COMPANION TO HISTORIOGRAPHY

EDITED BY

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ROUTLEDGE
London and New York
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Fifteen years ago, when I first considered mounting undergraduate courses in ‘historiography’, most students and not a few colleagues possessed barely more than a blurred notion of what the word meant. Secretaries pronounced it ‘historiography’. Professors, some of them major figures in their fields, advised that one could not teach the subject at all, either because it was too difficult and would ‘confuse’ the students, or, more radically, because the subject had no existence outside the particular historical problem one might be considering. It made sense, they said, to talk about the historiography of the French revolution or the historiography of feudalism; but there was no category, ‘historiography’, that one could discuss or teach. They were not aware, perhaps, that a sequence of ideas that had gained some currency in the 1970s and 1980s had already undermined these certainties in interesting ways.

A first subversion dwelt on the startling insight that students might understand more and develop faster if someone set about ‘confusing’ them with some serious thoughts about what they assumed the study of the past involved -- assumptions which were and are often painfully innocent of all forms of criticism and lacking context for the historical writing that students regularly encounter. The second took into account important changes in how ‘historiography’ had modified as an idea both in its nature and its status. It had lost its character as a form of sophisticated description and moved in the direction of an explanatory discussion that demanded answers to hard questions about why historians write what they do and why past historians did it differently. The need for a book like this one echoes these perceptions even when many of its contributors would not wish to be associated with some of the styles of thought reflected in contemporary approaches to history or social science.

In its older form, historiography (or, as it was usually called, ‘the history of historiography’) reminded readers that historical writing has fashions in its method and approaches and attempted to provide as comprehensive an account as
possible of the writings of previous historians. Some saw the task as a philosophical one and battled against the tightening grip of 'scientific' models – none more resolutely than the Italian Idealist thinker, Benedetto Croce.¹ But most writers went in the direction of 'empirical' study. This could be done with a Germanic commitment to systematic treatment, as in the pioneering account of Eduard Fueter at the turn of the century.² It could become an unconcealed mode of holier-than-thou liberalism in the hands of George Peabody Gooch.³ It could become a concealed mode of Christian apologetic, as it did for Britain's most self-conscious historiographer, Herbert Butterfield.⁴ It could turn into an annotated bibliography determined to list anyone who ever wrote anything: the besetting difficulty of a highly-scholarly compendium by the American historian Harry Elmer Barnes.⁵

Each of these examples had its own point of view and distinctive tone; but joining them all together (and linking them with many other studies from the same period) were a series of characteristics which no longer go unchallenged. For what they all assumed was that the task of the historiographer should be seen as biographical, expository and corrective. Their books took shape from the lives and writings of the 'great historians' they wanted to bring to the reader's attention. Typically, they presented arguments about a single individual who had, by acclamation, become part of the historical canon from Thucydides to Gibbon and then onwards into the congested list of major professional historians working in

¹ Noteworthy in Croce's astonishing output from this point of view are Storia della storiografia italiana nel secolo decimonono (2 vols, Bari, 1921); Filosofia e storiografia (Bari, 1949); La dialetta hegeliana e la storia della storiografia (Bari, 1952).
² Eduard Fueter, Geschichte der neuren Historiographie (Munich and Berlin, 1911).
³ G. P. Gooch, History and Historians in the Nineteenth Century (1913) 'Happy in the treasures of his monastic library, the pious chronicler did not stop to investigate their value ...' (p. 1). 'Gibbon constructed a bridge from the old world to the new which is still the highway of nations, and stands erect after every other structure of the time has fallen into ruins' (p. 7). His judgement of Michelet's French Revolution is a memorable paradigm of this sort of criticism:

'... though the book possesses unique merits [it is 'a contribution to knowledge as well as to interpretation'], his judgement of the Revolution is unacceptable ... [H]e is too tender to the masses, he is too harsh to the Church. He regards the Revolution as a struggle between two conceptions of life, the life of rationalist democracy against Christian monarchy ... The execution of the work is not less faulty than its general conception. Some events are described with infinite detail, others no less important are scarcely noticed. The book swarms with errors, and suffers from exaggeration and effervescence'

(pp. 183–4).

Nobody ever accused Gooch of effervescence.
⁴ Herbert Butterfield's oeuvre concentrated, most atypically for his generation, on historiography rather than substantive historical writing. As G. R. Elton commented with not untypical sweetness, Butterfield rarely troubled the editors of learned journals. See in particular The Whig Interpretation of History (1931); Christianity and History (1949); Man on his Past (1955); George III and the Historians (1957).
⁵ Harry Elmer Barnes, A History of Historical Writing (1937, revised edn, NY, 1962). The index to this formidable work of scholarship is over thirty pages long and contains between 1,500 and 2,000 names.
the shadow of the magisterial figure of Leopold von Ranke. Their investigations
gave rise to a text which abbreviated and epitomized what those historians had said
— reducing the sixty volumes of Ranke to a few pages of pith, expanding the terse
passages of Marx or Burckhardt to give the reader more idea of what the author
intended to convey. Once beyond this expository role, they then turned to
criticism; but the critique had a particular flavour. It showed where the authors
under review had in some sense gone wrong. The point of the exercise lay in
showing the ‘modern’ reader how historians of past ages, unblessed by the clinical
judgement of the critic’s own epoch and usually cursed by ‘bias’, had mangled the
‘truth’ through incompetence, wilful manipulation or — the ultimate patronage —
through no fault of their own. Fingers were wagged at religious zeal, ears tweaked
for thin or purblind research, heads patted for insight in advance of their time.
The historiographer gave his contemporaries some reassurance that history
advances towards truth by avoiding the mistakes committed by wanton predeces­sors. He (for there were no female historiographers of note in this period) told the
fellow members of a professionalizing discipline what they wanted to hear and
reinforced the claim of history to stand beside science as a means of appropriating
reliable and permanent knowledge.

Thus historiography became, like theology, the study of error. And the result of
that self-image has proved ruinous for the subject. If modern writers are correct
and past writers are wrong, what is the point of bothering with what Condorcet or
Theodore Mommsen or Bishop Stubbs happened to believe? They have, after all,
been ‘superseded’. If one does bother with them out of a respect for civilized
values and the importance of having an educated mind — every sound Englishman
should read Macaulay, every true Frenchman Michelet, and so on — then what
can be done with those texts beyond paraphrasing them or telling the young to
read them on the same grounds that one might tell them to read Shakespeare or
Molière? The difficulty has proved intractable for generations of teachers,
especially those who graduated in the West before about 1960. Approaching the
subject with this baggage, they presented courses in ‘historiography’ that
supposedly represented the leading edge of historical thought only to find that
students deemed them pointless and campaigned for their removal to make way for
something more ‘relevant’. Many readers of this Companion will remember
seminars in ‘historiography’ whose leaders had read the ‘text’ of the week while
lacking any clear notion of what to do with it. Indeed the sad and brutal facts are
these: historiography cannot be effectively taught or learned without a prior
interest in epistemology; and no one is likely to take the trouble with challenging
philosophical problems of that kind until he or she has come to appreciate that
history is a theoretical subject.

At some point between 1960 and 1975, in most countries of the West, history
took a turn towards theory. Why it did so raises difficult issues that we shall
examine later in this volume. The point to be pressed here is that the arrival of
theoretical models of one sort or another brought upon professional history an
embarrassing sense of self-consciousness. Historians quite deliberately made
models and drew graphs, spoke in -isms and -ologies, with a 'sixties contempt for those who did not. They began a journey (still continuing) away from telling the 'truth' about 'the' past towards a view that there are infinitely many sorts of past to talk about and towards a deep scepticism about the possibility of discerning the truth about most of them. For some, it has been a frustrating, even appalling transition and the period certainly provided more than one instance of chic, superficial fashion overriding patient research. Others, however, have found in it a new plausibility for the subject and a liberation for the individual attempting its study. Both points of view will appear in this book but, regardless of the point of view one may hold about the shifts in method and approach over the past thirty years, what seems clear is that a revolution in the understanding of historiography has proved one of its central consequences. Not only has historical writing itself received an irresistible impetus, that is to say, but the idea of historiography found itself swept along on the tide. The question, how and why did previous generations see their past(s) in ways different from those current now, became a serious issue for those trying to evaluate present perceptions rather than an optional visit to the National Portrait Gallery. The problem of how historical work ought to be done in a changed intellectual and political climate threw light backwards on how other generations and cultures had gone about it. The patronizing of past historians for having got the story wrong (or for having written stories at all) turned into a genuine curiosity about why their pictures and models look so strange to us and why they seemed persuasive to the particular audience for whom they had been intended. Historiography began to look like a first-rank subject in which, it was increasingly thought, serious historians needed encouragement and training— a training that would stimulate students to go beyond their immediate period or topic and examine the broader development of historical writing by relating it to various other forms of intellectual expression.

Acknowledging the challenge does not take one very far without a literature to which students and interested historians at all levels may be sent. And here there are profound problems. It is not that no literature exists: historiography is a subject whose bibliography has undergone enormous extension in the past two decades. But the material is frequently highly technical (especially in its more philosophical reaches) and its scope often turns out either too narrow to offer general guidance or so wide as to give the reader little more than a superficial impression. Perhaps this awkwardness lies so close to the heart of the subject that neither teachers nor books can change it. Whatever help secondary sources may provide, after all, it remains the case that the only way to understand historians is to read what they wrote; and any form of abbreviation or characterization will lose something. Many readers cannot spend weeks coping with a single author's historical works, on the other hand: they need urgent help in making sense of voluminous and elusive writers and of the schools and tendencies to which they may have contributed. Teachers no less urgently wish to have around them some basic texts to which they can direct pupils for initial orientation before writing that essay on Hitler or the coming project on the medieval family. For the general reader, intrigued by Marx as a historian or the Soviet
Revolution as a drama, some sense of context for such writers and events becomes the desideratum – a field-guide to the territory, as it were. These needs ought not to be beyond the capacities of a communicative and prolific profession. They have in some degree guided the modest ambitions of this Companion to Historiography.

Good books make good companions and the intention here has been to produce a volume that can be read rather than pulled from the shelf sporadically like an encyclopedia or compilation of historical ‘facts’. Indeed, this collection of essays is as far from a mere work of reference as we could make it. If anyone needs a column of print on Lord Acton or a column and a half on Ranke, he or she will find little difficulty in tracking it down in these days of historical ‘dictionaries’. Harder to find is an overview of developments within English or German historical practice within which the writing of these two writers might be located. So the starting point here has been that coverage of so vast an area as that bounded by historiography over the two and a half thousand years of its (Western) existence would prove less valuable (and less feasible in a single volume) than a series of studies whose analysis would remain relevant for subjects beyond the specific ones treated here. The emphasis would be placed, in other words, on approach and method as much as on content. By not including everything, conversely, it has become possible to insert important material about parts of the world – China, Japan, India, Africa – that frequently disappear in systematic treatments of European developments and which in no way replicate those experiences. Scaling down the space on the familiar canon of great historians has also allowed a greater concentration on changes since the Second World War. For young people trying to make sense of their own historiographical context, these years are the crucial ones because they made the perceptions of their parents and teachers and supplied the material against which many of the young stand in unconscious, or sometimes highly purposive, resistance. Balancing the competing claims of the general and the particular, of completeness and concentration, has proved no easier for this editor than it seemed to Ranke, who at least had talent on his side. But I have done my best within the parameters of the exercise and amid the often-acknowledged exasperations of a collaborative venture.

In the main body of the text we provide an understanding of development over time and a recognition of the spatial issues involved. The four sections dealing with ancient, medieval, early-modern and modern styles of historical writing operate in similar ways. Each has either an interpretative gloss on the essays that follow or, in the case of the later sections, a more extended introduction to historiographical changes within the period under discussion. In this way readers new to the period will find enough context to make intelligible the more detailed treatments of contemporary historiography reflected in the essays. Those essays offer a meditation by an expert on some aspect of the period’s more recent historiography – usually one that has attracted interest or debate in the last thirty years or so. It goes without saying that the choosing of issues to include has turned on a compromise between the general purposes of the volume and the particular interest of the author. Another editor and other authors could identify precisely the same purposes and yet
choose an entirely different range of topics to illustrate them. My own prejudice has been to concentrate on finding interesting and engaged authors rather than lurch towards the boring out of Calvinist relish. This procedure, compounded as any would have been by the defaulting of authors, leaves manifest gaps in the account but it also has enabled me to include some writing of extremely high significance and quality. Time having been satisfied, space receives its own treatment in a series of case-studies whose purpose is comparative as well as illustrative. No one should walk away from this volume thinking that the history of non-Western cultures is trivial or without its own special character. We then move on to think about more general contexts for the writing of history by situating the activity, first, against some other disciplines which have become especially important for how historians have seen their problems in the last few decades and, second, by examining some of the major new approaches to the subject that have often resulted from various forms of fertilization from beyond the acknowledged boundaries of the subject. The journey offered here is not the only one possible but I hope that readers with interests in any historical period or territory will find in it some of the stimulation I have received in watching it take shape.

The task of bringing forty essayists, each with a distinctive vision, topic and temperament, into a common framework would give pause to anyone familiar with the problems that beset the structuring of historical argument. To do so when the framework itself has frequently changed to accommodate a failure to commission a particular subject satisfactorily or, far more unsettling, to deal with someone else’s failure to produce an essay when contracted to do so, has given editor and contributors alike a lesson in patience and persistence. But the overwhelming sense from the editorial end is one of gratitude. Many of these contributors provided their essays longer ago than either of us wish to remember. Not a single one has complained about the delays and disappointments that, almost inevitably, dog the progress of a large-scale undertaking of this kind. I am immensely conscious of their tolerance and encouragement. Of those who came into the project very late in the day – often to fill the place of those who had promised to arrive by mid-afternoon – I cannot speak too highly for their professionalism and unselfishness. It is a pleasure to record help at early stages of the book’s preparation from Professor James Campbell and Dr Nicholas Purcell and throughout the enterprise from Dr Gerald Aylmer and Dr David Morgan whose expertise in, respectively, early-modern and Asian history has greatly enhanced the scope and content of what follows. I am also extremely grateful to Dr Paul Cartledge and Dr Julia Smith for supplying contextual glosses for the ancient and medieval sections and to a roll-call of friendly helpers at Routledge, most recently Samantha Parkinson, during the years of preparation. Finally but emphatically, I have to thank Jonathan Price. His was the original instinct out of which this volume arose and, during the period that I worked with him, he struck me as a figure rarely met with in academic publishing: a commissioning editor who is driven by the intellectual credentials of a project rather than its market-value or the demands of publishing as a streamlined, competitive branch of commerce. When others thought that this book might never happen,
Jonathan never wavered. When others lowered their sights, Jonathan always raised them again. When all around him were keeping their heads, Jonathan resolutely lost his — to the immense benefit of the book he inspired.

Michael Bentley
University of St Andrews
October 1996
I BEGINNINGS – EAST AND WEST
INTRODUCTION

Paul Cartledge

THE ANXIETY OF AMBIGUITY

History, the word, has a Greek etymology, being derived ultimately from a root meaning eyewitness, judgement and enquiry.¹ But only in our ‘natural history’ is that etymological connection at all closely maintained, and ‘history’ itself — whether the past, or the study of it, or of some of it — is and must remain a radically ambiguous term. Whence the coinage of ‘historiography’, struck in order to distinguish the study of and writing about some past facts from the facts themselves. But, since the distinction of facts from the writing about or of them is actually not at all clearcut — indeed is eminently contestable — a further meaning has been accorded to historiography, as meta-history or the study, from various standpoints, of the writing of history by others than the historiographer.² Both these senses are in play in Peter Heather’s chapter (5), on the historiographical invention of ‘Late Antiquity’ as a concept.

Ancient historians, however, that is historians of Antiquity, tend as a breed not to concern themselves primarily with such higher-order semantic or (still less) philosophical issues. For most of them most of the time, history is history is history, as it were, and at least until quite recently Antiquity has unproblematically meant Greece and Rome, that is the world or worlds of Greek- and Latin-speakers divided off chronologically from the more or less text-free universe of the archaeological prehistorians at the upper end, and at the lower end from the far less easily defined post-Roman ‘medieval’ universe: in round figures, from c.1000 BC (or — see below — BCE) to c. AD/CE 500. Introspection and doubts, however, are steadily creeping into even this fairly hermetically sealed scholarly domain. For example, the difficulty of a specification of ‘ancient’ that excludes China, source of the oldest

¹ References in Chapter 2, n. 3.
² Classic is White 1973. But see the rejoinder by Momigliano 1981b. See also below, n. 8.
BEGINNINGS - EAST AND WEST

historiographical tradition in the world, is manifest: hence in part the decision to print David Morgan's chapter (1) first below, as a small gesture towards historiographical rectification and recuperation. Another sign of altering perspectives is the increasing use by ancient historians of the non-Christian BCE/CE (Before/Common Era) chronographic notation, in belated recognition of the need to problematize and avoid a form of chauvinistic or ethnocentric cultural determinism and, conversely, of the desirability of fostering a non-teleological cultural pluralism.

However, respect for the primacy of texts, and texts written in at least two 'dead' languages requiring a lengthy period of linguistic and cultural immersion before they can be 'read' at even the most straightforward level, has tended to inhibit any Gadarene rush towards modernity, let alone postmodernity, of local or global interpretation in this field of historiography. Ancient historians have even been relatively slow to deploy systematically the 'ancillary' disciplines of archaeology, epigraphy and numismatics to eke out or contextualize their preferred literary sources, let alone indulge in the consensual interdisciplinarity and comparativism rightly desiderated below by Jairus Banaji (Chapter 6), across the whole range of thematic and material issues confronting them in their potentially highly heterogeneous area and period. As for theory - or Theory - that, despite (or because of) its Aristotelian pedigree, has typically always been at a discount among them, more so than among their literary Classicist colleagues.

Yet, as ever, there have been exceptions, and some of the exceptional ancient historians have made contributions within ancient history and historiography, the impact and import of which have been felt and acknowledged quite widely beyond conventional disciplinary boundaries (themselves in process of dissolution). Three such historians writing in English (but not only in English) deserve special mention here, all born - not coincidentally - within four years of each other (1908–12): the Piedmontese Italian-Jewish intellectual historian Arnaldo Momigliano (a refugee to England from fascist persecution, d. 1987); the concept-driven sociological historian Moses Finley (American-Jewish by birth, but British by adoption following McCarthyite witchhunting, d. 1986); and, still alive and writing, the Marxist Geoffrey de Ste. Croix (of Huguenot descent, born to Christian missionaries in Macao).³ Not for the first time one notices the impact of exile and transpatriation on ancient historiography, a trend set by Herodotus and Thucydides at its very outset. Only Momigliano, moreover, experienced fully the conventional, if dubiously beneficial, philological training of the typical classical historian.

NEW VARIETIES OF HISTORY: PROGRESS OR CHANGE?

To the ancient Greeks and Romans what was old, traditional, and ancestral was in principle good, what was new, the reverse: hence, Greek neoterismos

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('innovationism') or neotera pragmata ('newer transactions') meant revolution in an unwelcome negative sense, and precisely the same went for the Romans' res novae ('new things') (Finley 1986b). Of course, the Greeks and Romans did actually and inevitably innovate almost all the time, not least in historiography, which had after all to be invented and was so — for the Greeks — by Herodotus (fl. c. 450 BCE), and — for the Romans some two and a half centuries later — by Fabius Pictor (though he wrote in Greek, the then culturally fashionable literary language). But Thucydides' reception of Herodotus set the dominant tone and mode: one might legitimately hope to improve on the manner of one's predecessor or predecessors, but not usually his or their matter. The prescribed limit of wholly acceptable novelty was to find a subject not treated historically hitherto, namely the major event or events of one's own lifetime, and it was then sometimes — but not always — thought to be an advantage for the historian to have been an eventmaker rather than a mere observer. 'New' historiography in the sense of radically innovatory kinds of writing about the past was almost by definition considered bad or worse historiography.4

To us, in the sharpest possible contrast, new is cool, the newer is the better. That in itself is a revolution (in a positive sense). Since the 1960s wave upon wave of supposedly new histories have beaten against the supposedly hidebound bastions of traditional historiography — the traditions attacked having typically been invented in the later Victorian era of positivism and scientism, intellectually speaking, and of macho drum-and-trumpet activism, pragmatically speaking. There is, though, an element of caricatural rhetoric in the representation of both sides of this opposition: the post-Victorian traditions were not quite as one-dimensional as they have been painted, the new traditions (though that word was of course avoided like the plague) not quite as unprecedented as their proponents and exponents have liked to pretend. Yet there is also an element of truth, in the sense of correspondence to the facts, in the claims of the New. After about 1960 History did seriously and irrevocably begin to decompose into a plethora of smaller histories (social, economic, religious, intellectual, cultural, women's and so forth), narrative history of events (meaning typically large-scale public events of politics, diplomacy and war) did cede pole position to analytical accounts of deep structures and spatio-temporal conjunctures, and new -isms (especially feminism, comparativism and constructionism) have joined the older empiricist and Marxist tendencies. (Rabb and Rotberg 1982; Hunt 1989; Burke 1991; see also n. 5).

Indeed, the pace of change has been such that the fashion for social and economic history (e.g. history from the bottom up, attempting to restore a voice to the voiceless, whether they be earth-coloured rustics or other literally as well as metaphorically enslaved and silenced majorities) was quite quickly succeeded by a still apparent rage for cultural and intellectual history, and innovative modes such as

the women's history of the 1960s and 1970s have been fairly rapidly subsumed or superseded by even newer ones such as the gender/sexuality studies of the 1980s and 1990s (Scott 1988; Abelove et al. 1994). Worse still, from the point of view of conservatives, self-styled ‘progressive’ historiography, most noticeably in its postmodernist or New Historicism forms, not only has abandoned even the weakest versions of the nineteenth-century positivist claim that history was a science, no more and no less, but has even questioned the sacred notion of historical truth, in the name either of a rhetoric of discourse or of an ethical and/or cultural relativism.5

Of all this ferment the ancient world’s accredited historians were blissfully innocent and ignorant. After Herodotus, the father not only of history for the Greeks but also of what some moderns might approve as having a more than passing resemblance to Annales-style total history, political history - sometimes enlarged by consideration of social, economic, intellectual and cultural factors, but more usually not - ruled the ancient roost more or less unchallenged. A moral point of view was not to be hidden behind the mask of faceless objectivity but rather, at least in the Roman case, proclaimed as the historian’s ultimate task. Rhetoric, so far from being shunned as a shameful distortion or disguise, was praised and pursued as the necessary adornment of an essentially literary genre. Above all, history was regularly touted as useful, not merely diverting, and centrally important, a proper study for the ancient world’s movers and shakers to whom it was mostly directed.6

THE NEW ANCIENT HISTORY AND THE OLD

Modern historians of antiquity find themselves for the most part in a very different situation, addressing small, socially unrepresentative readerships whose hands are far removed from the levers of power. Classics, once thought just the thing for aspiring nineteenth-century imperial administrators, now must not only run the gauntlet of non-academic debunkers without but also face the friendly fire of canon-debunkers within. Orientalists in the old and supposedly harmless descriptive sense now find themselves accused of orientalism in its new, unambiguously charmless signification. In response, some defenders of the old faith rather petulantly ask why we should study the Greeks and the Romans in particular if not as our admirable and imitable cultural ancestors and models. But others more open-mindedly and more boldly take the attacks on the chin, investigating historiographically how and why it was that such phrases as the Glory that Was Greece and the Grandeur that was Rome could ever have attained such general currency. The relativists and anthropologizers among them, moreover, rather than merely knocking the Greeks and Romans off their factitious paradigmatic pinnacle, seek to account for their undoubtedly inspired as well as inspirational cultural achievements through the

medium of a more inclusive historiography than the ancients themselves could have
countenanced.\footnote{A subtextual reading of Herodotus, Thucydides and Xenophon is offered in Cartledge 1993.}

It is precisely in this context that the ancient historians themselves have come
under renewed critical scrutiny. Fathers of history and truth – or Fathers of lies,
fiction, and rhetoric? Such has been the emphasis on what for want of a better word
might be called the ancient historians' inventiveness that one leading student of
Greece has recently felt obliged to mount a 'defence of the Greek historians'; it is a
fair index of the nature and success of the attack that the burden of his defence is that
they 'were not so unlike modern historians that we cannot read them as historians at

It would be harder, as Michael Comber's chapter (3) shows, though not perhaps
quite impossible, to construct such a defence of the Roman historians, since they
were quite up front about the desirability, indeed necessity, for authorial pleading.
Not even the rhetoric of impersonal objectivity (cf. Novick 1988) was deployed.
Tacitus's famous prefatory \textit{sine ira et studio} ('without anger and partisanship'), for
example, had the strictly limited self-referential meaning that he himself personally
had no cause to feel resentment or take sides in the subject-matter he had chosen to
narrate – not that he did not feel indignation about the past or attempt vigorously,
and often most subtly, to sway the reader towards one interpretation of it rather than
another.

But if the manner of the ancient historians is controversial, their relative
narrowness of scope is not. The sorts of agenda that a historian of antiquity
influenced by recent turns in historiography more generally might wish to espouse
simply could not begin to be addressed, if the ancient historians were all we had left
to go on.\footnote{Compare and contrast Davies 1975 (recommending thirteen themes, area studies and approaches,
one of which could be studied or taught chiefly let alone exclusively from the ancient literary
historians) with the rejoinder by Brunt 1976.}

Consider, for example, religious history, the subject of Chapter 4. The
ancients differed considerably in the amount of interest they took in and importance
they allowed to religious phenomena, Thucydides occupying one end of the
spectrum, that of almost complete denial, Livy the other. But not even Livy gives
us nearly enough to begin to write a decent history of, say, religious change in the
Late Republic and early Empire, roughly the last couple of centuries BCE and the
first two CE. For that we must turn to archaeology, epigraphy and non-historical
literary sources, complemented or informed by liberal applications of modern
theory derived from comparative sociology and social anthropology.

\textbf{THE BLACK ATHENA DEBATE: IS EAST EAST AND WEST WEST?}

The liveliest current debate within ancient historiography, which is also the most
embroiled, and – worryingly – the one with the most potential practical relevance,
concerns Martin Bernal's 'Black Athena' project. Bernal is a professor of Chinese government studies and not a specialist historian, let alone a historian of classical antiquity. He has nevertheless devoted two major tomes (so far) and a number of ancillary studies to charging the dominant tendency within ancient Greek historiography since the mid-eighteenth century (Roman history and historiography are largely spared) with what he regards as a lethal combination of anti-black racism and anti-semitism. As a result, credit has not been given where the credit is in Bernal's opinion due for the original creation of the Western tradition of culture and civilization: namely, in the first place to Africa, and more particularly to Egypt (the relevant Egyptians were for him in some useful sense 'black', if not necessarily negroid), and secondarily to Phoenicia (the Phoenicians were a Semitic people). And this despite the classical Greeks' own willingness to acknowledge that it was from those countries and peoples that they had taken over many of their most basic cultural tools and ideas lock, stock and barrel.

The contemporary political immediacy of this debate stems from the circumstance that Bernal's general thesis, both as an exercise in intellectual-social historiography and as a positive reconstruction of how it supposedly was, has been adopted as ammunition, in very simplified and often distorted forms, by promoters of various versions of Afrocentrism on and off American campuses. (Jews have been noticeably less keen to embrace Bernal's supposed recuperation of Phoenician influence on Greece.) Scholars, including prominent classicists, have taken reasoned and reasonable issue with Bernal, on points of both method and fact. One of the most prominent, Mary Lefkowitz, has gone to the lengths of publishing a book defiantly entitled *Not Out of Africa* directed not only or primarily at Bernal but at the more detectably outrageous variants of a recognizably Bernalian line on the ultimate ancestry of Western civilization (Lefkowitz 1995; Lefkowitz and Rogers 1996; cf. Levine 1989).

As in the recent German *Historikerstreit* over the responsibility of Germany or Germans for Nazism and more especially the Holocaust, we have here another sadly spectacular illustration of Benedetto Croce's dictum that all histor(iograph)y is present histor(iograph)y. Inevitably, as Julia Smith has put it below, 'in one way or another, all history is always ideological, relative and zeitbedingt'. Sometimes, indeed, as I have myself attempted to show here, conscious or subconscious definition of the group addressed in ideological terms can be an important or even the major goal of historiography. Such public and heated debates as that concerning 'Black Athena' are not perhaps entirely deleterious to the historical profession, in so far as they may foster a necessary self-reflexivity and send us back chastened to re-examine our intellectual and methodological roots and conceptual presuppositions. But they do little, on the other hand, to enhance History's positive reception and perception by wider society, and they do nothing to support claims that 'progress' in historiography is a measurably attainable goal.

11 Note the inverted commas of Finley 1977, of which the first chapter of Finley 1985 is a slightly revised extract.
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Historians of China and Japan (London, 1961). The series also includes C. H. Philips (ed.), Historians of India, Pakistan and Ceylon (London, 1961) and D. G. E. Hall (ed.), Historians of South East Asia (London, 1961). Much of the material in these volumes is now inevitably out of date, but nothing has since been attempted on a remotely comparable scale. A much shorter but slightly less out-of-date survey is D. Sinor (ed.), Orientalism and History, 2nd edn (Bloomington and London, 1970). This contains essays on Islam by B. Lewis and on China by E. G. Pulleyblank. It should be noted that the title of this book dates from before the time when the word 'Orientalism' regrettably acquired a pejorative sense. Useful short articles on both Islamic and Chinese historiography, by R. G. Irwin and T. H. Barrett respectively, may be found in J. Cannon, R. H. C. Davis, W. Doyle and J. P. Greene (eds), The Blackwell Dictionary of Historians (Oxford, 1988). Individual Chinese historians are conspicuous by their total absence from that work of reference; the Islamic world fares very slightly better, being represented by Tabari (G. R. Hawting), Ibn Khaldun and Rashid al-Din (D. O. Morgan). On Islamic historiography, a well-established work of reference is F. Rosenthal, A History of Muslim Historiography (Leiden, 1952), but though massive this is much 21
more limited in its scope than its title suggests. There is valuable material in C.


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56 Leader in the Guardian, 29 January 1993, on the rumours surrounding John Major.

57 Cf. also Schama 1991.

58 Cf. Sallust, BC 3.2: 'acta dictis exaequanda sunt' (the words must be equal to the deeds). Also, Gorgias, peri phuseos or peri tou me ontos, 3.84 DK.

59 Now amply confirmed by the S C de Cn. Pisone PatTe. The amount of research Tacitus has done should not be underestimated, cf., e.g. Ann. iv. 11.4, iv. 53.3; Pliny, Ep. vi. 16,20.

60 Cf. Woodman 5. See also, Lukacs 1962. All ancient historians, of course, make their characters speak. It was, however, their practice to exclude verbatim speeches from their works, with the exception of brief quotations. All longer speeches, therefore, are composed by the authors themselves.

This holds good even when the actual speech was publicly available, as was the case with Claudius's speech to the Senate in 48, substantial parts of which still survive on an inscription (ILS 212). Tacitus gives his version at Ann. xi. 24, and here, at least, he appears to have admirably reproduced the gist, the xumpasa gnome, as Thucydides would have it (i. 22.1), of what Claudius really said. Our problem is that
we have no idea which speeches follow this pattern, and to what extent.


I am grateful to Dr C. B. R. Pelling for allowing me to read his lecture-notes on the Roman historians.

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35 The interaction between these sets of motives is nicely shown by Riley 1984.

36 C] XI.52.1 (c.393), where Thracian landlords are legally empowered to control rural labourers 'et patroni sollicitudine et domini potestate'.

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18 Revisionism in Britain


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IV IV.1 Revolution and Ideology


Hunt, L. (1984) Culture and Class in the French Revolution,
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It is a pity that Steven L. Kaplan (1993) reduced the significance of the bicentenary to the anecdotal level by focusing on accounts of personal rivalries among French historians. Inspired on the whole by
the same concern to arbitrate in favour of the 'Jacobin' tradition, the recent synthesis by Jones 1996,
besides curious omissions (the influence of Jaures, the importance of religious problems), claims, in a
debatable manner, that a new orthodoxy, typically 'revisionist', would predominate today in the
historiography of the French Revolution. On the contrary, that recent work of revision has replaced a
dominant and justifying model by a plurality of interpretations more respectful of the variety of the
realities. 524

Sutherland, D. M. G. (1985) France, 1789-1815: Revolution and Counter-Revolution,
London.
The Ecclesiastical Oath of 1791, Princeton.
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There is no historical consensus about the Russian Revolution of 1917. Few events in modern history provide such a vivid picture of the relationship between contemporary political attitudes and the range of historical interpretation. Russia's history which is stormy enough is not the only issue at stake. Debates about socialism, economic development and the potential for an alternative to Western capitalism are all involved. The fate of the Soviet Union itself has influenced the historical perspective; its collapse in 1991 exacerbated a crisis in scholarship which had already been perceptible a decade earlier. This is a field whose development in the next ten years is likely to be controversial, unexpected and vigorous. In the following pages we shall examine the history of the debate and the most important
points of controversy before discussing its prospects in
the post-Soviet world.

The debate began while the February slush was still wet on
the Petrograd
demonstrators' boots (see e.g. Miliukov 1967 and Sukhanov
1955). Winners and
losers of all kinds had their stories, as did observers of
all nationalities and political
persuasions. What they were all clear about was that the
Russian Revolution would
have implications for the rest of Europe, and possibly
also for the future of the
developed world. I In this assumption they were correct. The Russian Revolution
did more than overturn one of the longest-established
autocratic regimes in Europe.

Russia became the country in which the nineteenth-century
positivist hope that
society could be perfected by enlightened human effort was
put to the test.

Eventually, too, it was to witness an attempt to accelerate
economic 'modernization', the introduction of large-scale mechanized
industry, without the human
costs witnessed in other European countries.

These utopian pretensions have attracted fierce
controversy. They have

'It has fallen to the Russian proletariat to have the great
honour of beginning a series of revolutions',
declared Lenin in February (P/noe sobranie sochinenie (=PSS), vol. xxxi, p. 9). An opponent, Yu. V.

Got'ev, regretted in July that 'we are only fit to be manure
for the peoples of higher culture' (1988: 28). 526
influenced assessments of the entire history of the USSR. The world-wide reaction to the fall of the Soviet communist regime indicates how closely linked its reputation has always been to that of the socialist ideal. For some commentators, the very idea of an explicit ideology informing politics has always been unacceptable; they condemn the Revolution outright for its flawed ambition. But other assessments have been influenced by the subsequent history of the Soviet Union. These do not dismiss the whole project out of hand, but trace a direct line through 1917 to the evils of Stalinism and to Brezhnevite stagnation, attributing the continuities to a range of strategic political errors or to pathological flaws in Russian political culture.

The result of this outlook has been a series of changing fashions in historical appraisal. The first reaction of most developed countries after 1917 was to reject the Bolshevik regime out of hand. Hostility and suspicion remained the dominant responses throughout the 1920s. But by the 1930s, when most Western economies were in deep recession, it was also possible to admire the Soviet Union for its rapid economic growth and pretensions to full employment and a just social system.

Details of the privations involved in the collectivization of agriculture appeared only
slowly, as did the reality behind the much-publicized show trials of 1936-8; all contributed to an unfavourable picture of the regime. The war hushed this criticism for a time. By 1942, when Russian troops were bearing the brunt of the Nazi military effort, British and American publications began to praise Stalin and his patriotic supporters in a language unthinkable five years before. The Cold War would change all this within three years, but even at the time of Stalin’s death the man’s reputation as a war hero and socialist pioneer exercised as strong a hold on many people’s imaginations as did the allegations of his brutality. Depending on one’s position in these controversies, the Russian Revolution could be seen as a dramatic act of popular liberation or the first in a series of disastrous steps to repression and social regimentation.

The horrors of collectivization and famine and the appalling excesses of Stalin’s purges remain a stumbling-block for any historian writing about the revolutionary period. Conservative historians are correct to point out how rapidly they followed the Revolution, and how many continuities exist between Leninism and its murderous successor. No one seriously disputes that Stalin’s was a brutal polity, its values distorted by suspicion and violence. The difficulty has been to transcend this
judgement in order to arrive at a better understanding of the processes involved.

Studies which fail to dwell on the horrors of Stalinism are too easily accused of

2 A recent example is provided by Pipes 1990: 132-3. For a Russian statement to the same effect, see Shcherbakh on Russian political culture (1987).

3 This is a theme pursued in the second volume of Robert Tucker's biography, Stalin in Power. The Revolution from Above, 1928-1941 (1990).

4 As Lenin predicted, 'terrible clashes between the Soviet republic and the bourgeois states are inevitable', PSS, vol. xxiv, p. 122.

5 The most famous example is Webb and Webb 1935. 527 apologizing for it. The debate about the appropriate weight to be given to the 'moral' questions of Soviet history, which racked American historians of the early Soviet period in the 1970s and 1980s, has yet to be resolved. 6 Some of the most exciting new research complements the question of high politics and political manipulation by exploring political and social history 'from below'. As we shall see, historians of this school have raised vital questions about the origins and course of the Revolution, as well as developing a comparative sense of labour and revolution any history in the twentieth century.7 This school's ethos has rightly been described as 'libertarian' rather than pro-Leninist (see Acton 1991). But while the Cold War
critics of Soviet communism remain locked in their belief that those who do not condemn the regime must be apologists for it,8 the subtlety of the distinction will escape the average textbook reader. Indeed, the most widely marketed of the recent crop of textbooks on 1917 deliberately ignores the whole school, thereby depriving its readers of the fruits of the best recent research (Pipes 1990).

For historians outside the former Soviet Union, ideological considerations like these have complicated the study of 1917 and its aftermath. But until recently it was inside the Soviet Union that the problems were most severe. Official censorship and control of research funds compounded the difficulty shared by Western scholars of access to archival records. There was almost no contact between Soviet and Western specialists. From the late 1920s to the 1960s, little scholarly work on the modern period was possible in the USSR. The scope for original thinking was so narrow that serious historians often preferred to study medieval or ancient history, on which the ideological restrictions were more lax. A brief 'thaw' in the late 1950s and early 1960s saw the preparation of a number of more serious pieces of work, but the gradual resumption of censorship after 1964 meant that much of this material remained unpublished. With few exceptions, history-writing
remained shackled to official formulae until the late 1980s.

The eclipse of the Communist Party changed all this in two or three years. Since 1988, it has become possible for historians in Russia to question virtually every aspect of their revolutionary past, including Lenin's role and the viability of the tsarist system. Attention initially focused on the alternatives to Stalinism in the 1920s and 1930s. The Communist leadership encouraged historians to look again at such figures as Bukharin, Rykov and even Trotsky. A major national effort went

6 See, for example, the debate in Russian Review, 1986, vol. 45.

7 As Diane Keonker points out, this applies above all to social historians of the family and of urbanization; Keonker et al. 1989: 54.

8 An outrageous example of this kind of criticism appeared in the Times Literary Supplement's commemoration of the seventy-fifth anniversary of the October Revolution. In the lead article, Richard Pipes wrote that 'revisionist' academic monographs, 'as unreadable as they were irrelevant to the understanding of the subject', were the fruit of a deliberate policy of academic manipulation by the Communist Party of the Soviet Union, the object of which was 'to have western historians accept the cardinal principle of Communist dogma, namely the legitimacy and inevitability of the October Revolution and the regime that issued from it.' TLS, 6 November 1992, p. 3.
One of the finest accounts of this 'mental revolution' is given by Davies 1989. 528 into commemorating the victims of the purges. 10 Scholarship was still used as a political tool. It was vital for the future of perestroika that the so called 'blank pages' of Russian history be filled in such a way as to rescue Soviet socialism from the stigma of its past. 1 At the same time, history enjoyed a period of genuine public prominence. For a year or so, controversies about the foundations and history of the Soviet system appeared on the front pages of popular newspapers and featured in television chat shows.

The fall of communism changed the whole picture. It provoked a new interest in pre-revolutionary culture and history, a fascination with the Romanov dynasty and with nobility, the Orthodox Church and traditional customs. 12 At the popular level, much of this was escapism; economic and social conditions in Russia were growing steadily worse, and in most national republics the search for a non-Soviet identity inevitably evoked a pre-industrial, 'traditional' culture. 3 Some of it was also an understandable reaction to the monotony of the recent past. Romantic images of tsarist Russia were accompanied by a rejection of the Soviet period as a failed experiment. But as the country’s political and economic difficulties worsened, the
widespread popularity of history gave way to a 
preoccupation with survival. Serious

historical research alone has benefited from the change. Censorship is not a problem

at the moment. It is low wages, high production costs and the paper shortage which

now hold back the publication of new work in Russia.

The end of the communist era in the USSR and Eastern Europe must encourage

a reappraisal of 1917 among scholars of all persuasions. But for now, the most lively

historical controversies in East and West focus on one cluster of questions. These

have a marked teleological emphasis. Explicitly or not, and whether their emphasis

is on high politics or history 'from below', they concentrate on the overall picture of

Russian 'modernization' and on the implications of the Revolution for what

followed it. Was 1917 'necessary'? Could the tsarist regime have reformed itself

from within, and could Russian economic development have brought sufficient

prosperity to avert disaster if the war had not intervened? And if revolution could

not have been avoided, might not some more humane outcome have been possible

than the establishment of a one-party state, the repression of minorities and the

mass murder of millions of peasants?

The question of the 'necessity' of 1917 is fundamental to all these debates. The
counter-argument rests on the idea that tsarist Russia was showing signs of
reforming itself successfully from within from the late nineteenth century onwards.

10 The Union-wide organization, Memorial, attracted large numbers of supporters in 1989-90,
many of whom participated in its campaign to set up memorials to the victims and perpetrators of
Stalin's purges and to establish a photographic and memoir record of them.

II The delicate position of historians expected to defend the Communist Party while condemning
Stalinism is illustrated by the contributions to the Round Table on Soviet history, Voprosy istorii
KPSS, 1989, no. 2.

12 For an article praising the pre-revolutionary value of charity, see Granin 1987.

13 The exception is the Baltic republics, which were absorbed into the Soviet empire after the Second
World War. 529

About the reforms themselves there is no doubt. Prompted by defeat in the Crimean
War in 1856 and by Russia's perceived backwardness compared with Britain, France
and, later, Germany, successive tsarist ministers attempted to modernize the
Russian economy, beginning with the emancipation of the serfs in 1861. 14

Constitutional reform, which had accompanied economic growth in western
Europe, followed much more slowly. It took the crisis of the 1905 revolution to
force the Tsar to accept change in the form of the October
Manifesto (see Harcave 1965 and Hosking 1973). Although the concessions this granted were limited (and many of them were subsequently revoked), the argument runs that a crucial step had been taken. The autocracy was finally compromised, and if circumstances had permitted a peaceful evolution, some form of just constitutional monarchy would eventually have emerged.

No one (except perhaps extreme monarchist politicians in Russia today) has seriously argued that the Tsar himself was an intelligent or capable head of state.

The historians who hold that the tsarist regime had the potential to reform itself from within point to 'tendencies' which might have blossomed in 'normal' conditions. Whatever the limitations of the Tsar, several of his ministers displayed dedication and energy, often in the face of hostility from their monarch and colleagues. Sergei Witte in the late nineteenth century and Pyotr Stolypin in the early twentieth both introduced significant reforms aimed at encouraging economic growth and social stability. Stolypin in particular has emerged from the recent literature as an enlightened statesman whose reform programme, including a scheme for the gradual creation of a yeoman peasantry in place of the post emancipation commune, puts him in a direct line with
current thinking about the
need for privatization in the countryside. His efforts
appeared to bear fruit; the
Russian economy enjoyed a period of rapid growth between
1909 and 1914. 16 Some
of this growth was financed by Russian, as opposed to
foreign, capital. In some
sectors this had largely replaced the foreign investment
which had driven earlier
boom periods. 17 The growth of domestic funding was a
positive economic
development, while in social and political terms it
signalled the emergence, at last,
of a native Russian bourgeoisie, the potential backbone for
a constitutional
monarchy. It is possible to conclude from this information
that a stable social and
political structure was snatched from the Russian people
only by their involvement
in the war.

The problems with this line of argument are obvious.
However successful
Russian industry may have become by 1914, major problems,
amounting to a
14 Excellent essays on the emancipation of the serfs,
together with an overview by the editor, are
given in Emmons 1970.

15 The best biography of Witte remains von Laue 1963. A
recent reappraisal of Stolypin, entirely
laudatory, appears in Pipes 1990: 166-94. See also Voprosy
istyoni, 1988, no. 3.

16 Figures are analysed in Gregory 1982: 56-7.
The most optimistic assessment is that of Gerschenkron 1965a. More recently, see McKay 1970: 28. 530

potential political crisis, were all too apparent. Historians disagree about the rate of growth of the entrepreneurial class in tsarist Russia. Most new industry was still financed by foreign capital. And the bourgeoisie which had established itself may have had little reason to support the regime by 1914, denied as it was from participating in a democratic process, hindered as often as it was aided by tsarist regulations. Outside the narrow ranks of the Petersburg elite, peasant discontent and working-class radicalism threatened to boil over into rebellion at any time. 19

The peasants' problems do not seem to have been resolved by the Stolypin reforms. The take-up for his private farms varied from region to region, but in general, fewer than 20 per cent of peasant households had left the commune by 1914. Of these, less than half had succeeded in consolidating their holdings. The rest continued to farm scattered strips of land (Pipes 1990: 175-6; figures from Dubrovskii 1963). Partly for this reason, there was little sign that Russian agriculture was transforming itself along the desired west European lines. While overall agricultural output was rising in the decade before 1914, grain production
far outstripped that of livestock, farming methods remained
primitive and chronic
rural over-population threatened peasants’ livelihoods
(Wheatcroft 1990). The
boom of 1909-14 should be attributed primarily to a
coincident world-wide
economic surge rather than to domestic Russian policies.
In the cities, rapid and uneven economic development had
created new
industries, new factories and a greatly expanded workforce
without the infrastruc
ture to support them. The literature on the working class
in this period is
voluminous; historians even disagree about the
applicability of the term 'class' to so
heterogeneous a social group (for a discussion, see Haimson
1988). Some were
'hereditary' workers from families which had lived in the
city for more than one
generation. Some were highly skilled. But others were
rural migrants, not all of
whom lived permanently in the city. Many workers of all
types retained ties with
the village, either holding land there or returning
seasonally to help with the
harvest. 21 Whatever word is used to describe this
workforce, however, all historians
agree that its living and working conditions were among the
worst in Europe. By
1905 there were many people in Russia who were not content
to let this situation
continue unchallenged. The last decade of tsarism was
marked by frequent and
coherent expressions of worker protest. The regime's response, which was usually to use troops, was inflammatory and brutish. 22 Sooner or later, it can be argued, some kind of confrontation on a large scale was almost inevitable. 23

18 This less optimistic view of the period may be found in Gattrell 1986.

19 The seminal article on this subject was Haimson 1964-5. On peasant unrest, see Atkinson 1983.

20 Again, this is the subject of controversy, with historians discussing 'tendencies' rather than achieved results. For a more 'optimistic' appraisal of the Stolypin reform, see Gregory 1982.

21 A description of the workforce in the textile industry in the early 1920s appears in Ward 1990.

22 The most infamous use of troops, likened in impact to 'Bloody Sunday' in 1905, was the Lena goldfields massacre of 1912.

23 The classic presentation of this view is Pokrovsky 1933.

The nature and mainsprings of this worker protest have been the subject of intensive historical debate. The traditional Soviet view, crudely based on Lenin, was that Russia's urban workers were 'conscious' or 'unconscious', depending on their willingness to accept a Bolshevik political programme. 'Consciousness' came with a reading of the Marxist classics, an appreciation that revolution was the only solution and a willingness to make sacrifices to achieve it. The professional
revolutionaries' role was to encourage this consciousness and prevent compromises from being struck which might defer the ultimate achievement of the Revolution.

The 'vanguard party of a new type' plays a central role in all traditional Soviet accounts. Without it, the argument runs, the elemental forces of proletarian discontent would have remained incapable of concerted political action. 24

Paradoxically, this view was accepted in broad outline by the Soviet Union's bitterest critics, many of whom presented the Russian Revolution as an exercise in Jacobin putschism. 25 It is only in the last thirty years or so that the research of social historians has challenged this position seriously. Studies of the Russian factory confirm that conscious working-class radicalism was a prominent feature of late tsarist society.26 Historians disagree about the extent of its political purposiveness.

Some have argued that the revolutionary parties, and above all the Bolsheviks, played a negligible role in the growth of working-class activism. 27 But the idea that the whole movement was 'spontaneous' or undirected is unconvincing. Scholars who have studied workers' demands and the politics of the strike movement in detail reveal that it was far more complicated than schematic Soviet accounts allowed (see, in particular, Keonker and Rosenberg 1989). But they point to a
number of collective goals, such as a consensus about certain issues of social justice.

They also challenge the 'revolution from above' model by pointing out that skilled workers, and above all those in the metal industry, were becoming increasingly radical by 1914, and that their demands were no longer limited to 'economic' goals, such as better wages or working conditions, but extended to a radical political programme. 28

All this suggests that the tsarist system was in difficulties before 1914, and that the war may even have deflected criticism away from the regime for a while at the outset. But the war was not a deus ex machina in Russian affairs. 29 From the

24 For a relatively recent re-statement of this line, see Velikii oktyabr' 2: 1987.

25 This view was restated in TLS, 6 November 1992, articles by Richard Pipes and Leszek Kolokowski.

26 To list every contribution to this debate would be impossible in this short review. However, in addition to Leopold Haimson's seminal articles of 1963-4, attention should be drawn to Bonnell 1984; Engelstein 1982; Glickman 1984; and Johnson 1979.

27 This is the thread running through R. McKean's massive St. Petersburg between the Revolutions (1990).

28 Russian historians (and indeed Lenin himself) would agree about skilled workers' consciousness.
The difference is the weight they would give to the role of the party. For two accounts, one Soviet and one western, see Volobuev 1964 and Haimson 1964-5.

29 The contrary view is suggested by Gerschenkron 1965b: 141. 532

nineteenth century onwards (and certainly throughout the Brezhnev years),

Russia's commitment to great-power status influenced its diplomatic policy and its economic development. In neither case was the country's domestic political life made more secure. It was tsarism's great-power pretensions which involved the country in the Russo-Japanese War of 1904-5, defeat in which provided the spark for the 1905 Revolution. The same pretensions encouraged a concentration on military-related industries (such as arms and machinery) where wider diversity might have provided a more secure base for industrial growth and social stability.

Russia's involvement in the First World War was not accidental; the crisis it provoked was a crisis of the tsarist system as a whole. 30 The February Revolution brought down the autocracy and opened the way for an alternative style of government as well as alternative policies. The brief interlude of the Provisional Government left historians with a problem: was there ever a moment when democracy might have triumphed in Russia, and if not, why? Clearly
this is a question invested with considerable wishful thinking. Especially at the time, liberal historians expressed frustration that first the Provisional Government and then the freely elected Constituent Assembly (which the Bolsheviks dissolved in January 1918) failed to take hold in Russia (Ulam 1981). Underlying their remarks was a belief that democratic institutions might in some way have been the 'normal' development for Russia if the war and revolutionary crisis had not blocked their path. Latterly this view has been expressed by Russian historians too; the Kerensky regime is enjoying a new popularity in a country eager to find alternatives to chaos or dictatorship (for a discussion, see Startsev 1990).

The most pessimistic riposte has been to suggest that Russia was somehow destined to be ruled by autocrats. This explanation comes in several degrees of determinism. At its mildest, it consists of an assumption that countries without democratic traditions have difficulty in establishing elected institutions, and that the moment for doing so in Russia was also one of the most troubled in her history (Ulam 1981: 69-70). But stronger versions also exist, in many cases attributing to Russians a desire to be ruled by despots and an inability to resist 'Tsar' figures.

Such explanations are often couched in racist terms; Russians are seen as 'feeling
uncomfortable' if they are offered 'too much freedom' (Laqueur 1989: 8).

Adherents of this school often make connections with the distant past which are tenuous, speculative and unproven. But the explanation has considerable appeal, for it maintains a sense of Russia's exoticism and excuses current and recent generations for seventy years of Bolshevism.

Seductive though these overarching explanations of Russian authoritarianism may be, reality, as always, was more subtle. The Provisional Government's failure has to be explained in terms of Russia's economic and military collapse, and of its own devotion to a system of property ownership and parliamentary elitism which held no charm for the bulk of the population. By 1917 it was too late to ask the Russian peasant to wait for land legislation. It was also no time for a government to array itself behind the small industrial class against the underfed, insecure and frustrated workforce. By August, the fear had become widespread that the prime minister, Alexander Kerensky, intended to institute some form of dictatorship, and his involvement with Kornilov's attempted coup appeared to prove the point. It was
not the lack of democratic tradition which brought down the
Provisional Government

ment (and how any government can appeal to tradition in a
revolutionary situation

is a moot point) but a combination of external disasters,
short-sightedness,
mismanagement and a growing impatience among soldiers and
sailors as well as
civilians with the continuing war. All these difficulties,
moreover, were symptoms

of a greater structural crisis. As early as February 1917,
the Provisional Government

had been only one of two potential loci of power in
Russia. 'Dual power', the other
element of which was the Petrograd Soviet, deprived it of
the ability to build

legitimacy. By October, it was the Soviet, and not
Kerensky's, government which

commanded mass, multiparty support (see Ferro 1980).

The Provisional Government's collapse, therefore, hardly
came as a surprise. The

Executive Committee of the Petrograd Soviet had been a
credible alternative for

several months. But was the October 'revolution' really a
coup d'état? This view, a

staple of a certain type of Western historiography for
many years, would have been

unthinkable for a Soviet historian of the old school. But
now it is openly expressed

in Russia with more conviction, if anything, than it has
been recently in the

West. 32 The argument rests on three sets of assumptions.
The first that the
Bolsheviks created the revolutionary situation from above we have already examined. The second, to which it is related, is that Bolshevism was an alien ideology smuggled into Russia by a tiny group of revolutionaries, an ideology, moreover, which commanded very little grass-roots support. And the third is that Lenin's party (or rather, a fraction of its leadership) pre-empted a democratic coalition of the popular socialist parties from assuming power on behalf of the Petrograd Soviet.

The idea that Leninism was an alien system is especially popular among the new Slavophiles in Russia today. Marxism can be made to appear technocratic, industry obsessed, out of keeping with the Russian peasant soul. But it is true that it had little to offer to peasants determined to remain on the land, wedded, as Marx would put it, to 'the idiocy of rural life'. It also, in practice, had little appeal for women, even factory workers, who generally felt excluded and overawed by the atmosphere generated at party meetings (Glickman 1984). But Marxism undoubtedly attracted thousands of Russians from the late nineteenth century onwards. In the variant

32 As Alan Wood explained in the introduction to the special anniversary edition of the European History Quarterly, vol. 22, October 1992, 'The Bolsheviks, the Baby and the Bathwater'.
Marxism was a German product, of course, and Lenin, by the same token, may have been the recipient of 'German gold' in 1917. This is a point which continues to fascinate his detractors. For a recent example, see Argumenty i /akty, August 1992, nos 29-30, p. 5. 534
debated and refined by dozens of exiled revolutionaries in the cafes of Vienna, Paris and Geneva, it fitted admirably with pre-existing millenarian ideas, with a desire to see wholesale changes to a regime which no longer made any kind of sense, with a belief that Russia, which Lenin came to see as the 'weakest link' in the world imperialist chain, was special, that its sufferings would eventually make it the beacon of world socialism. 34 For Russia's workers, discussing their collective future in very different circumstances, Marxism made sense of a bewildering present while offering a vision of the future built on the sacrifices they were making and the lessons they had learned in city and factory. It did not seek to escape from industrialization. Rather, like the science fiction which workers were so avid to read (Stites 1989, esp. ch. 1), it offered a cleaner, more just version of capitalism.

Latterly, as the Russian workers' world has begun to come to life for historians, a good deal has been written about the relationship between the emigre revolutionaries' vision of Marxism and that of the workers back in Russia. One school shows
them as largely separate; its advocates point to the 'unreality' of much emigre

thinking (including Lenin's). And there is no doubt that revolutionaries who

remained active in Russia, such as Tomskii and Rykov, had frequently to remind

their comrades in Vienna and Geneva about the needs and priorities of the people

whom they sought to guide (McKean 1990). There were points where the emigres

and the Russian workers disagreed, for example about party discipline, about

participation in the Duma, and in many cases, about the correct response to the

outbreak of war. Moreover, tensions were created by the fact that emigre

intellectuals spent their lives discussing politics and designing general schemes (and

resenting, in many cases, the fact that they were exiled from their country), while

activists at home, who ran a daily risk of arrest, had to deal with a living working

class movement in the factories. But the two movements were not sealed off from

each other. There was a genuine interaction between the different segments of the

revolutionary movement. 35

The events of 1917 itself accelerated and emphasized the political radicalization

of Russia's workers, soldiers, sailors and peasants. Bolshevik opposition to the war

had seemed like unpatriotic lunacy in April. By October it was one of the most
popular planks of Lenin's platform. The Bolshevik image of 'extremism', particularly unattractive after the abortive July uprising in Petrograd, was one of their most compelling features after the Kornilov putsch. It is true that Lenin's policy was not generally understood in all its details, that part of the Bolsheviks' appeal was that they were not identified with a government whose policies were all too clearly apprehended by the average citizen, but the fact remains that the Bolsheviks' popularity was rising by October to a point where they commanded majorities in the local and city soviets in many industrial areas (Keonker 1981: 208-27).

That there was widespread even mass support for the Bolsheviks in October 1917 is no longer in doubt. It is unlikely, however, that

34 For a discussion of these theories, see Harding 1977-81, especially vol. 2, ch. 3, 'The Theoretical Basis.'

35 For an account of how the 'Russian' section of the party began to take the initiative after 1909, see Geoffrey Swain's introduction to Protokoly soveshchaniya rasshirennoi redaktsii 'Proletariya': iyun' 1909 (1989). A different approach is adopted by William Chase and Arch Getty, who have demonstrated, by quantitative analysis, that in Moscow at least the Bolshevik Party had come to represent the bulk of the working population by 1917 (Chase and Getty 1978).
this support extended to the

idea of single-party rule. Recent Russian research
confirms the Western social

historians' finding that the overwhelming majority of
trade-unionists opposed the

'dictatorship of the proletariat' throughout the
revolutionary period (Kiselev 1991).

Even workers who favoured the Bolsheviks over the so-called
'moderate' socialist

parties did not, as far as can be established, imagine
that the latter would be

excluded from government and ultimately outlawed. Many of
the Bolsheviks' own

rank and file did not advocate this solution. The split
between the Bolshevik and

Menshevik factions, for example, had always been less
emphatic inside Russia than

among the leaders in emigration. In some towns, 'combined'

socialist parties still

operated in the summer of 1917. Life may have been
difficult for these alliances,

but where activists were thin on the ground, some kind of
general co-operation was

preferable to isolation and enmity. After October, the
Bolsheviks were to be

castigated repeatedly for the vigour with which they
excluded and even persecuted

members of the other parties. A recent study of the
Mensheviks shows how rapidly

support for them grew after the Revolution as the reality
of Leninist single-party

rule began to emerge (Brovkin 1987). It was not so much
that voters wanted to see
the implementation of the Mensheviks' policies (if indeed they understood what

these were); the vote was an overwhelming demand for the fulfilment of revolution

ary promises, including soviet democracy.

The rank and file was not alone in resisting the imposition of one-party rule. It is

likely that the majority of the Bolshevik elite had little idea that Lenin proposed to
circumvent the Soviet entirely in October. Within the Bolshevik Central Commit

tee, Lev Kamenev, Grigorii Zinoviev and several others deplored the direction

Lenin's policy took from September. Their resistance overflowed from the closed

meetings of the Central Committee to the national press before the insurrection,

and for several days after it Kamenev and others worked to secure a coalition

government (Bone 1974). These negotiations, and the general possibility of

coalition after October 1917, demand more attention from historians. At the same

time, however, there can be no escaping the conclusion that the odds were heavily

stacked in favour of the Leninist solution. One of the main barriers to coalition was

the attitude of the socialist parties themselves. Their leaders entrenched themselves

in positions from which there was no obvious retreat. A condition stipulated at the

negotiations by the 'minority' socialist parties, for example, was that Lenin and
Trotsky be excluded from any future government. While Kamenev, eager to
prolong the talks, appeared to entertain this idea for several days, it was clearly

36 According to O. V. Volobuev (1964), as late as 1917 10-20 per cent of local Social-democratic
party organizations remained 'unified'. 536

unacceptable to the people who had successfully organized and carried out the
insurrection. Trotsky took advantage of the impasse to abandon the talks and
further humiliate the minority parties. 37 But even if the talks had succeeded, the
minority parties did not have workable solutions to the major issues which
confronted them, beginning with the pressing question of the war. These were
serious problems, although they might not, on their own, have excluded the
possibility of co-operation altogether.

The core of the Bolshevik leadership, however, was bent on single-party rule.

Their record after 1917 was a grim one. Within three years of taking power, the
Bolsheviks had dissolved, at bayonet-point, the only national body ever elected by
universal suffrage, they had initiated a state-directed 'red terror' and they had
subjected the mass of the population to coercive economic regulation, either
through grain requisitioning or through labour regimentation and rule by ration
card. Few Russian historians would now claim that there
was anything laudable

about 'War Communism'. Most see it as a makeshift response
to crisis. Some would

go further, and might emphasize the opportunism with which

Lenin's government

pre-empted future opposition while securing victory over
its overt political enemies

(Buldakov 1992). Newly released evidence suggests that

Lenin himself perceived

the importance of terror as a state-building tool; what we
know of his enthusiasm for

the summary execution of priests, for example, is hardly
consonant with traditional

'Soviet' accounts of the ultimate humanitarian and father
of his people. More

research into this period is necessary, for it was a
formative stage in Soviet history. 38

Many of the institutions hastily formed in 1917-18 (and
believed by some

Bolsheviks to be temporary) were to survive for the next
seventy years.

These new discoveries will encourage the view,
traditionally held by commu

nism's bitterest opponents, that Stalin's murderous
policies of mass collectivization

and mass terror were the logical sequel to Leninist rule.
39 The argument proceeds

through several stages. First there is the evidence of the
pre-revolutionary Leninist

party. Lenin was well known as an intolerant factional
leader; his quarrel with the

influential Bolshevik philosopher, Bogdanov, for example,
developed into a full
scale party split in 1909, and some of the language used by members of Lenin's wing calling for the extirpation of Bogdanovism from the party now has a sinister echo (Swain 1989). But from this intolerance and faction-fighting to Stalin's purges was a very large step indeed. Bogdanov was not 'purged' in the Stalinist sense. Other dissidents were tolerated, and in many cases Lenin had no effective control over his disparate and egotistical faction. The image of Lenin the puppet-master manipulating his followers is greatly overdrawn.

The second stage in the argument is based on the evidence of the coup itself. As

37 For a recent account of the negotiations before and after the insurrection, see Service 1991: 251-79.

38 For a preliminary discussion, see Keonker et al. 1989: section III.

39 The main lines of this debate are discussed in Tucker 1977.

40 Kamenev, for example, was reinstated within days of his public denunciation of the coup. We may never have been part of their scheme to share power with the other socialist parties. Like the factional intolerance of the pre-revolutionary period, this decision indicates that Lenin's goals were in no way democratic. From his point of view, only a Bolshevik dictatorship could ensure the future of the
world proletarian revolution.

It was not a matter of power for its own sake. But if he had wanted co-operation,
alternatives did exist, albeit on a local scale. Not all the rebel regimes in the former empire were dictatorships, though the long-term prospects for such bodies as the Committee of Members of the Constituent Assembly in Samara were questionable without some form of economic or political coercion and centralization. Power sharing with members of the other revolutionary parties, as we have seen, might also have been an option, but for the lack of faith (and sometimes active treachery) on both sides which ruled it out in practice.

Despite the criticisms and costs, however, there were good reasons why Lenin chose to rule by dictatorship in the first years after the seizure of power. The empire was falling apart, transport, and therefore supply and industry, was in chaos, and the country was losing a major war. One of his first tasks was to build some kind of state structure through which to impose order. Critics are correct to point to the fact that the force employed was occasionally excessive. 41 They might insist that the price paid for maintaining the empire was extortionate; a better solution, as recent history suggests, might have been to allow peripheral states to secede, to consolidate a core of European Russia and thus, because the need for
centralization would have been less, to open the way for possible democracy. But no revolutionary regime has managed to establish itself peacefully; reactions to the Treaty of Brest-Litovsk demonstrated how unpopular the ceding of territory might be, and the behaviour of the White forces in the areas under their control suggests that the most credible alternative to Bolshevism would not have been more merciful. To base an argument about the next twenty years of Soviet power entirely on decisions taken in the country's deepest crisis is futile.

The most serious evidence for the assertion that, had Lenin lived, something akin to Stalinism would inevitably have been established in Moscow is based on the leader's behaviour in the 1920s, when the revolutionary crisis appeared at last to be under control. The most important issues are the maintenance and even tightening of party discipline and the continuation, under another name, of the secret police.

In the past three years, the story of the prison camps on the Solovetsky Islands in the extreme north of Russia, camps which held political prisoners condemned by Lenin's government, has been explored in Russian literature and film. The Bolshevik mentality, even under Lenin, was not a tolerant one. Their political experience inured them to measures which other politicians
would have avoided;

41 Very different accounts of the Bolsheviks' first years in power are given by Service 1979 and Leibman 1975.

42 This view, associated with a good deal of anti-Islamic and Great Russian chauvinism, can be found in the political statements of Alexander Solzhenitsyn. They also knew that their tenure of power was uncertain and that reprisals awaited them if they failed. The stakes were high, for they believed that they were acting on behalf of a greater cause; extreme measures were justified to secure the future of the Revolution. The Civil War left many Bolsheviks and other combatants traumatized for life, but the memory of its more heroic moments encouraged a feeling, especially among those who did not see active service, that quick executive decisions at gun-point were the best way to solve intractable social and economic problems. All this helps to explain the political atmosphere which facilitated the emergence of Stalinist autocracy.

There is little justification for taking the case one stage further and arguing that Stalinism was somehow 'inevitable', however. In the first place, Lenin's last years were characterized by a search for less violent paths to growth. Within the party elite, this search was accompanied by vigorous debate; the introduction of the New
Economic Policy (NEP), for example, was heatedly opposed by some politicians, few of whom suffered in the short term for their views. 45 Second, the options facing the Soviet government were more varied than has sometimes been supposed. The main impetus behind Stalin's final victory in the party was the decision to embark on rapid industrialization and the state-directed collectivization of agriculture.

Whatever the economic rationality of this course, there were genuine alternatives as late as 1928. And after the four desperate years of the 'great turn' of 1928-32, there was no clear 'necessity' for the Moscow trials and massive purge of 1936-8.

There is probably no controversy about the Revolution more complex than the debate about the Soviet economy in the 1920s. To understand it, the different strands of the argument must be separated. The first concerns the long-term viability of NEP as a strategy for economic growth. Even if it were possible to agree on that, there would remain the difficulty of setting the whole debate in the context of the Bolsheviks' priorities and world-view. The picture is also clouded by the historian's judgement about the costs of the 'great turn', costs which few Bolsheviks could calculate as they debated their country's future in the winter of 1927-8.

Three basic positions characterize the debate about the economy. The first is that
NEP was a viable system and that its abandonment in 1928-9 was, from a purely economic perspective, unnecessary. The market economy which emerged in the 1920s, in this view, showed potential for medium-term stability, although most would agree that relations between town and countryside remained problematic.

This was the view so enthusiastically aired in the West by Stephen Cohen in the 1970s (Cohen 1973), and again in Moscow in 1988, the year in which Bukharin 43 Surveys conducted in the 1920s confirmed that large numbers of former members of the Red Army suffered from nervous and respiratory disorders, some connected with poor living conditions and others with the stress of the revolutionary years; Merridale 1990: 132.

44 Evidence for this is abundant; the language used by the new generation of activists, mostly too young to have fought, in 1928-31 was almost entirely drawn from the Civil War era.

45 As Bukharin put it, 'everyone argued, searched for ways and means, quarrelled and made up and moved on together.' Cited in Merridale 1990: 21. 539 became a posthumous national hero. A second school suggests that NEP was not viable because of the havoc wrought during the Civil War (Gershenkron 1965b: 144-60). While the tsarist economy may have shown signs of peaceful evolution, in other words, its successor was doomed because confidence had been shattered, and
the infrastructure destroyed, in the upheavals of 1914-21. A third group argues that NEP was collapsing by 1928, with the potential for co-operation between town and country exhausted and industry on the verge of a capital investment crisis from which there was no escape within the existing economic structure. Large quantities of evidence have been marshalled in support of all three versions and their variants, for economists working on the pre-Stalin period, enjoyed a relative abundance of archival material even during the Brezhnev years.

To some extent, however, the long-term economic viability of NEP is less important than the way in which it was perceived at the time. From its inception, it was never a policy which appealed to the Bolshevik cast of mind. Fears that the peasant countryside was holding the socialist city to ransom were voiced repeatedly in the 1920s. And the pace of economic growth appeared painfully inadequate.

Heavy industry, the home of the most advanced technology and the most conscious proletarians, could hardly make progress if the constant peasant demand for consumer goods had to be met. Stalin was correct to insist that his colleagues take note of the gap between Soviet and Western levels of industrial development. At the heart of the debate was the danger that the Soviet Union
could not protect itself
from a second round of foreign aggression. Heavy industry was the first prerequisite for a thriving arms-production sector. The force of these priorities is clear from contemporary material. Not only the Bolshevik leadership, but also a large section of the urban working class believed that rapid growth would be necessary in the relatively short term if the revolutionary regime were to survive (Merridale 1990: 59-67).

Stalinist industrialization, and the collectivization of the peasantry which preceded it, was not inevitable in 1917, then, but some form of state-directed growth was a likely solution to the problems faced by the USSR from about 1926 onwards. The manner in which the great turn was conducted, including such measures as the deportation of supposed 'kulaks' (an official category which bore little resemblance to the real victims of the campaign), owed much to Stalin's style of leadership and priorities, but much also to the desperation of local officials. Stalin's aides occasionally criticized the brutality or waste involved in the collectivization campaigns, and there was a good deal of discussion about the correct plan for industry by 1931, but the broad strategy of state-sponsored growth was accepted almost universally, and with it, with gritted teeth, many
of its excesses.

It is when the other major aspect of Stalinism is examined the purges of 1936-8 that the continuities with Leninism look thinnest. Such widespread brutality might not have been possible if the way had not been prepared by two decades of political violence, if there had not been scores to settle, and story-tellers to be silenced from the Revolution, the Civil War and the 'great turn'. But the Moscow trials of 1936-8 and the accompanying great purge, unleashed in 1937, owe much to Stalin's personal direction. Recent biographies confirm this using newly released material. 49 We may never fully understand the Soviet leader's psyche, but there is no doubt that ruthlessness, vengefulness and a tendency to paranoia were prominent features of it. 50 He may not have planned the purges from the outset, but his desire for revenge and personal insecurity led him first to remove his major rival, Sergei Kirov, murdered in 1934, and then to implicate his former allies, Zinoviev and Kamenev, in the plot. By the end of 1938 virtually no member of Lenin's Politburo (except Stalin himself and Trotsky) was still alive. Stalin's
responsibility for the wider purge is less clear; he did not personally order every
death, nor did he stipulate in detail where every enquiry should be made. But he
took advantage of the whirlwind to dispose of enemies and rivals and to break the
resistance of powerful institutions. His signature appears on hundreds of lists of
condemned victims, and conversely his word was sufficient to spare someone who
might otherwise have died. 51

There remain, however, a large number of unanswered questions. If the Soviet
experience is compared with those of other societies in revolution, the picture
changes immediately. For almost all revolutions are followed by a purge of some
kind and often by more than one. The reasons for this must be sought in the
problems of post-revolutionary state-building and in the social psychology of
populations which have undergone rapid change at the hands of millenarian
regimes. The answers remain tentative. 52 But it is clear that the purges cannot be
understood purely in terms of Stalin’s personality. This is an area where new
archival material must be used with imagination, for the obvious sources can tell us
only who signed which warrants and how many died. In the end the most satisfying
global explanations are likely to come from setting the best new research in a
broader, and possibly comparative, framework. Before this can be done, moreover,
it is vital that historians agree to a reduction of the moral temperature. The language
of the Grand Inquisitor, which flows so easily on this topic, was not designed for the
furtherance of scholarship.
The question of historical imagination demands discussion now. The sudden
availability of so much new material has led to a scramble for archives; buyers are
being sought, officially and unofficially, for the juiciest material, and the rush is on
to publish exciting findings. The secret police files remain closed to all but a few,
but the material already released will occupy historians for decades. Among the

49 The best of these is probably Tucker 1990.

50 Tucker (1990) develops the 'psycho-history' theme in his biography, as does Roy Medvedev in his outstanding Let History Judge: the Origins and Consequences of Stalinism (1989).


52 The best single comparative study is still Skocpol1979.

archives now opened, those of the Central Committee and Politburo, and many
local or institutional archives, offer new vistas of understanding, not only about the
elite, but also about the opinions and priorities of ordinary people. And the mere

fact that the archives have opened and that Russian
scholars can discuss their work freely has created a bolder intellectual atmosphere for everyone involved.

But all this is taking place against a background of crisis in the subject. Even before the Communist Party collapsed in 1991, the writing of Soviet history had reached an impasse; the debates tended to circle around the group of questions which we have discussed here. These were, and remain, central issues, but they are not the only ones which demand attention. The re-emergence of the 'nationalities' problem reminds us that a whole area of Soviet development was largely neglected until recently. Other 'blank pages' were blamed on the shortage of material, and this was a genuine problem, but as an excuse it may sound a little thin to archaeologists and students of the pre-modern world. Another difficulty was the isolation in specialist departments of historians in Russia and of Sovietologists in the West. This isolation restricted everyone's horizons. The Soviet Union came to be seen as a 'black box', as a remote and special case, separate from Europe. Fortunately, this state of affairs cannot continue. Parts of the former empire, after all, may well become member states of NATO or the European Community.

There will be many responses to this crisis. In Russia
itself, history is at a crossroads. The Brezhnev generation must adapt to the new freedom of debate or retire, while the young historians currently training have received little guidance from teachers as confused about historical techniques, and even about the basic facts of their own past, as they are. Many of the most talented have left the universities for more lucrative employment in commerce and industry. Those who remain are hearing from their colleagues in the West that archives are sovereign and ideology perilous. But is that all we have to offer them? Our own profession is as much in need of an infusion of new thinking as is theirs. If we are to benefit from the availability of documents and the ready co-operation of Russian and other former Soviet historians, it is surely time to start asking new questions. The writing of history is heavily dependent on the priorities and disposition of individual historians, and all have their own agendas for the future. But at this stage, there are several obvious areas for new research. Some are provided by the priorities of the present itself; the nationalities problem, racism, humane alternatives to socialism in a world where capitalism faces another serious crisis. But others arise from the general rethinking of historical writing outside the Soviet field since...
the Second World War; the rise of social history, broadly interpreted; the study of
cultural history; comparative history; the history of ideas. Many of the questions
which need to be asked about twentieth-century Russia such as those dealing with
its social history or the world-view of its citizens will cut across the political and
economic issues which we have dealt with here. They may not focus as sharply as
traditional historians might wish on the ethical problems raised by Stalinism. But in
the end they may help to make more sense of the Soviet experiment and more 542
generally of the twentieth-century history of the countries of what is now called the
Commonwealth of Independent States than the narrow range of issues which has for
Minutes of the RSDLP (bolsheviks), Aug. 1917-Feb. 1918, London.
Buldakov, V. P. (1992) 'The October Revolution: seventy-five years on', European History
Quarterly (October) 22.
cadres of 1917: a prosopographic analysis', Russian History 5(1).


Medvedev, R. (1989) Let History Judge: the Origins and
Consequences of Stalinism, rev. and expanded edn, New York.


Skocpol, T. (1979) States and Social Revolutions, Cambridge.


-(1991) 'What time is it in Russia's history?', in C. Merridale and C. Ward (eds)


Volobuev, P. P. (1964) Proletariat i burzhuaziya v 1917 godu, Moscow.


The Nazi period is in many ways unique as a historical epoch, since there are virtually no defenders of or apologists for a regime that has become a synonym for political infamy in the modern world. But this does not mean that there has ever been universal scholarly consensus about the history of National Socialism or its 'meaning', either in German history or in any wider historical or cultural context.

On the contrary, intense and sometimes acrimonious debates about the sources, nature and consequences of National Socialism have been common since the Nazi movement emerged in the 1920s. And although an enormous scholarly literature on the history of the Nazi movement and regime has accumulated since 1945, these arguments have not been exclusively academic in character. Some of the earliest interpretations of Nazism were advanced by its political opponents, and
enduring debates about the relationships among fascism, totalitarianism,
capitalism and mass society have their origins in the intense political climate of the
1920s and 1930s. The political controversies of the inter-war epoch persisted
beyond 1945 as well, their contours revised and sharpened by the impact of
Germany's partition, by the Cold War and by the two successor states' competitive
claims to legitimacy: a polemical context that sometimes had the effect of
exaggerating the distinctions between theories that in practice shared some
common origins and revealed certain overlaps. Most recently, the unification of
Germany has shattered one of these post-war historiographical traditions, and
presented new challenges of confrontation and reintegration, though it is still hard
to see what will emerge from this process. The political realities of the past
seventy-odd years have thus established the context within which native and
foreign historians alike have negotiated the meaning of the German nation's recent
history.

If the tenacious divisions of twentieth-century politics have marked one of the
critical fault-lines in the interpretation of National Socialism, another derives from
the texture and status of historiography itself in German scholarly culture. As 545
Georg Iggers (1968: 3) has pointed out, few national historiographies have been as self-consciously theoretical as Germany's has since the nineteenth century.

The Germans, as Marx once noted, were a nation whose capacity for philosophical self-creation outran its ability to establish itself politically. The same idea recurs, more delphically, in Hagen Schulze's recent comment that 'the identity of the Germans lies not in their present but in their past and their future simultaneously' (Schulze 1987: 1014). Both observations point, across a span of 150 years, to an original project of imaginative national self-creation; in the hands of early nineteenth-century thinkers like Herder, Fichte and Humboldt, this was an act of quintessential historical Idealism, and set the terms of the enduring German historicist tradition. Historicism, in its broadest meaning as a modern historical sensibility, was nowhere more at home or more pervasive in nineteenth-century Europe than in Germany, whose intellectual culture was saturated with debates about history and historical theory, especially as they applied to the Germans themselves. Historicism in its narrower sense as a philosophy of historical interpretation that calls for empathetic understanding rather than law-bound explanation, and that has characteristically been applied in the narrative reconstruc
tion of state-formation and policy-making was even more essentially Germanic.

Virtually without effective challenge, this paradigm dominated German historiography from the 1830s until well after 1945, and determined its research priorities:

state rather than society, foreign policy rather than internal affairs, the sequence of historical events rather than the analysis of structures and systems. Only the experience of National Socialism dislodged historicism from its pedestal in West Germany, and that belatedly. In the German Democratic Republic, the historicist tradition was challenged more dramatically by Marxist dialectical and historical materialism, though even there it took time for historiography to respond to the new political agenda established there in the early 1950s.

The inheritance of historicism, which will be discussed in more detail below, provides an important key to post-1945 developments in the German historiography of National Socialism. In the early years of both the Federal Republic (Bundesrepublik Deutschland; BRD) and the Democratic Republic (Deutsche Demokratische Republik; DDR), though for somewhat different reasons, the historicist tradition exercised a negative influence on the reconstruction of academic history, and inhibited the emergence of new methods and approaches for a
generation after 1945. Despite the radical change of political direction in the DDR,

academic history there was simply unequal to the challenge of developing a new

historiographical tradition. The absence of historians who were in political or

intellectual sympathy with socialism left the DDR without an appropriate

professional historical culture between the late 1940s and the early 1950s. This

strengthened the strategy, adopted by the leadership of the SED (Sozialistische

Einheitspartei), of conflating the academic history of Nazism with the politics of

popular antifascism. As a cadre of academic historians schooled in socialism

emerged in the 1960s, the tasks of the profession became more defined, yet

remained subordinate to the SED's political priorities. Whatever its value in helping 546

to legitimize the new state and in criticizing authoritarian traditions in Germany's

history, this political agenda also obstructed internal disciplinary innovation in the

DDR. The SED version of Marxism was, in effect, superimposed upon method

ological traditions that otherwise remained curiously conventional and undisturbed.

It was not until the 1970s that the practice of history as such began to acquire a

critical, self-reflective dimension in the DDR, and that research themes, objectives

and debates began to emerge from the internal disciplinary
process of intellectual exchange rather than from external instructions.

In post-war West Germany, meanwhile, professional and public resistance to 'coming to terms' with the immediate past meant that there, too, 1945 was less of an intellectual rupture than might have been expected. Most West German historians, like other intellectuals, appeared to be sufficiently free of direct responsibility for the intellectual and political climate of the Third Reich that they could resume their work undisturbed the more so once the brief flourish of denazification came to an abrupt halt in the Cold War. With few exceptions, West German historians, in the words of one critic, resumed the practice of 'a politically and morally tamed historicism' between 1945 and 1960 (Ernst Schilllin, cited in Klessmann 1987: 116).

A critical historiography of National Socialism emerged in West Germany only in the 1960s, by which time the contours of debate had already been established by international scholarship. Actually, the research that is usually taken as the evidence of this transition Fritz Fischer's controversial interpretation of Germany's responsibility for the outbreak of the First World War may have been iconoclastic, but methodologically it was quite traditionalist in its dense archival reconstruction of the governmental
decision-making process. It was the impact of theoretical models from mainly non-German scholarship that from the 1960s transformed both West German historiography in general, and research into National Socialism in particular. The irony here was that these models to some extent represented a reimportation of Weberian socio-structural scholarship that had previously been rejected by the German historical profession. And that had found a more congenial home in other cultures and disciplines. And even though the pattern of West German research into National Socialism since the 1960s testifies to a profound breach with earlier methodology in some circles, the historicist tradition has never been entirely abandoned, but has broken forth from time to time in renewed debates that have been as much political as methodological.

In this story, we shall see how debate about National Socialism has oscillated between integration into broader explorations of the nature of modern European society, and confinement to narrower questions about German national identity. In a related move, debate has also fluctuated between the historicist concept of a 'German path' (deutsche Weg), and the comparative concept of Germany's 'special development' (Sonderweg).

The task of summarizing the vast interpretative literature
on National Socialism

poses, of course, considerable challenges of thematization and periodization, and
every student will have his or her preferred solution. My own somewhat idiosyn
cratic response represents an attempt to highlight what I regard as the two most 547
intellectually engaging analytical literatures. First, there is the largely theoretical
analysis of National Socialism as a form of fascism. This approach emerged
simultaneously with the rise of National Socialism itself in the 1920s; originally
politico-theoretical in cast, it has since become partially incorporated into
historiographical debate, although not without controversy as to its scholarly status.
The second major body of literature German post-1945 scholarship on National
Socialism is more conventionally historiographical and professional in character,
but also raises, as I have suggested, important questions about historical method
ology and intellectual politics. Into this basically two-part structure of my chapter I
have inserted a brief discussion of the German historical profession under National
Socialism, a topic often neglected in surveys of this kind; and I have also aimed to
integrate as appropriate the research agendas developed in non-German scholarship
since 1945. My emphasis will lie on the broad intellectual issues of interpretation. I
will not rehearse in detail the numerous academic debates about specific aspects of
the period, for these have already been the subject of a number of admirable, up-to
date and easily accessible surveys. I

FROM POLITICS TO HISTORY: THE CHALLENGE OF NATIONAL
SOCIALISM IN INTERNATIONAL SCHOLARSHIP 1920s-1950s

Witnesses to the rise of National Socialism found it hard to resist the conclusion

that Nazism was a symptom of some wider crisis in the European system: that

National Socialism was to Germany what fascism was to Italy, and what similar

hyper-nationalist, anti-Bolshevik and anti-democratic movements and regimes were
to Spain and a number of central European states. But while this concept of a

European fascism was widely shared in the 1920s and 1930s, and was to remain the

guiding theme of Marxist historiography well into the 1970s, it always circulated in

relation to two other powerful theses which commanded respect in liberal and

conservative circles. One emphasized the specifically German origins and character

of National Socialism, seeing in it the culmination of deep-laid weaknesses and

failures in Germany's historical development. A second school identified Bolshev

ism as a related anti-democratic movement which was symptomatic of the same

tensions from which fascism arose, including the weakening of liberalism and of the
bourgeois social and cultural order in Europe at the turn of the century, the political mobilization of the masses, the fin-de-sie'cle intellectual crisis, and finally the drastic dislocations in European politics and society brought about by the First World War.

1 See Kershaw 1989, which includes in ch. 1 a valuable summary of the major historiographical issues; Hiden and Farquharson 1989, which concentrates on topical historical debates, and includes an on the theoretical debates since the 1920s; and Hildebrand 1991, which includes an extensive deT deutschen Geschichte (9th edn) offers a useful synthesis of the German literature to that date. See also Kehr and Langmaid 1982.

In its most coherently expressed form, this equation of fascism and Bolshevism developed into the theory of totalitarianism that emerged in the 1940s and survived in the academic literature of the West until the 1960s. I will survey in turn these interpretations of National Socialism as a version of fascism, as a German peculiarity and as a form of totalitarianism. 2 However, it should be borne in mind that these distinctions are partly a matter of convenience: in practice, theories grew up alongside one other, often sharing common origins, premises and conclusions, yet also repudiating these affiliations in the light of intellectual or political antagonism.

Within the theory of fascism as a comparative concept the
clearest divide lies

between interpretations which draw on some variant of Marxist class theory, and

those that do not. What all these theories share, however, is a strong sense of crisis

that had been building in European society since the late nineteenth century, of

which the fascist movements, ideologies and regimes that emerged after 1918 were

the climactic manifestation. Important here are the different roles played by the

concept of the 'irrational' in these theories. For Marxists, this crisis was

the inevitable and predictable consequence of the structural irrationality internal

to the capitalist mode of production at its imperialist apogee, an irrationality that

was, however, subsumed into the larger rationality of the logic of history, which was

hastening towards the final revolutionary epiphany heralded by the crisis. By

contrast, most non-Marxist theories tended to emphasize the irrationality of the

popular responses to the undeniably profound strains on European society between

the 1890s and the 1930s, but hoped for an eventual restoration of the basically

sound principles of the liberal social and political order. This divergence persisted

in the academic literature after 1945. Non-Marxist theorists and historians saw their

judgement vindicated by the restabilization of capitalism and liberalism in Europe,
and believed on the whole that the era of fascism was over. Marxists, on the other hand, continued to look for evidence of the ultimate instability of monopoly capitalism, and to see fascism as an endemic resource for capitalism in crisis.

Marxist theories of fascism first developed in the context of the direct confrontation between Europe's communist parties and the fascist movements and regimes of inter-war Europe. Their primary objective was to discover the social location and logic of the fascist movements, within an overarching theory of capitalism and class struggle on the one hand, and of the conjunctural politics of post-war revolutionary stalemate on the other. Structurally, orthodox Leninist theory held that capitalism

2 This does not exhaust the full range of interpretations of National Socialism, far less of fascism, but identifies the approaches that have sponsored the most important historical research into National Socialism. Ay"oberry 1981 is a valuable and wide-ranging summary of the major theories of Nazism since the 1920s. Comparative accounts of fascism include Nolte 1966; 1967; Carsten 1967; Woolf 1968a and 1968b; Kedward 1969; Lubasz 1973); Laqueur 1979; Payne 1980; Kitchen 1976; Larsen et al. 1980; Wippermann 1972; Poulantzas 1974; Schulz 1974; Cassels 1975; Saage 1976; Schieder 1976; de Felice 1977; O'Sullivan 1980; Mihiherger 1987; Kijhn 1990; Brooker 1991; and two issues of the Journal of Contemporary History devoted to fascism, I (I) (1966), and II (4) (October 1976). See also Rees 1984. 549 Z
had entered its final period of crisis in 1914, marked by the ripening of its economic contradictions and the growth of the revolutionary working-class movement.

Conjuncturally, after the end of the war, capital faced a worsening crisis of economic restabilization and political representation in all European nations, yet at the same time the revolutionary momentum had been blocked by the collapse of working-class unity in 1919, and by the Left's failure to gain the support of the social classes intermediate between capitalists and the proletariat. Nowhere was this alignment clearer after 1918 than in the defeated ex-power Germany, and in the quasi-loser nation of Italy. Fascism entered the political and historical scene in these countries as a mass counter-revolutionary movement of the socially disorganized classes, mobilized by capital in a bid to recover its political hegemony.

A crucial element in contemporary Marxist analyses of Italian fascism and German National Socialism was thus the distinction they drew between the class composition of the movements, and the class interests they served. As the Comintern theorist Karl Radek put it in 1934

Marxist analysis cannot of course disregard the social composition of any political organization, but it does not admit that the social
composition is the decisive factor in
determining the social character of a given movement; it
asks what class interests in the
last analysis are directing this movement. (quoted in
Cammett 1967: 158)

In the 1920s, Comintern worked through various versions of
this basic interpreta
tion, drawing first on the Italian experience of fascism
in the agrarian sector, and
castigating social democracy as fascism's helpmate. From
the point of view of the
German resistance to National Socialism, it was little
short of catastrophic that the
rise of the NSDAP after 1928 coincided with the climax of
Comintern's attack on
social democrats, in which the entire social and political
spectrum from social
democracy to National Socialism was characterized as 'one
reactionary mass'.3 It
was not until the disaster of National Socialism's seizure
of power in 1933 that
Comintern took official notice of alternative voices
within its ranks, and developed
an analysis that, belatedly, recognized the need for a
defensive front of all antifascist
forces. It was this shift that produced what became the
classic Marxist definition of
fascism, first formulated by Dimitrov at the ECCI Plenum in
December 1933, and
formally adopted at the 7th Comintern Congress in August
1935. Here fascism was
characterized as 'the open terrorist dictatorship of the
most reactionary, most
chauvinistic and most imperialist elements of finance capital [my emphases]’ a formula whose crucial superlatives finally opened the way to political collaboration with any groups short of these extreme 'elements' (Dmitrov n.d.: 40). It was this formulation that, as we shall see, was to preside over the bulk of post-1945 historiography in the German Democratic Republic, where it did service not only in accounts of the rise of National Socialism, but also in the explanation of the 3 For the role of the KPD in Germany, see Foukes 1984; Fischer 1991; Rosenhaft 1983 explores the tension in the communist movement between official and rank-and-file views on resisting the Nazis; on SPD attitudes to fascism, see Sturm 1986; Breitman 1989; Pyta 1989; Winkler 1990. 550 relationship between the Nazi regime and industrial capitalism after 1933, and of German aims in the Second World War.

It is, however, important to remember that these 'official' Comintern interpretations of fascism were by no means the sole contribution by Marxist theorists in this period. The 1920s and 1930s saw a wide range of alternative theories that used Marxist theoretical tools to probe the nature of fascism and the means of resistance to it. For example, the Austrian Marxist Qtto Bauer explored the way in which the extensive industrial rationalization of the inter-war years had provoked social crisis
by intensifying the contradictions between social
production and private accumula-
tion; the young German crypto-Marxist Alfred Sohn-Rethel
also stressed the
significance of the rationalization process, and
identified fascism as an attempt to
solve the consequent crisis of profitability by means of a
return to the production of
absolute instead of relative surplus value. Also important
were the so-called
'Bonapartist' interpretations of fascism, which drew upon
Marx’s well-known
analysis, in The Eighteenth Brumaire of Louis Bonaparte,
of the stalemate of class
forces that had permitted Bonaparte’s seizure of power in
1851; this interpretation of
fascism was most famously developed by Trotsky, but it was
first advanced by
Bauer and by the dissident German communist August
Thalheimer. 4 And just as
Comintern’s 1933/5 formula was taken over into East German
and Soviet orthodoxy
after the war so also it was the rediscovery of these
eredoxy interpretations from
the 1920s and 1930s that fuelled the revival of fascism
theory in the West in the
1960s and 1970s.

Beyond the official and dissident Marxist theories of
fascism, a third important
strand in German leftist thought was the attempt to
synthesize the theories of Marx
and Freud. Here Marxism offered, roughly speaking, a
structural analysis of the
dynamics of social process and historical change, and psychoanalysis a theory of the production, introjection and function of ideology in human agents. Wilhelm Reich, for instance, explored the relationship between patriarchal ideology, sexual repression and fascism and racism, using Marxist and psychoanalytic methods in increasingly heretical fashion; in the 1940s Erich Fromm developed an interpretation of the relationship between domination and submission in the psychology of the modern individual; and, most systematically, members of the Frankfurt School, notably Theodor Adorno and Max Horkheimer, began the intellectual journey that led to their theory of the authoritarian personality and also to their pessimistic explorations of the repressed relationship between Enlightenment rationality and the irrational, which reached its perverse climax in the extermination of the Jews. 5


and Held 1980. Despite the importance of sexual identity and pathology in these studies, they have
very little to say about women and the feminine in fascism; see Macciocchi 1976, also Theweleit

1987-9, 551

From the left reaches of the Frankfurt milieu came too Franz Neumann's powerful analysis of Nazi Germany as a quadripartite power system, built upon the competing pillars of party, army, bureaucracy and industry; first published in 1942, his study Behemoth remains one of the most compelling and insightful accounts of the regime. It was the Frankfurt School tradition that, although interrupted and redirected by exile and dispersal during the 1930s and 1940s, recovered after 1945 to become the strongest link between preand post-Nazi German theory; and its most eminent heir, the philosopher Jürgen Habermas, became deeply involved in the bitter historiographical controversy of the mid-1980s known as the Historikerstreit (historians' dispute), as we shall see.

The principal contemporary alternative to Marxist analyses of fascism, the 'mass society' thesis, had emerged towards the end of the nineteenth century as a description of the spiritually anomic, socially amorphous, and ideologically irrational tendencies of post-traditional European societies. While class antagonisms played a part in this image of mass society, they did not occupy the same determin
ing role as they did in Marxist analysis; anti-Semitism, for example, which Marxist theory tended to see in instrumental terms, was here given an ideological autonomy of its own. The imagery came into its own in the 1930s and 1940s as a plausible explanation for fascism’s appeal to the popular experience of loss, displacement and disillusion in the post-war period. 6 Nazism, in this view, was an extreme irrational response to the intolerable pressures of post-war German life, pressures which the Weimar state proved incapable of relieving. This approach reappeared in the postulate of a cultural and spiritual crisis of European civilization that was a popular recourse of West German historians immediately after 1945, as we shall see. Later interpretations of fascism as a form of resistance to modernization were also essentially refinements of this initial view; and Ernst Nolte’s important study Der Faschismus in seiner Epoche (1963), with its thesis of ‘resistance to transcendence’, was another heir to this tradition. 7 Parallel to the attempted convergence of Marx and Freud, the mass-society thesis offered its own synthesis of sociological and psychological theories to explain both the nature of social distress in the post-war period and the susceptibility of certain social groups to fascism’s solutions. Such interpretations echoed Marxism’s emphasis on the
crucial role of the petty bourgeoisie in fascism, but pressed beyond the question of its material status to examine the specific psychosocial pressures experienced by this most liminal of social groups (Geiger 1930; Parsons 1942/3a; 1942/3b; Lipset 1960). And with psychology as the new science in the early twentieth century, the idiom of pathology could also be directly applied to Hitler and his followers: ‘What is involved [in Hitler’s ideology] is an idee fixe of atavistic origin, which does away with the complexity of reality and replaces it with the uniformity of primitive combat’, wrote the Frankfurter Zeitung in 1928. ‘At bottom, Hitler is a dangerous madman’ (cited from Ay‘oberry 1981: 76). This psychopathological interpretation has obvious attractions to biographers seeking to understand Hitler’s personality,
and has been developed academically through the genre of psychohistory. Whether Marxist or non-Marxist, these theories begged the question of why it was Germany that apparently exhibited the most extreme symptoms of crisis. The idea of a particularly German susceptibility to fascism was not necessarily incompatible with Marxist theory; even the idea that National Socialism was a phenomenon of German backwardness was accommodated into Marxist thought, despite the logic of a theory that depended upon capitalism reaching its most advanced stage as the prelude to its final collapse. Intellectually, the transitional figure here was the Austro-German Marxist Ernst Bloch, who in the 1930s developed the concept of 'asynchronism', or the coexistence of 'non-contemporaneity and contemporaneity' (Ungleichzeitigkeit und Gleichzeitigen), to explain the strength of pre-capitalist survivals in Germany, 'the classic land of anachronism'. And as the world divided in war in the 1940s, and the evidence of the barbarous inhumanity of National Socialism accumulated, the search for the specifically German sources of National Socialism was further stimulated among its political and ideological enemies. Historians from the Allied nations (e.g. Rohan Butler (1941), Edmond Vermeil (1944) and A. J. P. Taylor (1946), and German
emigres (e.g. Hermann Rauschning (1939), Peter Viereck (1941), later Fritz Stern (1961) and George Mosse (1964), traced the roots of National Socialism back into earlier German history, and confected a logic of German development, or misdevelopment, that led almost ineluctably to Hitler. Many of these authors shared a tendency to emphasize the ideological origins and logic of National Socialism, as well as its peculiarly German quality, though they offered varying chronologies for its origins and rise, and differed in their explanations of this tradition of German political extremism. Typically, they focused on such issues as the shallowness of the Enlightenment in Germany: the uncontested power of authoritarian statism; the consequent weakness of rationalist, liberal and emancipatory values among the German elite, and a broader openness to ideas of aggressive nationalism, anti-Semitism and civic quietism among the popular classes; the anti-modernist, often nihilist 'cultural pessimism' that characterized fin-de-siecle German thought; and

8 See, e.g. Waite 1977; Binion 1976; Stern 1975; see also Ayberry 1981: ch. 10.

9 Ernst Bloch [1935]1962: 45-204. The Hungarian Marxist philosopher George Lukacs developed the idea that German philosophy provided uniquely fertile soil for the flourishing of irrationalism in Die Zerstörung der Vernunft (1953); for further comments on
the German backwardness thesis in Marxist thought, see Evans 1985, and Caplan 1986.

10 See also Barracplugh 1949; Kohn 1960; Glaser 1978, and Plessner 1959. Shirer 1960 is probably the most widely read popularization of this approach. 553

the alleged susceptibility of the masses to seductive political mythologies and defensive bigotry. Although these political and intellectual tendencies could be found throughout pre-1914 Europe, Germany strained morally and socially by the particularly intense pressures it experienced from industrialization and the rise of mass society was seen as offering the most fertile soil, so that it only took the further crises of war and revolution to convert this latent extremism into the overt form of National Socialism. In this way, the pathology of modern society was reinterpreted as a visitation concentrated in its most virulent strain on Germany.

Totalitarianism theory was in some respects a derivation from the mass-society thesis, though it abandoned the latter's previous broad interest in social dynamics for a more limited obsession with the mechanics of power. It emerged as a systematic political analysis in the 1950s, though the term itself had first been coined in the 1920s, in application to fascism and National Socialism. The term's
earlier currency among leftist analysts of fascism including Franz Neumann (1942), Herbert Marcuse (1967) and Ernst Fraenkel (1941) has largely been forgotten in the wake of its later appropriation as a generic description for both communism and fascism. This usage, which developed during the Cold War, deliberately challenged the Left's claim that there was an absolute antithesis between the two ideologies because fascism was, in class terms, a variant of capitalism, while communism was the overcoming of capitalism. II Totalitarianism theory does not necessarily deny the existence of major differences between communist and fascist political systems, but concludes that in the final analysis these differences are less significant than either what the systems have in common, or what distinguishes them from liberal democracy. The classic formulation, originally advanced by Carl Friedrich in the 1950s, identified six characteristics of totalitarian regimes:

- an official ideology . . .
- a single mass party ... a system of terroristic police control ... a technologically conditioned near-complete monopoly of control . . . of all means of effective mass communication [and] of all means of effective armed combat ... a central control and direction of the entire economy. I2

Marxism's emphasis on the class character of fascism was thus discounted, and in
general the social-structural approach characteristic of both the Marxist and

Weberian traditions of political analysis was discarded in favour of a narrower focus

on the political apparatus alone. A powerful exception was Hannah Arendt's The

Origins of Totalitarianism, first published in 1949, whose thesis depended upon a

devastating argument about the relationship between mass populations and mass

murder: that the raison d'etre of the true totalitarian regime was its capacity for the

annihilation of its unintegrated superfluous populations. Arendt's arguments

11 Kershaw 1989: 20-3, 30-5 offers an informed and judicious summary of the term and its

applications; see also Ay. 1981: ch. 8.


alone. 554

derived from her profoundly pessimistic assessment of the potential for responsible

politics in modern class societies, which she saw as an incoherent assemblage of

atomized masses on the one hand and an elite of leaders on the other. She traced the

historical emergence of this deformed polity, until its ultimate perversion as the

totalitarian state of her own day, in which terrorist domination became its own logic
and legitimation. Yet like other theorists of totalitarianism, Arendt found it easier to apply her model to Nazi Germany than to Stalinist Russia, even though she saw both of them as equally utopian (or rather dystopian) projects of social transformation in the name of history, freed from the constraints of divine, natural or human law alike.

Although in the 1920s and 1930s the concepts of totalitarianism and fascism were not seen as belonging to mutually irreconcilable theories, by the 1950s a polemical distance had been established between them by the exigencies of Cold War scholarship. Irrespective of their intellectual merits, both theories got taken up as political weapons in the stand-off between West and East, and were often grossly over-simplified by proponents and caricatured by opponents. In practice, academic historians in the West had little to contribute to this political stalemate unlike their counterparts in the East, or their own colleagues in Western political science departments, both of whom were heavily engaged in it. Nevertheless, when scholarly research into the history of National Socialism began to take shape in the 1960s, it was this debate that formed the initial framework though it was not long, as we shall see, before this research undermined the theoretical structures
within which it first emerged. But before turning to this, it will be helpful to review

the character of the historical profession under National Socialism, and the re

emergence of academic history after 1945. HISTORIANS

UNDER NATIONAL SOCIALISM

The history of the profession and practice of history between 1933 and 1945 is one

neither of principled resistance to the pressures of the National Socialist regime, nor

of enthusiastic political capitulation. 13 Before 1933, academic historians, conserva

tive politically as well as methodologically, were no more moved by the radicalism,

not to mention anti-intellectualism, of Nazism than they had been by any other

ideology or theory that threatened to disturb their own traditions. 14 But these

traditions also predisposed them to powerful reservations about the Weimar

Republic, and to an inner sympathy with National Socialism once it came to power.

Like the professoriate in general, academic historians were politically inclined to a

conservative nationalism that made them mistrustful of republican democracy.

13 On historiography in Nazi Germany, see Gilbert 1947; Heiber 1966, which is much more than a

history of the Reichsinstitut fur die Geschichte des neuen Deutschlands alone; Werner 1967; Werner

1974; Werner 1968; Franz 1981; Schreiner 1985; Burleigh 1988; Schulze 1989: ch. 3.

14 On academic historiography in the Weimar Republic, see Faulenbach 1980; also Hammen 1941;
Their intellectual practice privileged the history of the state, the role of the 'world historical' individual and the peculiarity of Germany's history by contrast with that of western Europe. Weimar appeared to them as an ahistorical imposition, a deviation from the path of German national assertion; anxious to see that continuity re-established, they had little interest in embedding the republic in any alternative logic of historical development. Thus a tendency to look for the emergence of a new German leader to overcome the political and spiritual crisis of the age was clearly discernible in 1920s historiography. On the whole, it seems hard to argue with the applicability to this group of German historians of the cynical observation made about an earlier generation, that 'they easily lose the ability to move their heads because of their always looking backwards, so that if any movement at all is possible, only movement to the right remains'.

Still, the fit was not necessarily a close one. Despite the status ascribed to history in Hitler's world-view (see Wippermann 1989: 98-105), the regime's own priorities lay more in mass indoctrination through popular propaganda and the lower echelons of the educational system. The esoteric researches of academic historicism were not
much use there. For their part, academic historians were not directly attracted by
the Nazis' insistence on the primacy of biological racism as the key to history; they
preferred to preserve their traditional standards of intellectual integrity, if more for
narrow and self-interested professional reasons than out of political principle. Like
other members of the bourgeois elite, their sights were set more on the restoration
of a familiar social and political conservatism than on the kind of radical changes
promised by the Nazis. But it was this very narrowness of perspective that made
most historians again, like members of other professions willing to shut their
eyes to Nazi radicalism, and to compromise with the regime as long as their own
professional activities were not too intrusively politicized. Few had been or became
enthusiastic members of the NSDAP, but at the same time few showed any scruples
about continuing their scholarly work with appropriate gestures to the new political
dispensation. On the whole, academic research remained relatively undisturbed,
and there was no sharp break in publishing practices (though Meinecke was forced
out of the editorship of the Historische Zeitschrift). In this sense the profession was
not thoroughly purged or Nazified yet this was hardly necessary, given the
underlying sympathy among historians for the regime. As Schreiner comments in
his discussion of academic research on the concept of the German 'Reich',

historians after 1933 presented evidence of 'historical connections [that] lent recognition and legitimacy to the third foundation of the Reich. ... Academic language coincided with the language of party propaganda. The lecture hall became a forum for völkisch ideology' (Schreiner 1985: 189-9).

Given this widespread stance, it was not surprising that the profession as a whole did little to protect those of its colleagues who did fall foul of the regime, most of whom were by definition already intellectual, political or racial 'outsiders' in this conservative milieu.

A number of liberal historians whose heterodox ideas had found little acceptance in the profession in the 1920s were forced into emigration, among them Eckart Kehr, Hans Rosenberg, Arthur Rosenberg, Hajo Holborn, Georg Hallgarten and Veit Valentin. Even though most continued to study the history of their homeland, few returned to it after 1945, and thus most of the discipline's liveliest and most imaginative minds were permanently lost to the German historical profession. 16 Some historians who were not professionally or politically radical, such as Ernst Kantorowicz and Hans Rothfels (both men of
impeccable conservative credentials), were obliged to flee as Jews. Others, such as Hermann Oncken and the Vernunftrepublikaner Friedrich Meinecke, remained in Germany and tried to distance themselves from the regime (and protect themselves from their critics) as best they could. Only a handful of those who remained, including Fritz Kern and Walter Markov, can be said to have resisted National Socialism either intellectually or politically.

On the other side, a minority of historians offered more active intellectual support to National Socialism. A handful of established scholars for example Heinrich Dannenbauer and Otto Westphal aligned themselves enthusiastically with the racial and geopolitical perspectives of the Nazi regime. Some younger scholars looked to the new regime for career openings beyond the frustrating limits of Germany's narrowly hierarchical academic system. Among these were the lesser or marginal scholars who were responsible for the political revision of educational textbooks. Others found a niche in Walter Frank's Reichsinstitut für die Geschichte des neuen Deutschlands, established in 1935 under the patronage of the Nazi ideologue Alfred Rosenberg. The ambitious and unprincipled Frank and his colleagues saw themselves as the intellectual pacesetters and political watchdogs for
the profession, but they were pretty much discounted by their fellow-historians and ignored by the Nazi leadership. Strangled by political and administrative intrigues that he had courted but was incompetent to control, Frank was evicted from his position in 1941, though the institute struggled on until 1945. More baleful was the contribution by historians to Ostorschung, or the academic study of eastern Europe, which had a more central role to play in Nazi policy. By nature interdisciplinary, Ostforschung included specialists in fields such as archaeology, anthropology and political economy, as well as history. Working often in close collaboration with the

16 Though they did influence the post-war generation of academic historians; see below p. 559. For the emigre historians, see Iggers 1974: a fuller list of those who either left as historians or trained in the discipline during exile includes some of the most eminent scholars in the field of modern German history, among them Felix Gilbert, Theodor Mommsen, Fritz Epstein, Golo Mann and Francis Carsten, also the younger Georg Mosse, Fritz Stern, Peter Gay, Fritz Ringer and many others: an outstanding harvest of talent, garnered mainly by the United States. Women historians, virtually ignored in existing studies of emigre scholars, are discussed by Epstein in a volume of essays based on the conference 'Women in the Emigration after 1933' held in November 1991 (Epstein 1993); see also in this series Lehmann and Sheehan (eds) 1991 and Lehmann and
van Horn Melton (eds) 1994.

17 On school textbooks, see Blackburn 1985, especially ch. 3.

18 See Heiber 1966 for a summary see Vierhaus 1968, 557

SS, and through organizations such as the Nordost-Deutsche Forschungsgemein

schaft, the Bund deutscher Osten, and other research institutes based in occupied Poland, the Ostforscher were deeply complicitous with the implementation of Nazi Lebensraum policies, as well as with the intellectual sink-hole of racism (see Burleigh 1988; Klessmann 1985; also Kater 1974). Their major intellectual contributions to the regime were 'the production of a leaden stream of propaganda literature justifying German hegemony in the occupied East', and research into the ethnic identity and composition of Germany's new subject peoples all in the service of racial discrimination, resettlement and ultimately extermination (Burleigh 1988: 206).

The tendency in West Germany after 1945 was to argue that academic historiography had remained more or less marginal to the activities of the Nazi regime, and that historians as a group had not deeply compromised themselves.

However, as Klaus Schreiner has pointed out, the question of who complied with, who resisted National Socialism is more than a matter of individual biography. The
significant question is how historians collectively allowed their intellectual principles to be replaced by or subordinated to the ideological agenda of National Socialism, and here it is impossible to ignore the importance of pre-1933 professional and political values in helping the historical profession to come to terms with the Nazi regime (Schreiner 1985: 167). In the words of the most recent student of the German historical profession, 'The discipline's basically national-conservative consensus and its acquired willingness to adopt "volkisch" perspectives in general created an effective web of internal relationships with the aims of National Socialism' (Schulze 1989: 40). 

The renewal of West German historiography after 1945 was conditioned by a series of somewhat negative factors: by a strong reaction against the disparaging image of Germany conveyed by Nuremberg and the Allies' denazification efforts, by the profession's own desire to assert its freedom from contamination by Nazism, and, consequently, by a return to traditional historiographical standards rather than the development of new critical approaches. Moreover, so successfully had the historical profession excluded heterodoxy in the 1920s and conformed to the political pressures of the 1930s that it had few internal
The most useful sources on the post-war development of West German historiography in general are Mommsen 1973; Conze 1977; Mommsen 1981a; Faulenbach 1981; Riisen 1984; Wehler 1984; Mommsen 1984; Faulenbach 1974; Klessmann 1987; Hein 1986; Schiilin 1989; Iggers 1975: ch. 3. For a detailed examination of the immediate post-war period, see Schulze 1989; and see also Ritter 1950.

Useful surveys of the early post-war historiography of National Socialism include Kwiet 1989; and Wippermann 1976. 558

draw for a critical reconstruction of its practice. And in 1945, even more than in 1933, continuity in academic appointments was the rule: only about 20 out of the 110 professors of history lost their university positions due to denazification efforts,

and even some of these found reappointment in the 1950s. Thus the Historische Zeitschrift, the profession's journal of record, published but a single article on National Socialism before 1950, and picked up the historical themes favoured before 1933, such as the 1848 revolutions, the tradition of local self-government, or Bismarck and unification. These were subjected to re-evaluation in the light of the failure of liberalism in Germany, but the framework of interpretation remained basically historicist, motivated primarily by intellectual and moral concerns, and not theoretically informed or interested in structural
analysis (Mommsen 1973: 127).

To a great extent it was the emigre generation of historians notably George Mosse, Peter Gay, Hajo Holborn and Fritz Stern who took up the challenge of reinterpreting the German past, fortified by the right of the victors to tell the story on their own terms. By the same token, the surviving generation of senior West German historians was reluctant to address directly in their research or teaching a history in which their own part had been less than glorious.

Yet German reluctance to address the Nazi past should not be exaggerated, for the immediate post-war years saw a spate of publications on National Socialism and the war, by journalists and independent authors as well as historians; it was not until the 1950s that fuller rein was given to popular desires to repress the recent past. 21 The titles of early works bristled with a vocabulary of national self recrimination and moral bafflement 'catastrophe', 'guilt', 'wrong turning', 'fate' and the like. 22 Among the most celebrated examples of this genre were two works by spokesmen for two generations of German academic history: the 85-year-old Friedrich Meinecke, and Gerhard Ritter, at twenty years his junior the new leader of the profession. Meinecke's Die deutsche Katastrophe (1946, trans. 1950) interpreted National Socialism as a manifestation of deep
faults in European society,

and ascribed its success in Germany to a series of
avoidable accidents. Not
dissimilarly, Ritter's Europa und die deutsche Frage (1948)
also emphasized the
European scope of the crisis of modernization that led to
National Socialism, and
viewed the Third Reich as an unpredictable moment of
rupture in the course of
German history (see also Ritter 1955). These titles also
suggest that, perhaps
unsurprisingly, the earliest publications in West Germany
were concerned more
with the moral implications of National Socialism than
with the search for broader
political or social explanations. This was true too of the
emigre Hans Rothfels's early
study of the German resistance first published in English
in 1948 and based on a
20 Schulze 1989: 124-5. The continuity in the profession
and methodology of Ostforschung was even
more blatant and reprehensible: see Klessmann 1985: 370-4
and Burleigh 1988: 300-21. For the
lineage of the historical profession in general, see Weber
1984.

21 Wippermann 1976 argues that the immediate post-war years
saw a lively debate, a point often
missed in reviews of the academic literature; for the
retreat from history, see Heuss 1959.

22 See the catalogue in Schulze 1989: 47; also comments by
Faulenbach 1981: 31-2. 559

lecture delivered in Chicago in 1947.23 Rothfels credited
the moral achievement of
the national-conservative resistance in spite of its evident political failure, and thus reversed the normal historicist criteria for judging success; to that extent his book represented a rejection of the inherited traditions of historical evaluation (Mom msen 1981: 153). Yet this emphasis on values and traditions perpetuated another element of the historicist tradition, and was evidence of a wider professional reluctance to embrace social and structural approaches to history. 24 In addition, Rothfels's exclusive emphasis on the national-conservative resistance ignored the contribution of the communist opposition, reflecting a broader tendency in West Germany to dismiss the leftist resistance to the Nazis as essentially traitorous a conservative position that was given new force by the emergence of Cold War polarities in the 1950s.

As research gathered steam in the 1950s, it did not shun genuine and difficult questions about the origins of and responsibility for National Socialism, yet pursued these in a somewhat limiting interpretative context which emphasized the ideological, criminal and anomalous character of the regime. This had the double effect of limiting research into the specific political and sociological circumstances of the Nazi rise to power, and of protecting the broader span of German history from a
more probing and critical re-evaluation. Thus Hans Buchheim's Hitler was portrayed as a demonic and more than Machiavellian character; the sources of his power were located above all in his ideological and manipulative skills, as the Fuhrer himself had claimed (Buchheim 1958; 1962). In a similar reflection of the Nazis' own imagery, the regime after 1933 was depicted as monolithic and totalitarian in structure, and its policies as the logical implementation of the leadership's ideological programme. While foreign and emigre historians, as we have seen, traced the roots of National Socialism far back into German history, the dominant West German historical interpretations insisted, with Meinecke and Ritter, on the character of the Nazi regime as an unforeseeable rupture in the course of German history, and/or as one local expression of common perils shared by the post-Enlightenment world. In so far as the triumph of National Socialism was not explained as the accidental victory of a conspiracy of brutal political thugs, German responsibility was broadly assessed in terms of the cultural weakness of German liberalism, rather than being assigned, as Brecht put it in his Kriegsfibel, to 'dunkle Miichte' dark powers who were not the anonymous emanations of fate, but had identifiable names and addresses. As Gerhard Ritter (1965: vii) pointed out, the
intended readership of these early publications was less the Germans themselves than a hostile world of foreign critics addicted to the concept of collective guilt. In Hans Mommsen’s phrase, Germany was represented as if it had been ‘occupied’ by National Socialism; the question of the specific institutional and social sources of National Socialism within Germany itself was thereby occluded. At the same time, historians were willing to contribute to the construction of a positive foundation for the new West German state, even if their temperament meant that they saw this less as a democratic than as an ethical project. 25

This period saw the foundation of the Munich Institut für Zeitgeschichte (Institute for Contemporary History), a research foundation that pioneered West German historical scholarship on National Socialism. 26 It is significant that this initiative took place outside the traditional university structure, and was even opposed as insufficiently scholarly by some members of the academic establishment,
Ritter included. But the Institute has remained the premier centre for research into National Socialism and contemporary history in general, and has sponsored a string of extremely important research and bibliographical projects, the publication of both primary sources and secondary research, and the issue from 1953 of the Vierteljahrshefte für Zeitgeschichte (first edited by Rothfels), which immediately established itself as the journal of record for research into the history of National Socialism. 27 The first short popular account of the National Socialist period aimed at a domestic readership was issued by the Institute's first head, Hermann Mau, in collaboration with Helmut Krausnick (1953); it was followed by a number of similar studies by Buchheim (1958), Helga Grebing (1959), and harbinger of a new generation Martin Broszat (1960), who was to become one of the most prominent historians of National Socialism and retained a lifelong association with the Institute. 28 Mau, who had written his Habilitation thesis on the use of the Nuremberg documentation as a historical source, also played an important role in the process by which the Allies agreed to return most captured documents to German archives more speedily than they had originally envisaged. 29 Under the editorship of Rothfels, and with an international
editorial committee, the
publication of the important series of Akten zur deutschen auswärtigen Politik
1918-1945 began in 1950, to be followed in 1958 by the independently published
series Ursachen und Folgen; the first popular edition of selected documents on
National Socialism was published in 1957 by Walter Hofer (300,000 copies were in
circulation by 1961) (see Kwiet 1989: 106). In general, the major themes of the first
25 For these points see Mommsen 1973: 129-33.
26 For the origins and work of the Institute, which until 1952 bore the name 'Institute for Research
into the National Socialist Period', see Gimbel 1965; Auerbach 1970; and Schulze 1989: 229-42. A
second non-university institute, the Hamburg Forschungsstelle für die Geschichte des Nationalsozial
ismus (led for many years by Werner Jochmann), was established after much delay in 1960.
27 It is impossible in the limits of this chapter to discuss the periodicals in the field of contemporary
history; the most important for contemporary history are, apart from the Vierteljahrshefte, Geschichte in
Wissenschaft und Unterricht, Internationale Wissenschaftliche Korrespondenz and the more recent
Geschichte und Gesellschaft. Important English-language periodical sources for the period include
Central European History, German History, Journal of Modern History, New German Critique.
28 Mau and Krausnick 1959 parts were originally published in Rassow 1953; Buchheim 1958;
Grebing 1959; Broszat 1960. For a broader listing of early

29 For the capture and return of German records, see Wolfe 1974; Henke 1982. 561

scholarly publications, German and foreign alike, were the
two and foreign policy,

and the ideological origins of National Socialism, notably
in modern anti-Semitism.

The internal structure and domestic policies of the Third
Reich did not become the

object of intense study before the mid-1960s, and until
then the interpretation of

the domestic structure of the regime was dominated by
totalitarianism theory.

Interestingly enough, despite the influence ascribed to
Hitler as the demonic power

in the regime, there was no major biography of the Nazi
leader by a German

historian until Helmut Heiber's 1960 study, and even this
was more workmanlike

than inspired. Alan Bullock's (1952) magisterial study
remained the canonical work,

and there was nothing comparable in German until 1973,
when Joachim Fest's

biography appeared.

The turning-point in the development of post-war West
German historiography
came in the early 1960s, with the publication of the
Hamburg historian Fritz

Fischer's research on the origins of the First World War.
30 Fischer's central

arguments that there was a significant continuity in
German foreign policy

between 1914 and 1939, even between Bismarck and Hitler,
and that foreign policy
emerged from domestic rather than geopolitical
considerations had been
anticipated to some extent in the early 1950s, notably by
Ludwig Dehio, but the
difference now was the presence of a new generation of
historians willing to pick up
this iconoclastic note and develop it systematically.
Fischer's thesis, which was
enthusiastically embraced by his younger colleagues, also
aroused a storm of
controversy among his own contemporaries, and for much the
same reasons:
because he shattered the prevailing academic view that
1933-45 could be safely
cordonned off as an anomaly in Germany's history, and
because he was engaging in a
form of national and professional self-criticism that was
deeply antagonizing to his
more conservative colleagues.
The broad impact of the Fischer controversy on the
reorientation of German
historiography is beyond the scope of this chapter, but it
was crucial in initiating
that process of critical historiographical reflection
which had been evaded by the
German profession in the immediate post-war era. 31 In
elaborating (and
hardening) his position in the course of a widely
publicized and increasingly
vituperative debate, Fischer moved away from his originally
ideological and
diplomatic interpretation and towards a more
socio-structural analysis of causation. In this, he echoed calls already issued by colleagues such as Theodor Schieder, Otto Brunner and Werner Conze for a new institutional and structural approach to historical studies in general, but invested them with a new moral energy that was also highly attractive to the rising generation of young historians.

Fischer's analysis of German foreign policy was thus extended by his students and disciples into a radically revisionist interpretation of modern German history.

30 Fischer 1959. For its significance, see e.g. Iggers 1975: 89ff.

Inevitably, this centred largely on Wihelmine rather than Nazi Germany, yet the approach was massively overdetermined by the desire to explain why National Socialism had been able to come to power in Germany: a project which, in the eyes of this new generation, had been deliberately and culpably neglected by the majority of their teachers. Influenced partly by American models of sociological and political analysis (domesticated in Germany most notably through the work of Rainer Maria Lepsius and Ralf Dahrendorf) and partly by the reimported influence of the emigre generation of German historians, a new and prolific generation of historians began their publishing careers in the 1960s. Including
Hans-Ulrich Wehler, Jurgen Kocka, Hans-Jurgen Puhle, Peter-Christian Witt, and Heinrich August Winkler among others, this group reconfigured the cultural and intellectual interpretation of German uniqueness into a new focus on the peculiar structure and continuities of social and political institutions and practices in modern Germany. 32

Characteristically, the work of these historians focused less on the ideological origins of antidemocratic tendencies than on the medium-term social and political consequences of the German experience of industrialization and modernization. According to this approach, German history has followed since the rise of industrial capitalism a 'special path' or Sonderweg by comparison with the nations of western Europe and the United States. Its development was peculiar in that it proved unable to develop the modern social and political institutions, ideologies and relationships that have normally been concomitant with the transition to industrial capitalism: a politically powerful bourgeoisie, parliamentary institutions, a modern class structure and set of social practices. Though Germany undoubtedly acquired an economically powerful bourgeoisie in the course of the nineteenth century, this class failed to identify itself with liberal social and political values, or to assert its
political leadership over the aristocracy and working classes. Instead, the pre-

industrial economic (agrarian) and social (aristocratic-military) elites continued to

exercise power in Wilhelmine Germany by means of the political repression of the

working class, and the manipulation of the intermediate petty bourgeoisie. In this

way the old elites blocked the development of liberal and democratic institutions,

and saturated society with their pre-industrial authoritarian values. Repression

sharpened social hostilities, so that when the Wilhelmine system finally collapsed in

1918/19, the bitterly antagonistic, mutually fearful and politically frustrated classes

confronted each other directly, without the mediating influence of democratic and

emancipatory traditions or a liberal consensus. The Weimar Republic was thus

saddled with a virtually fatal baggage of anachronistic institutions and expectations,

with which heritage it faced the recurrent climate of crisis in the 1920s and early

1930s. National Socialism was the mass political expression of this unbalanced

socio-political structure, in that it capitalized on the fears of the lower middle

32 Dahrendorf 1965; Lepsius 1969; 1973. For discussions of this literature, see Blackbourn and Eley

1984; Moeller 1983/4; Evans 1985; Iggers 1975: ch. 3. 563

classes caught between traditionalism and the pressures of modernization. In this
sense, as critics of this school have argued, imperial German history was rewritten as the prehistory of National Socialism, and National Socialism was seen as the outcome of a peculiarly German failure to accomplish a balanced process of social and political modernization. 33

This revisionist interpretation of pre-Nazi history as a crisis of modernization developed in tandem with a new historiography of National Socialism that also took shape in the 1960s. German historians themselves were thus recovering the initiative that had been forfeited to emigres and foreigners in the immediate post-war period. In an important sense, from the mid-1960s the writing of Germany's recent history became a shared international project for the first time, engaging British and US historians in particular and presenting a new convergence of research perspectives and objectives. Behind this process stood the momentous generational transition that took place in the 1960s, not only in Germany. Following hard on the emergence of the post-Fischer school of critical historiography came the cohort of the 1960s, the first truly post-war generation, who enjoyed a freedom to question conventions that had been unavailable to their parents. Not least, they insisted on challenging openly the absence of serious public education in the reasons
for the success of the Nazi movement and the impact of the regime on German society. The Eichmann trial in Israel in 1960 and the beginning of West Germany’s first major prosecutions of war criminals in 1964 also stimulated the desire to make up for the deficit of public knowledge and debate. More prosaically, the opening of the West German archives from the late 1950s and the expansion of the university systems in Germany, western Europe, the United States and Canada presented both opportunity and necessity for successive waves of doctoral candidates to embark on intensive monographic research projects. The result was an unprecedented outpouring of academic publications on the history of National Socialism, which began appearing in the mid-1960s and which has continued to the present.

This extension of research was also the beneficiary of an expansion and reorientation of historical perspectives and methodologies which, coming thick and fast upon the international scene in the 1970s and 1980s, influenced Germans and non-Germans alike. The process was initiated by the revival of debate about Marxist theory, which opened up in the later 1960s along with the emergence of a New Left as critical of the legacy of Stalinism as it was of the Cold War rigidities.
of Western politics. Marxist theories of fascism underwent a major process of

33 For a critical discussion of the implication of this argument for the study of fascism, which discusses the work of Kocka and Winkler in particular, see Eley 1989.

34 See e.g. Haug 1967; Uhe 1972. Kiihnl1973 is highly critical of West German textbooks, but does not cover National Socialism as such.

35 See Krausnick et al. 1968, which was originally presented as historical evidence in the 1963 Auschwitz trial; and for German prosecutions of war criminals see Riickerl 1980. 564

recovery and re-evaluation in this period, reaching far beyond the repetitive orthodoxy of official communist analysis. 36 In West Germany, left analyses of National Socialism drew much of their vitality from the claim that fascism was an endemic recourse of capitalism in crisis not least in contemporary West Germany itself, in which many on the Left saw troubling anti-democratic tendencies. These leftist critiques and histories were repudiated by liberal and conservative historians alike, for being too politicized or too abstract, in either sense too distant from the 'real' history of Nazi Germany (see especially Winkler 1978: Bracher 1976). These academic and political debates became very heated, reflecting a tense political climate in which democratic institutions in West Germany appeared to be under threat
though whether from the Right or from the Left depended upon one's perspective.

The revival of interest in Marxist theories of fascism, and the growth of empirical research into the history of National Socialism, were between them bound to put into question the claims of the main existing contender in the theoretical field, totalitarianism theory. This approach had been developed mainly by sociologists and political scientists rather than historians, yet with the eclipse of fascism theory in the 1950s it had presided over early interpretations of National Socialism, notably those advanced by Hans Buchheim and Karl Dietrich Bracher, and the early work of Martin Broszat. Yet having developed in something of an empirical vacuum, totalitarianism theory itself was vulnerable to the corrective onslaught of detailed research into the history of Weimar and Nazi Germany.

Critics of the theory claimed that it depended upon an illicitly retrospective reading of the history of National Socialism from regime back to movement; on an exaggerated estimate of the Nazi regime's capacity for integration and functional rationality; and on a highly selective comparison that reduced Soviet Russia to Stalinism alone, and established too simple a polarity between democracy on the one hand and all forms of dictatorship on the other. In turn, advocates of this model
attacked Marxist versions of fascism theory for subordinating empirical research to distorting political and theoretical premises, and especially for ignoring the substantial differences between National Socialism and Italian fascism especially.

36 For the revival of Marxist theories in West Germany, see, apart from the sources cited in n. 4 above, Rabinbach 1974. Pioneering the new Marxist work on National Socialism was the exchange of articles in the Berlin left periodical Das Argument, 1964-70. Numerous analyses of fascism from the left appeared in the late 1960s to the 1970s: for a sympathetic survey see Kuhnl 1990; for more critical surveys, see Wippermann 1975, and Thamer and Wippermann 1977. Among the representative German texts of the 1960s and 1970s: Kuhnl 1971; Allf 1971; Clemenz 1972; Kuhn 1973; Kuhnl 1974; Backhaus 1976; Pozzoli 1976; Hennig 1977. Poulantzas 1974, originally published in France in 1970, was an influential re-evaluation of Third International theories; see also Caplan 1976. The West German periodicals Geschichte in Wissenschaft und Unterricht, Neue Politische Literatur and Politische Vierteljahresschrift reviewed the new German literature extensively in the 1970s; see also International Journal of Politics, Winter 1972-3 issue on critiques of fascism theory from West Germany.

37 Buchheim 1962; Broszat 1960; Bracher 1955; [1969]1970. 565 the far greater significance of racism in Nazism, as well as the extraordinary political
dynamism of the Hitler-led regime. The renewal of interest in fascism theory was not confined to the Left alone. It received a highly influential reworking in the conservative philosopher Ernst Nolte's phenomenological analysis of fascism as an independent 'metapolitical' movement with a novel revolutionary ideology. Nolte's interpretation turned on his difficult concept of fascism as 'resistance to transcendence': an ideology that was opposed to the practical movement of emancipation and the spiritual striving of 'freedom towards the infinite' offered by Enlightenment tradition. Nolte based his account on a comparative analysis of the Italian, French and German fascist movements, but the philosophical magnitude of his interpretation stood in some tension with his claim that fascism was also, as the German title of his book proclaimed (Der Faschismus in seiner Epoche), an epochal event which was peculiar to the brief inter war period alone. Curiously, Marxist interpretations of fascism were at the same time embarking on a reverse journey: from excessively prescriptive statements about the nature and function of fascism, to a new concern with the historically specific conjunctures of its origins and success. Among the most prominent issues for Marxists was one that, for obvious reasons, had already engaged the Left's attention
in the 1920s and 1930s: the relationship between capitalism and fascism, both theoretically and in terms of industry's contributions to the Nazi rise to power.

Much new Marxist scholarship was devoted to exploring this relationship, often in highly abstract terms that allowed its critics to contend not only that no such relationship could be demonstrated, but also that Marxism as a method was incompatible with the conventions of empirical history. Partly in response to this criticism, and partly because of their own dissatisfaction with excessive theoretical abstraction, historians on the Left took up the empirical research which would test, and potentially support, the validity of the theories. Characteristically, they investigated such key questions for Marxist theory as the class composition of the Nazi movement, the socio-political circumstances of its rise to power, the position of labour under Nazi rule, and the relationship between economics and politics in the Third Reich.

The generation of West German historians who came of age in the 1960s thus inaugurated a remarkable period of research into the history of National Socialism.

38 The debate can be followed in Totalitarismus und Faschismus 1980, also in Kershaw 1967: 20-3, 30-5, and Ayvoberry 1981: ch. 8; for a critique, Mommsen 1971; for a defence, Bracher 1976.
39 Nolte 1966. This was one of a series of studies of fascism by Nolte, the others being Die Jaschistische Bewegungen (1966) and Die Krise des liberalen Systems und die Jaschistischen Bewegungen (1968).


The range of work published in the past thirty years has been enormous, and it is impossible to review it in detail here. 41 Much of it has revisited older themes,

including the social sources of mass support for the NSDAP before 1933, the role of the political and industrial elites in the rise of Nazism and in the Third Reich, the political structure and functioning of the Third Reich, Hitler's role and policies,

ideology and the propaganda system, foreign policy and the origins of the war, the economic system and rearmament, resistance, the legal and judicial system, and

anti-Semitism and the 'Holocaust' the latter began to attract serious attention
from German historian for the first time in the 1970s. By this date, a younger generation of historians, often influenced by the Marxist debates of the 1960s, was also opening up new issues to investigation, such as the social structure of Nazi Germany, the texture of everyday life (Alltagsgeschichte), the experience of marginalized social groups including women, youth and sexual minorities, culture and science under National Socialism, eugenic policies, and the persecution of groups other than Jews and political opponents of the regime.

The debate generated by new work on the political organization of the Nazi regime can be taken as symptomatic of the clash of the different theoretical and political perspectives adopted by those engaged in this research. Led by Martin Broszat and Hans Mommsen, historians subjected the structure and functioning of the Nazi state to intensive re-examination, and concluded that the Third Reich, far from being a totalitarian monolith, was a kind of disorganized 'polyocracy' in which power was in practice fragmented and decentralized, and policy-making was unsystematic and improvised. The much-vaunted Nazi Volksgemeinschaft in fact masked rather than reconciled antagonistic social and political interests; these emerged in distorted and displaced forms, and created a factious and competitive
political system in which rational policy-making was impossible. 42 This claim led to further arguments about the role of Hitler, especially the issue of whether the policies of the Nazi regime were the planned implementation of the Fuhrer's fixed ideology and programme, or whether they arose far less predictably and consistently from the anarchic and friction-ridden way in which the regime actually functioned. 43 This debate between an 'intentionalist' interpretation of Hitler's motives below.

41 The reader is referred again to Kershaw 1989, and Hiden and Farquharson 1989 for more below.


The debate about Hitler is extensively examined in Schreiber 1984; and see Kater 1981.

43 The terms 'intentionalist' and 'functionalist' were put into circulation by Mason 1981. For the intentionalist argument, see Bracher 1979 and 1978; Jackel 1979 and 1984, and the linked studies, Jackel [1969] 1972 and 1988; also Hildebrand 1981. Other critical surveys of the literature from an intentionalist point of view include Hofer 1957 and Hillgruber 1982.
and role, and the 'functionalist' or 'structuralist' arguments advanced by Broszat,

Mommsen and others echoed an older argument between the British historians

A. J. P. Taylor and Hugh Trevor-Roper about the extent to which the outbreak of

the Second World War was the effect of Hitler's deliberately calculated policy. In

its new form, however, the argument came to a climax on the horrific but crucial

question of the extermination of the Jews and Hitler's role in it. The intentionalist

argument is that the 'Holocaust' represented the planned climax of a fixed policy of

genocide conceived by Hitler in the early stages of his political career, and

systematically put into practice under his direction after 1933: from the boycotts,

purges and legalized discrimination of the early years, to the total exclusion of Jews

from Germany's economic and social life by the end of the 1930s, to the deportations, ghettoization and Europe-wide 'Final Solution' of the war years; also that it

was Hitler himself who orchestrated this sequential process. The 'functionalists', on

the other hand, have argued that Nazi anti-Semitic policies were far more ad hoc

than the intentionalist model allows, that the documentary record shows that

different leaders and agencies in the regime took the initiative at different times and

for varied motives, and that the liquidation of the Jews
was neither planned in
advancement of its initiation in mid-1941 nor personally
directed by Hitler. Rather, anti
Semitic policies are seen as mostly improvised responses
to a variety of political
pressures; the move to physical annihilation is
represented not as the barbaric
climax to a systematically planned policy of
anti-Semitism, but as the outcome of a
haphazard process of radicalization which translated
Hitler's metaphors of racial
cleansing into a terrifying reality. 45

This repositioning of an older argument from the conduct of
foreign policy and
war to the project of genocide signalled both the
durability of the major issues in the
historiography of National Socialism and the radically
changing context of
interpretation. The personality and role of Hitler have
continued to occupy a
prominent position in the historiography of Nazism, to an
extent unequalled for any
other 'world-historical' individual except perhaps
Napoleon. And it is the most
destructive and horrifying 'achievements' of the Third
Reich war and genocide
that have attracted lasting attention. But the angle of
vision has tended to shift from
the external policies and relationships of the regime to
its internal organization and
functioning. The evaluation of Nazi racism lies at the
heart of this debate, calling
into question the centrality of anti-Semitism, the primacy
of foreign over domestic

policy and the choice of a comparative framework of
interpretation for National

44 This debate is most easily followed in Robertson 1971,
which reprints some of the major

contributions, but see also Taylor 1961; and see Kershaw
1989: ch. 6, and Hiden and Farquharson

1989: ch. 5.

45 The literature here is of course enormous; a judicious
summary is Marrus 1987b; see also Marrus

1987a, Kulka 1985 and Friedlander 1984a. Intentionalist
accounts include Dawidowicz 1975 and

Fleming 1986. Major contributions to the functionalist
debate include Broszat 1985 (originally

published 1977) a devastating expose of Irving 1977;
Mommsen 1986; Adam 1972; see also the

important mediating works by Christopher Browning: Browning

Socialism. First, recent research has greatly strengthened
the case for seeing racial

politics as definitive of the Nazi regime; yet opinions
differ sharply on whether this

racism was essentially anti-Semitic, or whether it
embraced a broader regime of

eugenic and 'biopolitical' manipulations, involving
assaults on women as a sex, and

on a whole range of 'asocials' and other people judged
'unworthy' of life according

to Nazi criteria: homosexuals, Romani/Sinti, the
hereditarily diseased, habitual

criminals, alcoholics and so on. 46 Second, Nazi racism
can be construed as an effect

of either external or domestic policy, an ambiguity that
allows for very different
interpretations of its logic. Intentionalist historians have generally argued that Nazi
anti-Semitism derived its strength from Hitler's own fanatical ideology, and that its
ultimate logic was to power a war launched by Hitler as a campaign of territorial
imperialism and racial extermination. Structuralists, as we have seen, argue that
racist policy arose essentially out of the internal dynamics of the regime, and that its
evolution was characteristic of the conduct of a system that was functionally as well
as substantively irrational; some also take the view that the decision to go to war in
September 1939 was the outcome of domestic pressures that forced an economically
and militarily adverse choice on Hitler. Third, the extraordinary significance of
Hitler's personal rule, together with the unprecedented enormity of the 'Holocaust',
symbolized by the horror of Auschwitz, make the choice of a comparative
framework very delicate. To compare National Socialism with other dictatorial
regimes could lead to accusations of apologetics, relativization and trivialization; but
to see the regime as somehow sui generis, as so to speak uniquely unique, risks
removing it from historical explanation altogether.
The attempt to reposition the interpretation of National Socialism as a debate
about internal structures and policies rather than the
state and its leading personalities reflects one of the major new paths taken by post-war Western historiography in general, i.e. the turn from broadly political and narrative history to broadly social and analytical history. This was bound to be a hot issue in Germany, given the tenacious historicist legacy of concern with narratives of the state and its leaders, and with the interpretation of political motives by means of insight and empathy.

Intentionalist analysis was recognizably historicist in its concentration on Hitler’s political leadership and the primacy of high politics, especially foreign policy. By claiming too that Hitler and his anti-Semitic ideology were the keys to National Socialism, intentionalists also sought to argue that Nazism was not a variant of generic 'fascism': if anyone was comparable to Hitler, it was the twentieth century’s.


47 See, e.g. Hildebrand 1977 and Hillgruber 1972; the
debate is surveyed by Thies 1978.

48 See Mason 1975 and 1995. 569

other totalitarian dictator, Stalin. 49 (Hence Ernst Nolte's argument, to be discussed below, that the murderous results of Stalinist policies anticipated the extermination of the Jews: Stalin and Hitler become comparable as equal architects of genocide.)

The questions of the uniqueness of National Socialism, of Hitler's role in its history and of Germany's path towards it thus continue to occupy an absolutely central place, posing great challenges to contemporary historians, and repeatedly returning arguments to the topography of historicism.

The post-Fischer school of historians had, as we have seen, no argument with the view that modern Germany had undergone a special process of development that set it apart from the model of Western industrial capitalist democracy.

Whatever the merits of this approach, it did function as an explanation of the course of German history up to 1933; on the other hand, it left unanswered the question of the status of German history after the end of National Socialism. At the same time it displaced to an unspecified future the issue of whether an eventually 'normalized' post-Nazi history would also permit the renormalization of modern German history as a whole: a re-evaluation, in other words, of the
'peculiarity' of German history. That time came in the mid-1980s, when, fifty years after the Nazi seizure of power, a concerted attempt began to reconfigure the position of the Third Reich in the history of contemporary Germany. As on previous occasions, this was as much a political as an academic debate. In political terms, the mandate in the earliest years of the Federal Republic's existence had been to legitimize and anchor a newly constructed constitutional identity, under the reassuring rubric 'Bonn ist nicht Weimar' Bonn is not Weimar. Then the opening-up of West Germany's political and intellectual life in the late 1960s and 1970s led to an intensive engagement with the history and legacy of National Socialism, motivated in part by the belief that only by confronting this past could West Germans develop a critical perspective on their own national identity and, as some believed, resist the renewed threat of fascism. But the late 1970s witnessed another so-called Tendenzwende, or change of direction, in West German political life, moving the centre of the spectrum rightwards as the decade of reformism faltered and lost authority. The resulting debate among intellectuals was enacted as quintessentially a confrontation with Germany's past; and this Vergangenheitsbewältigung (‘mastery of the past’) was to
be construed, in Hans Mommsen's telling argument, not simply as a moment of catharsis or a single act of historical enlightenment, but as a continuous process of 'confrontation with the causes and operating mechanisms of fascist domination' (quoted in Habermas 1984: xxiii). Mommsen's injunction implied that the Germans would never be 'rid' of their Nazi past, and that any refusal to face this fact carried risky implications for the political present. It was against this image of a past that eternally dominated the present that reaction gathered in the 1980s, breaking out into a glare of publicity in the so-called Historikerstreit, or historians' dispute, in 1986. 50

What sparked the most intense phase of public debate was a press article by Ernst Nolte that suggested that Auschwitz was essentially a technically improved successor to the 'Asiatic deeds' of extermination carried out by the Turks against the Armenians and by the Bolsheviks in the collectivization campaign and the purges.

This was not the first time Nolte had made these arguments, which consummated his retreat from fascism theory by equating Nazi genocide with Stalin's 'Asiatic deed'. But, as with the Fischer debate twenty years
earlier, it was the existence of a receptive context that propelled the issue into public controversy. Nolte’s ideas seemed consonant with reinterpretations of Nazi history then being advanced by other conservative historians, notably Michael Sturmer (1988) and Andreas Hillgruber (1986; Hildebrand 1987). A raging dispute erupted which engaged virtually all the leading historians in the field, and was conducted through the newspapers and popular publications as well as in the specialist scholarly press.

Essentially, it turned on two issues: the uniqueness of Germany’s history, and the question of whether it could be made into a 'usable' past usable, that is, in the construction of a sense of national continuity and historic German identity. Should Auschwitz, for all its distilled horror, continue to be the bench-mark against which Germans measured their national consciousness? Against Mommsen’s argument that 'mastering the past' was a continuous process, Sturmer contended that 'We cannot live by making our own past ... into a permanent source of endless guilt feelings' (quoted in Evans 1989: 103-4).

The Historikerstreit revealed the strength of the historicist tradition in Germany, the durability of basic epistemological questions about interpretation and explanation, and the intricate and passionate inflection of
history and politics in West Germany. But its undoubted public political prominence was not entirely matched by its historiographical significance. In terms of its direct contribution to the agendas and methodologies of historical research, as opposed to political argument,

50 The background to open controversy included the Bitburg episode, discussions about a new German museum of history, and a newly acceptable consensus among conservative politicians and commentators that Germany had spent enough time regretting its guilty past and now deserved to develop a new sense of national identity and pride; see Hartman 1986; on the museum issue, see Heuser 1990. There are two excellent accounts of the Historikerstreit in English: Evans 1989, which offers a clear and comprehensive reading of the historical and political issues and a useful bibliographical essay, and Maier 1988, which takes a more philosophically reflective approach; see also Kershaw 1989: chs. 8 and 9, for a briefer but valuable summary. Baldwin 1990 is an important collection of German and English essays, but from the anti-Nolte side only; for Nolte's original essay, see Nolte 1985; but otherwise most of this literature remains untranslated. See also Habermas 1989; for the political background, see Eley 1988. Among the more important German contributions and surveys, see Backes 1990; Broszat 1987; Diner 1987; Habermas 1987; Hennig 1988, 1987; Meier 1987; Nolte 1987 and 1988; Senfft 1990; Wehler 1988.

51 For a careful evaluation of Nolte's arguments, and a
discussion of his shift from Nazism/fascism
to Nazism/Bolshevism comparisons, see Evans 1989: ch. 2.
the debate was somewhat sterile; it revisited ground that had already been opened
up by historians and it lacked the energizing intellectual quality of the Fischer
controversy twenty-five years earlier. 52 The major antagonists were members of the
now dominant generation of historians born in the 1920s and 1930s for the most
part who had already spent lifelong careers in the study of German history (the
same had been true of contributors to Habermas’s important 1979 conspectus of
intellectual trends). To be sure, younger historians were far from silent in the
Historikerstreit, yet their perspectives were often at odds with those of their senior
colleagues, and the scholarly and political divisions were multiple rather than
simply dualist.
The dominant generation of German historians had been formed in the context
of the Fischer debate, which had pitched historicism against structural analysis in a
first confrontation between West Germany’s political and intellectual inheritances.
But the younger generation had grown up among the diverse new political
movements of the 1970s and 1980s leftism, feminism and sexual politics, the civic
initiative and the green movements and many became
politically and academically

interested in topics quite foreign to that older confrontation. One particularly

striking and innovative result has been the recent convergence of academic and

popular interest in history from below, women's history and the history of everyday

life (Alltagsgeschichte). These new departures in a traditionally elitist historical
culture have been strongly influenced by the British history workshop movement.

They naturally raise special questions about the responsibility of a popular history

of Nazism not simply to pander to public and commercial tastes for entertainment,

as allegedly was the case with popular responses to the 1970s 'Hitler-wave' and the

TV series Holocaust and Heimat. The reception of women's history is also

problematic in an academic culture which remains notoriously inhospitable to

women, who occupy a mere handful of university positions, and who have

developed this field virtually without support from the academy and largely outside

it. And Alltagsgeschichte has raised the ire of the previous generation of radical

revisionists, who see in it a risk that the enormity of National Socialism will be

trivialized if subjective individual experience is granted as much authority to

generate 'meaning' in history as the reflected knowledge and theorized explanations
This point is made in Childers and Caplan 1992: Introduction.

For these new trends, see Crew 1989; Hull 1989; Jarausch 1989; Gerstenberger and Schmidt 1987; Niethammer 1983-5; Peukert 1986; 1987a; 1987b. The work of Martin Broszat has been the crucial crossover between generations here, notably the major project on the history of Bavaria under National Socialism led by him under the sponsorship of the Institut für Zeitgeschichte (1977-83); see also the essays in Broszat 1988, and his contributions to Baldwin 1990: 77-134.

See the special issues of New German Critique on the 'Holocaust'-reception, nos. 19 and 20.

See Hull 1989. The absence of women is registered for instance in Schulze 1989, whose ten-page index of names lists not a single substantive reference to a woman historian; further, its unremarkable ness is attested in Weber 1984, a sociological analysis of the profession that ignores the category of gender. For a relatively sympathetic discussion of the issue, see Puhle 1981. 572 produced by the traditional professional historian. 56 At the same time, the practitioners of the new social history (among others) are troubled by the latest 'postmodernist' turn in intellectual circles with its implications disturbing to the Marxist and the liberal traditions alike for a retreat from objective rationality and grounded explanation. These newer questions about the claims of subjectivity and representation emerged first out of a largely US intellectual debate, and while the
philosophical issues are familiar in Germany, their historical dimensions remain to be explored. 57

Prominent in these recent debates are two major challenges to the writing of German history which are likely to remain unanswered in any final sense for the immediate future: how the country's Nazi past can be 'historicized' without normalizing or trivializing it; and whether current critiques of Enlightenment rationalism and its regime of truth are compatible with the critical assessment of this most 'irrational' of political ideologies. Neither of these issues on its own is peculiar to the history of National Socialism: 'historicization' in a neutral sense is the inevitable product of all historiographical work, while philosophical debates about the criteria of historical truth are scarcely new in themselves. Yet we should not be surprised that their bearing on the history of Nazi Germany has become so problematic at just this moment. The passage of time, in bearing away Nazism's last surviving witnesses, leaves behind a new field for historical interpretation. For some, this change threatens to abandon the meaning of Nazi history to those who would apparently revise or deny the truth of lived experience. 58 For others, it is precisely this inevitable shift of perspectives that is posing new historical challenges.
that can neither be forestalled nor ignored, however onerous the responsibility of meeting them. German Democratic Republic

The development of the historiography of National Socialism in the DDR shows some parallels with that in West Germany, but rather more differences. First, as Gunther Heydemann has pointed out, there was for long periods an unequal relationship between the two historiographies: West German historians virtually ignored their East German counterparts until the 1970s, while the East Germans


57 One exception is Niethammer 1989; see also Caplan 1989; Hull 1989 and Jarausch 1989; some comments in Maier 1988: I 60ff. The issues are most directly addressed in Friedlander 1992;

Friedlander 1984 now appears as a forerunner to this debate. See also Lyotard 1988. It is not accidental that the two most public controversies on this issue the cases of Paul de Man and Martin Heidegger turn on the problem of National Socialism: see de Man 1988; Hamacher et al. 1988; Derrida 1988, and the symposium of responses in Critical Inquiry 15(4) (Summer 1989): 765-874; Farias 1989;


58 For the problem of 'revisionist' history proper, i.e. the denial of Nazi genocide as a historical event, see Vidal-Naquet 1989 and 1992. 573

engaged in a continuous though inevitably one-sided polemic with West German
historiography. Second, both historiographies took time to develop in new
directions after the end of the war, but their characters differed, and so did the
periodization and sources of change. Third, although these shifts in historiographi
cal disposition were related to broader changes in the political and intellectual
climate in both countries, the linkage to the political was looser in West than in East
Germany, where the political leadership established the goals and frameworks for
historical research at least into the 1970s. Finally, the Nazi period was the
overriding issue in DDR historiography, occupying a status that reflected the
importance of antifascism in defining the logic and character of the new state. For
both these latter reasons, the historiography of National Socialism occupied a more
public and dominant role in the DDR than in the Federal Republic.
The tendency to dismiss all DDR historiography as the polemical expression of
an undifferentiated and unchanging political dogma has fortunately been replaced
by a more nuanced sense of the relationship between the political/theoretical line set
by the SED (Sozialistische Einheitspartei) and the academic profession. With some
differences of detail, commentators on DDR historiography agree that there were
three distinct phases of development between 1945 and the
1980s: a post-war
transitional period between 1945 and 1948/9; a period of political construction and consolidation up to the end of the 1960s; and a period beginning in the 1970s in which a degree of professional independence was achieved and debates internal to the discipline began to emerge. 60

The immediate post-war period, preceding the constitution of the DDR as a state, saw comparatively little direct political intervention into academic historiography, and not much systematic research into contemporary history either, given the more pressing tasks of institutional and curricular refoundation. Inevitably, the historical profession was still dominated by non-Marxist academics, and an antifascist consensus rather than a specifically Marxist orientation was the order of the day. Political and intellectual leaders tended to the view that Germany's history was so negative as to be virtually useless to the project of socialist construction (Heydemann 1987: 18). To a greater extent than in the West, historians and other academics in the Soviet zone conceded not only Germany's responsibility for the war, but also the fatality of a German past that had culminated in National Socialism; in this sense, their views were closer to those of their non-German than their German colleagues. 61
This rather open situation dissipated once the DDR had been founded as a state,

and from the beginning of the 1950s through to the mid-1970s central direction by


Introduction. For surveys by DDR historians, see Berthold et al. 1973; Lozek 1989; Bramke 1988.

60 This follows Heydemann's (1987) periodization; Iggers offers a similar three-phase process, while Jarausch and Kuppe suggest five roughly decadal phases.

61 For examples of early publications, see Lozek 1989: 200-2. 574

the political leadership was far more visible. The SED claimed, in the name of

Marxism-Leninism, a monopoly of intellectual leadership; non-Marxist academics were eliminated from the universities as far as possible, and compulsory courses in
dialectical and historical materialism were instituted. 62 In 1951 the party, after consultation with members of the historical profession, declared the opening of a

new phase of ideological-political struggle: in a speech to the October Central Committee meeting, Walter Ulbricht proposed in effect a dual reading of German history, leading up to the divided nation of the present. The Federal Republic was represented as the embodiment of the reactionary element in German history,
carried by Junkers, industrialists, militarists, anti-Semites, fascists and the like.

Against this stood the positive and progressive strand in German history, identified with the peasant wars, the war of liberation, 1848, the origin of the working-class movement, the foundation of the KPD and the anti-fascist resistance, and culminating in the foundation of the DDR as the first German socialist state. The SED initiative obviously reflected the intensification of the Cold War, and set the scene for a growing organizational and intellectual divergence between the professions in East and West (the East Germans left the Verband der Historiker to found their own association in 1958, after some years of increasing friction). It was accompanied by other specific moves to establish a new historical tradition which would assist the legitimation of the East German state. These included an official injunction against historicism, and the substitution of historical materialism as the theoretical underpinning of historical scholarship; the establishment of a museum of German history in Berlin (opened in 1952), of historical institutes at the universities in Berlin, Leipzig and Halle, and of the Deutsche Akademie der Wissenschaften; the decision to embark on new canonical histories of the German people and of the German working-class movement; and the inauguration (in
1953) of the Zeitschrift der Geschichtswissenschaft as the journal of record for the DDR historical profession (Lozek 1989: 202-7). Through the 1960s, most scholarship was carried out in these research institutes by collectives of historians charged with the production of definitive official texts a structure that was not conducive to debate, innovation or resistance to the official line.

As far as East German research on National Socialism was concerned, these were the years of domination by 'Stamokap' theory, and of maximum polemic with Western scholarship.63 The presiding concept of fascism in the continuing era of state monopoly capitalism was taken from Dmitrov's classic formulation in his 1935 speech to the 7th Comintern Congress, as 'the open terrorist dictatorship of the most reactionary, most chauvinistic and most imperialist elements of finance capital' (Dmitrov n.d.: 40). The success of National Socialism was represented as the effect of the interests of monopoly capitalism, an approach that focused attention less on


63 'Stamokap' is the German contraction of 'state monopoly capitalism', i.e. the existing stage of capitalist development. For critical surveys of DDR theory, see Thamer 1987; Dorpalen 1988: ch. 8;

Jarausch 1991. 575
the form of the Nazi dictatorship than on the junction it performed for capital.

Consequently, a strong element of continuity was asserted between Weimar and the Third Reich, and between the Third Reich and its alleged successor state in West Germany. The major themes in DDR historiography were the relationship between the state and economic interests before and after 1933, the role of the antifascist resistance, and the political economy of the Second World War. Each of these topics was the subject of monographic publications which drew on, and sometimes reprinted, documentary sources not readily available to Western scholars (see e.g. Eichholtz and Schumann 1969; Eichholtz 1969; Eichholtz 1974; Bartel 1956). By contrast, topics which depended upon a different conceptualization of the relationship between the economic and the political than that offered by historical materialism, or which treated social history as an autonomous sphere, were largely neglected: these included important themes which were beginning to attract Western scholars in the 1960s, including the precise trajectory of the transition from Weimar to the Third Reich, the internal political structure of the Nazi system, the centrality of racism and genocide to Nazism, and the structures of social integration and control in the Third Reich. For more
contingent political reasons,
some specifically difficult topics such as the Nazi-Soviet Pact, or the full complexity of resistance and collaboration after 1933, were also barred from critical assessment. Research was also designed to refute the claims of West German 'bourgeois' historiography, in a context in which genuine debate was, unfor
tunately, rare on either side. 64

The recovery of disciplinary independence and the emergence of internal
historiographical debates were products of the consolidation of a politically secure historical profession in the universities in the 1970s, as well as of shifts in official political ideology. The SED was obliged to acknowledge the fact that the rigidity of the party's ideological stance had so sterilized official history that it was neither creative nor popular, and was forfeiting the legitimatory political role it had been assigned. The party's 7th congress in 1967 established the new goal of creating a 'socialist consciousness', and this inaugurated a new period of debate about the tasks of history a debate which, as Gunther Heydemann (1987: 21) has pointed out, reinvigorated the concept of debate itself as a scholarly method. At a more remote but extremely important level of political determination, the succession of Erich Honecker to the DDR leadership in 1971, and the opening of
a new era of

East West detente, marked a highly significant shift in the DDR's self-representation. Politically, this was signalled in an abrupt and unexpected
declaration of German national unity at the SED's 8th party congress in 1971, and
decisions acknowledged the reality that the DDR was not to be 'a brief transitional
phase' (Fulbrook 1989: 199) before the establishment of a socialist state on all

64 The official Zeitschrift for Geschichtswissenschaften carried critical reviews of West German
publications. The contributions by DDR historians to the fascism debate in the West Berlin journal
Das Argument in the 1960s were unusual; see also Czichon 1967. 576

German soil, but that it was a nation in its own right, with its own future and its
own history. The meaning of nation was premised on a shared experience of class
and social structure, rather than on language and ethnicity. The dual heritage of the
German past that prior orthodoxy had assigned on the one hand to the BRD (seen
as reactionary) and on the other hand to the DDR (seen as progressive) was
reconceptualized, and the hindrances to the achievement of socialism in the DDR
were no longer represented as residues of a soon-to-be-superseded past. Instead, the
complexities and contradictions of German history were conceded a constructive
status within the history and prehistory of the DDR itself, which in this sense could now claim all German history as its own. Cast into the mould of a semantic distinction between 'Erbe' (heritage: the undifferentiated historical past) and 'Tradition' (the selective elements of 'our' national identity), this history now became the source for a new construction of national identity which embraced Luther, Frederick the Great and Bismarck, as well as Marx, Engels and the SPD.

As Georg Iggers has pointed out, the Erbe / Tradition debate was not in itself methodologically innovative, for it exhibited more associations with traditional German historiography than with the genuinely new trends then gaining ground in the West. Yet it did encourage an expansion of the range and depth of historical enquiry, including into National Socialism. With the admission of previously discounted points into the historical compass, it became possible to explore seriously for the first time topics such as the role of the non-communist resistance, including its bourgeois and conservative participants, or the history of racial persecution under National Socialism (see e.g. Bramke 1988, and refs. therein; Bramke 1989; Buuck 1988; Finker 1989; Groehler and Drobisch 1989; Drobisch et al. 1973; Petzold 1983). The wider sweep of this more recent historiography was
well illustrated in the important volume
Faschismusforschung, edited in 1978 by two
of the senior historians of the period, which included
essays on Nazi ideology, the
Nazi state, anti-Semitism and genocide, and comparative
fascism. By the 1980s,
contacts between DDR and Western historians were becoming
closer and more
frequent, with joint conferences and seminars and a growing
sense of dialogue. As a
result partly of this, and partly of the internal move
towards a more popularly
accessible historiography, DDR historians began to take up
some of the innovative
approaches that had been at work in the West since the
1970s, notably Alltags
geschichte. Yet this growing interest in social history
was most characteristic of
nineteenth-century studies, and was only just beginning to
make headway into the
65 Discussions of this important and ramified debate can be
found in Iggers 1992: Introduction;
Jeismann 1988; Kuttler and Schleier 1983; Meier and
Schmidt 1989 the latter two titles are by DDR
historians.
66 Eichholtz and Gossweiler 1980; see also e.g. Petzold
1983, and Gossweiler 1986. For a critical
review of this literature, see Piitzold 1992.
67 For reviews of the progress of social history, see
Iggers 1992: Introduction (where the range of
contacts between DDR and Western historians in the 1980s is
also briefly reviewed on p. 36); Kocka
Nazi period when the DDR collapsed. Feminist history, the history of sexuality, the new history of eugenics, the reconceptualization of the relationship between power and ideology: these most recent tendencies in Western history had encountered few parallels across the border before the unification of the two Germanies coercively transformed the context of scholarly exchange.

It is easy enough to point out that the late 1980s political revolutions in the Soviet Union and central Europe, and the unification of East and West Germany, have transformed the tasks and conditions of research into the history of National Socialism. Less simple is to predict what the results will be, but I will offer a few hostages to fortune. First, the specific history of Weimar and Nazi Germany is likely to lose some of its immediate interest as the 'lost' history of the last forty-five years claims a greater share of academic and popular interest a trend that will be strengthened by the passing on of the last generation with direct personal experience of the 1930s and 1940s. This too is likely to reinforce an already visible tendency for more research questions to emerge out of the process of scholarly exchange as such, rather than from political and moral agendas, and this will be more practically
effective in 'historicizing' the Nazi past than was possible by means of the agonized debates of the 1980s. The history of eastern Germany and eastern Europe under National Socialism will be reconsidered as new archival sources become available, and this may generate new conclusions about the nature of the German imperium in the East, the process of German military defeat, the liberations and the experience of the Germans under the advancing Allied armies. This is likely to lead to new insights into the relationship between wartime and immediately post-war Germany.

As research into the history of the DDR and of the other communist states expands, the nature of totalitarianism will also be opened to scrutiny once again. This will no doubt stimulate renewed debates about the generic concept of totalitarianism: to what extent should it be based on the Soviet model, and what would be the relationship between such a model and the nationally specific histories of the DDR, Poland, Hungary, Czechoslovakia and the rest? As a result, fascism theory too may well undergo a revival and revision, especially given the grim likelihood that nationalist movements and regimes in the new Europe reveal their own dictatorial, racist or imperialist tendencies. Indeed, as I write, the brutal campaigns of 'ethnic cleansing' being carried out upon the body of the ex-Yugoslavian state are already
68 Jacobeit 1992 is a rare example of this.
70 Let Martin Broszat (d. 1989) speak for his generation: 'If! myself had not been a member of the generation of Hitler Youth, if I had not lived through its very specific experiences, then I probably would not have felt such a need after 1945 to confront the Nazi past so critically' (Baldwin 1990: 122).

being widely compared with anti-Semitic genocide, and numerous European countries, including Germany itself, yield horrifying examples of racist and nationalist intolerance. And as the 'developed' world faces, or more likely tries to evade or resist, the twin challenges of world economic inequalities and new patterns of international population movements, the equally grim precedent of the Nazi state's management of its own surplus populations may, as Hannah Arendt feared, Abraham, D. [1981] (1986) The Collapse of the Weimar Republic, 2nd rev. edn, New York.
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From the fall of Mussolini in 1943 to the dramatic political changes of recent years, debate on Italy's future has been driven by reinterpretation of its past. Fascism has been the single most important issue in these debates even when it has not been addressed directly, and much of the debate on unification and the liberal state has revolved around the search for the roots of fascism and the collapse of Italy's parliamentary democracy after the First World War.

This chapter will chart briefly the ways in which these debates have developed, with particular reference to the contributions of non-Italian (especially British and American) historians. As a broad generalization, in the first two and a half decades after the fall of Mussolini the concern was to set fascism in the context of what had preceded it. More recently the focus has shifted to relate fascism to what came after, not least because as Italy began to acquire a history since the fall of Mussolini it has made increasingly less sense to assume as earlier studies necessarily did that 1943 had been the end of an era in Italian history.
Historiographical shifts are not easily pinned down to neat dates, but in this case
1968 and the wave of protests against Italy's post-war political system are more than
a benchmark. Until then, at least some of the optimism that had accompanied the
founding of the Republic survived, and with it the notion that the collapse of the
regime, the struggles of the Resistance and the founding of the Republic had
marked a new beginning anno zero, 'Year Nought'. But the political and cultural
climate in Italy changed significantly in the 1960s and 1970s. On the Left,
Stalinism was losing its appeal, while the papacy of John XXIII and the first
openings towards the Left signalled profound changes. The decade ended with the
protest movements of 1968 and 1969. The terrorist attacks inspired by the extreme
Right that began with the Piazza Fontana bombing in Milan in 1969 in turn triggered the response of the Red Brigades and the spiral of violence that gripped
Italian society throughout the 1970s.
It was in this climate that Italian historians began rethinking the longer-run
patterns in Italian history, posing new questions and challenging formerly
persuasive interpretations and models. New perspectives in historical research and
writing played a part in this, as did an awareness that the problems of Italy's post
war democratic Republic seemed to have little to do with economic failure. Indeed,

post-war Italy had experienced perhaps the most dramatic economic and social transformation of any western European state. This meant that the emphasis on

Italy’s relative backwardness that permeated much of the earlier debates on Liberal Italy and fascism became questionable too. In this new climate, the continuities

between fascist Italy and post-war Italy began to attract closer attention. POST-WAR PREMISES: FASCISM AND THE RISORGIMENTO

History and politics were inseparable in the painful process of political reconstruction in Italy after the fall of fascism. Each of the political parties that had been part of the antifascist Resistance and participated in the founding of the Italian Republic in 1946 sought to establish its particular identity and democratic legitimacy by appealing to the past. Gaetano Salvemini, a distinguished antifascist exile, former militant socialist and a professional historian, opened a debate in the constitutional Consulta in 1946, for example, with the question ‘Was Pre-fascist Italy a Democracy?’ This set an agenda for historical debate for over a decade that focused on the political programmes of Giovanni Giolitti, who had dominated Italian politics in the decade before the First World War. Had Giolitti been working to strengthen
Italian democracy by introducing welfare and franchise reforms? Had his overtures to the Socialist Party been genuine or merely a new variant of the traditional cross party gerrymandering known as trasformismo? How did the invasion of Libya in 1911 and the introduction of universal suffrage fit into this picture? Had Italy's entry into the First World War in 1915 against the wishes of the parliamentary majority signalled the collapse of parliamentary government in Italy even before the post-war crisis? Had fascism, in other words, overwhelmed a 'democracy in the making', or was liberal democracy in deep crisis well before the advent of Mussolini's blackshirts?

Arguing that, despite Giolitti's unprincipled and corrupt management, Italian political institutions had in these years become more democratic, Salvemini was endorsing the reformist strategies of the pre-1914 Socialist Party although at the time he had been amongst their most bitter critics. In one of the first English language studies to appear after the fall of fascism the Italian-American scholar A. W. Salamone supported Salvemini's interpretation. The Communists naturally disagreed, and their leader Palmiro Togliatti argued that although Giolitti had been a genuinely progressive bourgeois liberal, his attempts at reform had been blocked
by Italy's backward and reactionary capitalist classes. Togliatti's purpose was of

course to discredit the reformist policies of the Socialist Party (PSI) earlier in the 592
century while endorsing the revolutionary programme advocated by the Commu

ists (PCI) who had broken away from the PSI in 1921.

More moderate and conservative liberals took their lead from the philosopher

historian and antifascist of international renown, Benedetto Croce, whose History of

Italy from 1870 to 1915 (1929) had refuted Mussolini's claim that fascism was the

true heir of the Risorgimento. Arguing that fascism was simply a 'parenthesis', a

phenomenon without roots in Italian history and a product of the terrible upheavals

and trauma caused by the First World War, Croce emphasized the achievements of

the Liberal state, which he believed had reached its highest levels in the years of

Giolitti's political supremacy before 1914.

The debate took on wider dimensions when the prison writings of Antonio

Gramsci were published. A co-founder of the Italian Communist Party and one of

the major Marxist thinkers of the twentieth century, Gramsci's reflections on the

Risorgimento were written in fragmentary notes to avoid detection in fascist jails.

For Gramsci, the struggle for Italian unification had resulted in a 'passive' or

'missing revolution' ('rivo/uzione mancata') because
Italy's small and immature bourgeoisie of landowners and professionals had been unable to carry through a political revolution on their own. To overthrow the absolutist Italian rulers, Gramsci argued, the middle classes were forced to make an alliance with the reactionary Piedmontese monarchy and its army (Gramsci 1971).

Italy's relative backwardness in economic and social terms also explained why the Risorgimento radicals had failed to offer a democratic alternative. Lacking a significant following among the middle classes, in Gramsci's view the radicals could only have created a social base by harnessing the force of peasant unrest in Italy. That would have meant adopting reforms to meet the peasants' demand for land, something that Gramsci acknowledged the radicals were unable to do because it would have threatened their own class interests. Cavour's more moderate political programme was, therefore, unbeatable. But for Gramsci the alliance between the propertied classes and the reactionary Piedmontese monarchy on which Cavour's strategy was based compromised Italy's liberal revolution from the outset in ways that hindered the development of modern capitalism, left the working classes as well as the bourgeoisie divided and hence opened the way to fascism.
The historical debate on the strengths and merits of the rival radical and moderate political programmes in the Risorgimento moved back to the reform movements of the Enlightenment in Italy. Croce had always insisted that nineteenth-century moderate liberalism was heir to eighteenth-century reformism, while socialist and communist historians now looked to the more radical figures of the Enlightenment for the origins of an alternative Italian democratic tradition. But this field was soon to be dominated by the towering intellectual figure of Franco Venturi, who warned against simplistic attempts to turn the Enlightenment into an intellectual prehistory of nineteenth-century Italian politics. For Venturi, the openness and richness of the intellectual, political and economic debates of the eighteenth century were symptoms of the disintegration of the ancien regime world. This was a unique moment of cosmopolitan enquiry that moved backwards and forwards between abstract ideas and practical proposals and across territorial and dynastic frontiers. Venturi’s decision to bring his studies to a close before the French Revolution underlined his conviction that no subsequent political movement or philosophy could claim to be the sole ‘heir of the Enlightenment’ (Venturi 1971; 1972).
The first major reworking of Croce's defence of the Risorgimento came from Rosario Romeo's study of the Risorgimento in Sicily (Romeo 1954). Romeo's central theme was that Sicily had been too backward to achieve political or economic progress unaided in the nineteenth century. Neither the peasants nor the middle classes had sufficient influence or vision to formulate new political programmes capable of commanding broad support. For Romeo, only the inspiration provided by the liberal values inherent in the broader movement for Italian independence and unity had made it possible for Sicilians to overcome Sicily's social and material backwardness.

In his later and monumental three-volume biography of Cavour (Romeo 1969-84), Romeo elaborated this defence of the progressive and constructive force of Risorgimento liberalism. Understanding that liberalism offered the only alternative to the political extremes posed by the Jacobin legacy of the French Revolution and the equally destructive reactionary and absolutist politics of the Restoration, Cavour came to accept that constitutional government was the essential premise for political stability on the Italian peninsula. Since a constitutional monarchy proved difficult to consolidate within the narrow confines of the
Kingdom of Savoy, above all because of opposition from the Church, Cavour's
determination to press forward with liberal reforms led him to seek the wider
support of the propertied classes of northern and central Italy and hence to adopt
the nationalist cause against Austria. Refuting categorically the claim that the
Liberal state had been flawed from birth, Romeo insisted that Cavour's policies
were the necessary prerequisites for Italy's political, economic and cultural
modernization: the falling-off had come later and had resulted from the failure of
Cavour's successors to understand and uphold the original values of Italian
liberalism.

Denis Mack Smith (1954) was the first British historian to enter this new debate
with his study of the relations between Garibaldi and Cavour. This was conceived as
a critique of the heroic terms in which G. M. Trevelyan had depicted the struggles
for Italian unification for British audiences at the beginning of the century
(Trevelyan 1907; 1909; 1911). For Trevelyan, the struggle for Italian independence
had been one of the great, if not the greatest, achievements of nineteenth-century
liberalism and individualism. But that interpretation was discredited by fascism,
and Mack Smith's study told a very different tale. As A. J. P. Taylor noted, 'with
brilliant, though well-founded perversity, Mr. Mack Smith turns everything upside

down' (Taylor 1967: 83). Although sharing Trevelyan's admiration for Garibaldi,
Mack Smith contrasted Garibaldi's idealism with the wily but more effective
Realpolitik of Cavour. For Mack Smith, Cavour's triumph in 1860 fatally
compromised the liberalism of the new state and in his Italy: A Modern History 594
(1955) he drew out the broader consequences. In Cavour's misuse of parliamentary
procedures Mack Smith saw the origins of the weaknesses of Liberal Italy and
continuities that linked Cavour to Mussolini.
Mack Smith's study spoke to two quite different audiences. For British readers, it
explained how Italian fascism was rooted in the failings of the Risorgimento, while
for Italians it offered a coherent critique of Italian liberalism that was not tainted by
Marxism. In Italy, however, Mack Smith's most severe critics most notably
Rosario Romeo were on the Right not the Left. But his work also drew responses
from non-Italian historians and in 1968 Christopher Seton-Watson published a
detailed history of Italy from liberalism to fascism that challenged Mack Smith's
analysis at every turn. Arguing that Italy had succeeded in strengthening its liberal
institutions and politics at least until the crisis after 1918, Seton-Watson's
conclusions came closer to those of Salve mini and Croce (Seton-Watson 1968).

While other British historians turned to examine the ways in which British diplomacy shaped Italian unification (Beales 1961), the debate on the politics of the Risorgimento was also taken up by American scholars. They were influenced less by Trevelyan than by earlier American studies such as the pre-First World War study of Cavour by J. A. Thayer but above all Kent R. Greenfield’s remarkable 1934 study of economics and liberalism in Lombardy in the Risorgimento (which was republished with an introduction by Romeo in 1965).

Greenfield’s book was written as a critique of the Marxist claim that Italian nationalism was linked to the emergence of a new capitalist bourgeoisie, and showed that the demands for political reform and independence from Austria had come from intellectuals rather than from the Lombard commercial and entrepreneurial classes.

The study of Austrian administration in Lombardy after the Restoration of 1814 was taken up by R. J. Rath (1969), who showed how the exclusion of the Italian propertied classes from power soured relations with the Lombard patriciate. In his important study of the Italian National Society, Raymond Grew (1963) also examined the ways in which the propertied classes of Northern and Central Italy
influenced the political outcome of the struggle for independence. The National Society was founded in 1857 by Cavour's supporters in central Italy to prevent the radicals gaining control of the Italian Revolution. Grew showed how the 'success' of its conservative supporters 'spoiled' the broader democratic ideals present in the struggles for independence. The radicals and their programmes were the subject of three books by Clara M. Lovett, which included biographies of Carlo Cattaneo (1972) and Giuseppe Ferrari (1979), the leading advocates of federalist solutions for an independent Italy, and a more general study of the social and cultural back ground of the Risorgimento radicals and their destinies after Unification (Lovett 1982).

Interest in the political programmes of the Risorgimento radicals also drew attention to the ideas of Antonio Gramsci, and following the pioneering work of included H. Stuart Hughes's (1958) masterly study of European intellectual history in the late nineteenth century, a useful brief essay by James Joll (1977) and more recently detailed studies of Gramsci's political thought by Joseph Femia (1981) and Walter Adamson (1980). Two studies of the Italian revolutions of 1848 were directly inspired by Gramsci's notion of the 'missing revolution'. Taking another of
Trevelyan's subjects, Manin and the Venetian Revolution of 1848, Paul Ginsborg (1979) argued that Manin's leadership of the Venetian Revolution well illustrated Gramsci's analysis of the political weakness of the radicals. Ginsborg's claim that the refusal to arm the peasants and adopt a programme of agrarian reform had wrecked Manin's attempts to join forces with the constitutional regimes in Milan, Turin, Florence and Rome was contested, however, by Alan Sked (1979) who approached the revolutions from an Austrian perspective. David LoRomer (1987) adopted a similar Gramscian analysis in his study of the 1848 revolution in Livorno. MOVING FORWARD: LIBERALISM AND MODERNIZATION The debate on Liberal Italy that began with the post-war exchanges on Giolitti came to focus on the relationship between liberalism and economic growth, or in broader terms liberalism and modernization. This was partly because of Gramsci's insistence on the immaturity of the Italian bourgeoisie as a class and his claim that the 'missing revolution' of 1860 was reflected in the reactionary alliance between the northern industrialists and the southern landowners that took shape in the late 1880s in support of tariff protection for agriculture and industry. For Gramsci this alliance was the key feature of the Liberal state and remained the
principal obstacle to both political and economic development down to the 1920s.

The rejoinder came once again from Rosario Romeo, for whom unification had been the premise for Italy's economic as well as political modernization. Arguing that after 1860 agriculture, through taxation, demand and invested profits, had financed the infrastructures (roads, railways, banks) on which Italy's industrialization depended, Romeo claimed that an agrarian reform would simply have preserved an outdated peasant agriculture and hence have retarded Italy's economic modernization (Romeo 1961).

The response to Romeo came not from the Left, but from Alexander Gerschenkron who claimed that Italy's relatively late and internally patchy industrialization was the result of state intervention and the creation of new mixed investment banks (the Banca Commerciale Italiano and the Credito Italiano) in the 1890s and not of capital transfers from agriculture. What was at stake in this debate was nineteenth-century Italy's capacity for independent economic growth.

Gerschenkron believed that in Italy, as in other industrial latecomers (such as Germany and Russia), spontaneous economic growth had been weak. Like Germany's notorious Sonderweg, Italy's path to the twentieth century was as a result
marked by a process of industrialization from above that was not accompanied by
broader forms of social modernization and indeed exacerbated the existing internal
dualisms between an industrializing North and the still backward and predomi
nantly agrarian South. It was to these internal contrasts that Gerschenkron attributed the political disasters that would beset all the industrial latecomers Russia, Germany, Italy and Japan in the twentieth century, thereby offering a
non-Marxist explanation of the structural weaknesses of Italian liberal democracy
(Gerschenkron 1966).
Gramsci's notion of an incomplete bourgeois revolution and Gerschenkron's
inclusion of Italy in the category of industrial latecomers were in many ways
complementary, and both linked the problems of Italian liberalism to incomplete
and imbalanced modernization. These arguments established a broader context for
debate, much of which focused on the limits of Italian liberalism one of the
central themes in the essays published in the volume on Gramsci and Italy's Passive
Revolution (Davis 1979). But the economic record of Liberal Italy also found
defenders and F. J. Coppa's (1971) detailed study of the renegotiation of Italy's
protectionist tariff regime in the early twentieth century put up a stout defence of
Giolitti's economic and political liberalism.

The other principal field of debate was foreign policy and nationalism. Had Italy's entry into the Triple Alliance in 1882 distanced her from liberal Britain as well as from republican France, or was the German-Austro-Hungarian axis dictated purely by diplomatic considerations? The terms of the debate were set by Federico Chabod (1953), who emphasized how the new Italian state had been born in the shadow of the Paris Commune and hence at a moment when European liberalism as a whole was rapidly losing its progressive and constructive features. Reconstructing the tensions between liberal and nationalist aspirations and the links between domestic and foreign policy, Chabod underlined the narrow constraints within which the new Italian state had to steer its foreign policy after unification.

Studies by Decleva, Di Nolfo, Vigezzi and others focused on the diplomatic and ideological aims of Italian foreign policy before 1914 and the contrast between the cautious diplomacy of Visconti-Venosta and the more aggressive and anti-Austro Hungarian policies adopted by Di San Giuliano. Industrialization and its influence on foreign policy became a major issue in this field as well. Were the first colonial initiatives of Crispi's government linked to Italy's industrial expansion? Did the
social legislation of the Giolittian era go hand in hand with the emergence of new
forms of industrial imperialism? The American historian Richard Webster (1974),
for example, argued that Italian colonialism before 1914 had been driven by the
network of financial and industrial interests that converged at the beginning of the
twentieth century around the Banca Commerciale Italiana, although this has been
challenged. In Italy Least of the Great Powers (1979), the Australian historian
Richard Bosworth argued that Italy’s economic backwardness made it impossible for
her to sustain her pretensions as a Great Power. Bosworth saw this as the principal
cause of the adventurist tendencies in Italian foreign policy that he believed were
present from Crispi's early and disastrous bid for colonial expansion in the 1890s, to
Giolitti's invasion of Libya in 1911 and finally to Mussolini's imperialism in the
1930s. In parallel with this, the studies by J. A. Thayer, A. Saladino, A. De Grand
and Richard Drake explored the origins and influence of nationalist ideas in Italy
before 1914. 597 FASCISM
It was in the 1960s that the debate on fascism itself began in earnest. The first
studies based on extensive archival sources began to be published, while in
international terms a broader debate on the cultural roots and sociological contexts
of totalitarianism was stimulated by the publication of studies like Barrington Moore Jr. Social Origins of Dictatorship and Democracy, and Ernst Nolte's Three Faces of Fascism. The two volumes edited by Stuart Woolf (European Fascism and the Nature of Fascism) and the launch of the Journal of Contemporary History are all indications of this widening interest.

In Italy, Alberto Aquarone's study (1965) of the institutional organization of the fascist regime, Roberto Vivarelli's analysis of the post-war crisis, and the first volume of Renzo De Felice's biography of Mussolini appeared in quick succession. De Felice's work in particular aroused great controversy. Like Romeo, De Felice chose biography as a means to write a general study of the period and the evolution of Mussolini's regime, but whereas Romeo had stressed the European breadth of Cavour's ideas and culture, De Felice insisted on the essentially Italian character of fascism, although denying that fascism and Mussolini-ism were identical. Many historians on the Left saw the claim that Mussolini's politics were rooted in his experience as a revolutionary socialist (De Felice 1965) as an attack on the integrity of the antifascist alliance on which the post-war Italian Republic was founded. But De Felice's arguments also
offended a historian like Vivarelli (1967) who blamed the revolutionary rhetoric of the Socialist Party for heightening the political crisis in Italy after 1918, but insisted that the ideological matrix of fascism lay in the pre-fascist nationalist movement.

The controversy grew as successive volumes of De Felice's biography set out an interpretation that many believed sought to rehabilitate and 'relativize' Italian fascism. The claim that Mussolini's regime had in its heyday commanded the active support ('consensus') of the majority of Italians and that the regime reflected the aspirations of a rising political class that had previously been excluded from the patrician politics of the old elites sparked off a particularly heated controversy (see also M. Ledeen Interpretations). The later volumes continue to cause controversy, not least because of De Felice's insistence that fascism had firm roots in Italian society, was not in every respect negative (although he denounced its dictatorial and imperialist aspects) and bore no substantial similarities to German National Socialism.

English translations of general studies on the origins of fascism like Salvemini's Origins of Italian Fascism, Angelo Tasca's Rise of Italian Fascism and Paolo Spriano's...
Occupation of the Factories attracted non-Italian historians to study the post-war crisis and the fascist seizure of power. Many focused on the failure of the Left, and Martin Clark’s Antonio Gramsci and the Revolution that Failed examined how the fragile unity of the industrial workers, the unions and the socialists collapsed during the factory occupations in the summer of 1920. 598

Paul Corner’s study of fascism in Ferrara (1974) was concerned to explain the collapse of the militant agricultural labour unions in the Po Valley after 1918. Much to Mussolini’s astonishment, it was here that fascism first found a mass following and Corner reconstructed the ways in which fascism emerged as a response to a class war that had been endemic in the rural Po Valley since the beginning of the century. During the war, the highly organized and militant labourers’ unions had made great conquests, but once the war was over the landowners looked to claw back the concessions they had made and the incursions of black-shirt squads into the agrarian landscape provided the means. Despite its rural setting, Corner’s study points firmly to the links between fascism and capitalist modernization in rural Italy. Other regional studies, by Anthony Cardoza (1982) (the province of Bologna), Frank Snowden (1989) (Tuscany and
Apulia) and Alice A. Kelikian (1986) (Brescia), developed these arguments and showed how the fascist movement inserted itself in a variety of different local conflicts.

At stake was the claim that fascism had developed after 1920 as a form of capitalist counter-revolution to destroy labour organizations and reformist political parties (including the new Catholic Popular Party) at the behest of big business and the landowners. Charles S. Maier's Recasting Bourgeois Europe (which deals with Italy in detail, as well as France and Germany) questioned that interpretation, while setting Italian fascism in the context of a broader abandonment by the European bourgeois and managerial elites of pre-war liberalism for new corporatist strategies.

The timing of the crisis in Italy in Maier's view was determined by the relative weakness of the economic and political institutions of the Liberal state. Douglas Forsyth (1993) has recently argued that the financial and economic demands of the war made it impossible for Italy's post-war governments to maintain or develop the welfare programmes and social legislation adopted before 1914 by Giolitti. In these terms, fascism is seen as a consequence of the failure of post-war Italian liberal governments to adapt to modernization, social and economic change in conditions.
when it was no longer possible to pursue a model of welfare consensus, in ways that anticipated the later fate of Weimar Germany.

Relations between the fascists and Italian industrialists were the subject of a detailed study by Roland Sarti (1971), who argued that although Italian business leaders were not opposed to Mussolini, many remained wary of the movement's revolutionary tendencies and kept their distance. But once the dictatorship was in place, Sarti claimed that the Italian industrialists proved better able to retain their corporate autonomy than any other interest group.

Of the principal political groups and tendencies that converged in the fascist movement, David D. Roberts (1979) has examined the role and influence of the revolutionary syndicalists and shown how Mussolini never supported a genuinely syndicalist programme. Alexander De Grand’s study (1978) of the nationalist movement from the founding of the Italian Nationalist Association in 1911 to the merger with the fascists in 1923 reveals how the Nationalists played an often critical and conservative role in shaping the fascist state. Christopher Duggan’s Fascism and the Mafia (1989) examines the ways in which the regime attempted to establish a political base in western Sicily and the political alliances that this involved.

Adrian Lyttelton’s Seizure of Power (1974) offered a model
for these approaches,
emphasizing the heterogeneous nature of different local situations while seeking to
explain how these converged into the fascist movement, how they held together and
how this cohesion was preserved in the transition from political movement to
regime. Critical of De Felice's argument that the creation of the fascist state entailed
no major break with the past, the underlying theme of Lyttelton's study is that
fascism resulted from the failure of the liberal ruling classes to adapt to the realities
of mass politics. Accepting that the regime was in essence 'Mussolini's personal
dictatorship' (p. 432), Lyttelton shows how Mussolini played different factions off
against one another but avoids identifying the dictator and the regime too closely.

Lyttelton also describes the ways in which the Fascist Party was gradually
subordinated to Mussolini and the state bureaucracy in the power struggles after
1925, although Emilio Gentile (1984) has recently suggested that the Party retained
important functions within the fascist state.

A central theme in Lyttelton's study is the degree to which fascism was a political
movement driven by clear yet often competing political and economic interests,
rather than an exercise in mere political eclecticism, opportunism and chicanery.

The staunchest champion of the second interpretation which
Salvemini's was Denis Mack Smith, whose biography of Mussolini appeared in 1981. Directed primarily against De Felice's insistence on the rationality of Mussolini's politics, Mack Smith portrayed the Duce as a bombastic artist in propaganda, self and national deception whose power ultimately rested on graft and corruption.

Mack Smith's critics believed he exaggerated the incoherence of fascist policy, and the recent revival of right-wing political ideologies in both Europe and the United States explains why historians have tended to take the ideologies of fascism more seriously now than in the past (see Griffin 1993). But even amongst those who argue that fascism did have a coherent ideology there is still disagreement. The Israeli historian Zeev Sternhell (1993) has recently revived an argument first suggested by Ernst Nolte (1966) that the ideological matrix of Italian fascism lay in the ideas of the pre-1914 French Action Française and French syndicalism, but his critics argue that fascism drew on wider ideological and cultural sources. The Italian historian Emilio Gentile (1990), for example, has developed George Mosse's argument that fascist ideology was part of a wider reaction against the secularization of European societies in the nineteenth century and can be
interpreted as an attempt to forge new forms of secular religion through nationalism, myth and public display.

On similar lines, Walter Adamson (1993) has also argued that the avant-garde of anti-liberal and anti-positivist writers and artists associated with the pre-1914 Florentine journal La Voce prefigured much of the content and thrust of later fascist ideology.

The role of ideology is also the central question addressed in studies on the regime's policies once in power. For the American historian, A. J. Gregor (1979), Mussolini's regime was a 'developmental dictatorship' bent on modernizing the Italian economy and society. Jon Cohen (1988), however, has cogently argued that such an interpretation risks taking the regime's claims too much at face value and is not substantiated by Italy's economic performance in the 1920s and 1930s. Indeed, it is still far from clear whether it is possible to define a 'fascist' economic policy.

Some argue that the regime did have a clear policy and that the revaluation of the lire in 1927 intentionally precipitated a deflationary trend that sacrificed consumption and consumer goods industries to strengthen Italy's chemical, iron, and steel sectors, marking the beginning of a bid for economic autarchy and the restructuring of Italian industry that laid the basis for Italy's
post-war economic miracle. Others claim that changes in the Italian economy owed more to the Depression and the international economy than to Mussolini's policies, that after revaluation Italy's dependence on US loans increased, and that massive increases in state intervention in the economy through the rescue of the banking system and the creation of state holding companies (IRI, IMI) in the early 1930s were crisis measures that did not form part of any longer-term or coherent economic plan. Both those interpretations are rejected by those who argue that Mussolini, like Hitler, subordinated economic policy to his imperialist ambitions, forcing Italy into disastrous economic subordination to Germany and dragging the country into a war that it lacked the resources to fight. Vera Zamagni's recent Economic History of Italy 1860-1990 (1993) offers a clear and balanced guide to these debates.

Lack of reliable data makes generalizations about the performance of the economy in the 1930s difficult, and this has caused debate on the role of ideology to focus on the regime's social and cultural policies. A recent general study by D. Thomson (1991) in some respects updates Tannenbaum's older book (1972) on fascist social and cultural policies, but the best insights come from detailed studies. Philip Cannistraro's research on the Ministry of Popular Culture,
for example, shows clearly the limited success of the regime’s attempts to generate consensus through propaganda (La Fabbrica del Consenso) (1975). Marla Stone’s analysis of the 1932 exhibition to commemorate the anniversary of the Fascist Revolution emphasizes how the regime at this time was attempting to project a pluralist and inclusive image.

explores the ways in which the regime deployed propaganda and the broader question of whether there was a 'fascist' culture. The relationship between the regime and Italian artists and intellectuals has been the subject of major studies by Italian historians, and the recent biography of Margherita Sarfatti, Mussolini’s Jewish mistress, by B. R. Sullivan and P. Cannistraro (1993) contains much information on intellectual life and the arts in fascist Italy. This is also the subject of a recent article by Ruth Ben Ghiat (1995) that examines the ways in which many leading Italian writers and artists who subsequently disclaimed any contact with the regime were more closely and actively associated with official fascist culture than they chose to remember after 1943.

The debate on the cultural policies of the regime has always been closely linked to De Felice’s claim that in the early part of the 1930s Mussolini’s regime achieved broad consensus amongst Italians. Shifting the focus from
Victoria De Grazia's Culture of Consent (1981) examined the relationship between the regime and the working classes in the broader context of the search throughout the capitalist economies for ways to respond to the advent of mass society. De Grazia argued that the regime's objectives in establishing the network of workers' recreational associations (Dopolavoro) were an offshoot of principles of scientific management pioneered by industrialists like Henry Ford and adopted in Italy by leading Italian industrialists like Giovanni Agnelli after the war. Fascist attempts to redraw the boundaries of private and public life by bringing the state directly into the leisure activities of Italian workers were one variant on a wider range of responses to the advent of mass society.

Early optimism soon gave way to more realistic aims partly, De Grazia argued, because Mussolini's Italy lacked the material resources to attempt anything comparable to the later Nazi 'Strength through Joy' organizations, but also because its policies were fundamentally contradictory. The regime never decided whether it wanted to exploit a nascent consumer economy or to block its growth (on the grounds that it was not 'fascist'), while its strategy of mobilization ran up against the fascist emphasis on obedience, hierarchy and deference:
as De Grazia pointed out, it proved difficult to mobilize the masses to be passive. Early aspirations to turn Italians into fanatical fascist men, women and children soon gave way, therefore, to more minimalist but not ineffective attempts to associate everything that was traditionally Italian sports, pastimes and entertainment with fascism. A recent article by Emilio Gentile (1993) also shows how the regime's ambivalence over consumerism was reflected in fascist attitudes to America. Fascist education has been studied by T. Koon (1985), while following earlier studies by Alexander De Grand (1976), Leslie Caldwell (1986) and Emiliana Noether (1982), Luisa Passerini (1979) has studied women's memories of the regime and the Duce. A recent study by Perry Willson (1993) offers a detailed reconstruction of the work and experiences of the mainly female workforce of a leading Milanese engineering company that was also deemed to be a model 'fascist' enterprise. But the broadest study of the regime's policies towards women is Victoria de Grazia's How Fascism Ruled Women (1992), which again sets the regime's policies in the broader comparative context of the ways in which all advanced industrial societies were responding to fears that the advent of mass society would cause birth-rates to fall. In the Italian
case, De Grazia emphasizes how the regime’s policies evolved hesitantly and, despite a deep-seated misogyny, ambiguously. On the one hand, the regime wanted to remove women from the workplace, deprive them of freedom of choice and bend them to its demographic needs. But at the same time fascist attempts to push back the boundaries of the private gave many Italian women a new freedom to lead lives outside the family and enter into a consumer culture unknown to their mothers. As in its attempts to organize leisure, the regime’s specific objectives were not achieved (the birth-rate did not increase and the number of women in the workforce did not decline) and provided a further illustration of the contradictions inherent in the fascist desire to achieve economic modernity within a context of traditional social values and institutions.

De Grazia’s study also deals at length with the conflicts with the Church that resulted from the regime’s policies on education, the family, maternity, gender, race and eugenics. Relations between the regime and the Church have been the subject of studies by Jemolo (1960), Binchy (1941), R. Webster (1960) and most recently by J. F. Pollard (1988), while R. Kent has written on the Vatican’s policies towards the dictators. Attention has also focused on the relationship
between racism and fascism,

and in a recent comparative essay Maria Quine (1996) has
argued that racial theories

and eugenics influenced the regime's policies more deeply
than has been recognized.

The regime's anti-semitism has also attracted new attention. The Jewish communities

and their role in Italian intellectual life have been studied by H. Stuart Hughes (1983)

while Alexander Stille (1991) has vividly described the contrasting political outlook

and fortunes of five Jewish families in fascist Italy. The background to the racial laws

of 1938 and the origins of fascist anti-Semitism are the subject of Meir Michaelis's

Mussolini and the Jews (1978), while the idea that Italian fascism was free of anti-Semitism and racism and that the racial laws of 1938 were imposed by the alliance

with Germany has found many critics. Esmonde Robertson, for example, has

emphasized the importance of racism in Italian imperialism and the links between

racism and anti-Semitism. On the other hand, both Susan Zucotti (1987) and

Jonathan Steinberg (1990) have shown that the 1938 legislation against the Jews was

deeply unpopular among many Italians, and that down to 1943 the Italian authorities

refused to co-operate with their German allies in deporting Jews either from Italy or

from the Italian-occupied territories in Croatia and southern France. But for Meir
Michaelis (1989) the regime's open advocacy of imperialism, racism and violence made fascist Italy no less an accomplice in the destruction of European Jewry.

The regime's policies on Jews, minorities, workers and women raise the issue of opposition to fascism, which has attracted little attention in English since Charles Delzell's pioneering book on Mussolini's enemies (1961).

Paul Corner (1990) has explored the reasons for the lack of opposition amongst industrial workers in Italy before 1943, while Luisa Passerini (1979) has studied forms of covert working-class opposition to the regime. Both J. P. Diggins's study Fascism: The View from America (1972) and H. Stuart Hughes's Italy and America (1979) contain extensive reference to Italian political exiles in America, as does the biography of Margherita Sarfatti.

The Italian antifascist diaspora in Europe has not yet been studied, although interest is growing in the ideas of Carlo Rosselli (1994) and the Justice and Liberty movement which he and his brother Nello helped to found in Paris in the late 1920s.

Debates on racism and imperialism take us back to the regime's foreign policy, a topic on which contributions by non-Italians have understandably been particularly numerous. The earliest studies focused on the origins of Mussolini's alliance with Germany and the diplomatic relations between Rome and Berlin (Elizabeth
The debate on the coherence of Mussolini's foreign policy was taken up by Alan Cassels in his study (1970) of Mussolini's early diplomacy, where he argued that Mussolini's expansionist objectives were present from the beginning but were held in check by Italy's old guard professional diplomats in the Foreign Ministry. Once the dictatorship was secured and the rise of Hitler had thrown the Versailles settlement into question, Mussolini was able to adopt increasingly overt revisionist strategies.

Esmonde Roberston (1977) also stressed the internal consistency of Mussolini's policies, which he argued found their logical outcome in the bid to create a new Roman Empire, whereas Denis Mack Smith (1976; 1981) depicted Mussolini's foreign policy as a series of improvisations determined by the regime's need to maintain its popularity at home. This argument was extended in his biography of Mussolini which echoes Gaetano Salve mini's earlier description of Mussolini as a 'sawdust Caesar' (1927; 1936) and argues that the Duce's foreign policy was throughout an extension of domestic propaganda. Admitting that Mussolini's North African ventures after 1935 were motivated by the need to strengthen the regime's image in Italy, Renzo De Felice (1990) rejected the claim...
that Mussolini's policies
lacked consistency and argued that Mussolini looked to
achieve his revisionist aims
by playing off the Western allies against one another, a
task that was initially made
easier by the rise of Hitler. De Felice's claim that
Mussolini's foreign policy was
driven by revisionist rather than ideological
considerations is supported by the
conclusions of John Coverdale's study of Italy's
intervention in the Spanish Civil
War (1975), a move that was dictated not by ideological
affinity with the National
ists in Spain but by the need to protect Italy's
established interests in the western
Mediterranean.

MacGregor Knox's Mussolini Unleashed (1982; 1984) offers
an interpretation that
differs from all these, however, and insists that
Mussolini's foreign policy was
throughout guided by the clear intention of establishing
Italian domination over the
entire Mediterranean. Rejecting the idea that Mussolini's
foreign policy was
incoherent or dictated by domestic considerations, Knox
argues that as soon as the
opportunities arose Mussolini attempted to implement this
plan even at the risk of
alienating public opinion and exhausting Italy's fragile
resources. However,
Mussolini's 'parallel war' failed precisely because of
those inadequate resources,
inadequate preparation and planning. With little or no
inter-service co-ordination,

Mussolini's generals proved reluctant to take risks and indeed even to take the

offensive. In this light, Italian fascism appears no less aggressively imperialist than

German National Socialism, although Italy lacked Germany's material resources.

Mussolini proved less successful than Hitler in bending Italy's armed forces to his

will not because the Italian generals, navy and air-force commanders were hostile

to fascism but because they were beset with internal rivalries and unwilling to

innovate.

Rivalries within the fascist leadership are the subject of other recent studies.

Segre's biography of Italo Balbo (1987), an early leader of the fascist movement who

became the head of the air-force before being sent virtually into exile as governor of

Libya by a jealous Mussolini in 1934, throws light on the internal politics of

Mussolini's Roman Empire (like Segre on the colonization of Libya), and on the

growing opposition to Mussolini's leadership amongst leading figures in the regime 604

that was fuelled by the alliance with Germany. But jealousy amongst the fascist

hierarchies and fear of the Duce meant that this opposition was never articulated.

The rivalries and tensions within the fascist leadership in the late 1930s are also the

Mussolini's decision to declare war on France and Britain in June 1940 began the process that led to his fall in July 1943 following the Allied landings in Sicily. The armistice of 8 September then marked the beginning of a two-year tragedy which turned Italy into both a lingering battlefield and the theatre of a violent civil war as fascists and anti-fascists fought one another against the background of resistance to the Nazi occupation in central and northern Italy.

English-language studies have concentrated primarily on the military operations and the allied administration of Italy down to the liberation of Milan in April 1945.

Carlo D'Este's studies (1988; 1991) of the Allied landings in Sicily and Anzio show that personal rivalries and poor military planning were by no means a monopoly of Mussolini's regime. David Ellwood's analysis (1985) of the Allied administration in Italy underlines the areas of dissent between the British and the Americans, not least in their attitudes towards the Resistance in the North. Denis Mack Smith's study of the Italian monarchy (1989) although covering the entire period from Unification also describes how Victor Emanuel Ill's abrupt abandonment of Rome, the army and the administration in 1943 contributed to the opprobrium that resulted in the abolition of the monarchy in 1946.
American economic relations with fascist Italy have been studied by G. G. Mignone, while the United States' role in the economic and political reconstruction of Italy at the end of the war is the subject of excellent books by James E. Miller (1986) and John Harper (1986). Acknowledging the impact of the Cold War and Washington's hostility to the Italian Communists, both suggest that the Christian Democrats and the Vatican needed little external assistance to defeat their political rivals on the Left. This confirms the conclusions of Sidney Tarrow's earlier analysis of the ways in which after 1945 the Christian Democrats outmanoeuvred the Left and in particular the Communists by exploiting widespread peasant unrest in the South. An earlier but still valuable collection of essays on Italian reconstruction edited by Stuart Woolf (1972) is now supplemented by a more recent volume edited by C. Wagstaff and C. Duggan (1995), while Paul Ginsborg's History of Contemporary Italy (1990) offers English readers a comprehensive guide to this period.

British and American historians have understandably tended to focus on the military, political and diplomatic contexts of the war, and have shown less interest in the internal history of Italy during the war. F. W. Deakin's Last Days of
Mussolini (1966) is the only study of the Republic of SaId in English, and is concerned primarily with Mussolini’s relations with Hitler. There is a wealth of first-hand documentary and fictional writing on the war, including works of outstanding literary merit like John Horn Burns’s The Gallery, Norman Lewis’s Naples ’44 and Eric Newby’s Love and War in the Apennines. The trials of leading figures in the fascist regime after the fall of Mussolini are the subject of a new book by Roy Palmer Domenico (1991). Another recent study by Roger Absalom on the ways in which Tuscan peasants helped often at great personal risk to shelter Allied POWs also touches on an issue that has been most fiercely debated in recent years by Italian historians. In a remarkable book published in 1990 the Communist historian and former Resistance militant, Claudio Pavone, argued that it was time to make a new and dispassionate assessment of the Resistance and its political legacies in post-war Italy. His book attempts to reconstruct the choices made by those Italians who after Italy’s surrender on 8 September 1943 chose to join the Resistance. He challenges the long-held claim that the Resistance was a combination of a patriotic war against the German forces of occupation and a class war to create the basis for the revival of democracy in Italy. While accepting that
both elements were present, Pavone argues that the Resistance was also part of a
more confused civil war in which alignments and loyalties were more accidental,
and which gave rise on both sides to acts of systematic violence and mass executions.

For many historians on the Left, Pavone's study was seen as a timely coming to terms with the realities of the past, one that was particularly relevant at a moment when the fortunes of the Italian Communist Party were declining and the whole orientation of Italian politics seemed to be moving towards something new. But Renzo De Felice (1995) and other critics seized on Pavone's conclusions to argue that the Resistance's claim to represent the most constructive, progressive forces in Italian society was a myth. According to De Felice, only a minority of Italians had been actively militant for either the Resistance or Mussolini's Republic of Salò. The majority constituted what he termed a 'grey zone' of uncertainty and confusion, in his view because Italy's military defeat on 8 September 1943 had brought with it a crisis of national identity. That argument has become the subject of heated recent debate in Italy, since implicit in De Felice's argument is the idea that Mussolini's regime did claim to represent the Italian people and that in failing to acknowledge
this the Left has weakened the sense of Italian national identity and has been responsible for the political failings of post-war Italian politics.

Like the French historian François Furet, De Felice believed that the Russian Revolution of 1917 brought about the crisis from which fascism and National Socialism emerged and also gave rise to a left-wing political mythology that in his view has choked off liberalism and individualism. There are echoes here of the German Historikerstreit and in Italy champions of the contemporary new-right look to reassess fascism in ways that underline the 'relativist' character of Mussolini's regime, challenging the 'myth' of the antifascist Resistance and blaming the political shortcomings of the Italian Republic on the political parties that drew their legitimacy from the Resistance. At this point the historiographical wheel seems to have come full circle: after 1945 Italy's post-war political parties looked to the pre-fascist past to establish their legitimacy and roots in Italian democracy. In the crisis that since 1990 has overwhelmed the political parties that had dominated Italy since 1945, claims for political legitimacy must once more be established by reinterpreting Italy's more recent past.
The recent debates on fascism and its place in Italian history since the 1970s have also brought new questions to bear on the history of nineteenth-century Italy. One of the first debates focused on the labour movement and was partly an offshoot of the heated controversies within the Italian Left in the 1960s and 1970s over the leadership and policies of the PCI. The terms of the debate were set by the studies on the origins of the industrial proletariat by Stefano Merli and the German labour historian Volker Hunecke. Louise Tilly’s study of class formation in Milan (1992) (although published much later) was written in the context of these debates, as was Donald Bell’s analysis (1986) of labour and politics in the Milanese industrial suburb of Sesto San Giovanni before 1914. But the unique importance of the Italian Communist Party in post-war Italy, and then in the 1980s the re-emergence of the Italian Socialist Party under Bettino Craxi, also drew the attention of non-Italian historians to the history of the Italian Left. Studies by Spencer Di Scala (1980), James E. Miller (1990) and Alexander De Grand (1986; 1989b) re-examine the achievements of the PSI’s pre-1914 reformist strategies and explore the alternative paths of Italian socialism. Since the end of the Cold War, the ideas of anti-Marxist socialists such as Carlo Rosselli have also begun to
attract new interest.

The debate on the Italian labour movement and politics was also about modernization and the emergence of new social forces, themes that have dominated recent research on nineteenth-century Italy much more widely. The idea that fascism was linked in some way to Italy’s experience of ‘difficult modernization’ called into question notions of ‘backwardness’ and began to reshape debate on nineteenth-century Italy in ways that were clearly signalled in the Introduction to Raffaele Romanelli’s 1979 History of Liberal Italy (Italia Liberale 1979). The highly differentiated realities of Italian society were in many ways best approached through local or regional studies, Romanelli argued, and were difficult to reconcile within a framework determined by national unification. While not denying its importance, Romanelli also acknowledged that the history of the Italian ‘state’ was essentially a history of the political projects of the Italian ruling classes, almost an ‘autobiography of the Italian bourgeoisie’, and hence only one of many possible histories of Italy and of the Italians (c. Rosselli 1994).

Instead of assuming that the creation of a nation-state was the natural outcome of political unification, Italian historians -like historians more generally were now beginning to explore how social and political identities
were shaped, how nations
were 'constructed', how the relationship between state and
society, state and nation,
nation and state have developed over time. As well as
asking new questions this
meant doing new types of history. The multi-volume Einaudi
Storia d'Italia that
was published in the 1970s reflected a growing interest in
Italian social history that
was captured in the subtitle of Stuart Woolfs History of
Italy 1700-1860: The
Social Constraints of Political Change (1979) (which first
appeared in Italian in vol. 3
of the Einaudi Storia). A number of new journals came into
being to promote research on social history (Quaderni Storici, Movimento
Operaio, later Passato e
Presente, Società e Storia, Donna-Women-Femme, Memoria,
Ricerca e Storie and
most recently Meridiana), and in the 1980s the multi-volume
Einaudi Storia delle
Regioni offered important new outlets for this research.
Accepted images were soon called in question, not least
that of nineteenth
century Italy's social and economic 'backwardness'.
Economic historians pointed out
that earlier assessments of Italy's economic performance
had taken too much
account of the most advanced sectors of Italian industry
and too little account of less
spectacular but broader and in the end equally effective
forms of economic growth.

Luciano Cafagna (1973), for example, has argued that
agriculture, and especially
the cultivation and production of silk (nineteenth-century Italy's most valuable export), sustained capital accumulation, commercial expansion and new manufaturing enterprises that by the end of the century had generated relatively dynamic and spontaneous economic expansion throughout much of northern and central Italy.

The dramatic success of many small family-based firms like Benetton in the 1980s focused attention on the longer-term role of small enterprise in Italy's modern economic growth, and Paul Corner has explored the role played by peasant farms in the transition from agriculture to dynamic small family-based enterprises. Italian entrepreneurial capacity has also been reconsidered in ways that stress the rationality and 'modernity' of entrepreneurial strategies. Rather than a sign of backwardness, for example, the ruralization of Italian industry can be seen as a rational entrepreneurial strategy that enabled Italian manufacturers (especially in the textile sector) to respond to constantly unstable external markets by exploiting a cheap rural labour force composed mainly of women and children. Others have emphasized the aggressively modernizing and self-sufficient features of the rural capitalist entrepreneurs in the Po Valley and the Veneto in
the late nineteenth century (Davis 1996a). Nineteenth-century Italy, in other words, was not backward although its experience of capitalist development and modernization had taken a path that was in many respects distinctive and modernization had always been 'difficult'.

Social historians were making similar points, and focused above all on Italy's hitherto much-maligned but until recently virtually unexplored middle classes. Abundant new studies now reveal the presence of a self-conscious new class with tastes and values that differed little from their counterparts in other western European societies, which in turn reopens the debate on the relationship between politics and society. Challenging the idea that 'modern' capitalism and liberalism were necessarily interdependent, Silvio Lanaro (1979) and Alberto Banti (1989) have argued that modernization bred a particularly assertive and self-sufficient form of capitalist enterprise in Italy. If Italian capitalists showed little interest in liberalism, this was because many successful Italian capitalists in the late nineteenth century and early twentieth century believed that nationalist and corporatist programmes were more 'modern' than liberalism and offered more effective solutions to political and social conflict in advanced
industrial societies. 608

From a different perspective, Raffaele Romanelli has also challenged the idea that

the Italian political system (or any other) should necessarily have imitated that of

Great Britain as some historians had always implied. Whereas Prussian iron and

steel forged the new German state on terms dictated by Bismarck and the Prussian

elite, Piedmont had never played a similar role in Italy and as a result, Romanelli

has argued, Italian unification took the form of a settlement negotiated with the

regional elites. The Italian political system was not then an aberration from some

general (yet never defined) European norm, but rather the product of Italy's history

and the particular balance of political and social forces on the Italian peninsula: a

system that was not necessarily 'good' or 'bad', 'better' or 'worse' than others,

merely different.

The essays in the volume on Society and Politics in the Age of the Risorgimento

(1991) show how the debates on the social history of nineteenth-century Italy have

in recent years become increasingly international in ways that make it difficult to

distinguish clearly between 'Italian' and 'non-Italian' approaches. This is partly the

result of new international debate and research projects (for example, on the

comparative study of the European bourgeoisie), and also
because more Italian studies are now appearing in English. One of the principal themes has been the bourgeoisie, and recent studies are discussed by Meriggi (1993) and Adrian Lyttelton (Davis and Ginsborg 1991). The upper boundaries of the post-Unification middle classes are charted by Anthony Cardoza's work on the Piedmontese aristocracy (1993), that looks to validate Arno Mayer's thesis on the 'persistence of the Old Regime' in Italy down to 1914. In the other direction, Jonathan Morris's study of Milanese shopkeepers (1993) draws boundaries at the other end of the social scale and defends the petty bourgeoisie against the charge that their politics were inherently anti-liberal. A new collection of essays in English edited by Maria Malatesta (1995) on the professions explores the middle ground.

There are also many studies that touch on the emergence of new middle-class tastes and life-styles: John Rosselli's book on the opera industry (1984), for example, provides a particularly vivid description of the new theatre-going urban middle classes. Mary Jane Phillips-Matz's biography (1993) of Giuseppe Verdi and Roberta Olson's study on the iconography of Risorgimento art (1992) offer additional insights into the politics of nineteenth-century Italian bourgeois culture.
The volume on Italian Art in the 20th Century (Braun 1989) also contains essays and culture and society by Lyttelton on the Giolittian era, by Cannistraro on the fascist period and by Woolf on post-war Italy.

The distinctive role of the family in the development of Italian society and politics has also begun to attract attention, and in addition to the general study by David Kertzer (1991), Marzio Barbagli offers a useful overview (Davis and Ginsborg 1991). The history of the family in Italy is closely linked to regional differences and especially those between the North and the South. Here, too earlier theories of modernization have come under concerted attack in ways that are well illustrated in recent studies devoted to the origins of the Mafia. The anthropologists Anton Blok (1975) and Jane and Peter Schneider (1976) were among the first to link the emergence of the Mafia in Sicily in the late nineteenth century to the development of commercial capitalism, while an important recent study by the sociologist Diego Gambetta (1994) emphasizes the economic rationale of Mafia enterprise.

Rather than a form of 'primitive rebellion' or an example of the social and cultural backwardness of the South, these studies link the emergence of the Mafia to the economic modernization of the South. Marta Petrusewicz's recent re-evaluation
(1996) of what has long been considered the epitome of the backwardness, the vast latifundist estates, argues that what made economic activity and behaviour in the South different was not the lack of modern management but the chronic insecurity and uncertainty of the markets on which Southern producers depended. Robert D. Putnam (Making Democracy Work. Civic Traditions in Modern Italy, 1993), on the other hand, has revived the older claims that the disparities between North and South have been rooted in cultural differences. For Putnam, the key differential lies in the lack of civic traditions in the South, where from an early date feudalism and foreign rule precluded the development of the 'civic' cultures typical of northern and central Italy from the time of the Renaissance. But this theory of cultural determinism extending over centuries runs counter to the principal thrust of recent research on the Southern economy.

The role of the South and the 'Southern Problem' in the formation of an Italian national identity has been explored by Daniel Pick (1989) and John Dickie (1992), who argue that the negative stereotyping of the South after 1860 formed part of a conscious strategy of cultural standardization and constituted the 'otherness' against which Italy's new elites sought to establish new collective identities and cultural
norms (see also Davis in Levy 1996). The 'construction' of national identities in wider terms is also the subject of an important recent study by Silvana Patriarca (1996) on statistics and nation-building, while Adrian Lyttelton (1993) has suggested new perspectives on Risorgimento nationalism.

The emphasis on the processes of state-formation and changing social and political identities has kept the relationship between state and society at the centre of the debate on nineteenth-century Italy. But by focusing on the emergence of modern forms of bureaucracy and administration and the ways in which this changed the distribution of power between rulers and elites and redefined the contours of public and private, Unification ceases to be the isolated miracle portrayed by Trevelyan to become a variant of a broader European historical process of state-formation.

This is the central theme of Stuart Woolrs Napoleonic Integration of Europe (1991a), which measures the impact of Napoleonic rule and reform on the Italian states, and of my own study on Conflict and Control: Law and Order in Nineteenth Century Italy (1988), which is concerned with the changing distribution of power between the elites and the state from the crisis of the ancien regime rulers in the late eighteenth century to the political crisis of the 1890s. Confrontations between local
elites and rulers are also centre stage in Steven Hughes's study (1994) of policing in Bologna, which shows how the Papal government's assumption of new powers to put down public disorder clashed with the interests of the propertied classes and contributed to the political crisis of the Papal States. For the period after 1860, Mary Gibson's study (1986) of prostitution reconstructs an important chapter in the history of Italian working-class women and the development of the Liberal state, and sets the Italian debates on public health, gender and crime in a broader European context. Women's history in nineteenth- and twentieth-century Italy is the subject of a recent essay by Michela De Giorgio (1996), while Alice Kelikian (1996) and Margherita Pelaja (1996) have explored new aspects of the history of women, gender and family in Italian society and culture both before and after Unification.

Thanks to these studies we can for the first time begin to map out the contours of social change in nineteenth-century Italy, to see how social forces and politics were related, and so set Italy's social and political developments down to 1914 in their broader comparative European perspective. This does not mean that the political history of unification has been neglected. Denis Mack Smith returned to the fray in
1985 with a study of Cavour in which he challenged the positive assessment of the Piedmontese statesman in Rosario Romeo's biography. Harry Hearder (1994) has also written a new biography of Cavour. The second volume of A. J. Reinerman's magnum opus (1979; 1989) on Austrian policy in Italy, and especially the relations between Vienna and the Papacy, has now also been published, while F. J. Coppa (1990) has made important reassessments of the politics of the curia of Pius IX.

Denis Mack Smith's recent biography of Mazzini (1993) shows how contemporary concerns are also setting new perspectives for the political history of the Risorgimento. His is the first major English biography of the prophet of Italian nationalism since Bolton King's study written at the turn of the century (and still worth reading) and the translation of Salvemini's more complex work. Indeed, Mazzini has long been out of favour, in part because Mussolini unjustly appropriated him as the prophet of fascism, but also because Mazzini had quarrelled furiously with the Risorgimento socialists. It is partly for this reason that Mazzini's search for a form of democracy that was not tainted by Jacobinism has recently begun to attract new interest (Urbinati 1996). Mack Smith, not surprisingly, finds Mazzini a more sympathetic subject than his political rival Cavour. As well as
underlining the impact of Mazzini's contacts with English politicians and intellectuals during his exile in England, Mack Smith emphasizes how Mazzini's programme anticipated the more contemporary idea of a united Europe composed of independent nations.

As in the case of fascism, the debates on nineteenth-century Italy have opened out to accommodate new questions and new agendas. Social and cultural history has made its mark in ways that have drawn Italy into more general comparative debates on the processes of modernization and state-building in nine teeth-century Europe.

But as Lucy Riall (1994) has rightly pointed out, this has meant that issues such as nationalism and the reasons why Italy achieved political unification have tended to be sidelined. However, a number of important recent studies have attempted to set cultural and social history more firmly in the context of political processes (for example, Patriarca 1996). But this also owes much to the rapidly expanding literature devoted to Italy's history since the Second World War (see especially Ginsborg 1990 and McCarthy 1995) which has reopened debate on the longer-run 'peculiarities' of Italy and the Italians. The deconstruction of the older stereotypes that linked the peculiarities of Italian history to 'backwardness and under
development’ has served to bring into sharper profile the specific features that in the longer term seem to have epitomized Italy’s particular path to the twentieth century features that include the particular character of the Italian state, and its political systems and administrative structures from its origins in the nineteenth century down to the present (Romanelli 1995), the complex relationship between centralism and regionalism (Levy 1996), the development of a ‘national’ identity, the problems posed by Italy’s generally vulnerable position in changing international markets, the nature and role of its bourgeoisie, the specific role of the family in Italian society and politics, and not least the long-term role and influence of the Catholic Church and religion. It is around these themes that the attempt to understand the nature of Italy


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In 1978, Edward Said, an American-Palestinian professor of English and comparative literature, published Orientalism, a book which became the focal point of a broad international and interdisciplinary debate. It was a provocative study of Western discourses, academic and other, on the Islamic Orient which, Said claimed, can be regarded as a Western style for dominating, restructuring, and having authority over the Orient (Said 1991: 3). Although many of Said's arguments had already been voiced by earlier critics, none had discussed such a wide range of materials from different fields and disciplines to demonstrate the basic features and continuities of this discourse. And it was precisely the polemic nature of Said's book, the fact that 'he unceremoniously dumps every Westerner who has ever studied the Arabs into one big basket', as one of his reviewers remarked.
sourly, 4 which ensured the success of the book.

In part, Said’s success can be measured by a change in terminology. ’Orient
alism’, which had, in English, hitherto been a more or less neutral denotation for

’Oriental scholarship; knowledge of Eastern languages’, 5 has taken on a pejorative

I would like to thank Gabriele vom Bruck, Gerhard Dannemann, Christoph Herzog, Jamal Malik

and David Morgan for their comments on earlier drafts of

this paper.

2 Edward Said (1991), Orientalism. Western Conceptions o/the Orient. Penguin (London etc.). 1st edn,

Routledge & Kegan Paul 1979. For an analysis of the early debate on Said’s book, see Mani and

Frankenberg 1985.


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meaning in English refers to ’Oriental character, style, or quality’, whereas the German term

’Orientalismus’ signifies the preoccupation of nineteenth-century artists with Oriental motifs. It is not

mentioned in La Grande Encyclopedie (Librairie Larousse) of 1975. 620

connotation and is now mainly used ’to refer to older European scholars who

combine a conservative methodology with what is felt to be a contemptuous or

prejudiced attitude towards the peoples of the Near
Incidently, Said’s book has thus accelerated the process of replacing the term ‘Orientalism’ by ‘human sciences in Asia and North Africa’, as had been suggested by the Orientalists themselves at least since their international congress in Paris in 1973. A similar, and probably somewhat earlier, transformation can be observed for the Arabic term, istishraq, as a result of extensive discussions about the redoubtable merits of Western scholarship.

From the beginning, Orientalism has been discussed with a number of aims and has addressed widely varying issues. This chapter, which gives an overview that by no means claims to be comprehensive, will concentrate on three broad aspects. First, it suggests reading the debate historically, as an expression of the cultural encounter or should one say confrontation? between the ‘West’ and the ‘East’; second, it will discuss major issues which were raised by Said and some of his predecessors; and finally, it will look at some of the more profound problems the debate poses for scholarly work on the ‘Orient’.

It seems in order to clarify some terminological problems before embarking on the debate itself. Not least as a result of the increased discussion of Orientalism, recent scholarship has elaborated on the imaginary quality of such terms as ‘Orient’
and 'Occident', 'Easterner' and 'Westerner', pointing out that geopolitical boundaries are hardly objectively definable but remain very dependent on historically changing collective identities and perceptions. Furthermore, their heuristic value is limited in that they tell us more about our self-image through the definition of the 'other' than about the 'other' himor herself (Hentsch 1992: ix-21, 159-205; Carrier 1992: 197; cf. Chandhuri 1990: 22-8). If these terms are used in the present chapter, albeit with reservations, it is not only for the sake of convenience but also because they form the frame of reference, indeed the key, to much of the debate on Orientalism. The geographical imaginary 'Orient' has greatly varied within scholarly Orientalism, from wide definitions including all of Asia to a rather more limited interest in the area closest to the imaginary boundary which is now often known as the Near East (Waardenburg 1993: 735; al'Ayyad 1965: 161-4; Hentsch 1992: xi), as have the preoccupations and developments of scholarly activity on various parts of the 'Orient'. The following discussion limits itself mostly to that part of the debate from within and without which deals with North Africa and West Asia, for which the terms 'Orient' or 'Middle East' will henceforth be used. It should at least be mentioned,
however, that many of the
problems inherent in Orientalism have also been discussed
with regard to Western
6 Malti-Douglas 1979: 724. For a history of the term, see
Waardenburg 1993: 735f. and Reig 1988:
9-15.
7 The term and its history in the Arabic language are
discussed in al-Bustani1977: 11-14. For an
overview of the discussions about Orientalism in Arabic,
see Rudolph 1991. A useful anthology of texts
on Arab perceptions of the West from 1870 to 1990 is
al-KJittb 1991. 621
scholarship of Africa, showing the similarities and
discrepancies in the general
approach. 8
Similarly, with regard to the term 'Orientalism', certain
reservations are in order.
Not only did it undergo the aforementioned change in
meaning, it is also debatable
whether the discourse on the Orient has actually been as
congruous as Said wants us
to believe, or whether, as Maxime Rodinson claims, there
always was 'a multiplicity
of issues coming under the jurisdiction of many general
disciplines. These issues
emerge from varied phenomena found in certain countries
previously grouped
under the questionable rubric of the East' (Rodinson 1987:
81). What is fairly
undisputed, however, is the predominance, in older Western
scholarship on the
Orient, of philology. This has increasingly been
challenged by scholars who argue
that in order to understand historical, sociological or
economic developments, a

scholar needs to be equipped with the methodological tools
of these respective fields

instead of relying exclusively on the knowledge of
Oriental languages. Another

prominent feature of traditional Orientalism has been the
concentration on 'Islam',

often seen as an unchanging phenomenon, as a key to the
understanding of Middle

Eastern societies. (Halliday 1993: 1512-7; Philipp 1994:
166-9). 'Orientalism' will

be used with reference to scholarship marked by these
characteristics, rather than

making more problematic assumptions about the general
attitudes of the scholars

involved. HISTORICAL ROOTS OF A MODERN DEBATE

Although the European interest in and scholarly examination
of the Orient can be

traced back to the Middle Ages, 9 what concerns us here is
the nineteenth century,

when the European expansion brought an altogether new
dimension into the

relationship between East and West, and when, mostly as a
consequence of the

imperial project, Oriental and Islamic Studies became
established as an independent

field of studies Oohansen 1980: 88). To what extent even
apparently innocent

publications could feed into imperial ideology becomes
clear in the case of Volney

(1757-1820), author of a travel report on Syria and Egypt
and an opponent of the
French involvement in Egypt. By analysing the Ottoman Empire in Syria and Egypt as foreign despotism, enhanced by reliance on a religion that aimed at the absolute rule of the Prophet, he concluded that a change in political control was in the interest of the local populations as well as in that of the preservation of the historical monuments (Volney 1959: 156). Thus, Volney provided the arguments used by Napoleon when he invaded Egypt in 1798, and can be seen as the founding father of a liberal and republican version of the French mission civilisatrice (Laurens 1987: 78).

8 For the difference and similarities between Orientalism and discourses on Africa, see Miller 1985, particularly pp. 14-23. For Western discourse on Africa, cf. Mudimbe 1988. Andreas Eckert kindly

9 For overviews of the development of Oriental Studies, one may consult Waardenburg 1993; Rodinson 1987; Hourani 1991: 7-60; and Fiick 1955: 622

Consequently, the perception of the Europeans in general, and of the Orientalists in particular, underwent a major change in the eyes of the people they studied. In 1798, the Egyptians clearly recognized the French as foreigners, mainly characterized by their different religion, but otherwise considered them on a basically equal footing as normal human beings with their weaknesses and strengths. The French interest in science and knowledge, manifested in the enormous effort of the Institut
d’Egypte which had been established in Cairo, was commented upon very positively by the Egyptian historian al-Jabarti. However, at that time the European presence did not have any major impact on the wider Egyptian society. This changed in the second half of the nineteenth century with the increasingly direct European involvement and the establishment of more permanent forms of rule and influence in the Middle East. (Ende 1965: 86-8). More and more, the Europeans came to be regarded by the Arabs as the ‘other’ in relation to whom they tried to define themselves, to the extent that, according to the Moroccan historian Abdallah Laroui, the question of ‘who is the other and who am I’ became a major preoccupation for the Arabs (Laroui 1967: 15), in a vein similar to other ‘Oriental’ societies. Although this was somewhat analogous to the function ‘the Oriental’ fulfilled for those Europeans concerned with the Orient, the situation cannot be fully compared. The very real difference in power relations and the depth of European penetration forced the encounter upon Oriental societies on a scale unknown in Europe, where the consideration of the ‘Orient’ as the relevant ‘other’ remained, in the nineteenth and twentieth centuries at least, to some degree a matter of choice (Said 1991: 204). Although the Europeans might not, at large, have realized
this until the second

half of the twentieth century, they came under intensified scrutiny by the

supposedly so passive 'Orientals' (cf. Abu-Lughod 1963 and Lewis 1982a). It is not

intended to give a detailed overview of how the perception of Europeans in the

Middle East changed. It is worthwhile noting, however, that from the inter-war

period, the peak of direct European domination and growing cultural influence in

the Middle East, the belief in basic common humanity, upheld in spite of many

criticisms of the Europeans, began to be replaced by a more outright rejection of

things European, culminating in the period of struggles for independence and

political self-determination.

Orientalists formed an integral part of the Europeans observed. An interesting

early account of such a personality, characterized by his knowledge of Arabic, is

given by the Egyptian writer 'Ali Basha Mubarak in his novel 'Alam aI-Din which

was published in 1882 (Wielandt 1980: 48-72 and Alleaume 1982: 6-9). The

Orientalist in his novel was an intermediary between Orient and Occident, a

propagator of European ideas in the East and an advocate of Orientals in Europe.

10 Wielandt 1980: 17-33. Wielandt's book gives a very wide-ranging and differentiated overview of

how the perception of Europeans changed from the early
nineteenth century to the 1960s.

11 For a similar argument concerning India see Inden 1990: 3.


He continuously enhances particular judgements and the world-view deemed important by 'Ali Basha, thus playing an inherently positive role. 13

Around the same time, however, some Oriental intellectuals started to respond more critically to Orientalists, and, more specifically, to their writings. Although they were, at first, isolated individuals, they are significant in that they challenged, albeit on a modest scale, the Orientalist claim to an explanatory monopoly about 'the Orient'. Thus, they are predecessors of the 'writing back' of the 'objects' of European observation 14 that set in with the more systematic and general critique of Orientalism which started in the 1950s.

One of the earliest instances of such a response to an Orientalist interpretation of Islam, aimed not only at pointing out minor errors to an author but at openly opposing his views in front of a wider public, is the famous exchange between the French Orientalist Ernest Renan and the pan-Islamist activist Sayyid Jamal al-Din al-Afghani. 15 Renan had argued in a lecture at the Sorbonne, published in the
Journal des Dibats in March 1883, that Islam constituted a severe obstacle to progress. The Arab lack of scientific advancement was, in his eyes, enhanced by the inherent hostility of the Arab race as opposed to the Aryan one to science.

What was known as Arabic science was, according to Renan, either pure linguistics or else due to the influence of Christians (taken not to be Arabs) and Persians.

While al-Afghani accepted the limiting influence of religion on scientific exploration, he stressed that this was an intrinsic problem of narrow religious thought as opposed to philosophy which could be found in all faiths, rather than being a characteristic trait of Islam. Al-Afghani also rejected Renan’s racist approach, opting instead for an evolutionary explanation which underlined that all people underwent various developmental stages before adopting science and philosophy.

Among the Arab authors who obtained wide-ranging knowledge of Orientalists and their works and thus came to a differentiated evaluation was the Syrian scholar and journalist Muhammad Kurd ‘Ali (Hermann 1990: 83-94 and Escovitz 1983: 95-109). Kurd ‘Ali held many Orientalist works in high esteem because their concern with and their editions of classical Arabic works had contributed largely to
the renaissance or nahda of the Arab language and literature in the second half of
the nineteenth century. For his history of Syria, Khi\{
al-Sham,16 Kurd 'Ali
consulted Orientalist scholarship and made a conscious
effort to write a modern,

13 This function has been termed 'europiischer Bestiitiger
vom Dienst' by Wielandt 1980: 57.

14 Clifford 1980. The term 'writing back' was coined by
Michel Leiris in an article called
'L'Ethnographe devant le colonialisme', Les Temps Modernes
149-82.

15 Renan 1883. For comments on this debate, cf. Hourani
1983: 120-3 and Keddie 1983: 84-95 and
181-7, for an English translation of al-Afghani's answer to
Renan. There had been earlier instances of
minor criticisms, often with regard to philological errors
committed by Orientalists. Cf. Rudolph
1991: 15.

16 6 vols, first published Damascus 1926-8, 2nd edn
Damascus 1983. About this work, cf. Freitag
1991: 144-56. 624

secular, emancipatory history, discarding old taboos and
employing an analytical
style. Kurd 'Ali encountered criticism for his alleged
adoption of the Orientalist
method and the fact that he at times openly recommended
Orientalist scholarship to
his fellow Arab scholars. It has to be noted, however,
that he himself employed it
almost subversively to write from an Arab perspective
against the neglect of Arab
history by Arabs, regretting that most accounts of Syrian
history so far and by this
Kurd 'Ali seems to refer to the kind of analytical Syrian
history he tried to provide
had been written by Westerners (Kurd 'Ali 1983: I, 3).
Kurd 'Ali maintained no
illusions about the innocence of at least part of
Orientalism. He recognized
colonialism as one of the three factors which had
stimulated the growth of Oriental
Studies, the other two being religion and scholarly
curiosity. Furthermore, he
realized that many Orientalists, while their training
enabled them to mediate
between Orient and Occident, put their knowledge at the
disposal of the political
interest of their respective countries. This however, did
not occur to him as
something morally inappropriate but rather as normal,
because, he argued in 1935,
Muslims did the same thing. (Kurd 'Ali 1935; quoted after
Reid 1987: 58). In a
similar vein, his Egyptian contemporary, the writer Zakl
Mubarak, defended
Orientalism in a public newspaper discussion entitled 'Did
Orientalism do more
harm than good?', by arguing that even those Orientalists
who wrote negatively
about Islam still often contributed worthy publications
and, in any case, spurred
public interest in this religion.
Such a line of argument shows clearly that the thought of Kurd 'Ali and some of
his contemporaries, while being more critical of the
Orientalists than 'Ali Basha Mubarak had been, was based on a strong self-confidence.
Kurd 'Ali's controversy
with Henri Lammens over the latter's book on Syrian history
(Lammens 1921)
shows in an exemplary way this tolerance towards other
interpretations, provided
they comply with basic rules of fairness, something which
Kurd 'All misses in
Lammens:
We do not demand that the author should write about the
Islamic scholars without
criticism and comment, or that he should curtail his
freedom of judgement. We also do
not expect that he believes in the religion [Islam] in the
same way as its followers do.
What we do expect, however, is that he judges fairly and
distances himself from emotions
which are brought forward under the pretext of scholarship
.... He has said things about
the Quran ..., its interpretation and language that were
first brought up by medieval
fanatics in monasteries. We had hoped that he would not
follow their opinions, after his
non-Muslim contemporaries have already rejected them. His
words degrade a religion of
approximately 300 million followers. (Kurd 'Ali 1922: 275)
Kurd 'All was neither the first nor the only Arab scholar
who came under attack
from conservative circles for following Orientalist
methodology. When the new Egyptian University was established in 1908, it extended invitations to Italian, French and German Orientalists to give lectures on topics such as early Oriental languages, Egyptology and classical studies. At the same time, an increasing number of Egyptian students were sent to Europe for higher education. Attacks on those who, under the influence of Orientalist methodology and scholarship, started to question time-honoured historical and religious interpretations led to a series of controversies and affrays over personalities such as Jurji Zaidan (1910), Taha 'Iussain (1926) and Mubammad Abmad Khalaf Allah (1947). Although the end of the Second World War was accompanied by widespread independence in the Arab World, the establishment of Israel and the Cold War in the Middle East increased rather than decreased the heat of the cultural confrontation. Numerous books on Orientalism as a discipline and its deeds came to be published in Arabic for an Arab audience, attacking Orientalism as a mode of cultural imperialism, that is, as an attempt to exercise control even after formal political independence had been achieved. This criticism came partly from a
religious, partly from a Marxist perspective. While it can be argued that the critique in Arabic countries constituted at least partly an internal debate about the future orientation of the respective societies now that independence had been achieved (Rudolph 1991: 9), the same arguments were taken up by Oriental scholars in the West and published in European languages. These publications were clearly directed at a wider European and scholarly audience, a sign that the 'writing back' had reached a new phase.

The first major critique of Orientalism in English was by the London-based Palestinian scholar Abdel-Latif Tibawi and came from a religious perspective. Many of Tibawi's arguments can be found in similar writings in Arabic and other languages (Algar 1971; al-Siba’i 1968; Rudolph 1991: 30-42). His argument is based on the assumption that Orientalist scholarship was, in its early phase, inspired by religious hatred, coupled with a Christian desire to proselytize among Muslims. With the imperial expansion, and the development of anti-colonial nationalist movements, these movements became the new focus of the West's feud with Islam. While acknowledging the contribution of Orientalists to the editing of Oriental manuscripts, Tibawi bitterly accused Western students of Islam of
doubting its inherent value by presenting it mainly as a deviation from Judaism and Christianity. This was not only a defamatory, Eurocentric approach, Tibawi claimed, but also distorted Islamic history. Besides scrutinizing the work of some British Orientalists such as W. M. Watt, K. Cragg, B. Lewis and P. M. Holt from this perspective, Tibawi makes some further points worth mentioning.

The first might be considered as providing some insight into the motives of the 'writing back'. While Arabs held, Tibawi notes, high respect for Westerners, they were not treated on an equal footing and had no chance, at least in Great Britain, of obtaining equally high academic positions. Hamid Algar (1971: 104-6) developed the point further by suggesting ways in which Muslims could, in such a position of hierarchical inferiority, react to Orientalist scholarship by challenging the Orientalist's claim to objectivity. Underlying both authors' criticisms are strong doubts about the possibility of any outsider's capacities for understanding things.
Tibawi's second argument raises a major methodological issue: the author claims that many Orientalists not only had insufficient knowledge of the Oriental languages they claimed to master, but also lacked the training in social sciences required to justify their judgements on the Middle East.

This latter argument is taken up at great length by socialist critics of Orientalism such as Anouar Abdel-Malek. Abdel-Malek argues that the 'crisis of Orientalism' came about because of the independence not only of the countries but also of the people who had formerly been the 'objects' of Orientalists' studies. This development had challenged the Western assumptions about the essential difference between 'Westerners' and 'Orientals' who had been regarded as passive, non-autonomous and non-sovereign, ideas which often had been conflated with racism.

Although Abdel-Malek noted that since 1945, (neo-)Orientalism had somewhat adapted itself to the changing conditions, for example by legitimizing the study of modern developments, the Orientalist's 'Orient' still remained the passive object of a field that did not admit 'Orientals' into its ranks. Abdel-Malek (1963: 120-30) contrasted this with the approach chosen by socialist countries. He outlined as its characteristics, first of all, a critique of Euro-
centrism; second, political solidarity and co-operation including on the academic level with the peoples of Asia, Africa and Latin America, thus transforming them from 'object' into 'subject'; and third, a new focus on contemporary sociological and political problems. Such studies were carried out by professionals trained in social sciences and humanities with additional linguistic skills as opposed to the primarily philological training of traditional Orientalists.

At this level, Abdel-Malek's arguments tied in with paradigmatic changes in mainly, but not exclusively Western leftist and anti-imperialist circles which had been, as Abdel-Malek had already noted, deeply influenced by the independence of the formerly colonized peoples after the Second World War and further stimulated by the opposition to the Vietnam War. This critique of conventional scholarship started in disciplines which were much more directly confronted with political changes than the more textually oriented Orientalists. Thus, in 1950 Michel Leiris had analysed the relationship between anthropological knowledge and colonialism and had provoked a debate about how European knowledge about other parts of the

Abdel-Malek, see Cahen 1965 and Gabrieli 1965.

21 This latter point was also criticized by Tibawi 1979: 25f.

22 The following discussion draws on Clifford 1980: 204f., Halliday 1993: 148f., Johansen 1980 and
world had been structured by a will to power (Clifford 1980: 205). In the political
sciences, the critique of the Eurocentric traditional and behaviouristic approaches in
the 1960s encouraged the development of a more comparative, theoretical,
humanistic and problem-orientated approach which was 'Third World'-centred and
can be classified as 'post-behavioural' (Chilcote 1981: 57). This debate spilled over
into the domain of Orientalism, primarily on the level of studies directed at the
contemporary world, stimulated by political concerns as much as by a desire to
reorientate research in a way that was linked more directly with mainstream
scholarship in specific disciplines (I:Iamis 1981: 63-88). For example, historicism
began to loose its paradigmatic influence on German Oriental Studies in the 1960s
Oohansen 1980: 93f.). In France, Maxime Rodinson had suggested that general
questions pertaining to social, economic and political development and inspired by
Marxism should be considered for Muslim countries, thus breaking free from the
paradigm of religious specificity and integrating the discussion into a global context
from younger Orientalist academics, as Jacques Waardenburg’s study exemplifies, which analyses the approaches of five Western scholars to Islam in view of the problematic relationship between subject and object (Waardenburg [1963] 1970). The already mentioned debate about the future name of international gatherings of scholars working on the Orient was just another reflection of the extent to which the debate had reached the scholars concerned. Edward Said: The Renewal of a Debate

With regard to Orientalism, one of the critics remarked that 'Like the title, the very act of writing the book is a political statement and a literary fact which may prove to be more important than the author's arguments themselves' (Malti-Douglas 1979: 724). Many of the criticisms of 'Orientalism', for Said a field encompassing virtually all Western expressions about the (Middle) East, from politicians' statements such as Balfour's to poetic works by Goethe and Flaubert and to treatises by scholars as far apart as d'Herbelot and Louis Massignon, well as the collective effort of societies and scholarly institutions, can indeed be found in much of the critiques outlined above, as well as in others that were in print at the time when Said was writing his book. In away, Said popularized and polemicized the earlier debate and carried it
from the relatively isolated field of academic discourse into the expanse of popular Western discourse—an area which this debate, I would argue, had already reached with regard to an Arabic speaking audience in the context of the debate about decolonization and neo-imperialism. The publication of Said’s initial articles on the topic, as well as his exchange with Bernard Lewis in the New York Review of Books, indicate the kind of public he was aiming at (Said 1976 (for comments, cf. Little 1979: 118-31); Lewis 1982b; Said 1989).

However, what was new in Said’s analysis was that he made use of the analytical framework of theories of discourse and power to integrate such a wide-ranging body of material. This allowed him to reformulate some of the questions raised by earlier contributors to the debate in more fundamental terms. While the link between Orientalist knowledge and imperial power had already been recognized, Said expressed more serious doubts about the possibilities of Westerners obtaining knowledge about the Orient, arguing ‘that political imperialism governs an entire field of study, imagination, and scholarly institutions in such a way as to make its avoidance an intellectual and historical impossibility’ (Said 1991: 14). He
problematizes the counter-argument, i.e. that individual scholars were trained in various disciplines and not by necessity linked immediately to the imperial project, by locating such supposedly individual discourses within the more general context of their time, pointing to the fact that 'each work on the Orient affiliates itself with other works, with audiences, with institutions, with the Orient itself (ibid. 20; cf. p. 96). Furthermore, he observes a historical continuity from traditional Orientalism to modern political thinking which conceives of cultural differences as essential, thus, in the final analysis, resulting in a confrontational desire to contain, and possibly rule, the 'other' (ibid. 47f.).

In accordance with the observations made by Abdel-Malek, Said recognizes the changes which took place within Orientalism, namely the influence of the Western cultural crisis in the inter-war period which led to a re-examination of 'the Orient' in search of a new Western self-definition, and of the social sciences after the Second World War (ibid. 257, 284). Whereas in most humanities this caused a serious rethinking of Western superiority, Said claims that this did not happen in Orientalism. In spite of sympathy for the 'Orientals', scholars such as Gibb and Massignon nevertheless upheld the claim to speak for 'the Oriental', whom they
considered to be inherently unable to represent himself (ibid. 260-3, 271). In contrast to Abdel-Malek, who perceives the socialist approach as a viable alternative, Said's tone is more pessimistic. He sees little room for change in the 'discursive consistency' (ibid. 273) of Orientalism, the cause for which Said locates in the constant reproduction of a culturally hegemonic discourse (ibid. 321-4). This has prompted the Syrian philosopher Sa'diq JaJal al-'A?:m to reproach Said for a renewed essentialism in the form of an 'Orientalism in reverse',24 although Said himself emphasizes that he is highly critical about conceptions pre-supposing such essentially different entities (Said 1991: 322).

REACTIONS TO SAID'S ARGUMENT

Said's work has received a mixed response. This has been facilitated by his very wide definition of 'Orientalism' as well as by his sometimes contradictory and often less than systematic and methodologically rigorous approach. 25 Much of this criticism moves on the level of the correction of particular errors and a discussion of his omission of German and Russian scholarship,26 points...
which are of little interest
within the current context.

Bernard Lewis, who himself was attacked by Said for his pro-Zionist stance,
polemically compared the criticism of Orientalism to a
situation
in which a group of patriots and radicals from Greece
decides that the profession of
classical studies is insulting to the great heritage of
Hellas, and that those engaged in these
studies, known as classicists, are the latest manifestation
of a deep and evil conspiracy ...
(Said 1982b: 49)
and concluded that 'The most rigorous and penetrating
critique of Orientalist
scholarship has always been and will remain that of
Orientalists themselves' (ibid.
56). Thus Lewis reiterated the exclusivist Orientalist
stance already criticized by
Tibawi and Abdel-Malek, implicitly reconfirming once more
the idea that only
outsiders that is, Orientalists could really represent 'the
Orient' and were the
only ones competent to review their own scholarship.27 Such
a position, as well as
the continuing topicality of cultural essentialism in
international politics, as
exemplified in Samuel Huntington's recent article on the
confrontational nature of
civilizational differences,28 might alone suffice to
underline the ongoing importance
of works such as Said's which provide the basis for a
widespread, broad and critical
debate.
However problematic many of Said's generalizations may be, what seems important is not only the popularization of the debate about Orientalism, and thus a spreading awareness of certain problems, but also the new kind of research and questions which it has instigated in the field of Oriental Studies. Besides critical investigations into the state of the art which might have been on the agenda in any case (e.g. Kerr 1980; Reig 1988), and alternative accounts of Orientalism, Said's work has prompted more detailed research into the history of the discipline and its relationship with the imperial project (e.g. Hourani 1992; Rodinson 1987; l:Iamis 1981; Juha 1982; 1983; al-Muna'ama and al-Khalij 1985). On the level of discourse analysis, such varied topics as the intellectual origins of Napoleon's expedition to Egypt, the development of studies on Algeria, and the Western imagination of the Middle East have been investigated (Laurens 1987; Leimdorfer 1992, Hentsch 1992), to give but a few examples.

Besides adding to our knowledge, such studies have also allowed us to reformulate the questions which were raised by the critics of Orientalism in a more precise manner. 'How true is it that Orientalists were the main ideological authorities of

25 This has been pointed out most systematically in the critique of Said by Ahmad 1992: 159-219.


27 This latter position had been criticized by I:Iamis 1981: 8.

28 Huntington 1993, for comments cf. the contributions by Ajami, Mahbubani, Barthey, Binyan and Kirkpatrick in Foreign Affairs 72. 4 (1993). 630 I ~ ;

Orientalism in nineteenth-century Germany?', asks Johansen in his seminal study of German Orientalism, only to come to an analytical distinction between 'Orientalism as the imagination or scholarly judgement that produces the Orient as an object of knowledge and domination that is clearly separated from the Occident' and the scholarly field of Oriental Studies (Johansen 1980: 75).

While Johansen's study shows the manifold links between these two categories, the distinction allows him to differentiate between the two and analyse their relationship. Thus he can show that while early representatives of Oriental Studies in Germany, such as Friedrich Ruckert and Joseph von Hammer-Purgstall, became the main authorities of romanticist Orientalism, and while German Oriental studies since the 1830s came under the paradigm of classical philology, Orientalism became embodied in the views of Leopold von Ranke, the leading German historian...
of the time. He adhered

to a particular kind of historicism, then the leading
paradigm in historical and

political studies, which held, inter alia, that 'a history
of the human kind does not

and cannot exist' and that 'only some peoples form part of
world history' (ibid. 80).

On this basis, and against the background of the 'Eastern
Question', Ranke

developed the idea that Europe, and more specifically the
Germanic and Romanic

peoples as embodiments of civilization, had to control the
barbarous Ottoman

Empire. Ranke became the main historical and political
'expert adviser', to use the

modern terminology, on the 'Eastern Question' or, in Said's
terms, 'the leading

authority in the field of Orientalism'.29 'It is',
Johansen pointedly argues, 'for this

reason that he did not care for the results of Oriental
studies research' (Johansen 1980: 82). In a very similar vein, one can observe even
nowadays that public opinion

about the 'Orient' in countries like Germany is dominated
by self-styled 'experts'

who have little, if any, knowledge of the field of
Oriental Studies. 30

It has been remarked that because Said limits his critique
to studies of the Middle

East, the usage of 'Orientalism' implies that there is
something special about the

Middle East, and thus reiterates the very notions Said set
out to attack, while many
of the methodological approaches chosen by Orientalists, as well as the notions of superiority and inferiority, racism and even particular stereotypes can be found in much of the writing about anything 'non-Western' (Halliday 1993: 158). Said's claim that Europe's experience with the Middle East or Islam has been in many ways specific (Said 1991: 17), did not deter scholars in fields related to the study of other parts of Asia from a critical scrutiny of their own scholarly traditions in the light of his work, and thus incited further debates about scholarly traditions and their consequences. Thus, Ronald Inden (1990) has set out to destroy the notion of an essential, passive India locked in its unchanging caste system and often described by metaphors such as female, dream or jungle (see also Inden 1986). Similarly, Richard Minear (1990), has demonstrated that Said's analysis of Orientalists' use of the abstractions 'Oriental' and 'European' with its prejudice in favour of the latter and the ideas about past Oriental greatness and present degradation can provide a...
fertile basis for a critique of Western studies of Japan.

Minear’s investigation points to another field of enquiry that has received renewed attention in the wake of Said’s study, which involves fundamental epistemological questions about the conditions, possibilities and limits of obtaining knowledge about other cultures. Said, and a number of his adherents, have linked the specific picture painted by Orientalism to the imperial project. Minear, however, argues that very similar results emerged from scholarship on Japan in the absence of a similarly long-standing relationship with the West, Western participation in the Japanese rediscovery of its own past and, most importantly, in the absence of actual Western domination. Minear suggests, therefore, 'to focus less on the specific historical setting be it Said’s Orient or our Japan than on the general one: the encounter between Europe and America on the one hand and the "non-Western world" on the other' (Minear 1980: 515). Thus, Orientalism points once more to questions that had already been asked by anthropologists, namely, as J. Clifford put it, the key theoretical issue raised by Orientalism concerns the status of all forms of thought and representation for dealing with the alien. Can one ultimately escape procedures of dichotomizing, restructuring and textualizing in the making
of interpretative statements about foreign cultures and traditions? (Clifford 1980: 209 f.)

It may well be, as has been elaborated on the basis of these questions, that in the final analysis, this problem is not even limited to intercultural communication, as has been pointed out by James Carrier: 'Essentialization appears to be inherent in the way Westerners, and probably most people, think and communicate. After all, to put a name to something is to identify its key characteristics and thereby essentialize it' (Carrier 1992: 207). However, this is almost certainly aggravated in situations of intercultural contact through the mutual definition of the 'other' in opposition to the 'self through 'Orientalism in reverse' or, as Carrier has called it, 'ethno-Orientalism' (ibid. 190). It has been feared that the recognition of such a fundamental issue might easily lead either to a relieved or a resigned attitude in view of an apparently insoluble problem (Mani and Frankenberg 1985). However, the epistemological impasse plays the ball back into the field of politics: 'The problem, then, is not of essentialism itself. Instead, the problem is a failure to be conscious of essentialism, whether it springs from the assumptions with which we approach our subjects or the goals that motivate our writing' (Carrier 1992: 207; cf. Clifford

Although it is certainly true that reflections of this kind do not add anything to our factual knowledge about the Middle East (Halliday 1993: 151), they are nevertheless of great importance in that they stimulate not only a certain self-reflection among Orientalists but contribute, as a body of discussion, to an increasing awareness of the historicity of knowledge in Oriental Studies. This need not be confounded with the absolute arbitrariness of any knowledge in the humanities and social sciences that Hamid Algar (1971: 105) seems to imply when he writes with regard to Oriental Studies 'that the whole claim to objectivity, academic method, impartiality and so forth ... is basically false. After all, the whole concept of objectivity is in this case inapplicable'. However, if one assumes the existence of a reality, one has, in its investigation, not only to comply with certain rules, commonly thought to be the academic method, but also to accept that such an endeavour is by definition an open and unfinishable enterprise. 

Thus, a certain scepticism about uncritical reliance on the academic silsila or chain of transmission, so well described by Albert Hourani, and a critical reconsideration of earlier scholarship in the light of the circumstances surrounding its production are in place.
While this may, by now, be regarded as self-evident in disciplines such as history (cf. Koselleck 1977), Said's study provides ample evidence that such a reminder might still be well in place in Oriental Studies, as well as in any field, West or East, that is dominated or influenced by the search for an essential, timeless 'truth'.

This problem is all the more real because so much of Orientalist scholarship is conducted on the basis of texts. This is particularly problematic for those who are working on discourses of identity, be they inspired by secular or religious ideologies. More often than not, the sources themselves reflect the notions of identity held by their authors and are thus, by the very nature of this particular genre of texts, predominantly rooted in essentialist and exclusivist notions. Scholars can be easily tempted into uncritically reproducing these notions, and thus easily perpetuating old prejudices of essential differences between various groups of humankind. 33 The following example of Huntington shows in an exemplary way how such notions of Orientalists and Orientals alike can be used and, for political purposes, misused very conveniently to reinforce each other, when he writes, On both sides the interaction between Islam and the West is seen as a clash of civilizations. The West's 'next confrontation,' observes M. J. Akbar, an
Indian Muslim, 'is definitely going to come from the Muslim world. It is in the sweep of the Islamic nations from the Maghreb to Pakistan that the struggle for a new world order will begin.' (Huntington 1993: 32)

It thus seems that the current trend to consider the analysis of discourses as the methodological non plus ultra harbours a certain danger of backfiring so as to bolster old prejudices in a new disguise (cf. al-Azmeh 1993: 19). This danger is enhanced by the criticism of ignoring or diminishing the importance of authentic self


32 Hourani 1992: 32f. and 61f. The same point was made by Abu-Lughod 1987, who speaks of Orientalist 'jsnati' (p. ISS).

33 See, for example, the criticism of al-Azmeh 1993: 1-28 and Prakash 1990. Expressions which Oriental adherents of various brands of essentialism direct at those Orientalists who try to avoid this trap by looking to other paradigms for an explanation of these ideologies. This 'Scylla of the secularists and Charybdis of the religious', as it has been called (Fahndrich 1988: 183), can probably not be entirely avoided. It might, however, be more important than ever in such a context to integrate 'Oriental studies' into the relevant humanities and social sciences to avoid
the tautology of treating something as 'Oriental' simply because it originated in 'the Orient'. On this basis, topics such as Islamic history can be fruitfully reconsidered in the light of the critique of Orientalism.

Another important aspect is certainly that discourses and their analysis can be approached in different ways. Gyan Prakash (1990: 384) has made a strong case for treating the Third World (and, one should add, all other worlds as well) 'as a variety of shifting positions which have been discursively articulated in history' instead of seeking a new 'nativist romanticization' (ibid. 406). While such positions are easily exposed to criticism on the grounds that they seem to threaten the unity of the anti-imperialist camp, a critical exchange about and constant review of scholarly results seems to be one of the main prerequisites for a further deconstruction of Orientalist conceptions. To realize this, a crucial condition is the globalization of the field to encompass not only Western Orientalists, or scholars of Oriental origin who are integrated into the Western system, such as Edward Said, but also scholars, Eastern and Western, who are based in 'the Orient'. While Said is certainly correct in observing a Western-dominated hegemonic discourse, it is certainly no longer true if indeed it ever was that Western scholars can afford to ignore academic
developments in the Middle East or elsewhere,37 in spite of the often serious problems, such as a lack of resources or the widespread and lamentable absence of academic freedom. 38 Besides the gains and stimulation for scholarly insight, such a co-operation may be regarded as a necessary corrective of Occidentocentrism. Although this might seem to be a small step considering the practical and epistemological problems of intercultural understanding outlined above, it might nevertheless prove to be a significant one, as the Syrian author Mikha'il Rustum already observed in 1895. During his travels in the United States, he had come across the books of a certain Dr Henry Jessup whom he had known as a Christian missionary in Syria. Disappointed and outraged by Jessup's descriptions of Syria, Rustum wrote: 'I believe that had he known that the Syrians will read his book, he would have desisted [from his defamatory statements], and instead written the truth.,39 It is 34 Besides earlier cited advocates of this strategy, cf. al-Azmeh 1981. A slightly different Arabic version was published under the title If'āq al-istishraq. al-Mustaqbal al-'arabi (1981), pp. 43-62. 35 See, for example, the suggestions made by Schulze 1990. 36 This has been vehemently demanded by Benaboud 1982: 12; Djait 1985: 171; and I;Iamis 1981: 88.
37 Said 323. This point has been strongly made by Malti-Douglas 1979: 731.

38 This is being extensively discussed in the Arab literature, e.g. al-Nabi Iṣṣaif 1982; and Iṣamis 1981: 120f. For a study of the problems of Arab academics, cf. Sabour 1988.

39 Rustum 1895: 88. The episode is related by Musallam 1979: 22. 634 ' l t'.06 ii th. ,39 If al.J I: lti i I~! I:1 i 

true that, as Musallam (1979: 22) observes, Said is separated from Rustum by the Balfour Declaration, the Sykes-Picot Agreement and other historical events, as well as by current unequal power relations which have severed the optimism expressed in Rustum's words. On the other hand, the increasingly general global experience, exemplified by such travellers between cultures as Said, might well improve the chances for the development of a shared imagination of 'Orient' and 'Occident' amongst those concerned with the human past, be they from the 'East'.


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It is widely accepted by historians that modern Japanese history begins with the Meiji Restoration of 1868 when the government of the Tokugawa shoguns was ended and direct imperial rule in theory at least restored. A few scholars, for example Professor Hane (1983), argue that the majority of the Japanese people, the peasants and women, were so underprivileged and oppressed until the reforms of the American Occupation of Japan between 1945 and 1952 that modern Japanese history only begins after the Second World War. Others would see the Occupation
as the beginning of 'contemporary' rather than modern Japan, but in any event

stress on the Occupation makes a point with which few historians would disagree:

the Occupation is one of the two greatest turning-points of recent Japanese history,

the Meiji Restoration being the other.

If we argue that the Meiji Restoration was the beginning of modern Japan, a

survey of modern Japanese historiography should begin with discussion of the

Restoration's origins. There would be a good deal of agreement among Western

historians that the Restoration was a result of three interlocking sets of factors: of

long-term social and economic developments within Japan notably changes in the

composition and wealth of the samurai and merchant estates and of the peasantry; of

the intrusions of Western powers from the 1850s onwards; and of political disputes

within the ruling elite, the samurai. The same historians would, of course, differ

greatly as to the relative importance to be attached to each of these sets of factors.

The political disputes, though they came to a head during the 1850s and 1860s,

were longstanding. The domains of Choshu and Satsuma, which took the lead in

the overthrow of the shogunate, had been its traditional enemies since the battle of

1 This chapter deals with works written in English or translated from Japanese into English and, with
rare exceptions, with scholarship published since 1945. The very high percentage of books published in the United States reflects the continuing importance of Japanese history in America since the Occupation of 1945.

Sekigahara in 1600, though it was the circumstances of the Western intrusion in the mid-nineteenth century which gave their long-term political resentments an opportunity to make themselves felt. The social and economic developments were centred on the rise of the merchants (Sheldon 1958) and the changing composition of the peasantry (Smith 1959), with the growth of a wealthy peasant elite which by the early nineteenth century had become a body of entrepreneurs, often referred to as rural merchants. The bulk of the peasantry have traditionally been seen as existing in a state of increasing immiseration during the Tokugawa period, but persuasive revisionist work notably by Hanley and Yamamura (1977), has painted a very different picture, suggesting that most peasants were becoming more prosperous even while the rural elite of peasant entrepreneurs considerably outstripped the others in growing wealth. Meanwhile the samurai were becoming relatively though probably not absolutely poorer (Yamamura 1971). The ruling group within the shogunate was therefore already under severe pressure when the Western
powers made their appearance from 1853 onwards, and the
difficulties already
described were compounded by the lack of really able
shoguns there was no
outstanding ruler between Yōshūmune in the early
eighteenth century and Keiki
who got his chance, too late, only in the last year of
shogunal rule; by disputes
within the shogunal bureaucracy; by the growing threat from
the imperial
institution, deprived of all power during the Tokugawa
period but, at any rate in
Japanese theory, representing an unbroken line of emperors
going back for two
millennia and, since the eighteenth century, the subject
of analyses by men like
Motoori Norinaga and Hirata Atsutane who stressed imperial
legitimacy and drew
attention to the delegated role of the shogun (Webb 1968;
Earl 1964; Harootunian
1970). The Western powers, which entered this scene of
political, social, economic
and intellectual instability in the 1850s, demanded the
opening of Japan to Western
trade and settlement, and the upheavals which resulted from
their demands led in
1867/8 to the overthrow of the shogunate and the
theoretical restoration of direct
imperial rule. The splendid syntheses of Totman (1980) and
Beasley (1972) which
discuss these events in detail, though from rather
different angles, stress the strands
in historical thinking which see the Restoration as
essentially a nationalist revolt
against the Western intruders. This interpretation, which
goes back to a notable
article published in 1956 (Yoshio and Hall 1956) and which
has received and
continues to receive wide support from Western (and now as
well from some
Japanese) experts on the Restoration, can incorporate work
like that of Craig (1961)
on Choshu, which stresses the local particularism which
still existed amidst this
growing Japanese nationalism, but other historians like
Norman (1940) and Huber
(1981) have laid more emphasis on internal social and
economic developments in
explaining the events of 1867/8. Norman, much influenced
by Marxist theory, saw
the Restoration as a revolt by lower samurai and merchants
at a time of serious
peasant unrest against a 'feudal' system dominated by
shogun and samurai, while
2 There has been vigorous debate among historians about the
nature and, indeed, the very existence
of Japanese feudalism. A good introduction is Hall 1968.
Huber argues that these lower samurai should be regarded as
a merit intelligentsia
excluded by their relatively undistinguished birth from the
upper reaches of a social
and political system dominated by high-born samurai who
were often of limited
abilities.
While vigorous debate continues about the relative weight
to be attached to each
of the major sets of factors which led to the Restoration, it is also unabated about
the Restoration's consequences. The new ruling elite of lower-ranking samurai
dominated by men from the Choshu and Satsuma domains were determined to
strengthen Japan in order to achieve as soon as possible both domestic security and
equality with the Western powers in international affairs. As a result they embarked
on a major programme of reforms which so transformed Japan's domestic and
international situation that the country emerged by the end of the Meiji emperor's
reign in 1912 secure at home and a colonial power in its own right, which treated on
terms of equality with the West, an equality symbolized by the Anglo-Japanese
treaty of 1902, which has been discussed in a fine work of scholarship by Ian Nish
(1966).
The Japanese word ishin, usually translated into English as ‘restoration’, should
more properly appear as renovation and it is significant that the latter term, which points to the future as well as to the past, is pleasing to those historians who see the Restoration as a revolution, stressing the long-term political, social, economic, military and intellectual changes which flowed from it. 3 A powerful school of Japanese historians, influenced by the Marxist thought
which became so widespread among Japanese intellectuals in the period between the two World Wars, have debated, both during the 1920s and 1930s and in the years after the Second World War, whether the Restoration should be regarded as a successful or as a failed bourgeois revolution. The question had practical implications for the attitudes of left-wing political activists between the wars, as historians of the Rono-ha school, who believed that the Restoration was a successful bourgeois revolution, were able to dream of an immediate socialist revolution, while those of the Koza-ha school, who believed that it was a failed revolution, maintained that both bourgeois and socialist revolutions were still necessary in Japan’s historical evolution. Some of these debates came to be conducted in highly theoretical terms but the dispute also produced a flood of detailed research on nineteenth-century Japanese social and economic history. This has until recently been largely inaccessible to those Western scholars who do not read Japanese, but a recent book by Germaine Hoston (1986) now provides a good English introduction to the literature. It is hardly surprising that these Marxist interpretations had little appeal for the empirically minded American historians who came to the forefront in Western studies of Japan in the years after the Second World War,
a period dominated by
the 'Cold War' against the Soviet Union. In the 1960s, following a conference at
Hakone in Japan, they launched a series of studies based on modernization theory.

3 In this connection, the subtitle of a recent collection of interpretative essays is significant, Nagai
which, they hoped, would enable them to produce 'value-free' work on post
Restoration history.4 During the Meiji period Japan not only entered the
mainstream of international life but also recast many of its domestic institutions
along Western lines. This was, in the eyes of both Japan's rulers and of foreign
observers, an attempt to catch up with the great modern powers of the West, and
the modernization school of Western historians which has been so influential since
the 1960s has had plenty of material to work upon: the creation of a centralized
nation-state to replace the divided Tokugawa polity; the abolition of samurai
privileges and the introduction of a conscript army; effective reform of the taxation
system; education reforms which created an almost entirely literate population; the
creation of a constitution and of political parties; fundamental legal reforms based
on Western models; the creation of a national civil bureaucracy and of a modem
military establishment; the development of Japanese industry, so that by the end of the nineteenth century Japan had made the 'take-off' into self-sustaining industrial growth. These domestic reforms, in tum, enabled Japan to transform her international position between 1868 and 1912 from one of weakness and inferiority to the Western powers to one in which she dealt with them on broadly equal terms.

By examining these dramatic changes in terms of modernization theory the historians involved undoubtedly added to knowledge and understanding of post-Restoration history. On the other hand, from the 1970s onwards critics mounted increasing attacks upon their judgements, pointing out that they tended to take a far more favourable view of Japanese developments not only in the Meiji years but over the whole period from 1868 to 1945 than the majority of their Japanese colleagues working in the same fields. Critics challenged above all the claims of members of the modernization school that they had gone far in introducing value-free judgements in their studies. Dower (1975) and Hane (1982), especially, have produced powerful critiques of their methods and conclusions, arguing that they have implicitly if not explicitly favoured the elite groups who controlled Japanese society at the expense of the mass of the population: the peasants, the industrial workers,
women and outcasts, all groups which, Hane (1982) argues, led lives which varied from the barely tolerable to the totally wretched. No reader of his descriptions of the conditions of the poorest peasants or of female textile workers can fail to appreciate the very heavy costs which modernization imposed on the most vulnerable sections of the Japanese community, but it is perhaps also fair to point out that wretched conditions were also common within advanced Western nations during the processes of industrialization and modernization.

English-language scholarship, so full of the ideas of the modernization school, is, 4 A number of influential volumes were published by Princeton University Press as a result of the researches of the modernization school: M. B. Jansen (ed.) Changing Japanese Attitudes Toward Modernization (1965); W. W. Lockwood (ed.) The State and Economic Enterprise in Japan (1965); R. P. Dore (ed.) Aspects of Social Change in Modern Japan (1967); R. E. Ward (ed.) Political Development in Modern Japan (1968); D. H. Shiveley (ed.) Tradition and Modernization in Japanese Culture (1971); J. W. Morley (ed.) Dilemmas of Growth in Prewar Japan (1971). 662 as we have seen, only beginning to absorb the details of the Marxist debates of the 1920s onwards about the course and meaning of the country's modern history. It is only beginning, too, to absorb the ideas of the 'people's
history' which has been influential in Japan since the 1960s but has only recently begun to appear in English translations, though it was drawn to the attention of English-language scholars over a decade ago in a notable article by Carol Gluck (1978-9). About the same time as the American modernization theorists were launching their views in the scholarly world a group of Japanese historians, rejecting both Marxist and modernization theory, began to search for what they called an 'integral' or 'indigenous' approach to Japanese history, centreing on 'the people'. People's historians divide society into two parts, the elite and the rest of the population. In their concentration on the bulk of the population they are obviously closer to the Marxists than to the modernization theorists, but they criticize Marxist historiography as being too concerned with endless ongoing debates, such as that over the nature of the Meiji Restoration. They themselves have traditionally concentrated on rural history, seeing the real Japan as village Japan, and stress the importance of studying everyday life in the localities. They have devoted special attention to village communities, symbols of the interdependence of rural people, and have shown their rejection of Marxist theory by abandoning analytical frameworks in favour of the accumulation of descriptive
material about the people and communities which they study. As Professor Gluck emphasizes, because the Western world in a general sense and Western historians in particular are so often identified with the establishment and the elites in Japan, people's history, by de-emphasizing that establishment and these elites, also de-emphasizes the role of the West in Japan's modern history. The American modernization theorists, in contrast, who so often stress the role of the West as exemplar, can point, inter alia, to the creation of the Meiji constitution of 1889 as a clear example of extensive borrowing from Europe. The detailed studies which have been published about the events preceding and surrounding the drafting and promulgation of the constitution (Beckmann 1957; Akita 1967; Pittau 1967; Siemes 1966) have revealed especially the strong German influences throughout the process, influences very obvious during the mission to Europe in 1882-3 of Ito Hirobumi soon to be Japan's first prime minister to survey Western models. He visited Austria, France and Britain as well as Germany, but after he returned to Japan it became obvious that the major foreign impact would be German. When the actual drafting of the constitution began in 1885, six men played major roles. Four of them, including Ito himself, were
Japanese; the other two were Germans. The final document has been appositely described by one Western scholar as 'a remarkable combination of western political technology and traditional Japanese political ideas' (Hall 1970: 297). The political theory behind it was firmly Japanese: it was freely granted by the divinely descended and sovereign emperor to his loyal subjects. There was only one slight loophole in this implied absolutism. Article 4 provided that the government should be conducted 'according to the constitution' and this opened the way for theorists to discuss the possibility that there might be a law above the emperor. The prime minister and members of the new Cabinet, which had been created in 1885, were responsible to the emperor alone and not to the Diet. The constitution provided the legal framework for what scholars have come to describe as 'the emperor system', the pre-1945 Japanese political system within which the emperor theoretically wielded supreme power. In reality, though everything was done in the emperor's name, he himself did not normally make political decisions. In that situation a number of elites competed for power in the state and important
scholarship has been produced analysing these elites and debating their respective
influences at different times during the fifty or so years before the Second World
War. There were at least five elites: party politicians; the civil bureaucracy; the
military; big business; and the imperial advisers, with whom can be associated the
peerage and the Privy Council. In addition, there has been debate about how much
influence Emperor Hirohito did exercise and could have exercised in the dramatic
events of 1931 to 1945.

The beginnings of political parties in Japan can be traced to the popular-rights
movement of the 1870s, but throughout the period up to the end of the First World
War cabinets, though they might contain party politicians, were dominated by
ministers drawn from the bureaucracy. From 1913 onwards, however, when there
were two major competing parties in the Diet, pressure for government through
party cabinets grew, and between 1918 and 1932, with the exception of the period
1922-4, Japan had party cabinets, with the premiership passing between the leaders
of the two main political parties. During this period too the previously very small
electorate was substantially expanded, first of all in 1920 and then, much more
significantly, in 1925, when all men over 25 were given the vote. From 1932
onwards, however, with serious economic difficulties and growing violence within Japan and with Japanese military ventures on the Asian mainland, which brought international condemnation, there was a reversion to non-party cabinets, headed by either military men or civilian bureaucrats. Finally, in 1940, all political parties were dissolved. This rise and fall of the parties in the late nineteenth and early twentieth centuries has been described and analysed in some excellent works (Scalapino 1962; Duus 1968; Berger 1977), and it should be noted that it was underpinned on the theoretical side by Minobe’s theory of the constitution, often referred to as the ‘emperor organ theory’. Minobe, a law professor at Tokyo imperial university, argued that sovereignty lay not in the emperor personally but in the state and that the emperor ruled as the highest organ of the state. The Diet, he maintained, was an organ representing the people and grounded directly in the constitution, an idea which both encouraged and legitimated party cabinets. Minobe’s theory was very influential though never universally accepted in academic circles during the 1920s, but in the mid-1930s ultranationalists mounted a ferocious campaign against his ideas, accusing him of lese-majeste. He was driven out of academic life and narrowly escaped assassination in 1936. The career of Minobe, which has been well...
dissected by Frank Miller (1965), provides a vivid commentary on the varying fortunes of Japanese political parties during the 1920s and 1930s.

A second elite was the civil bureaucracy. Tokugawa Japan had a long tradition of bureaucratic rule, both in the shogun's lands and in the daimyo domains and after the Restoration, in the 1880s and 1890s, a national civil service recruited by competitive examinations was established. This meritocracy, responsible to the emperor and subject to only limited control by the political parties, conducted the day-to-day government of the country. The highly trained upper civil servants numbered about 15,000 in 1927 and scholars who have analysed their role all agree on their vital importance in the government of Japan between the late Meiji period and the end of the Second World War (Quigley 1932: ch. 9; Silberman 1974; Spaulding 1971).

The power of the third elite, the military, was entrenched, first of all, in the 'right of autonomous command' and, second, in the effective veto which the leaders of the army and the navy held over the formation of cabinets. The first of these rights, which dated from the creation of a General Staff System in 1878, meant that generals in the field could take operational decisions there without consulting the
cabinet, even though these operations might have the most important political consequences. This right, when it was exercised in China in the 1930s, had the most profound consequences for Japan. The veto over the formation of cabinets arose from the right of the armed services to approve the choice of war and navy ministers, who were always generals and admirals. These huge powers, exercised at the expense of civilian politicians, seem even more dramatic in view of the very limited influence of the party politicians in military affairs. The whole situation was complicated further by the endemic rivalries both between and within the army and navy, and the work which has been done on the military has, therefore, revealed an elite which was both immensely powerful and very divided (Maxon 1957; Crowley 1962; Hackett 1964).

A further elite was big business. The great zaibatsu cartels, huge conglomerates with financial, industrial and commercial interests, which reached the height of their power in the inter-war years, had important connections with both the bureaucracy and the political parties. Bureaucrats in post who provided help to businessmen could expect sinecure jobs in business when they retired, and the zaibatsu contributed large sums to the campaign funds of the political parties, with
the two greatest zaibatsu, Mitsui and Mitsubishi, being traditionally linked with the

two main political parties, the Seiyukai and the Minseito (Tiedemann 1971; Roberts


Finally, the emperor's advisers wielded great influence. They consisted of the
genro and the leading members of the imperial household, and there is some

excellent scholarship on their roles. The genro (or elder statesmen) were the most

important of the Meiji oligarchs. Until 1901 one or other of them headed the
government of the day. After that they acted as elder statesmen, advising the

emperor on the appointment of prime ministers as well as on other important affairs

of state. After 1924 only one genro, Prince Saionji, was left alive and in the 1930s he 665

fought an increasingly hopeless battle against the increasing power of the military. 6

The imperial household ministry, set up in 1885, not only served the emperor's

personal needs but provided him with advice on political and military affairs and

Titus, in an important book (Titus 1974), has shown how its higher officials carried

out their duties. A Western-style peerage had been created in 1884 and a Privy

Council, an advisory body to the emperor, had been set up in 1888. Both can be

regarded as part of the imperial establishment.
One subject which has provoked considerable debate in the West as well as in Japan is the role of Emperor Hirohito in the events of 1931-45. Some scholars, like Sheldon, stress that he saw himself as a constitutional monarch, normally obliged to follow the recommendations of his ministers and other advisers, but argue that in private he consistently advocated a moderate line of action, always doing his best to curb the more reckless ambitions of the military. When the political machinery of the state was in disarray in August 1945 and he had to make a personal decision he voted for Japan’s unconditional surrender to the Allies at a time when the preservation of the imperial institution itself seemed to be endangered by the wrath of the victors. Other historians, like Bix (1992) in a recent article, have been much more ready to assert his war responsibilities and it is clear that the issue will provoke continuing debate.

Another subject of controversy is the precise balance of power within the elite groups at any given moment during the pre-1945 era. In the Meiji period attention has usually been focused on the genro, in the 1920s the political parties are often seen at the centre of the stage and in the 1930s the armed forces seem predominant.

The interpretation of the 1920s as 'Taisho democracy' (the Taisho emperor reigned
between 1912 and 1926) has been understandably attractive to Western historians, who have stressed that this was the era of party governments and who have seen the introduction of universal male suffrage in 1925 as a pivotal event. It has also been argued, however, that the political parties of the Taisho period were corrupt, selfish and limited in their conceptions of democracy they were unwilling to widen it to envisage social and economic justice for the peasants and the urban proletariat. In this latter line of argument the 'democracy' of the 1920s was largely a sham and there was much more continuity between that decade and the militaristic 1930s than the proponents of 1920s democratization would allow. 7 What does seem clear is that the elite structure persisted throughout the Pacific war. Professor Shillony (1981) has demonstrated this beyond much doubt. He draws attention to the restraints placed on Tojo's power during the war years by the bureaucracy, the Diet, big business and the imperial institution. Japan never became a totalitarian state and Tojo's position never remotely resembled that of Hitler or Mussolini. Shillony's picture undoubtedly gives comfort to the large band of Western historians who are uncomfortable with descriptions commonplace 6 Hackett 1968; Hackett 1971 Yamagata was one of the most important of the genro until his death
in 1922; Connors 1987.

7 For discussions of the controversy, Wray and Conroy 1983: 171-98; Hoston 1986: llff. 666

among Japanese historians (e.g. Masao 1969) of Japan as a fascist state during the 1930s and early 1940s. In a particularly notable and convincing essay in scepticism Professors Duus and Okimoto (1979-80) argue that it is meaningless to speak of a fascist political system in Japan the emperor's position ensured that no supreme leader like Hitler or Mussolini could come to the fore and there was no mass fascist party and suggest that 'we should stop pondering why democracy failed in pre-war Japan, and consider instead why fascism failed'.

If the constitution of 1889, which provided the legal framework of Japan's pre-1945 political system, was part of the Meiji oligarchs' modernizing measures, so in a sense was their embarkation on an imperialist policy in Asia. When the oligarchs looked at the modernized nations of the West which they wished to emulate they saw that all of them were imperialist powers. It is hardly surprising, therefore, that imperialism seemed a natural as well as an attractive path to follow. Japanese imperialism was so bound up with the country's foreign policy that it is artificial to separate the two and Western historians have written with distinction both on Japan's foreign policy in a general sense here we have,
especially, Professor Nish's

(1977) analysis and on the motives, chronology and results of Japanese

imperialism in a narrower sense, where Professors Beasley

(1987), Myers, Peattie

and Duus have made notable contributions (Myers and

Peattie 1984; Duus et al. 1989). These authors agree that Japan's imperialist

ambitions were derived from a

variety of motives among which questions of economics and

security inevitably

loomed large, and in that judgement they echo the views of

the writers of more

specialized monographs, such as Professors Crowley (1966)

and Barnhart (1987).

Barnhart's recent book, for example, is a good

demonstration of the mingling of

economic needs, foreign policy and the quest for empire during the 1920s and

1930s, leading up to the dramatic confrontation with the United States at the

beginning of the 1940s.

Japan's constant interactions with the outside world in the years between 1868

and 1945, such a contrast to the isolationism of the Tokugawa period, continued

after the Second World War, but on a very different basis. There was almost as

dramatic a discontinuity in the country's foreign policy in the years after 1945 as in

the period after 1868. Japan's pursuit of an imperialistic and independent foreign

policy between 1868 and 1945 was replaced by a clear
rejection of all imperialist

ambitions and, at least for a time, by an almost complete
dependence on the United

States in international affairs. Japan's new rulers set out
to achieve world respecta

bility through economic strength and the measure of their
success can be seen in

Japan's crucial position in the world today. The exhausted
and impoverished state of

1945 has become the economic superpower of the early 1990s,
poised between East

and West. As such, Japan has established itself as a new
world power centre and

found a role for the twenty-first century (Saito 1990;
Drifte 1990).

The dramatic discontinuity of Japanese foreign policy in
the post-1945 era was

one result of the country's defeat in war and of the
American occupation. The extent

of Japan's plight in 1945 has been well described by Thomas
Havens (1978). The

war had had a steadily increasing effect on the life of the
Japanese people, with 667

mounting inflation, poor, scarce and rationed food, fewer
consumer goods each

year, higher and higher taxes, forced labour, separated
families, long casualty lists

from both the war zones and the home front and, from 1944,
carpet bombing by

American 'planes of the Japanese home islands which left
swathes of destruction in

almost all the major cities. The Occupation which followed
has been described
in many general studies, of which Kazuo Kawai's (1960) is probably the best. The American authorities were determined to achieve both the demilitarization and democratization of the country. Japan was deprived of all of the great empire which she had acquired since 1868, her armed forces were dissolved and there was a purge from positions of leadership of those who were believed to have 'participated in Japanese expansion'. On the creative front a new constitution, based on entirely different principles from the Meiji constitution of 1889, was promulgated in 1946, and, among many other reforms, the most important were probably those in land tenure and education. The constitution completed the theoretical demythologizing of the emperor's position which had begun in early 1946, when he personally renounced any claims to divine or quasi-divine status. Now the constitution vested supreme authority in the people 'with whom resides sovereign power'. The emperor was merely the 'symbol of the state', deriving his position from the will of the people. This was a revolution in the traditional concept of the Japanese polity and the emperor's new role has been well analysed by Titus (1980), though it should be noted that not all Japanese have been happy with the reduced status of the imperial institution, and
Hirohito’s death in January 1989 produced much speculation both in Japan and abroad about the future of the throne (Crump 1989). The constitution created a Cabinet system with ministers responsible to the Diet, guaranteed the vote to women on the same basis as men (over the age of 20), provided for an independent judiciary, decentralized local government, gave elaborate guarantees of human rights and in the most famous (or notorious) of all its clauses formally renounced the right of Japan to wage war. The establishment of responsible ministerial government seemed to decide once and for all the major problem which had plagued the pre-1945 political system: where did effective power in the state lie? Two of the pre-1945 elites, the military and the imperial advisers, were abolished. Big business was temporarily in eclipse (it revived to rejoin the elite system later), but the civil bureaucracy remained throughout the Occupation and thereafter as a functioning elite. In theory it was now firmly subordinated to the politicians, but in practice things were not so simple and scholars are still discussing vigorously today the actual balance of power between bureaucrats and politicians (Koh 1989).

There was also a fundamental reform of land tenure. Following the creation of a new taxation system for the land at the start of the Meiji period the amount of land
held in tenancy rose steadily, reaching about 45 per cent of the total by the early 1940s. The traditional picture of tenant farmers has stressed the difficulties and hardships of their lives as part of that 'underside' of modern Japan about which Hane (1982) has written so eloquently, and recent attempts to present their fate in a more favourable light (Waswo 1977, and especially Smethurst 1986) have not met with general acceptance. That debate seems certain to continue but, whatever its outcome, there is no doubting the fundamental importance of the 1946 land reform, well described by Dore (1959). The abolition of absentee landlordism and the selling of large quantities of land to tenant farmers at easy prices meant that within a few years only about 10 per cent of land was held by tenants. The reform created a group of independent, prosperous farmers who were to be a fundamental support of the conservative political forces which have dominated Japan since the war.

The education reforms built on the very high level of basic literacy which prevailed in pre-war Japan. Already during the Tokugawa period literary levels were high among men (Dore 1965), and the introduction of universal primary education for both boys and girls during the Meiji period produced an almost wholly literate population by the early twentieth century, although
advanced secondary and university education was confined to a small minority (Roden 1980). The whole pre-war system was closely controlled by the Ministry of Education in the interests of the state (Hall 1973) and pupils were taught, in ethics courses, the virtues of loyalty to the emperor. The post-war education reforms, in contrast, stressed education for all on a decentralized basis. The school-leaving age was substantially increased, the control of the Education ministry over schools was drastically curtailed and the content of education fundamentally altered, especially at the primary level, with the elimination of the old ethics courses and stress on the values of democratic citizenship.8 Since the war a higher percentage of children have stayed on at school beyond the compulsory leaving age and more of these have continued their education to a further stage within the vastly expanded university system. Many authorities now argue that the Japanese are the best-educated people in the world and 'objective' tests in subjects like mathematics seem to confirm this view. On the other hand, much of the education consists of hard memory-work and the pressures for achievement clearly have implications for the psychological development of individual Japanese. Recent books by Rohlen (1983) and Duke...
stress the efficiency of and striking results obtained by Japanese schools, but

Cummings's (1980) argument that the post-war school system has created a more egalitarian society has been strongly challenged. Rohlen, for example, has argued powerfully that the school system creates a good deal less social mobility than is sometimes thought.

Such divisions of opinion about the specific results of the educational reforms are part of a wide-ranging debate about the significance of the Occupation as a whole, a debate which Ward has surveyed in his introduction to a recent volume of essays (Ward and Yoshikazu 1987). As he points out, most historical judgements on the Occupation, especially those by Western scholars, have been favourable; but while the essays confirm this in general they also show the extent to which experts differ over how far the Occupation realized its full potential, especially where democratization is concerned. Here the position of women in modern Japan is clearly relevant.

Passin 1965 provides an excellent survey of educational developments from the Tokugawa period to the early post-war years. 669 Late Tokugawa Japan was a patriarchal society in which women's inferiority to men was clear in both theoretical and practical terms, with a woman subject successively during her lifetime to the authority of her
father, her husband and her
oldest son. In the period after 1868, despite the growth of
a feminist movement
which has been chronicled by Sharon Sievers (1983), basic
attitudes were little
changed. The Meiji constitution excluded women from direct
participation in the
political process, and the civil code of 1898, in all its
clauses relating to women,
'reinforced their subordinate, subservient position in
Japanese society' (Hunter
1989: 143). This legal inferiority, which was maintained
throughout the pre-1945
period, was in theory transformed during the Occupation.
Women were given the
vote in 1945 and their right to the franchise was
enshrined in the 1946 constitution,
which guaranteed sexual equality. The new civil code of
1948 formally ended
women's subordination to men, asserting their equal rights
in all areas of life. Here,
however, the words of the Occupation decrees and social
realities have remained very
far apart. Study of the contemporary Japanese rural scene
shows how hard the life of
a farm woman can still be, and Alice Cook and Hiroko
Hayashi, concentrating on
urban life, have concluded that the modern Japanese
employment system 'probably
exploits women more extensively than is the case in any
other industrialized country'
(Cook and Hayashi 1980: 2). An important book on
middle-class Japanese women
shows that, though they are certainly less confined to the house than their mothers were, their primary role continues overwhelmingly to be that of wife and mother; all other daily activities have to be fitted in around that central task (Imamura 1987).

The early post-war years, the period of the Occupation, were a time of severe economic difficulties for the prostrate Japan of 1945, but the greatest historical phenomenon of recent Japanese history has been the 'economic miracle' of the second half of the twentieth century which has transformed the country into the second largest capitalist economy in the world Gapan achieved that position in 1968 when she overtook West Germany in Gross National Product) and arguably the most successful. Since the 1950s her economic performance has continually outstripped that of the other major powers, and during the 1980s experts were debating when rather than if she would overtake the United States in total economic output. This dramatic economic advance has inevitably led to a huge Western literature, both describing the phenomenon and seeking to explain it.

It is worth noting that the remarkable economic growth of the period between the end of the Second World War and 1990 was the accelerated phase of a trend which has been evident throughout the twentieth century. Except for the collapse
during the Second World War and for some minor blips during the early 1990s

Japan's economy has grown continuously since the Meiji period and historians, in
detailing the advances of the second half of the century, are now stressing as well
the more modest growth of the first half. One major study which emphasizes
continuity between pre-war and post-war developments is Nakamura's (1981)
balanced and lucid discussion, which tells the story until the end of the 1970s. The
collective works edited by Patrick and Rosovsky (1976) on the one hand and
Yamamura and Yasuba (1987) on the other are very comprehensive and
between them cover the period to the 1980s, while Minami's (1986) important
volume adopts a quantitative approach. Among a wide range of valuable studies
which discuss more specific aspects of the Japanese economy and also throw light
on the general reasons for Japan's success are Cusumano's (1985) study of the
automobile industry, which shows, inter alia, how sharply Nissan and Toyota have
differed from each other in their development, a point which may give pause to
those who have talked too glibly of a single 'Japanese management system';
Gordon's (1985) work on labour relations, which shatters some myths about
relationships between managers and workers; MacMillan's
workings of the industrial system, stressing its positive aspects; and Chalmers

Johnson's (1982) book on the role of the Ministry of International Trade and Industry in economic development. These very different books, taken together,

demonstrate beyond much doubt the number and complexity of the factors which

explain the economic successes of the post-war years and the difficulty of assigning

primacy to anyone of them. An interesting additional perspective is provided by

Morishima (1982) who believes that in searching for the secrets of Japan's modern economic success it is necessary to go back to religious and cultural developments in the very early periods of Japanese history. Such studies emphasize the success story, but in recent years some authors have begun to question the reality or the endurance of the economic miracle. The works of such sceptics as W oronoff (1986) and Reading (1992) are doubtless useful antidotes to the Japanophile writings of Vogel (1979) and Kahn (1970,) but their more extreme scenarios, notably Reading's prediction of a coming Japanese 'collapse', will not despite the economic difficulties of the early 1990s readily convince those who find their work very one-sided.

Most commentators would agree that Japan's remarkable
economic success in the post-war period was a major reason for the dominance of Japanese politics by conservative forces from 1945 to the 1990s. That dominance was symbolized by the Liberal Democratic Party, formed in 1955, which only yielded power in 1993, and then to a coalition composed of seven opposition parties. There are good studies of the Liberal Democrats and the reasons for their success by Thayer (1969) and Fukui (1970). Books by Ward (1978) and Stockwin (1982) are among the best studies of the general workings of the political system, while a recent volume of essays, edited by McCormack and Sugimoto (1986), takes as its theme what the contributors see as the gaps between the democratic theories of Japan's 1946 constitution on the one hand and the authoritarian and nationalistic realities of present-day Japanese society on the other; this is a book which will give much comfort to historians who wish to stress that the Occupation had only limited long-term success.

The broad history of modern Japanese politics can, therefore, be seen as a story of stability achieved, then lost, then achieved once more, and then lost again: the control of the Meiji oligarchs was succeeded by the competing elites of the early twentieth century. These in turn were replaced by the dominant Liberal Democrats 671
of the post-war years. They themselves have been succeeded by unstable coalitions

and whether a genuine multi-party system will develop from the present situation

remains to be seen.

The dominance of the Liberal Democrats for a period of about forty years in

what is, avowedly at least, a democratic state is unusual, but certainly not unique

the Christian Democrats dominated the post-war political scene in Italy to almost

the same extent, and also lost power in 1993 but many Japanese are fond of the

word 'unique' to describe their history and culture. They use the term nihonjinron,

which can be translated as 'theorizing about the Japanese',

to describe the huge

Japanese literature which is devoted to what the authors see as the very special

qualities of their customs, history and beliefs. The general message which

nihonjinron is intended to convey is one of marked contrasts between most things

Japanese and most things Western with usually the added point, made either

implicitly or explicitly, that Japanese ways are better. In such thinking the Japanese

economic miracle could never have happened in a Western country because so much

of it stemmed from Japan's own peculiar cultural attributes, such as the pre

eminence of the group rather than the individual. A good deal of Western literature
on Japan has absorbed, sometimes almost unconsciously, the less extreme ideas of nihonjinron, but the concept has also been attacked head on by some scholars, not least by Dale (1986) in a recent book.

It is probably best to steer a path midway between the extreme views of the most enthusiastic proponents of nihonjinron it is unlikely, for example, that Japanese brains work in an entirely different way from their Western counterparts! and the equally extreme views of diehard sceptics like Dale. There is a powerful argument to be made that, in the broad context of world history, modern Japan is unique. Meiji Japan was the only non-Western country successfully to copy the Western great powers of the day, transforming itself into an industrialized, imperialist power, and the industrialized Japan of the second half of the twentieth century has consistently outstripped these same Western rivals in economic growth over a period of forty years. Moreover, studies of Japanese society, of which those by Nakane (1979), Smith (1983) and Hendry (1987) are among the best, stress the continuing importance of the group as opposed to the individual to an extent which would be unthinkable in similar studies of a Western society. Supporters of nihonjinron can also point to the peculiarities of Japanese religion. It is true that contemporary Japan, just
like so many Western countries, is often described as a 'secularized society', but,

whatever the truth of this judgement, its whole religious background, a mixture of
Shinto, Buddhism, Taoism, Confucianism and folk religion, with additions of 'new
religions' which were derived from Buddhism and Shinto and came to the fore in the
nineteenth and twentieth centuries, is very different from that of any of its economic
rivals. Christianity has never been a powerful force in Japan and today only a very
small minority of the population are Christians. 10

10 Good surveys of the Japanese religious scene are Kitagawa 1966; Ellwood and Pilgrim 1985; Byron
Earhart 1982; Thomsen 1963. 672

The works mentioned in this survey represent a tiny fraction of the historical
research and scholarship which has been published on modern Japan a recent and
from the 1860s to the 1980s, the vast majority of them published since 1945 (Perren
1992) and some scholars have taken time from their more specialized work to
produce general surveys of modern Japan, incorporating both their own researches
and those of fellow-scholars. Among the best are books by Beasley (1990), Duus
(1976), Storry (1982) and Hunter (1989). Beasley is especially strong on the mid
nineteenth century, a reflection of his expertise in the Meiji Restoration; Duus, a
specialist on Taisho political parties, deserves special
mention for his treatment of
the politics of the inter-war period; Storry, who wrote a
notable book on right-wing
politics in the 1930s (Storry 1957), has a brilliant
general chapter on that decade;
and Hunter, who eschews the essentially chronological
approach of the other three
writers in favour of a thematic treatment, is especially
good on economic and social
history, in which she has research interests. Very recently
The Cambridge History of
Japan has begun to appear, with volumes on the nineteenth
and twentieth centuries,
published in 1989 and 1988 respectively (Gansen 1989; Duus
1988). The primary
purpose of the work, as described by the general editors, is 'to put before the
English-reading audience as complete a record of Japanese
history as possible',
though it is also stated that a decision was taken to
leave out 'the history of art and
literature, aspects of economics and technology and
science, and the riches of local
history'. The volumes undoubtedly have great merit they
sum up much
scholarship on the topics with which they deal as well as
presenting readers with a
good deal of new research and ideas but it is also fair to
point out, as Hunter has
done, that they were commissioned about twenty years ago
and that the history
which they contain is mostly 'mainstream history as it was
broadly accepted in the
early 1970s ... , the history of national politics, macro-economics and international relations' (Hunter 1990: 286). Where, she asks, are the women, the workers, the underdogs? This is fair comment, even though it says no more than that the volumes are works of their time, which means the time of commissioning rather than the time of publication. Despite that perhaps inevitable drawback they will be


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In all former European colonies intellectual elites have sought in history a body of
myths of origin to legitimate their independence. This has been the most powerful influence on the development of post-colonial historiography. Yet Indian historical writing has a deeper lineage than this might suggest. At several points over the last two centuries India edged from the margins towards the centre of international historical interest. The discovery of the Enlightenment that Sanskrit was an Aryan language and interest in oriental despotism as a mirror of Western absolutism ensured that late Mogul India engaged the attention of the international scholarly community. Ironically, it was the British conquest of the subcontinent, and particularly James Mill’s ferocious denunciation of the barbarities of Indian society in his History of British India (1817), which pushed it out to the margins again.

Fifty years later, Sir Henry Maine’s Village Communities in East and West (1871) initiated a discussion about the status of village, commune and property right in historic civilizations which drew upon some of the arcane lore of the Indian district officers. In the 1960s, again, the debate about preconditions and 'take-off' in industrialization generated an interest in Indian economic history which was matched by a fickle admiration among the Western intelligentsia for Nehru and the Non-Aligned Movement.
Generally, however, Indian history has seemed exceptional, mainly because it fitted into no obvious paradigm of development, whether liberal or Marxist. It seemed to have failed to follow the 'right' form of industrial development under both Raj and Republic. Neither the Raj nor the Indian National Congress had been able to forge a solid and economically effective modern state above the melie of factions, castes and local bosses. Fierce disputes between Hindus and Muslims, or between Sikhs and the central government, seemed still to mark it out as a fragile civil society always on the edge of being wiped out by ethnic violence, religious obscurantism and 'amoral familism'. Every decade since the death of the Mogul Emperor Aurangzeb in 1707 seemed to have been 'the most dangerous decade' for India, and its historical writing, too, limped from the examination of one peak of famine, slaughter and civil unrest to another. Now we all seem to be in the same boat. The capacity of the modern state and international economy to erode localism, to suppress inter-ethnic violence and religious extremism seems everywhere in doubt. The collapse of the Soviet Union, the West's crises of deindustrialization and the rapid rise of Asian economies towards global dominance (though not, as yet, India's), eroded many of the old
teleological certainties of economic history. Now, too, we all live in plural societies balanced on the edge of ethnic and religious violence. We have all become citizens of little Indias.

These changes in the world outside have been mirrored in the cultural battles of the media and the academy. While cultural and historical discourse remained 'centred', India with its myths and ballads, linguistic and semantic complexities, seemed like some worn old Afghan patchwork, eye-catching but impractical as an area of study. Even the Marxists despaired, and invoked the 'Asiatic' and 'colonial' modes of production to explain why India seemed different. But now that 'decentred discourses', 'thick description', 'cultural self-representation' have become the norm in the academy, particularly in America, India has come into its own again. Indian historians were postmodernists before their time, though often unknowingly or unwillingly. It is not surprising therefore that able young expatriate Indians have entered the historical and literary culture-wars of the United States with great vigour. This revolution of the Green Card-holders has begun to surface in Europe and New Delhi. Indian novelists writing in English have produced texts bursting with historical comment, while, at their head, Salman Rushdie has himself
become an exercise pit for international cultural combat. Yet despite its slow advance to international significance, Indian historical writing is still deeply immured in the debates and controversies of the past. This chapter seeks to uncover both the continuities and discontinuities in the writing of modern Indian history. THE ORIGINS OF MODERN INDIAN HISTORY

It is often said that India, and in particular Hindu India, lacked a strong tradition of historical writing. For Hindus, time is supposed to have been 'fuzzy' or even 'cyclical'. History in its modern form, according to this view, came with the modern state or in India with the colonial state. These statements are dubious at best. Hindu India expressed its historical memory through legends and ballads, a true representation of popular constructions of the past. Yet even Hindu kingdoms had elaborate records, genealogies and annals which could be as precise as those found in other early modern societies (Tod [1819] 1982: I, 1-3). Likewise, an Indo Muslim tradition of historical writing, drawing on the precision and concern for correct testimony of Arabic and Persian exemplars, developed under the patronage of

1 Harrison 1960 was a classic of this genre.
2 For a discussion of these concepts and the concept of feudalism in Indian history see Byres and Mukhia 1985. 678
of the Mogul and other Indian Muslim courts. This tradition continued to flourish during the late seventeenth and eighteenth centuries. Persian histories of dynasties and religious teachers, biographical dictionaries of writers and poets, local histories of landed clans and small towns were written in great numbers, often by Hindus of the scribal classes who had taken Mogul service.

While one of the main themes of these histories was the evanescence of human might in the face of God's greatness, other more 'secular' themes emerged (Hardy 1960). Eighteenth-century indigenous historians were deeply concerned about the corruption of Mogul public office and the decline of virtue. Such writers were by no means innocent of historical explanation; it is simply that their explanations differed from twentieth-century predilections, though not necessarily from eighteenth century European ones. The Mogul conquerors had lost their virtue, it was said, in the great well-watered and well-provisioned cities of the Indian plains; their place was usurped by hard men, usually Hindus from the dry, unwatered parts of the Empire. The Empire was like a human organism; its humours were out of balance, with the dry and choleric overwhelming the damp and bilious. 4

These themes were quickly adapted to the new conditions created by the rise of
British power after Robert Clive's seizure of the revenues of Bengal in 1765. Persian histories, notably Ghulam Hussain Tabatabai's Siyyar al-Muta 'akhkhirin (1790), chronicled the gradual British takeover of the Mogul domain by stealth. This and similar works sometimes lauded the stability the 'Franks' brought, but often denounced their brutality and incivility. These historians wrote of a revolution in power which had brought the 'drunken and licentious butlers' of the English to power. In the early nineteenth century grand chronicles were matched by large numbers of local histories, often produced by the old literati under British direction. These works informed the new British collectors and land-revenue managers, but they also embodied claims for status and land-rights on behalf of particular groups of landlords and local government servants in the new British districts. Indians were, therefore, consciously producing their own histories of 'modern times' during the very period of British conquest. Over recent years, these have provided the primary materials for a number of histories which have sought to portray the Indian eighteenth and early nineteenth centuries in a more rounded and complex light than the old stereotypes of the Time of Troubles. These works, many of American origin, have shown how dynasties created local and regional states, how
these states functioned through the redistribution of 
resources and honour, and

3 Abul Fazil's 'Akbar Namah' was a model, as was Inayat 
Khan, 'Shahjahan Namah', tr. W. Begley 
and Z. Desai, Delhi, 1990: some eighteenth-century 
representatives of this tradition were Mustajab 
Khan Bahadur, 'Gulistan-i-Rehmat' tr. C. Elliot as The 
Life of Hajeez-ool-Moolk, London, 1821; Abu 
Talib Isfahani, 'Tafzih-ul Ghafalin', tr. W. Hoeyas 
History of Asaf'ud Daulah, Nawab Wazir of Oudh, 
Allahabad, 1885; Mahomed Faiz Baksh, 'Tarikh-i Farah 
Baksh', tr. W. Hoey as Memoirs of Delhi and 
Faizabad, Allahabad, 1889.

4 Ghulam Ali Azad Bilgrami, 'Khazana-i-Amira'; large parts 
of this are translated in Rao 1963, esp. 
p.227. 679

how they in turn provided much of the scaffolding for the 
East India Company's 
rule, both in the areas it controlled directly and in the 
princely states under British 
Paramountcy (e.g. Barnett 1980; Fisher 1987; Cole 1988).

IMPERIAL HISTORIES AND THE CIVILIZING MISSION 

Early British writing on India drew heavily on the 
indigenous historians either 
directly through their writings or from verbal informants. 
The local histories passed 
into the body of genealogical and revenue lore which were 
later embodied in the 
British District Gazetteers, Reports on Revenue 
Settlements and other statistical and 
topographical enquiries which remain the staple of Indian 
social historians. But by
the 1830s there was a sea change of mood. The new vigour and arrogance associated
with the anglicizing bureaucracy of Macaulay's period had less time for Indian
histories and their pretensions. Sir Henry Elliot, of course, fathered a twelve
volume work, The History of India Told by its Own Historians ([1869] 1958). But his
aim was specifically to disabuse the 'bombastic young baboos' [English-educated
gentry of Calcutta], of the idea that Mogul times had been superior to British rule.
After James Mill's History, Anglo-Indian historians voiced few objections to the
British conquest of India and were content to chronicle the divisions, disorganiz-
ation and depravity of a society which seemed to them to have invited foreign
conquest. It is true that the Mutiny Rebellion of 1857 resulted in violent disagree-
ments between those who blamed the military authorities and those who claimed the
civil authorities were to blame (Stokes 1986: 1-35) but no one of significance
doubted the legitimacy of Britain's civilizing mission in the East.
On the surface, Anglo-Indian historical writing in the later nineteenth century
followed the same plan. Social history was rendered down into 'folklore' and
disquisitions on 'tribes and castes' or bottled up in gazetteers. Economic history was
severed from political and immersed in land-revenue settlement reports and
British Indian political history was ensconced in Oxford University Press's Rulers of India Series which memorialized a succession of British men of action, prayer and thought, in direct succession to the Great Mogul Emperors. Yet Anglo-Indian history, whether written in a bureaucratic or public mode, was already under pressure. Since the 1840s Indian historical writing had revived, especially in Bengal. In the early days much of this writing was sycophantic and the most interesting development was the increasing blackness with which Hindu writers portrayed the period of Muslim (though not yet British) rule. Many of these histories were produced under the auspices of the newly formed Committees of Public Instruction. But even here many small barbs were beginning to be stuck into the carcass of the Raj. The tools of scientific historical criticism which were being used by Western orientalists to dissect India's past could quickly be turned against the presumption of Britain's civilizing mission.

5 e.g. Prasad 1851, which is largely a precis of Cunningham's History of the Sikhs, 1852. 680 INDIAN HISTORIES AND THE NATIONALISTS

The foundation of the Indian National Congress and the emergence of 'economic nationalism' in the 1890s was accompanied by a spate of works which denounced the
British for loading the peasant with intolerably heavy revenue demands. They also attacked the government for repatriating the wealth of India in the form of the Home Charges levied by the government of India. In the realm of political history, there was an increasing desire to show that the Moguls and regional dynasties, such as the Hindu Maratha states which preceded the British, were wise and humane.

Indian nationalism was now matched by the growth of regional feeling, especially in Bombay and western India. But the seminal work in the new Nationalist history did not appear until 1908 when young radicals in the Congress had made a lurch towards more radical and insurrectionary types of politics. This was V. D. Savarkar's The Indian War of Independence 1857 (finally published in 1947), which portrayed the movement of that year not as a mutiny, but as a justified response to British tyranny and racism. At this point the main lines of historical debate both for the last days of the Raj and for the years after Independence were already laid.

In British writing about India, the early twentieth century saw a slow adjustment to the growing power of the Indian National Congress and its rival, the Muslim League, which had precipitated mass civil disobedience and the constitutional reforms of 1909, 1919 and 1935. Yet the Cambridge History of India and Vincent
Smith's Oxford History of India both remained remarkably British-centred and colonialist in their basic premises. Even radical histories such as Edward Thompson and G. T. Garrett's The Rise and Fulfilment of British Rule in India (1934) continued to laud Britain's initial civilizing mission and also to assign to her a residual role in arbitrating between the supposedly homogeneous blocs of Hindu and Muslim opinion.

More interesting was the emergence of Indian social and economic history during these years. Much of the best work was based on official and local sources and hidden away in the growing number of local historical journals such as Bengal Past and Present, The Indian Antiquary or The Journal of the Andhra Research Society.

The dominant figure here was W. H. Moreland, whose The Agrarian System of Moslem India (1924) was the founding text of pre-colonial and, by default, early colonial Indian economic history. Moreland's work arose from his period as Director of Land Records in the United Provinces (see Moreland 1912), and it was in direct descent from the history of Sir Henry Elliot sixty years before in that it sought to show that the pre-British economy was small and hampered by poor transport, and that its long-distance internal trades and external commerce dealt in
little more than luxury commodities. Indian historians connected with Aligarh Muslim University have worked hard to correct these stereotypes (notably Habib 1963), but they are still dominated by Moreland’s influence in the topics they choose. Indeed, despite the emergence of self-consciously Marxist, Gramscian and anthropological schools of history in the last twenty years, the ideas and material accumulated by the historians and sociologists of the Indian Civil Service remain critical to the writing of Indian history. Hardly a single argument or idea which has emerged from increasingly sophisticated writing on Indian history in the last generation lacks its precursor in the musings of a revenue official or the writings of a district judge. Yet Indian historiography is not yet mature enough to admit this debt, since a facile anti-colonialism flourishes throughout the academy in India and abroad. Alongside Moreland should be mentioned another founder of modern Indian history who also wrote on the immediate pre-colonial period. This was Sir Jadunath Sarkar (1901; 1919; 1955, etc.; see Gupta 1957) who initiated a dispassionate and largely apolitical strain of national history which had many imitators among the next generation of historians of the colonial period. The most typical work of this generation of ‘grand old men’ was The History
and Culture of the Indian

People (1951-69) produced by R. Majumdar and his

Radical nationalist Indian history was taking shape faster

in the 1920s and 1930s

against the background of the struggle for freedom. The

'economic nationalist'

interpretation of Indian history which had arisen amongst

the bourgeois nationalists

of Bombay and Calcutta in the 1880s and 1890s was now

crossed with a more self

consciously Marxist strain, though before Independence this

was banned in schools

and universities. M. N. Roy (see Bhattacharjee 1971) and R.

Palme Dutt (1940),

orthodox Marxists, laboured alongside Fabian socialists,

notably Jawaharlal Nehru

(1965), to show that India's struggle against the British

was part of an international

struggle against capitalism. Their assumption that

British-imposed free-trade wiped

out the Indian textile industry in the early nineteenth

century and stunted the

growth of Indian-owned machine milling in the later

nineteenth century provided a

historical justification for the economic autarchy which

Marxists of all hues argued

should constitute the policy of an independent India. 'Landlordism' was identified

as the problem in the internal economy. Historical evidence

was brought forward to

show that the British had favoured their collaborators

among the great landholders,
while poor peasants and small landholders had been expropriated.

This dominant leftist and Marxist tendency within Indian historical writing did not go unchallenged, however. Regional history flourished in the last years of the Raj, partly, one suspects, because the Education Department smiled on a history which appeared to fragment India into provinces, castes and cultures in a manner which matched the official political discourse. Regional patriotisms did, however, have a life of their own. In western India scholars put in train a large programme of publication from the records of the erstwhile Maratha rulers of the seventeenth and eighteenth centuries (e.g. Ranade 1900; Kincaid and Parasnis 1918-25; Sardesai 1946-8). Their interpretation of the Maratha kings and their ultimate fight to the death against the British was significantly different from the treatment accorded them by Bengali historians, who tended to see the Marathas as 'predators'. In the south, the Tamil literary revival gave rise to a historiography which claimed that the brahmans had subverted the cultural and political independence of southerners, a stance which matched that of the Justice Party, a non-brahmin conservative alliance which dominated the Madras Presidency in the inter-war years and proved to be a precursor of modern southern regionalism (Irschick 1986). This theme of 'brahmin 682
'dominance' was taken up in the caste and community histories of low-caste workers which were beginning to be written as labouring people and the poor began to be mobilized into politics in the 1920s and 1930s (Gooptu 1991). In these histories the British played an ambiguous role. Sometimes they were depicted as foreigners who had begun to break the yoke of brahmin priesthood; equally often they were held to have perpetuated it by educating brahmins in science and the English language.

The most politically important challenge to the writing of secular, leftist and nationalist history came, of course, from a section of the Muslim leadership. In the 1860s and 1870s Sir Syed Ahmed Khan, founder of the Aligarh College, had asserted that Hindus and Muslims were 'two nations' who merely shared the soil of India (Robinson 1974). An inheritor of the eighteenth-century traditions of local and dynastic history, he argued both from Islamic history and the history of 'modern times' that Muslims had an identity which set them apart from the Hindus, and that this entitled them to separate representation and separate leadership within imperial India. Many, but by no means all, Muslim leaders and literati elaborated these themes in the next generation. The celebration of the Islamic and west Asian cultural and racial heritage of
India's Muslims reached its apogee in the work of Maulana Muhammad Iqbal, a Persian and Arabic scholar, who argued on historical precedent that they should have a separate 'homeland' within the redrawn map of post-colonial south Asia (Hussain 1971). Ultimately, the creation of a separate history for Muslims resulted in the virtual abandonment of historical writing in the future Pakistan. If Islam, rather than the historical experience of Muslim people living amongst their Hindu neighbours, was to be the touchstone of national memory, then history was itself a valueless category. In Pakistan, modern history has largely been dropped in favour of the vacuous 'Pakistan Studies'.

THE POLITICAL CONTEXT OF POST-Independence Historical Writing

All new nations work to consolidate their myths of foundation and India was no exception. To a surprising extent history remains a fundamental constituent of contemporary political debate. Controversies ranged from banal polemic generated by the decision to remove the statues of British conquerors from public places to esoteric debates about the balance between exogenous and endogenous factors in India's nineteenth-century growth rate. This latter debate has remained lively. The reason is that the position scholars take on the historical record tends to be reflected...
in their stance on whether India should deregulate, liberalize the economy and plunge naked into the storms of the international market, unprotected by the regime of controls established by Nehru's Congress. When the American economic historian Morris D. Morris proposed in the early 1960s that colonial rule may not have been as dire for the Indian economy as commonly thought, he stirred up a massive controversy which still echoes on, partly for political reasons.

Resistance to the British also remains a hot issue. The Ministry of Tourism in Uttar Pradesh now offers tours to see the locations of the glorious deeds of the Indian 'martyrs' of the 1857 rebellion. Political parties jostle to appropriate the heroes who died on the scaffold or in hails of British bullets during the Independence movements. Sub has Chandra Bose, 'Netaji', who took up arms against the British alongside the Japanese in 1942, has become a national hero who dwarfs the pacific and saintly Gandhi. Ironically, the fact the India did not throw up a mass anti-colonial revolutionary movement, like Vietnam or Algeria, has served to radicalize its history. New village Hampdens are discovered lurking in every colonial police file.

Institutional changes inside the country and outside have also moulded the new
historiography of post-Independence India. Indian educational institutions and centres of advanced study have increased immeasurably since Independence.

Dedicated historical institutes such as the Nehru Memorial Museum and Library or the National Archives of India have co-operated with the Indian Historical Manuscripts Commission and the Indian Council of Historical Research to breed a new professional academic community. The 'golden triangle' runs between the Nehru Library, the campus of Jawaharlal Nehru University and the offices of the Economic and Political Weekly in Bombay. Congress patronage and the political stance of Indian youth has tended to ensure that this new professional community of Indian scholars has remained in the mode of its Nehruvian predecessors. The effort has been Marxist, anti-imperialist, anti-capitalist and more recently directed to 'history from below'. A glance at the articles in the most innovative of the historical journals, The Indian Economic and Social History Review, will show that the majority of them continue the tradition of anti-imperialist historical writing which was pioneered by Dadhabhai Naoroji's Poverty and un-British rule in India (1893; see Masani 1939) and was consolidated by Bipan Chandra's History of Economic Nationalism in India. The concern of Jawaharlal Nehru with the peasantry,
manifested in his Discovery of India (1935), has spawned a huge number of books and theses in recent years, seeking to reconstruct, largely from colonial records, the history of the 'people without history'. But it would be a grave injustice to believe that the domestic historical profession represented a kind of historical establishment of the sort that existed once in the Soviet Union and now exists in China. The vitality of Indian historiography in recent years reflects deep internal contradictions as much as the growing influence of historiographical changes overseas, notably in the United States, but to a lesser extent in Britain and Europe. Before we go on to deal with several important areas in recent historical writing, we will outline some of these fissures and influences.

While Indian historiography has been generally leftist, the Indian Left has been a house minutely divided against itself. Distinct pro-Chinese and pro-Soviet schools of historical writing loosely matched the political parties which espoused these causes. The Congress right and left also saw different lessons in the history of the nationalist struggle. All the while, regional political parties and their ideologies subtly influenced the style of history being written. There is no sense in which these political divisions were directly reflected in historical writing. But a tendency to
study the working class as against the peasantry, a predilection for jacquerie-like peasant movements, as opposed to Congress-led nationalism, often betray the political assumptions which Indian authors bring to bear on their work.

The issues of religion and region are only now beginning to surface in elite historical discourse. There has been an enormous gap between the professional, cosmopolitan, English-speaking elite and the vernacular schools of historical writing and teaching in which 95 per cent of the country's historical effort is concentrated.

Some of these non-elite institutions still use translations of old British textbooks. But others, particularly in states where anti-centre, non-Congress parties are in power, have begun to propagate forms of local chauvinist and fundamentalist religious historiography which have alarmed and galvanized the historical establishment. Most significantly, the country's main rightist Hindu party, the Bharatiya Janata Party, has over the last decade made an organized effort to infiltrate professional institutions and to propagate its own version of history. This version holds that Nehru's socialist secularism is a distortion of India's true Hindu identity, and no more than a continuation of the anti-Hindu agenda of India's British rulers. They depict Muslim rule as a scourge, Muslims as foreigners
in India, and the partition of India in 1947 as a betrayal by Gandhi and Nehru. Some of India's leading 'secular' historians have taken up arms against this revival by the Hindu Right of the nineteenth-century Muslim assertion that Hindus and Muslims in India were 'two nations'. It was colonial rule, they say, which 'constructed communalism', that is Hindu-Muslim conflict. It is too early to say where this cultural struggle will lead, but it has already influenced the writing of Indian history to the extent that 'communalism', that is Hindu-Muslim and other types of religious conflict, has become a critical area of research, overtaking an earlier obsession with anti-colonial nationalism. 6

Finally, what are the international professional influences that have come to bear on the domestic Indian historical scene since Independence? The most important development has, perhaps, been its emancipation from the influence of British historiography (cf. Kumar 1989). While many young Indian historians still come to Britain for postgraduate studies (almost exclusively to Oxbridge and London), the colonial monopoly has been broken. A significant number now follow MAs in Jawaharlal Nehru University with doctoral work in the United States, Australia, France and Germany. In Chicago, Pennsylvania and Berkeley
they come into direct contact with the tradition of historical anthropology which has dominated Indian studies in America since the 1960s. As disciplinary boundaries break down in US academia, they are much more likely to exchange ideas with the new generation of literary theorists and 'discourse analysts'. Besides this, most US orientalists come out of a tradition of study in literature and civilization which insists that doctoral work has a strong component of indigenous language material. This was never the case in Britain, or, ironically, in India itself, where many capable dissertations were written without benefit of any indigenous language sources.

6 See the recent collection by Pandey 1993. 685

On balance, this pedagogical tilt towards America, and the matching awareness of French historical and social theory (the Annales School, Foucault, Derrida, etc.), has been invigorating, not least to the writing of Indian history in Britain. Yet some things have also been lost alongside gross British historical empiricism. Few students have much knowledge or understanding of the political and social theories which the colonial rulers brought to bear in Indian life, let alone any grasp of British social and economic history. Similarly, for all the antediluvian emphasis on constitutional history and sturdy narrative, the old school of British imperial history
did at least provide a framework for studying issues of imperialism and resistance at a global level. Now one often gets the impression that authors believe that India was the only country to have been colonized, while naive essentializations of 'the British' or the 'colonial state' have replaced the alleged orientalist reduction of India to a pot-pourri of essences. Another measure of the internationalization of Indian historiography has been the relative decline of economic history. While India produces some of the best economists in the world, economic history, especially quantitative economic history, has recently suffered a distinct loss of status as the brightest young minds in India hitch their colours to the standard of an ill-defined concept of 'culture'.

PRESENT DEBATES AND OBSESSIONS

It would be wrong, it must be said again, to think that contemporary Indian historiography has succumbed completely to political or intellectual fashion. Both inside India and outside an equally powerful force has been the growth of professionalization of history. In the United States and India, regional studies associations have come into being. Their annual conferences and newsletters chart the development of the history of Bengal, the Punjab or Tamilnadu. Urban history has its own Indian association, 7 while the Indian Council of Historical Research, the...
Indian Historical Records Commission and the National and state archives have continued programmes of documentary publishing. Environmental history, while clearly responding to contemporary worries, has become established as a serious branch of Indian historical writing. The history of science in India now shows signs of throwing off its early need to show that all major scientific discoveries in the West were anticipated in India.

Some of the older fields of study have continued to grow without scholars very obviously looking over their shoulders at contemporary political disputes. One area which has moved from strength to strength since the 1960s has been the study of the British conquest and initial impact on India. Echoes of the broader debates on imperialism and British motivations in conquest, spurred to life again in the 1960s by Robinson and Gallagher's Africa and the Victorians, took some time to reach India's shores. When they did, interesting variations were discovered. Some historians continued to argue that the British conquest was essentially one part of a
geopolitical battle for supremacy in the wars with the French. Others argued for an expansion dominated by economic interests, whether it be the interests of the East India Company itself, or private merchants in cloth, raw cotton and opium, or the interests of the Indian merchant entrepreneurs who financed much of the British trade. More sophisticated studies of the politics and motivations of the East India Company officials and Indian rulers produced arguments that stressed the indirect influence of political or economic crises. For instance, it has been argued that the key to British expansion was 'military fiscalism'. The eighteenth-century British had to finance their colonial armies with local resources. In India this meant forcing or cajoling Indian powers to pay for the Company's presence through the mechanism of the so-called subsidiary alliance system. The need to payout so much cash to foreigners forced Indian states either to resist and be conquered or to hand over large tracts of their territory. In either case the British frontier inexorably expanded both across the map and down into Indian society. This was a local variant of Galbraith's notion of the uneasy frontier. At all events, the recent emphasis on local agents of expansion, whether the British official, private traders, the East India Company's army or the Indian communities which served and financed the
expansion, has proved productive of much new material and argument.

Indian resistance to British colonial expansion, as well as to the mature Raj, has been another topic which has attracted great interest. In the eighteenth century the emphasis has generally fallen on the last independent Indian states. As in the case of Africa, the propensity of post-Mogul Indian rulers to resist or capitulate has been closely calibrated to the social politics, resources and ideologies of their ruling elites.

At the same time the stark dichotomy between 'collaboration' and 'resistance' has been eroded. For the nineteenth and twentieth centuries resistance has been seen to be the response of specific communities on the land, fortified by their sense of space, their symbols of kingship and their indigenous definitions of proper government. Indian historians' debts to international peasant studies, especially to the works of Eric Hobsbaum, E. Le Roy Ladurie, E. P. Thompson and James Scott, have been very great. There have been many academic scuffles about the social and economic status of the resisters. Were the rebels of 1857 rich peasants, or small landlords? Or, on the other hand, were they the wretched of the earth, the impoverished smallholder or dispossessed rural labourer? 10 The same sorts of arguments have been raging about the peasant movements of
the later nineteenth century and those of the Gandhian era. In the world of industrial labour there have been interesting investigations of the origins of the extraordinary stamina with which Indian working people pursued their disputes in the twentieth century (see Chandavarkar 1994; Chakrabarty 1986).

However, in the last few years an important group of historians has switched away from social structural and economistic arguments about the origins of peasant and working-class resistance. Disillusionment with orthodox Marxism and with the notion that the Indian National Congress ever really played a leading role in the 'mobilization of the Indian masses' have been rife, as Congress and the Communist parties themselves seem to come to represent the new establishment in the Republic of India. The so-called Subaltern Studies group of mainly younger Indian historians have consciously employed the arguments of Gramsci to stress the role of culture in domination and resistance, and to loosen the grip of social-structural understandings of the motivations of the poor and the oppressed. This work has been productive of most interesting details and observations about the life and struggles of peasants, rural labour, Indian 'tribal groups' and the
underclasses of the towns and many sophisticated arguments. Yet the Subaltern Studies Group of historians and their imitators have had considerable difficulty in integrating their histories from below into a picture which accounts for historical continuities and change. Despite a Gramscian agenda, the dominance of the ruling elites, and indeed of the imperial state itself, remains unexplained. There also appears to be a clash of agendas. One part of the project stresses the anti-imperial heroism of the 'subaltern', and tends to essentialize and even romanticize resistance. Another, influenced by literary deconstruction, is sceptical of the existence of anything outside 'discourse' in an ultra-Foucauldian mould. The etiolation of conjunctural economic history has made it difficult to discover why anything occurred when it did. Another set of problems arise when historians attempt to combine standard archival sources with increasingly sophisticated collections of oral traditions. Meanwhile, a growing number of studies of Indian women from a feminist perspective have argued that even history from below has tended to ignore the role of Indian women, denying them agency and 'alternative discourses' of resistance to male oppression. One of the reasons why history from below has found it difficult to come into
focus is the relative neglect of the role of elites and nationalism in recent scholarship. This in turn is a reaction against the heavy focus on these themes that characterized the 1960s and 1970s. During those years biographies and analyses of the regional politics of nationalism were written in great numbers. The majority of works produced in the United States saw Indian nationalism as an aspect of the 'modernization' of Indian society. Works produced in Britain, especially those of the so-called Cambridge School, emphasized the factional struggle for power and office among a very fragmented set of regional and local Indian elites. Indian historiography naturally tended to be dominated by the idea of nationalist struggle and failed to address the many things that imperialists and nationalists had in common across the divide of race and power. There are some signs that a more nuanced history of the thought and social life of the nationalist elites is coming into being now. Their construction of an Indian ideal family life, their understandings of modern science, and of their conceptions of new 'public' Hinduism and Islam, have begun to appear. But it must be admitted that modern Indian intellectual history hardly exists, let alone a history which tries to...
situate and contextualize ideas in society. On the contrary, the 'anti-elitist' bias of
the younger generation of historians, and their attempts to dismantle
'foundationalist' historical narratives which are supposedly complicit with
capitalism and the modern state, have deflected attention from this very important
need. While the new history of 'decentred discourses' persistently evokes the
concept of power, the sophistication of its treatment of those without power
contrasts strangely with its simple-mindedness about the sources and projection of
that power.

One area where this attempt has been more successfully made is in the history of
religious conflict, and especially of Hindu-Muslim communalism. In part this is
because there has been a steady accretion of excellent works on modern Indian
religious life (e.g. Jones 1992; S. Bayly 1989). The personalities and institutions of
the nineteenth-century religious revival in the major Indian religions have been
portrayed with great success, as has been the continuing syncretic culture of many
localities. The emergence of religious conflict in public space can thus be situated
against a much clearer understanding of how religions were taught and learned. A
lively if somewhat crude controversy has also arisen about the extent to which
conflicting religious identifications were products of colonial policies and structures, and how far these competing identities grew out of older ideological and social roots. The vitality of this writing clearly reflects the immediacy of the experience of Hindu-Muslim and Hindu-Sikh rioting to both Indians and foreigners. Yet this remains a baffling and difficult area of study. The precise relations between the 'high politics' of communalism in the councils of the Congress, Muslim League and Hindu Mahasabha, and the conflicts in the streets and villages is yet to be satisfactorily explained. If anything, the rise of contemporary Hindu militancy has made that task harder. Much of the secular elite among Indian historians is working so hard to show that communalism is modern and politically inspired that it cannot bear to consider evidence of older and deeper imbalances, ambivalences and conflicts among Indian religious groups. The result is that the most obtuse religious chauvinist can often appear to win the argument by default, simply by showing, for instance, that Muslim monarchs did sometimes destroy Hindu temples.

This last example explains some of the strengths and weaknesses of modern Indian historiography. The closeness of religious conflict to the ordinary citizen of
Ajodhya or New Delhi and even Birmingham or New York makes of Indian history a powerful political artefact. Indian debates are now immediately relevant to those general cultural questions about representation and self-representation which are being raised in all fields of history. Should the historian point to pre-colonial religious conflict if this might indirectly feed into the armoury of the Hindu and Sikh right-wing or Muslim fundamentalists? Can Westerners represent Indians? Can elite, English-speaking Indians represent the peasants and the poor simply by appealing to an implicit racial solidarity? Such questions have given a powerful stimulus to historical research and controversy. Equally, they have given rise to an inordinate amount of political posturing which has distracted attention from our

This is not a full bibliographical list, only a sample of relevant works. More comprehensive bibliographies are provided in the volumes of the New Cambridge History of India, ed. G. Johnson, Cambridge, 1986; see also, C. H. Philips (ed.) Historians of India, Pakistan and Ceylon, London, 1962, and J. S. Grewal, Muslim Rule in India: The Assessments of British Historians, Calcutta, 1970. Oxford University Press India's 'Themes in Indian History' series, ed. N. Bhattacharya et al., also provides bibliographical essays and a sample of important articles.


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Prasad, B. S. (1851) Sikhiyon Ka Udaya aur Asta, Benares.
The discovery of the African past has been the most important advance in historical study since the Second World War. It has flourished when scholars have seen Africans as the creators of their own history and have relied on sources which Africans have produced: their material remains, languages, traditions, institutions and written documents, Whenever the
subject has deserted those principles, its vitality has waned. John Iliffe, Professor of African History, University of Cambridge, in History Today, September 1992

MATERIAL EVIDENCE AND THE MILLION-YEAR PERSPECTIVE

One of the first and most famous pieces of African historical popularization was titled ‘Old Africa Rediscovered’ and in 1959 it explored the archaeological researches into the African Iron Age and emphasized the often-neglected fact that Africa, like Europe, did have a history and even a prehistory (Davidson 1959). In that same year, 1959, the most notable of Africa’s archaeologists (or palaeoanthropologists) went even further back in time and captured the front page of the East African Standard with the banner headline ‘Leakey Discovers The Missing Link’ (Leakey 1961). The reference was to Darwin’s theory of evolution from ape to man, and thereafter no scholar seriously doubted that Africa was the probable location in which the origins of the human species should be sought. Olduvai Gorge, in what was to become the Republic of Tanzania, was the first site where early hominid fossils and footprints were discovered, but soon others were to be found in Kenya and Ethiopia as well, thus reinforcing Africa’s claim to be the cradle of mankind and pushing the origins of the species back beyond the million-year threshold (Richard Leakey 1981). The times were exciting as academics discussed
Davidson's book and listened to Leakey on the subject of hominid dentition and diet. 692

Historiography in Africa had to develop a whole series of new scientific tools and techniques to cope with the questions that historians began to ask. From being the backwater of the historical sciences Africa became the pioneer, using new forms of dating. The relative chronology of archaeological layers and geological time-spans was strengthened by the development of absolute dating through physics and chemistry. The discovery of potassium-argon dating enabled inorganic materials with human associations to be dated, and what is more it enabled them to be dated for much earlier periods than was permitted by the radio-carbon dating of organic material. Radio-carbon dating itself was refined so that errors of measurement created by sun-spot activity or other influences could be discounted and 'calibrated'; dates could be given with higher degrees of probable accuracy. The development of dendrochronology meant that patterns of tree-ring growth could be recognized in old timbers to provide an even more accurate absolute dating in given vegetational and climatic zones. The African Stone Age was mapped out from the development of regular patterns of human tool-making and tool-using to the origins of spoken

The study of language in Africa, and the associated study of material and social culture, involved the development of yet further historical skills, this time in the field of statistics. Glottochronology, the measuring of the time over which languages evolve and diverge, was never a precise science but it enabled historians to create patterns of human association over the last 10,000 years that were far more plausible than those based on earlier concepts of 'race'. The 'Hamitic Hypothesis', which the first generation of anthropologists had fed into African history, had imagined the swarthy sons of Noah invading the land of their wholly black brethren, bringing light into darkness (Seligman c.1930). The linguistic statisticians were much more refined in their cultural partitioning of the continent into zones of very ancient common ancestry such as northern Africa and the Middle East and zones of relatively recent linguistic colonization such as central and southern Africa (Greenberg 1963). In the north it was recognized that the languages of the Middle East had their roots in Africa long before the emergence of the Egyptian dynasties. Later these very old African languages returned to Africa in their new Semitic forms of Ethiopic and Arabic. Meanwhile quite different languages evolved in the
Niger basin of West Africa and, during the first millennium before Christ,

spilled over into the Congo basin as well. In central Africa, and in southern and
eastern Africa as well, the great Bantu family of languages evolved at much the
same time as the Germanic family of languages evolved in northern Europe
(Guthrie 1967-71; Dalby 1970; Ehret and Posnanski 1982). It was among the older
communities of the north and the west of tropical Africa, however, that a food
producing revolution began during the new Stone Age. From there it was diffused
throughout Africa, requiring historians to turn to yet another branch of scholarship,

palaeobotany.

The history of agriculture became and remained one of the central themes of
African studies. Although food foraging has remained of greater or lesser import
ance to many peoples in many places over the past 10,000 years, the development of 693
regular patterns of food production affected the majority of African peoples and has
attracted the attention of large numbers of historians. Explanations for the change
no longer assume that the farming way of life was naturally more secure than the
foraging one, but where population densities grew, or reliable rainfall dwindled, the
intensification of land-use evolved. Old ideas of agricultural diffusion from a cradle
in the fertile crescent of the Middle East had to be modified as botanists established that many of the food crops of Africa evolved from indigenous plants (Harlan et al. 1976). The concept of 'vegeculture' emerged among the societies which had discovered that wild tubers such as yams could renew themselves if carefully tended. Tree harvesting for oil seeds developed in both palm groves of the forest and orchards of the savanna (Davies 1967). But above all the domestication and ennoblement of the wild grasses of Africa provided the basis for the cultivated cereal crops of millet, eleusine, sorghum and teff, which formed the staple ingredients for beer, for porridge and for bread (Lewicki 1974; Miracle 1966).

Historians have always been interested in differentials of wealth and ostentation, and in Africa such differentials were often associated with the supply of protein to supplement the diet of foraged and farmed vegetable foods. The history of fishing, fish-drying, fish-marketing and fish-transporting dominates the history of many African societies over the last 10,000 years. One archaeologist went so far as to refer to the 'fish-stew revolution' that occurred along the streams of the southern Sahara as former hunters began to settle down and wait for a riverine prey to swim past their villages rather than themselves living as hunters.
constantly on the move

(Sutton 1974). On the lower Niger river fishermen became so adept at the carving and manning of heavy transport canoes that they became large-scale entrepreneurs in carrying crops and salt over long distances (Northrup 1978). On the middle Niger, above the rapids where Mungo Park met his death, other fishermen developed a naval might that enabled them to challenge the land-based rulers of empire and create a military kingdom of their own which linked the rice plantations of the upper river with desert gateways of the caravan routes to the Mediterranean (Rouch 1954). On the lower Congo (or Zaire) river large states did not evolve, but the management of economic opportunity created a network that spread ideas, technologies and ornaments along a thousand-mile chain of villages (Harms 1981).

On the upper Congo (or Lualaba) fishermen did create an early empire whose wealth was based on the management of lakes and waterways that supplied dried fish that could be sold over long distances. The fishermen bought preserving salt from the dried-lake pans of East Africa and adopted currencies that had evolved from the eighth-century AD copper mines of the Zambezi watershed (Birmingham and Martin 1981; Reefe 1981).

Domestic animals were as important as fish in the creation
of new prosperity in Africa and have attracted the same attention from historians. Household cats, ferrets and hunting dogs were followed by domesticated sheep, goats, pigs and hens which spread readily to become an essential part of the agricultural way of life of most African societies. Of even greater if more localized importance was the development of cattle ranching, which can be traced through art history as well as bone middens. Those parts of the Sahara that had adequate pasture 6,000 or more years ago saw wild cattle become tamed herds, and the herdsmen drew magnificent pictures of their prize beasts on the walls of their rock shelters (Lhote 1959).

Smaller cattle, often herded by nomadic specialists, were taken south to the very fringes of the tsetse-infested forest, and in the highlands of East Africa long-horned cattle and humped cattle spread through the highlands to reach South Africa during the early centuries of the Christian era (Phillipson 1977).

The history of cattle became intimately linked to the history of social organization among many Bantu-speaking peoples. In the highlands of East Africa the Iron Age kingdoms were associated with the management of herds and pasture. The ownership of cattle gave the wealthy a better diet, better health, a taller stature and political status (Karugire 1971). Cattle were the primary
form of wealth in the
fourteenth-century AD empire of Zimbabwe whose 'barons' leased out their
livestock to their 'villains'. The royal Zimbabwe court was surrounded by a curtain
wall 60 feet high topped by chevron decorations that were built in the thirteenth
century. Provincial governors quarried granite or alternative local stone to build
imitation courts where they tended herds of cattle right up to the margins of the
disease-infested lowlands. The royal regalia were ornamented with leopard skins
and the royal banquets were served on Chinese celadon, paid for with gold dust
quarried from deep pits by women and children while men devoted themselves to
the prestige task of nurturing and protecting the herds (Garlake 1973). Further
south, cattle played a key role in clan relations and cows became the preferred form
of bride-wealth when women passed from one community to another in marriage
(Kuper 1982). Cattle also played an important role in the history of transport.
Bullocks drew carts across the Roman Sahara several centuries before the
introduction of the pack camel and 2,000 years before Boer waggons, spanned with
sixteen oxen apiece, were drawn across the South African high veld (Bulliet 1975;
Walker 1934). RECORDED HISTORY AND ORAL EVIDENCE
African historiography is even better known for its
pioneering work on the use of oral evidence than it is for its innovative use of material evidence to recreate early history (Vansina 1985; Henige 1982; Miller 1980). One of the first historians to collect oral evidence from Africa was Herodotus, who gained knowledge of the interior from the Greek colonies of modern Libya. The Carthaginians had oral accounts of epic voyages down the African coast and perhaps even heard of a circumnavigation of the continent. The Roman senator Pliny exclaimed that something new was always coming out of Africa. A thousand years later it was the interface between literate Muslim travellers and oral chroniclers that brought details of the medieval empires of West Africa to the world of Mediterranean scholarship. (Levtzion and Hopkins 1981). Timbuktu and Kano became centres of historical learning as the Arabic alphabet spread into Africa and caused orally transmitted legal precedents, constitutional agreements, dynastic successions and international peace treaties to be committed to writing. In the fourteenth century the emperor of Mali made the pilgrimage to Mecca to enhance his credentials as a Muslim and his caravan was so lavishly endowed with West African gold that its passage through Cairo caused turbulence on the money markets (Bovill 1933; Levtzion 1973).
The northern interface between oral historical records and written ones was matched by a similar interface in eastern Africa, along the monsoon lanes of the Indian Ocean. East Africa’s early history was recorded in boat-building technology brought from Indonesia, along with the Indonesian languages still spoken in Madagascar, Indonesian crops such as the vegetable-banana, Indonesian diseases such as elephantiasis and most dramatically Indonesian music as played on huge wooden xylophones with gourds for resonance chambers (Oones 1964; Kent 1970).

The first literate observers to report on the exotic wealth of East Africa came from Rome, in the first Christian century, and later ones came from Persia, from Arabia, from India and from Muslim Spain until in 1415 a Chinese embassy arrived in Africa and went home to parade a live giraffe before the Ming emperor (Freeman Grenville 1962; Snow 1988). Some of the visitors used, wrote and diffused the local Swahili language of the north Kenya coast until it became a lingua franca along 2,000 miles of sea-shore and eventually spread into parts of the interior as well.

Historical legends were recorded in Swahili, long-distance traders wrote their autobiographies in Swahili and eventually German conquerors adopted Swahili as the official language of modern colonial government (Nurse and Spear 1985;
Whiteley 1969; Pauwels 1987). The oral and literary records are supplemented by

the inscriptions on the foreign and local coin which numismatists decipher and by

the artwork on the coral palaces of sultans in the medieval city-states of the coastal

islands and enclaves (Chittick 1974; Garlake 1966).

Africa's third frontier, the western one facing the Atlantic, was very different from

the northern and eastern frontiers in that it did not have open communication with

the classical world of antiquity or with the culture and literature of Islam and of the

Asian empires. The historiographical dialogue between the custodians of oral

information and literate visitors from across the seas began at a much later date, in the

fifteenth century, and some societies did not meet literate observers until the colonial

period of the twentieth century. It was therefore here, in Guinea and West Central

Africa and also in the deeper interior of eastern Africa, that the modern skills of the

oral historian were perfected. The first oral historians thought that they were

collecting individual memories handed down by old men and women from generation
to generation. Later they realized that the oral records could be more formalized,

structured or coded accounts of social and political systems. The traditions could be

passed on for centuries where the institutions to which they related survived with
such longevity. But formal oral records also needed to be modified by their custodians so that each generation understood its society’s history in ways that made sense to the current political or cosmological view of the world. Moreover the explanations of oral tradition could become stylized and stereotyped, making their interpretation by outsiders particularly difficult. Episodes of historical change became cloaked in conventional stories of migration, epitomizing a remote and prestigious ancestry, or of hunting and salt-gathering, epitomizing power and success in times of hardship, or of cruelty and barbarity, epitomizing domination by force and ritual. Capuchin missionaries of the seventeenth century and Belgian anthropologists of the twentieth each perfected their own way of interpreting the oral records which they heard in Central Africa (Cavazzi 1687; Vansina 1978; Ogot 1967).

The most extensively collected and printed oral records from Africa relate to political systems, and historians of Africa have been accused of being excessively preoccupied with the history of states and empires, of kingdoms and chiefdoms. The majority of Africans, it was pointed out, lived in villages and hamlets with few overarching institutions to connect them. Part of the historian’s preoccupation with states derives from the nature and richness of the oral chronicles relating to
institutions which need to prove their legitimacy and pedigree. Some of the oldest

known oral records, for instance, belong to the rain priests of the Angolan grassland

whose magico-religious authority evolved into structured chiefdoms and then

kingdoms. The power of prayer, and the control of the supernatural, preceded the

military mobilization of regiments of drilled archers, and the economic domination

of mines of iron and salt, in the fourteenth and fifteenth-century origins of the

kingdom of Angola (Miller c.1974). Elsewhere states kept records relating to their

constitutions, their legal systems, their boundaries, their property-ownership, their

dynasties, their kinship alliances and their wars. African historiography has taught

the historical profession how to decode material which at one time or another has

been conveyed by oral means. Classical scholars now look at the interface between

the spoken and the written form in Greece and Rome with new eyes, and modern

historians have made strides in recovering the invisible past of women and men who

did not take part in the literate culture of decision-making.

One of the great problems of oral tradition, like that of material evidence, has

been the problem of supplying absolute as opposed to relative dating. Where

generations are counted some help is available, but in
polygynous societies the

children of young wives may be very much younger than
their fathers, and in

matrilineal societies heirs may be brothers or nephews and
not sons at all. Average

reign-lengths may provide rough guidance, though with the
same variables, but an

excessive concern with rulers can lead to the padding of
king-lists and the confusing

of proper names with titles. Attempts to identify
remembered and datable natural

phenomena, such as total or annular eclipses of the sun,
have not carried the matter

very far. Thus the dating of social events, wars,
accessions and droughts has lagged

behind the dating of material artefacts in Africa.
Absolute dating, like literacy, has

come largely with outside contact, and the most pervasive
outside contact has come

with the spread of world religions from the Near East.
Although Islam and

Christianity have been extensively studied, the history
of Judaism in Africa is one of

the more neglected of the continent's historiographical
fields. In Ethiopia the black
Falasha long practised Judaism and subsequently gained
admission to the state of

Israel. In Morocco Jewish communities prospered and were
enriched by refugees

from Spain when Castile conquered Granada in 1492. In
medieval Cairo Jews

provided a class of merchants who retained their beliefs
and practices alongside 697
their Christian and Muslim neighbours. Yet Judaism did not gain the hold over
Africa that Islam and Christianity were to have, nor has it yet gained its rightful
place in the scholarly literature.

The birth of Christ in Nazareth, and the flight of Muhammad from Mecca 622
years later, provide the fixed points around which most Africans have built their
calendars. Christianity reached into Africa at much the same time it reached into
late Roman Europe. It spread down the Red Sea, up the Nile, along the Mediter-
ranean coast, into the desert oases. The Coptic Church flourished in Egypt and
became the state Church of Ethiopia for more than 1,000 years. In North Africa the
Donatist Church, named after a fourth-century bishop of Carthage, gave the region
a distinctive religious style, albeit one that others would consider heretical. In the
person of Saint Augustine of Hippo, Africa also provided the Western Church with
one of its most influential theological fathers. Islam, however, spread farther and
faster than early Christianity and in many places overwhelmed it, in North Africa,
up the Nile and in the lowlands surrounding the mountain core of Christian
Ethiopia. Eventually Islam even overwhelmed Christianity in the last bastion of the
Roman Empire, Constantinople, which was conquered in 1453. Thereafter Islam
was given a new stimulus in Africa by the rise of the
Ottoman Empire and its
confrontation with the new Christian empire of Portugal at
the opposite end of the
Mediterranean (Groves 1948; Holt ed. 1970; Lewis 1966;
Trimingham 1962;
Doressse 1957). FOREIGN EMPIRES AND ARCHIVAL RECORDS

There are two great repositories of early modern archives
for Africa, and neither of
them has been fully exploited. The first is in Istanbul and
relates to the spread of
Ottoman imperial influence through northern Africa in the
sixteenth century. It has
rarely attracted the attention of historians interested in
Africa. The second set of
archives is in Lisbon and was seriously disrupted by the
earthquake of 1755. Only
slowly did historians come to recognize the wealth of
materials relating to the
Atlantic face of Africa with some material on the India
Ocean as well (Ryder
c.1965). The two spheres of early imperial influence in
Africa overlap and interact
both on land and at sea in Morocco and in the Red Sea
basin. Both empires sent
diplomats deep into the African interior and both created
creolized African elites
who spoke Turkish or Portuguese and became the local agents
of colonial
administration. The archival records of the Portuguese
literate urban cultural
enclaves around the African shore have been partially
quarried by historians and the
surviving creole communities have been studied by anthropologists. The most

striking surviving cultures are to be found on the islands which were used as

pioneering bases for the European development of colonial plantations growing

wine, cotton and sugar in the tropics (Boxer 1964; Russell-Wood 1992; Mercer

1980; Duncan 1972; Hodges and Newitt 1988). Hard on the heels of the Ottomans

and the Portuguese came a third wave of merchants and settlers from The

Netherlands. They gained access to the gold emporia of West Africa, the slave and 698

ivory grounds of Central Africa, and the temperate farmlands of South Africa,

creating ordered historical records which the first generation of modern African historians used to sharpen their historiographical skills and write their doctoral

theses (Daaku 1970; Martin 1972).

Planters and merchants were followed by Muslim and Christian preachers who

spread literacy, ethics, jurisprudence and belief along the long-distance trade routes

and settled missionaries in the great urban centres of politics and commerce. The

city of Kano in West Africa became a centre of learning as well as a focus for the

trade in textiles, metals, livestock and slave labour. One travelling scholar provided

the city-state with constitutional guidelines for good governance and another hoped
to buy a literate slave to take home to Muslim Spain as his secretary. In the
nineteenth century one British visitor was asked if he could supply copies of the
Greek works of mathematics missing from the sultan's library. Victorian scholar
adventurers became profoundly interested in African Islam and their awe passed to
both British and French administrators of the next generation (Bovill 1964-6; Fisher and Fisher 1970). In the Christian sphere of influence the sixteenth-century
kings of Kongo hired clerks who kept their ledgers and wrote on theological matters
to the popes of Rome. A royal archive was steadily built up until a column of
marauding colonists from Portugal burnt it to the ground in the mid-nineteenth
century. The loss of the Kongo archive facilitated the dissemination of the
European myth that Africa had no history and, by implication, that conquest in the
name of 'civilization' was morally justified (Hilton 1985; Thornton 1983).
The early attempts to Christianize Africa gradually subsided as European
interest in the continent became increasingly dominated by the slave trade. The rise
of the modern slave trade became a major preoccupation of historians. The trade
was not a new phenomenon when Europeans entered it in the fifteenth century. In
the north of Africa black slaves were bought by Romans to supplement the white
slaves who came from the eponymous Slav territories in Europe. The Carthaginians probably also had black slaves, and the name Guinea which came to be applied to the west coast of Africa derives from the Carthaginian word for black.

Arabs developed more reliable routes across the Sahara and bought not only male labourers but also attractive young women who could fetch high prices in the harems if they survived the ordeal of the desert crossing. The Turkish Empire went on buying slaves into the nineteenth century and in exchange supplied West Africa with firearms for its infantry and chain-mail for its cavalry. The first Portuguese also bought slaves for their newly conquered Muslim estates in the Algarve and paid for them with metal goods and horses (Thompson and Ferguson 1969; Boahen 1964; Saunders 1982). The trade in people from eastern Africa was as varied as the northern trade but has been less well illustrated in the literature. Ethiopian slave wives seem to have been prized in Persia and child pearl-divers were taken to the Persian Gulf in large numbers. Colonial regiments in India were sometimes reinforced by black slave soldiers from Africa, though the scale of the trade to India has not been adequately quantified (Clarence-Smith 1989; Austen 1987; Russell-Wood 1992).
The modern extension of the slave trade to the Americas became a topic of virulent academic controversy. Much of the early writing about the slave trade came from the pens of polemics who wished either to condemn its morality or to justify it as a means of redeeming souls from the perceived barbarism of indigenous society. Historians were slow to adopt a properly critical approach to this data and to delve into the archives to find authenticated evidence about such a sensitive subject. When they eventually did so, driven by American scholars rather than by either European or African ones, they unleashed a spate of new research and publication. Measuring the Atlantic slave trade has been the concern of polemics for two centuries, but it was not until 1969 that serious scholarship prevailed and Philip Curtin, a founder of the American school of African history, published his historiography of slave statistics and his own estimate of the scale of the Atlantic trade. His evidence was subjected both to emotive condemnation and to statistical refinement, culminating in a consensus that 11 or 12 million people had been taken from Africa to the Americas in less than four centuries (Curtin 1969; Klein 1978; Inikori 1982). One of the great controversies over the slave trade was the measuring of its impact on Africa not only in statistical terms but
also in terms of demographic change, commercial initiative, military destruction and political innovation. Several diaries of slaves and of slaving captains were discovered, edited and published. In the South Atlantic the evidence of the Brazil trade shed light on how slaves were procured, on the methods of credit used in the trade, on the ownership of slaves during the middle crossing and on the rivalries of shipping interests (Lovejoy 1983; Miller 1988; Curtin 1975; Edwards 1967; Grant 1968). The other great debate among historians concerned the reasons for the outlawing and suppression of the African slave trade by the future colonial powers. Economic, moral and political factors were debated with increasingly sophisticated intellectual passion (Williams c.1944; Anstey 1975; Drescher 1977).

Traders, missionaries and slavers were followed by cohorts of invaders, settlers and conquerors, each of which acquired a specialist accompaniment of historians. The history of this imperialism was treated with some reserve by the post-war generation of new historians of Africa who were concerned to understand the African experience rather than to study the activities of Europeans in Africa. Yet the questions had to be raised concerning the reasons for the 'scramble for Africa', its diplomatic partition and the subsequent imposition of
'effective rule' by foreigners. An acrimonious debate occurred between those who saw imperialism as 'uneconomic', an extension of nineteenth-century European nationalism into colonial provinces, and those who saw it as a search for minerals and Lebensraum (Hammond 1966; Clarence-Smith 1985). 'My map of Africa', Bismarck was purported to have proclaimed in 1885, 'is in Europe', but Leopold, the king of the Belgians, would hardly have agreed as he carved out his million-mile fief in the Congo. Anglo-French rivalry over spheres of influence and methods of financing services for traders was another source of sharp disagreement that led to the creation of formal colonies with boundaries and tariff regimes. France won the upper Niger and Britain the lower Niger in a series of military expeditions. Further south treaty 700 makers and mineral prospectors sought land, labour and local alliances with African rulers (Slade 1962; Hargreaves 1974-85; Kanya-Forstner 1969; Oliver 1957).

Beyond the debates over local circumstance and agency was the wider debate about the world partition and Africa's place within it as a dark continent separating the two economic giants of South Africa and Egypt and providing a barrier on the sea road from Europe to Asia (Robinson and Gallagher 1961; Penrose 1975).
The African territory that required the most intense military effort to conquer it was South Africa. The second Anglo-Boer War of 1899-1902 was, with the possible exception of the Crimean War, the most brutal and expensive British war to be fought between Waterloo in 1815 and the Great War in 1914. British soldiers died of wounds and diseases, African auxiliaries disappeared without trace or concern, Afrikaner women and children were rounded up in desolate concentration camps, farmhouses were fired, heirlooms were looted, war reports were forged, fortunes were made (Warwick 1980; Cammack 1990). Despite the profound impact which the conquest of South Africa had on Britain and on British historians, the post-1945 generation of 'new' African historians found South Africa almost invisible. It did not feature in their syllabuses and did not attract their research students. South African history was an enclave of European history, a political debate among white males. Only two non-European politicians feature in the entire index to a history of South Africa put out by the English Universities Press in 1946, and one of those was the 'diabolically ingenious' Mr Gandhi who caused Mr Smuts so much grief (Williams 1946). The history of race relations in South Africa was
more commonly an account of the relations between the British and the Dutch races than an historical analysis of the so-called 'native' question or even of the 'Indian' question. Indeed it was officially put about in school texts and government publications that the black population of South Africa consisted of 'hordes' of 'warriors' who had come down from the north to try to wrest South Africa from the legitimate white settlers who had turned the wilderness into a garden. The beginnings of an effective challenge to this historical propaganda came not from a historian but from an anthropologist who traced back the records of the societies she was studying to discover that there were well-documented accounts of sixteenth century shipwrecked mariners being cared for by African farmers a century before the first Dutch settlers came on the scene (Wilson 1959; Boxer 1959).

A turning-point in South African historiography came in 1969-71 with the appearance of the Oxford History of South Africa (Wilson and Thompson 1969-71). Not only was the history of first-millennium AD cereal farming, cattle-raising and iron-working brought into focus, but archaeological evidence of trade with China was unearthed to demonstrate at least limited medieval contact with the outside world. Not only was the history of modern African society shown to be a long one,
but the organization of the society was shown to be more complex and sophisticated than the colonial conquerors had been willing to acknowledge. An Oxford chapter 701 on nationalism compared the aspirations of white Afrikaans-speaking nationalists with those of black Bantu-speaking nationalists, inviting a debate which the South African government did its best to stifle by censorship. From 1969 onwards, however, South African historiography began to grow apace. Afrikaner nationalists, it emerged, were not the only ones to be critical of the 'liberal' tradition of South African historiography. A new school of 'revisionist' historians were anxious to bring the experience of the black majority into the mainstream of historical writing. These radical historians included both Afrikaner and British scholars, though at first few Africans had access to the new centres of academic research. In 1982 a two volume social history of the Witwatersrand provided a path-breaking collection of essays on poor whites who came to the city, on Zulu guilds of washermen and rickshaw peddlers, on the role of alcohol and prostitution in attracting black and white workers respectively to the gold mines, on the distilling of Boer farm produce into hard liquor, and on the efforts of the Kruger government to meet the competing demands of farm capital and of mine capital,
thereby revising received ideas about the causes of the Boer War (van Onselen 1982). Labour history replaced financial history as the core subject of the mineral revolution in South Africa and studies focused not only on the mining demand for segregation, mine compounds and rural reserves for migrants' families, but also on the supply of cheap workers from outside South Africa and indeed from as far away as China (Wilson 1972; Turrell 1987; Richardson 1982; Levy 1982). Although the nineteenth-century mineral revolution and its legacy provided an early focus of revisionist historiography among South Africa's emigre historians, rural life, and often a very uncertain life at that, remained the experience of the majority of South Africans and attracted the attention of the growing historical profession. Introductions to the emerging themes appeared in a three-volume set of essays edited by Shula Marks and others (1980; 1982; 1987). Zulu history emerged from the colonial and anthropological mystification of the conquest to become a subject of thrusting historical controversy (Marks 1970; 1986; Guy 1979). The history of the rise and fall of modern black farming in the Cape concentrated on the competition between black and white farmers over access to land, transport and markets. As landless proletarianization advanced, so
historians turned to studying

social, political and trade union organization (Bundy 1979; Beinart 1982; Bradford 1987). The drama of rural South Africa reached its nadir in the Bantustans which bore the brunt of South Africa’s labour policy and had to carry the social costs of reproducing a labour force with minimal expense to industry or the state. Some of the heaviest burdens were borne by women whose men had gone off to the mines and plantations as they had done under slavery or colonialism in other times and other parts of Africa. Some women followed men into the labour market, but often under conditions of great poverty and isolation. In the 1980s the lives of many of these women were brought into the light in a new branch of South African historiography (Marks 1987; 1994; Walker 1990; Berger 1992; Cocks 1980).

Despite its pockets of racially confined wealth and its great development potential, much of South Africa remained poor and prone to disease. The history of 702 disease was slow to catch the imagination of historians and yet it is one of the prevalent experiences of South Africans. The mining industry brought new diseases to the continent and accentuated the effects of old ones. Disease brought poverty and death to former workers who had neither medication nor security. The great
pandemic of Aids appeared to spread in the male hostels of mine workers and the old plague of tuberculosis revived to become again one of the world's great killers (Packard 1989). Disease affected rural people and also their livestock. At an earlier date South Africa suffered a severe epidemic of rinderpest, and in East Africa cattle disease cleared whole areas of herders and led to ecological changes of deep importance to human activity and the encroachment of colonial conquest (Kjekshus 1977). In French equatorial Africa sleeping sickness killed many workers in the logging camps and led to acute shortages of labour and the harsh recruitment of forced conscripts (Coquery-Vidrovitch 1972). In Angola an epidemic of smallpox swept through village society in the early twentieth century and disrupted the expansion of the colonial coffee plantations. River blindness affected those who lived near snail-infested rivers and malaria killed more people than any other disease in all the lowland areas. Drought and famine became increasingly oppressive as the population of Africa increased in number during the twentieth century and caused a new wave of historical investigation of ecological factors. As deprivation and stress grew so the history of mental health became a preoccupation and records of the colonial attitude to psychiatry began to be investigated. The history of public
health, of sanitation and of medical science in Africa began to explore the change and lack of change brought by the twentieth century (Iliffe 1987; Vaughan 1987; Lyons 1992). THE PROFESSIONALIZATION AND POPULARIZATION OF AFRICAN HISTORY

The first major steps in the formalizing and disseminating of African history were taken in 1948. In that year the University of London took measures to bring Africa into the mainstream of the discipline by creating a quaintly archaic 'lectureship in the history of the tribal peoples of East Africa' at the School of Oriental and African Studies. London distanced itself from the old British Academy tradition which insisted that scholarship was exclusively literate and that Africa was only of reputable academic interest where it interacted with the civilizations of Greece and Rome, or Mecca and Jerusalem. The London initiative led to the hosting of two international conferences on African history in 1954 and 1961, to the founding of a Cambridge journal devoted to African history, and coincided with the establishment of history departments in the University's new colonial colleges on the Gold Coast, in Nigeria and in Uganda. Local journals of historical scholarship were established in Africa and local history degrees gradually introduced Africa into their syllabuses. London itself concentrated on postgraduate teaching and encouraged the
publication of hundreds of hardbound monographs that began life as doctoral theses. In 1961 London University began teaching African history at an undergraduate level and in 1966 it launched 703 American-style masters degrees as well. In 1962 the London pioneers published a Penguin history of Africa which has been in print in numerous editions and several languages ever since. They also began commissioning work for an eight-volume Cambridge history (Oliver and Fage 1962; 1975-85). Each author was awarded a chair in African history and within twenty years no less than fourteen British history chairs were held by Africa specialists. During those years African historical studies spread from British Africa to the ex-colonial universities of Belgian and French Africa as well. The African universities also sponsored new research by hosting international conferences and publishing the proceedings, with the help of the International African Institute, the Pan-African Congress of Prehistory and Archaeology, the Leverhulme Foundation and UNESCO. UNESCO also launched an ambitious General History of Africa which aimed to encourage African scholarship and disseminate it into educational syllabuses. The eighth and last volume was published in 1993 and abridged paperback editions have begun to appear (Mazrui
1993). African schools, however, were slow to change their neo-colonial customs and adopt even subsidized new textbooks. African history made better progress in American schools where many teachers had gained familiarity with Africa through their Peace Corps service.

In the 1960s historians of Africa were almost overwhelmed by the demand that they explain the surge of anti-colonialism that was presumed to herald the advent of nationalism in Africa, on the model of the nationalism that had grown in Europe during the unification of Italy or the dismemberment of Austria. The first tide of nationalist history was informed by an American school of political science that developed its own language and its own interview materials (Apter 1955; Coleman 1958; Young 1963). The second development in nationalist historiography was more profoundly historical as scholars sought to find the roots of nationalism in the early colonial experience by recovering the submerged 'authentic voice' of Africa. Historians looked at the history of primary resistance, as Africans struggled against the advent of imperialism, and at secondary resistance as they tried to maintain their identity under the colonial yoke (Ranger 1967; 1970; Iliffe 1969; Crouder 1971; 1968). The third phase of nationalist history came later, once the colonial archives...
had been opened to scrutiny, after judicious shredding by the departing powers.

Historical documentation significantly challenged the triumphalism of the 1960s and brought a more nuanced interpretation to the controversial conflicts of decolonization. The anti-colonial war in Kenya was revisited by British scholars while remaining virtually a taboo subject for Kenyans, and even the profoundly traumatizing war of independence in Algeria began to be studied with liberated hindsight in France (Kanogo 1987; Throup 1987). One way in which the popularization of history was expected to grow was through the publication of national histories on the European model, but in fact very few of the fifty-odd ex-colonial nations in Africa have been covered by single-volume, single-author histories that are both scholarly and readable. A few notable exceptions include Tanzania, Nigeria, Zambia, Ethiopia, South Africa and Algeria (Iliffe 1979; Isichei 1983; Zewde 1991; Thompson 1990; Ageron 1991). 704

When the history of Africa began to be studied and written in the 1950s it was reasonable to expect that its popularization would include the publication of biographies of leading Africans, just as biography became a major branch of wider history in Europe. One of the first great classics of the new historical school was just
such a life-and-times biography (Shepperson and Price 1958). This path was not
followed by many later historians and the great African biographies were more
commonly devoted to the lives of the colonial proconsuls than to their indigenous
partners and protagonists. (Perham 1956-60; Flint 1974). In South Africa the
biographical tradition was stronger and covered both black and white South
Africans (Willan 1984; Thompson 1975; Paton 1964). In tropical Africa some of the
heroes of the anti-colonial resistance have yielded up documentation that has led to
studies of a quasi-biographical kind. (Robinson 1985; Person 1968-75). Modern
political leaders have been the subject of biography or of autobiography that may be
more or less ghost-written. (Murray-Brown 1972; Nkrumah 1957). The biographies
of common individuals are even more scarce than those of leaders but those that
have been published are richly illuminating (Smith 1954; Crowder 1988).

By the 1990s African history had gained its place in the mainstream of the
historical profession, even though the current work devotes only one chapter to the
continent as compared to four devoted to Asia. African history has been presented
on television in two eight-part series, one made by the pioneering Basil Davidson
and the other by the Kenyan scholar and Reith Lecturer, Ali Mazrui. In Africa itself
historical publishing has suffered grievously from closed frontiers, restricted currencies, paper shortages, impecunious students; and much historical work prepared in the 1970s and 1980s therefore still awaits an outlet. In Britain, however, two of the founding fathers of African historiography reviewed their life work in books of enormous vision and vitality (Oliver 1992; Davidson 1992). At the younger end of the historical profession students throughout Europe and North America continue to turn to Africa as a source of new evidence, ideas and methods through

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Probably the written history of few peoples has been so influenced by the present as that of Americans. As a people admittedly more tuned to the future than the past,
they have, none the less, found it necessary to draw upon history to explain themselves. All nations, of course, do that; few, however, need it as much as Americans. From the beginning Americans have been a heterogeneous people, and their relatively brief national existence fits entirely within the confines of 'modern times'. The sources of American nationality, unlike those, for example, of England or France, are not lost in the mists of time; rather, they seem to have been laid down only yesterday.

These forces that have shaped American historiography have never been more obvious and potent than over the last three decades when a minor revolution has reshaped historians' conception of the American past. Part of that renovation derives from a problem inherent in all historical study, namely, the tension between the collective and the individual. We want and need a history that describe groups of all sizes from clubs and organizations to cities and states, to nations and empires. Yet the larger the group the more diverse are the individuals who make it up. By what means can a historian generalize about a large diverse nation like the United States? That problem occasioned the first turn of the wheel of American historiography. The object of attack was the American Studies or consensus approach to
the past.

Ever since the beginning of American history, like that of most nations, a history of the whole country has been both necessary and commonplace. Usually the story has been shaped around politics or government. During the 1930s and 1940s, however, some venturesome students of the American past moved beyond that narrow conception of the past; they sought to integrate American literature, sociology, psychology and even some economics into the nation's story; they called their approach 'American Studies'. By taking such a multidisciplinary view of American history, they believed they could more accurately encompass the breadth and diversity of that past. Among historians that approach came to be called a 'consensus' view since it sought to identify the values and attitudes that characterized Americans in the course of their history.

Several of those studies are worth examining briefly because they provide concrete examples of the approach. Perhaps the best-known and certainly the most influential was Richard Hofstadter's The American Political Tradition and the Men Who Made It, which appeared in 1948. Hofstadter's point was that deep and enduring social divisions had been absent from the American past, a historical homogeneity that he seemed mildly to deplore. Daniel
Boorstin's The Genius of American Politics, which appeared ten years later in 1958, happily argued that the central clue to appreciating and understanding the American past was its lack of ideology or dogma. Instead of conflict, he contended, Americans had been primarily concerned with what worked, with responding to the moment, with adjusting to the unexpected. Some historians interpreted this to mean that Boorstin discerned no principles, no ideological commitment in the American story. A less explicitly provocative effort to explain a large part of the American past by discerning a central theme was advanced in David M. Potter's People of Plenty (1954). His contention was that the American experience could best be understood by recognizing the central role played by material abundance in the making of American history. By abundance Potter meant more than just the fertile land, and plentiful mineral and plant resources; he also pointed to the immense amount of wealth produced by Americans beginning soon after the first settlements. Because they had lived amidst so much wealth, as compared with other nations of the past and present (he was writing, of course, after the devastating destruction of Europe's wealth during the Second World War), many of their characteristics, such as wastefulness, emphasis
upon privacy, individualism and democracy could be explained or accounted for by that abundance.

Like all efforts to explain a society and its history through a single operating force or consensus, Hofstadter's, Boorstin's and Potter's efforts were criticized for ignoring those events and developments not easily accounted for by central principles or values. But the objection most frequently raised against the consensus approach to the past was the absence of any depiction or even recognition of conflict or diversity in that history. To be sure, that was precisely what the approach intended to do: to move beyond conflict and contradictions by articulating what that past had meant in general once whatever passing conflicts there may have been had been resolved by time or events. Though probably not intended by the several authors to be a justification of the American way, in practice their books could easily be read as such, especially since the United States at that time was at the peak of its economic abundance as well as being the only untouched victor in the most devastating war in history.

As we will see in a moment, self-satisfaction with national achievement was not the only sentiment emerging from the Second World War. But before we turn to that, one other aspect of David Potter's People of Plenty
is worth noting in any discussion of American historiography: the attention Potter paid to the social sciences in the course of his effort to uncover the sources of the American character.

Among other things, he turned to social psychology, particularly that of Karen Horney, a contemporary neo-Freudian psychologist. In fact, in this book and throughout his career in subsequent years, Potter looked to social sciences such as anthropology, political science as well as psychology in seeking to understand the American past. That part of Potter's work, unlike his attempt to identify American national character, proved to be only the beginning of a broad interest among American historians in turning to the social sciences for new insights into the nation's past. Thus Stanley Elkins in his 1959 study of American slavery depended heavily upon the interpersonal psychology of Henry Stack Sullivan, Lee Benson in 1961 deliberately looked at Jacksonian Democracy through the lenses of political scientists, while Charles Strozier (1982) boldly employed psychoanalysis in trying to understand Abraham Lincoln. Herbert Gutman's Black Family in Slavery and Freedom, 1750-1924 (1976a) drew upon the insights of cultural anthropology in analysing the history of black families. Probably more than any other field of
history, recent students of the American past have worked
to bridge the otherwise
broad gap between their discipline and the social sciences.
One additional measure
of that continuing interest was the founding in the 1960s
and 1970s of scholarly
journals such as Historical Methods Newsletter (1967),
Journal of Interdisciplinary
History (1970) and Social Science History (1976), all of
which continue to be
published.
Important as these new methods were in reshaping American
historiography,
they did not constitute what needs to be seen as a minor
revolution: the transform
ation of the historians' conception of the American past.
That derived, as suggested
already, from a fundamental dissatisfaction with the
absence of conflict in the
history presented by historians such as Boorstin, Potter
and Hofstadter. Two forces
seems to have given rise to those criticisms.
The first was the altered nature of the America of the
1960s. Rather than an
America of wealth, international power and internal
progress, which had spawned
the American studies or consensus approach, the United
States of the 1960s was a
society in turmoil. The struggle on behalf of equal rights
for blacks that had begun
in 1954 with the Supreme Court decision outlawing
segregation had become by the
early 1960s a fully-fledged struggle to eradicate
discrimination against minorities

and women. At the same time, the growing war in Vietnam had
spawned an

opposition that was increasingly vocal and effective,

particularly among students

and faculties at colleges and universities. In such a
climate, a history that lacked

depictions of a contentious past seemed unrealistic at

best and hypocritical at worst.

Besides, at a time when ethnic and sex discrimination were

increasingly seen as

unAmerican, it was hard to deny that such discrimination

must have been present in

the past as well as in the present; surely such strongly

held feelings could not have

sprung forth without antecedents.

The second development standing behind that minor

revolution was the

changes in the composition of the historical profession itself. For most of the

twentieth century academic historians were much more likely to have been 711

recruited from among 'old' Americans, that is, middleor

upper-class, white

Protestants of English or northern European background,

than were the scholars

in some of the social sciences, such as anthropology and

sociology. After the

Second World War, however, when many veterans took

advantage of federal

stipends and tuition for graduate education, the

traditional demographic

composition of the historical profession changed
dramatically; sons and some
daughters of working-class America, whose parents may well
have also come from
eastern or southern Europe backgrounds, joined the
historical profession. They
began to see their country's past with different eyes from
those who had come
from traditional backgrounds.
The emerging view of the American past was, as might be
anticipated, more
varied, and more detailed. It was therefore less willing to
draw broad conclusions
about the nature of Americans in general; it was the
diversity of America that now
seemed central to the nation's past. Americans had long
been aware of their diversity
of peoples, of their being a nation of immigrants. Yet
behind that traditional view
stood the anticipation that in due time the immigrants
would melt into the general
population, that a composite American population would
emerge from the 'melting
pot'. Beginning in the 1950s, however, some historians
began to recognize that the
'melting' had not yet been completed, even though the
massive immigration of
the decades before the First World War had been almost
entirely shut off by the
immigration law of 1924. In 1952, for example, Samuel
Lubell's Future of American
Politics documented the ways in which immigrant background
still shaped electoral
politics in the United States, setting a pattern for
subsequent historians and political scientists when they sought to explain American political behaviour. Lubell pointed out, for example, that much of the isolationist areas of the country just prior to the entrance of the United States into the Second World War coincided geographically with concentrations of voters with German or Scandinavian ancestry.

Traditional historians of immigration may have looked towards a 'melting pot' as the future of American diversity, but they had also begun to recognize, as in Oscar Handlin’s *Uprooted* (1953), the difficulties encountered by European peasants seeking to make their way in the strange and threatening United States of factories and cities. Scholars who came after Handlin did not ignore the newcomers’ difficulties, but in line with their new vision of a revised American past, they shifted from seeing immigrants as victims to emphasizing the newcomers’ group solidarity and ability to make a life to their own liking. Thus Tamara Hareven’s *Family Time: Industrial Time* (1982) demonstrated the way in which co-operation among immigrants could shape the work patterns in a modern factory situation, while Virginia Yans-McLaughlin’s (1977) investigation of Italian immigrant families revealed how family behaviour varied according to ethnic or immigrant origins. In
place of a 'melting pot', many historians began to give recognition to the persistence of ethnic differences by invoking instead the symbol of the 'salad bowl'.

Nowhere is the recognition of the persistence of immigrant diversity more strikingly measured than in a recent essay by Frederick C. Luebke, a historian of German immigration. In his Germans in the New World (1990) he called for the study of the ways in which immigration shaped American society outside the political sphere, where, ever since Samuel Lubell's book, it has long been recognized. 'Would the United States have been a different society if the largest immigrant group had been French instead of German?' he asked.

What Luebke was referring to in regard to changes in political history as a result of a new recognition of American cultural diversity was the emergence in the 1960s of what came to be called an ethno-cultural interpretation of political history.

Traditionally, historians had been most inclined to uncover economic interests in explaining the policies of political parties and the winning of elections. Beginning with Lee Benson's pioneering work, The Concept of Jacksonian Democracy (1961), political analysis took a new turn. After Benson argued for cultural values and religion as sources of division between the major political parties and their voters,

Over the years historians of labour have also been acutely aware of the diversity of peoples when attempting to explain why unions have been weak in the United States as compared with other industrial nations. Sometimes the emphasis has been placed upon employers' exploiting ethnic antagonisms in order to control workers; at other times historians have argued that in certain periods class has been able to unite even a workforce composed of diverse ethnic allegiances. But the divisive power of ethnic diversity rarely remained far beneath the surface when historians endeavoured to account for the relatively low proportion of industrial workers in the United States who actually joined labour unions. It is that historical heterogeneity of the workforce, contended Gary Marks (1989) in his comparative study of unions in politics in Britain, Germany and the United States, that has been a 'recurring, dynamic process' in keeping American unions from reaching the level of member
ship achieved by workers in Germany or Great Britain. Ethnic diversity has also been a well-recognized explanation as to why the United States has historically lacked a socialist movement comparable to those in the major industrial countries.

One recent historian of labour, Lizabeth Cohen, has described this lack of socialism as a measure of the conservative character of American labour, which she summarizes as workers' belief in a 'moral capitalism'. This outlook, Cohen contends, has differentiated the American working class from the more anti-capitalist outlook of European labour traditions. In a sense, Cohen and Marks are reviving an older interpretation of American labour and American historiography in general, namely, the idea that America is 'an exception' to a European pattern of development. Most recent historical writings have stayed away from, or even denied outright any kind of 'exceptionalism' but, as Cohen's and Marks's books make clear, exceptionalism still occupies a useful place in the minds of historians of the American past.

The new social or demographic diversity of the American
historical profession

has caused scholars to be interested in more than simply
documenting the way in

which immigration has affected the working class or impeded
the spread of labour

unions. Labour historians of late have been especially
concerned to describe and

analyse the worker's life in the workplace. Traditional
American historians had been

principally concerned with the structure and nature of
labour organizations. The

more recent labour historians, who are frequently imbued
with sympathy for

the workers' lot, want instead to portray the lives of
workers on the shop floor or

in the community. The late labour historian, Herbert
Gutman, led the way with a

series of articles on nineteenth-century workers, now
collected in his Work, Culture,

and Society in Industrializing America (1976b). Following
in the footsteps of E. P.

Thompson's Making of the English Working Class (1963),
Gutman stressed the role

of social values, such as religion, in shaping the American
worker's conception of his

role and his work. By combining labour and immigrant
history, Gutman also broke

down the academic barriers that had once artificially
separated historical investiga

tions of the working class into different fields. Gutman's
approach is exemplarily

carried forward in Lizabeth Cohen's Making a New Deal,
where she breaks fresh
ground in depicting the interactions among immigrants, natives and employers in
creating during the Great Depression a modern, politically
conscious American
working class.

Less social and more ideological in interpreting labour's place in the American
past has been the work of the Yale professor David
Montgomery, whose approach culminated in his Fall of the House of Labor. The
Workplace, the State, and American Labor Activism, 1865-1925 (1987). The ideological and
social approaches to working-class life and work are apparent, too, in Alan
Dawley's Class and Community: the Industrial Revolution in Lynn (1976) and in
Sean Wilentz's Chants Democratic: New York City and the Rise of the American
Working Class, 1788-1850 (1984), both of which treat as well the transition from
pre-industrial society to factory life. Both studies exemplify, too, the new emphasis
upon the detailed study of individual communities or cities rather than of states,
regions or nation.

An additional reason why the 'melting pot' interpretation of American ethnic
diversity or cultural pluralism came under criticism was its failure to recognize that
the different races in the United States never 'melted' into the mainstream. Blacks,
Amerindians, Japanese, and Chinese were always set apart, either by law, as in the
case of African-Americans in the American South, or by custom. Here, too, the political and social changes set in motion by the Great Depression, and its offspring, the New Deal, along with the social changes in the course of the Second World War, radically reshaped historians' conceptions.

The Supreme Court's repudiation of legal segregation in 1954 unleashed an unprecedented academic inquiry into the history of African-Americans. Black historians had long been pursuing that story, but largely without either much assistance or interest from white historians. But once racial discrimination became a primary political as well as legal and social concern, the social roots of discrimination took on a new urgency among historical researchers. By the time the excitement and interest had subsided in the mid-1980s, American social history in general, not only African-American history, had been transformed.

The transformation began in connection with the writing of the history of slavery in the United States, a subject that seemed to many observers to lie at the heart of the question of why blacks had been kept apart from whites for all of American history. Slavery, after all, had begun the separation, and in the works of some early historians provided the primary justification for the separation: Africans were
naturally suited to be slaves. By the 1950s, however, a simple racial justification for either slavery or social and economic discrimination was no longer acceptable to most citizens. What, then, was the role of slavery in shaping the position of blacks in a white-dominated America? Should it be seen as simply a historical institution that had been eliminated and could therefore be forgotten, or had its impress been so deep that it continued to shape the fate of African-Americans in the present?

Kenneth Stampp in his study of ante-bellum Southern slavery, *Peculiar Institution* (1954), meticulously depicted the harshness, even the cruelty, of bondage, while emphasizing the essential similarity of whites and blacks. It was this last point the lack of inherent differences between the races that clearly marked his study as fitting into the post-Second World War conception of US history. Earlier students of US slavery had not begun their enquiries from that point of view; they had generally accepted the idea that African-Americans were inherently different from other Americans. After the 1950s, no historian would start from that position.

Stampp's emphasis upon the harshness of slavery opened up a quite new exploration of the nature of human bondage in the American past. 'Harshness' of treatment is a relative, not a precise term. Harsh in relation to what: freedom, or
other kinds of oppression? Obviously slavery was harsh, not to mention horrible,

when compared with freedom, but that was not the real issue since earlier
discussions about slavery had recognized that only black people forced immi
grants from Africa were slaves. From that recognition the contention had arisen
that slavery was not so horrible because only a people who were not considered the
same as white people fell within it. So the question became: was US slavery harsh as
compared with slavery in other parts of the world?

A beginning had been made in this regard even before Stamppp's book appeared.

In 1946 a Latin American historian named Frank Tannenbaum drew a comparison
between slavery in certain Latin American countries and in the American South,
from which he concluded that American slavery was harsher because it denied
blacks many of the protections and life conditions which the law and the Roman
Catholic Church provided in certain Latin American societies.

Tannenbaum's book had little impact upon American historians, for it came too
early on to the political and social scene. But a student of Tannenbaum, Stanley
Elkins, picked up Tannenbaum's comparison in 1959, and elaborated upon it. By
that date, popular concern over the history and fate of blacks in American society,
past and present, had reached a high point. Elkins's contention was that the denial of the essentials of humanity to the slave by the American system had been so severe that it had reshaped the psyche of the African-Americans who lived under its weight. As a consequence of slavery's almost total repression, black people had became infantile, irresponsible, happy-go-lucky and generally incapable of mounting serious resistance or protest against their bondage. The conclusion, in short, was that American slavery had been the most burdensome form of human bondage in the New World. It had psychologically unfitted blacks for life in freedom. Throughout the book Elkins made clear that he was not talking about a racial or biological difference, only a psychological or cultural transformation of Africans under the impress of US slavery, a transformation that was seen to be an accurate, if horrendous, measure of that institution's profoundly evil character.

Since, at the time, many historians and citizens alike were struggling to understand the background of the social and legal movement on behalf of equal rights for blacks through which the society was then moving, Elkins's imaginative depiction of the enormities of slavery found a wide acceptance among both white and black historians. The acceptance was reinforced by the

Yet, as often happens with interpretations of historical events, persons or institutions, a change in social context soon raised doubts about Elkins's interpretation. If one was only interested in the severity of the impact of slavery on American black people, then Elkins's view might be convincing. But if one also wanted to know how blacks reacted to their burden of slavery, Elkins's interpretation left them largely victims, people unable to resist or to challenge their bondage. Elkins's conception of US slavery might help to account for the very few slave rebellions that broke out during the ante-bellum years, but it had no place for the many examples of individual resistance slaves exhibited throughout those years, nor for the hostile communal and social reactions of the slaves to their bondage. So a whole new historical literature on slavery began to emerge, depicting slaves not as victims, but as vibrant, often courageous and resourceful resisters and survivors of bondage.

Some of these historians were of African descent themselves, like John Blassingame, who wrote The Slave Community: Plantation Life
in the Antebellum South (1972), in which he traced the everyday life of the slave in 'the quarters' and in the fields, using that depiction, quite explicitly, to refute Elkins's conception of the black slave as a complaisant victim. Some other critics of Elkins were white, like Eugene Genovese, whose magnum opus, Roll, Jordan, Roll: The World the Slaves Made (1974), ranged over the whole life pattern of the slaves, portraying them as engaged in continual struggle with their masters to win a life of their own, to obtain sufficient 'social space' to escape that total control which Elkins had contended the masters exercised over them.

One consequence of the widespread interest in the history of slavery was a new willingness to use sources once seen as 'tainted' or improper for scholarly exploitation, namely, the hundreds of slave reminiscences, which had been recorded during the 1930s, some sixty years after the abolition of slavery. Kenneth Stampp, for example, in his book The Peculiar Institution, had refused to draw upon those sources because they were so dependent upon admittedly fallible, partisan and highly selective memories. He relied primarily, as had his racially orientated predecessors, upon contemporaneously written documents by white men and women. By the 1960s and 1970s, however, the need and the
desire to have sources from the slaves themselves, and the recognition that all sources are partisan and always require critical examination made the slave reminiscences quite acceptable as an entry into the everyday life of slaves. Genovese relied heavily upon those particular sources in his Roll, Jordan, Roll (1974), and George Rawick based his picture of life in the quarters, From Sundown to Sunup (1972), largely upon them. Probably the most imaginative effort at uncovering insights into the spiritual and mental life of slaves, who had few chances to leave written documents of their own, was Lawrence W. Levine's Black Culture and Black Consciousness: Afro-American Folk Thought from Slavery to Freedom (1977), in which he gave an imaginative reading of the slaves' spirituals, religious rituals and folk-tales. In a sense, Levine's book was the definitive cultural answer to Elkins's contention that slavery in America had been so oppressive as to obliterate the essential humanity of the Africans who had been forced to endure it. Herbert Gutman, whom we had last encountered in regard to labour history, provided a social response to Elkins's interpretation of the impact of slavery that almost equalled in definitiveness of Levine's cultural response. Gutman's retort to Elkins was his book The Black Family.
Like so much else, as we have seen, in the historiography of American slavery,

Gutman’s book on the African-American family was the direct outgrowth of a contemporary public concern, in this case a controversy over a social policy of the American government. In the 1960s, partly under the influence of Elkins’s work on slavery’s devastating effect upon black people, the administration of President Lyndon Johnson initiated a social programme to prevent the break-up of black families, a social phenomenon that was seen as a direct legacy of slavery. The most common measure of the disruption of families was the presence of mother-headed households, something that was thought to be characteristic of families under slavery since it was known that marriage between slaves was without legal support, and within families, fathers were thought to lack economic or psychological authority. Contrary to these widely held views, Gutman’s research showed that long-standing marriages between couples were characteristic of slave households. Moreover, by following former slave couples after emancipation, Gutman demonstrated that the idea of enduring marriages was something cherished by the slaves themselves rather than being an institution imposed on the slaves by masters.
or the cultural pressure of the white social order. Through a deft analysis of slave

children’s names, Gutman was able to show that fathers were more than solely
generators of offspring; they held positions of authority within the family not unlike

those occupied by white husbands. Finally, by carefully uncovering the lines of

kinship that ramified among members of slave communities, Gutman revealed in yet another way the complex lives the slaves had managed to create and maintain

even under the oppression of bondage.

If the awakening of historians to the need for a new history of African-Americans began with the enormous outpouring of studies of slavery, it did not stop there.

New biographies of historically important African-Americans began to appear,
among which was a monumental study in two volumes (1972, 1983) of the former

slave who became a noted educator and black leader, Booker T. Washington, by

Louis Harlan, and a study of twentieth-century black leaders (Franklin and Meier 1982). The Harlem Renaissance of the 1920s, the establishment of black communi
ties (the so-called 'ghettos') in northern cities and the story of the economic

success of African-Americans also became the subjects of scholarly works. (e.g.

Osofsky 1966; Spear 1967; Huggins 1971; Lewis 1981; Schweninger 1990). The
whole field of Southern Reconstruction history, which had been long interpreted as a measure or sign of the grievous errors of a too rapid and too egalitarian emancipation, now came under fresh scrutiny. This time around the emphasis was placed upon the achievements of former slaves in and out of the governments of the Southern states with full recognition of their long history of exclusion from a free life. The new emphasis was especially well displayed in Eric Foner's thoroughly documented Reconstruction: America's Unfinished Revolution, 1863-1877 (1988), in which, for the first time in an academically accepted volume by a white historian, the story is told more from the standpoint of blacks than from that of whites. 2

At the root of the burgeoning interest in black history lay the assumption that the proper history of the United States required the bringing into the mainstream of historical writing the activities of all minorities, not simply blacks. African Americans, it is true, constituted the largest minority in both the American present and the American past. But as young men and young women worked together in the 1960s on behalf of the civil rights of blacks, it soon became apparent to some of the young women, at least, that there was an analogy between the limits that were placed upon women's roles in society and those against which they were protesting
on behalf of the freedom of African-Americans. Obviously, the analogy was not precise: women, for example, had never been segregated. On the contrary, because they were a sex, they had been intimately 'integrated' with the dominant group. Yet historically it was true that, like blacks, women had been kept from voting, or participating in government, and denied access to certain kinds of jobs, and even a college education. When, in more modern times, higher education, politics and the vote had been opened to them, custom and family obligations had still confined most women to limited roles in life compared with those open to men. It was not far-fetched, then, for women in general and women in the historical profession in particular to ask that they, too, along with blacks and other minorities, begin to bring their past into the mainstream of US history. Unlike the situation that 2 Much is revealed about the changes in Reconstruction historiography over the last half-century when it is recognized that the black historian W. E. B. Du Bois had published his Black Reconstruction as long ago as 1935, and that it had been ignored by the great majority of white historians. 718 prevailed in regard to African-Americans, who had been segregated throughout American history until very recent times, women themselves were ready to take on the task of writing their own history. Their historic
'integration' with men meant that many already possessed the necessary educational and scholarly preparation for the job.

Some women, it needs to be said, had been writing for decades about women in the past, but their work had not been acknowledged as a part of the general historiography of the United States. That changed in the course of the 1960s. The earliest work in the new women's history, during the 1960s, followed a path not unlike that which Elkins had sparked when he depicted blacks as primarily victims under slavery. For women that meant emphasizing the ways, particularly during the nineteenth century, by which familial domesticity limited women's lives. The classic article that inspired many subsequent studies was Barbara Welter's 'The Cult of True Womanhood: 1820-1860', which appeared in 1966. Notable among those subsequent studies was Nancy Cott's Bonds of Womanhood: 'Woman's Sphere' in New England, 1780-1835 (1977). Some authors found within 'woman's sphere', as others had found within black bondage, signs of restlessness or outright resistance. Nina Baym identified it through an examination of Woman's Fiction: A Guide to Novels by and about Women in America, 1820-1870 (1978), where she showed how women writers and readers created a symbiotic relation that
sustained their gender identity. Domesticity itself could well provide a strong sense of identity, Kathryn Kish Sklar showed in her sympathetic biography of the leading home economist of her time, Catharine Beecher (1975). Firmly convinced of the strength of women even in the nineteenth century, Gerda Lerner made her point early in her vigorous biography of the anti-slavery and women's rights advocates, The Grimke Sisters (1967). Anne Firor Scott documented women's strength even within the domestic sphere in the South in her Southern Lady: From Pedestal to Politics, 1830-1930 (1970), but Suzanne Lebsock (1984), writing later about the ante-bellum women of Petersburg, Virginia, was not so sure that Southern women were that independent.

Lebsock also included black women in her story, taking a broader approach than some of the first books on women's history, which dealt primarily with middle-class white women. The problem of the division which class inevitably produced among women was well explored in Barbara Epstein's Politics of Domesticity (1980). By the late 1980s the sharp change that had taken place in women's conception of domesticity was thoughtfully documented in Glenna Matthews's 'Just a Housewife'. The Rise and Fall of Domesticity in America (1987).

By the 1980s it had become abundantly clear that the
category 'women' was a much more divided category than 'African-American'. Black people after all, had been largely confined to poverty because of their long history under slavery, but women from the beginning had been spread evenly throughout the social structure.

In addition to class and ethnic divisions among women, historians began to explore those parts of women's past that differed from men's. John Mack Faragher, *Women and Men on the Overland Trail* (1979) and Julie Ray Jeffrey, *Frontier Women: the Trans-Mississippi West, 1840-1880* (1979) not only documented women in the west, but stressed as well their commitment to the separate sphere for females. The quite different experience of black women at work and in families was dramatically and sympathetically told in Jacqueline Jones, *Labor of Love, Labor of S"OW* (1985).

Perhaps the most striking ways in which women's lives in the past differed from men's was in their work outside the home, and so numerous studies soon appeared documenting their experiences. The once familiar 'Lowell Girls' received a fresh and more realistic analysis from Thomas Dublin (1972), while domestic service, the most common form of women's work, was analysed by David M. Katzman (1978).

The inequalities and limitations that women encountered in the workforce are
historically considered in Alice Kessler-Harris, Out to Work; a History of Wage earning Women in the United States (1982). The primary social reason for the expansion of women's work was the growth of the factory and the city, which not only found a need for women's labour, but often provided as well an escape for women from the restrictions imposed by the traditional family. These aspects of the opportunities cities provided women were insightfully presented in Joanne J. Meyerowitz's imaginative Women Adrift: Independent Wage Earners in Chicago, 1880-1930 (1988) and, for an earlier period, in Christine Stansell, City of Women. Sex and Class in New York, 1789-1860 (1987). The traditional 'trade' of women is scrutinized in Ruth Rosen, Lost Sisterhood: Prostitution in America, 1900-1918 (1982).

Virtually all white women in the nineteenth century who worked outside the home were unmarried, but that pattern began to change dramatically in the 1930s and 1940s, and then became a revolution after the 1950s. Winifred Wandersee's Women's Work and Family Values, 1920-40 (1981) began the story. The movement of married women into the workforce in great numbers after the Second World War and the movement against job discrimination is well analysed by a professional
Prostitution became a subject of importance in women's history because it touched on a fact that must always distinguish women's past from that of men.

Sexuality, we must not forget, is the historical as well as the social link between men and women. Indeed, it is not at all radical to see sex as the source of women's subordination to men and, as such, to be a subject historians need to explore if they are to create a history of women. One of the earliest efforts to re-examine the nature of women's sexuality was a 1974 article of mine, which was based on a turn-of-the-century questionnaire answered by forty-five middle-class women on their sex habits. 3 The subject of sexuality has been expertly traced throughout American history in John D'Emilio and Estelle Freedman, Intimate Matters: A History of Sexuality in America (1988). The shift among historians from thinking about sex to thinking about 'gender', the socially constructed difference between the sexes, is

3 Degler 1974. All of the forty-five individual questionnaires have been published in Mosher 1980. 720
elucidated in important articles contained in Carroll Smith-Rosenberg, Disorderly Conduct: Visions of Gender in Victorian America (1985).

Given the reasons why women's history has generated a novel subject like sexuality,

it can come as no surprise that the content of history, under the prodding of women's history, has now expanded to include the history of childbirth and birth control.

Excellent in the first regard is Judith Walzer Leavitt, Brought to Bed: Childbearing in America, 1750-1950 (1986) and equally rewarding in the second is James Reed, From Private Vice to Public Virtue: The Birth Control Movement and American Society Since 1830 (1978). And if contraception has been added to history, one must expect that the addition of abortion is not far behind, as James Mohr's Abortion in America: the Origins and Evolution of National Policy, 1800-1900 (1978) demonstrates.

The unfolding of the once hidden subject of sexuality has added the history of homosexuality to the historiography of the United States, the pioneering work of Jonathan Katz, Gay American History, appearing as early as 1976. John D'Emilio's important scholarly study of the origins of homosexual political and communal activity appeared in 1983, to be followed a few years later by Allan Berube's (1990) thoroughly researched story of homosexual men and women during the Second
Finally, as a further offshoot from the upsurge in women's history, there needs to be mentioned the beginnings of a history of manhood. The aim of this most recent field of study is to do for men's past what the study of women's history brought to women's past: the discovery of the social construction of sex or gender, that is, in the case of men, to ascertain what it meant to be a man at various times and occasions in the past. Several such studies appear in the collection entitled Meanings for Manhood. Constructions of Masculinity in Victoria America (1990), edited by Mark C. Carnes and Clyde Griffen, and in Carnes's book, Secret Ritual and Manhood in Victorian America (1989), where he seeks to delineate the feelings of men nurtured by the highly popular adult fraternal organizations of the nineteenth century.

The growing recognition of the diversity of the American past carried one other group into the mainstream of the country's historiography: a people who had been present for the longest time of all: the First Americans or Indians. A few historians and many anthropologists had been studying and writing about Amerindians as long ago as the mid-nineteenth century, but most historians generally pushed aside their works, much as the Europeans had pushed the Amerindians aside as they moved...
westward across the continent. Just as blacks were often seen in traditional history as a source of conflict and ultimately civil war, so Amerindians came to be perceived as a source of problems, and, then, as their numbers declined and they were confined to reservations, as outside the American story. Indeed, in 1959, in writing a general interpretation of American history that took as its focus the relevance of the past to the present (Degler 1959), I deliberately omitted any discussion of Amerindians on the ground that in the 1950s they were not of contemporary concern. Within a matter of years, that perception was dramatically changed because in California and in South Dakota Amerindians staged large public protests that soon brought their plight and the nation's neglect to the attention of daily newspaper readers.

Today, the history of the First Americans is a burgeoning field of scholarship that draws heavily upon conventional historical sources as well as the more traditional anthropological sources. What has changed dramatically, of course, has been the point of view. Whether written by Amerindians themselves (who happen not to be active in the field compared with, say, women in the field of women's history or of African-Americans in black history) or by non-Amerindians, the writing of Native American History today has abandoned the 'white man's'
approach to the indigenous peoples, as Wilbur R. Jacobs's pioneering book,

Dispossessing the American Indian (1972), makes clear. Sometimes these new

historians do not even attempt to conceal their indignation, as in Francis Jennings,

The Invasion of America: Indians, Colonialism, and the Cant of Conquest (1975). The

aim, as James Axtell's The European and the Indian (1981a), emphasizes, is to

understand, not to judge, the divergent cultures that clashed on the North American continent in the course of some 300 years. One of the most striking discoveries of the new history of Amerindians is the devastating demographic consequences of the arrival of the Europeans, as Russell Thornton, American Holocaust and Survival (1987) summarizes in horrifying detail.

As one might anticipate, the rapid increase in historical subjects has also meant that some fields now begin to overlap as they cross-fertilize one another. A notable example of this is William Cronon's Changes in the Land (1983), a magnificent study of the effects of the physical environment of New England on the life of the indigenous population, and their impact, in turn, on the physical landscape. In another example of cross-fertilization, Theda Perdue brought together Amerindian history and Southern slavery in her wide-ranging book

Up to now in delineating the variety of new fields and approaches in recent American historiography, I have been emphasizing the influence of ethnic and national diversity in American society. That influence is real, but it should be perceived also as a surrogate for a less obvious, but no less significant influence, namely, concerns about the distribution of power in American society. By historical experience and definition minorities and women lack power and to a substantial degree still do in the present. The new emphasis on telling the story of minorities and women is a way of calling into question the power hierarchy of the present as well as that of the past. It takes the place, in part at least, of similar tendencies among European historians to emphasize class differences, for in their pasts class is the traditional identification of differences in power. Among Americans, however, class has always had less salience than ethnic and racial minorities as a means for criticizing the distribution of power in the past and the present.
4 De Loria 1969 is an exception, but it was not a scholarly work. Brown 1971 was not an academic work, either, but it measures the sudden upsurge of interest in Amerindian history in that it was a Book-of-the-Month-Club selection. 722

In conclusion, there remains one other aspect of the American past that has been sharply redefined as a result of changes in the society in which historians live.

That is the field of American diplomatic history. As with social history, the dividing line that cuts across the foreign-relations history of the United States is the Second World War. Before that event, most historians of foreign relations manifested a largely sympathetic understanding of the United States' dealings with other nations. The main dissent from that outlook had been objections to American involvement in the First World War, an enduring proponent of that dissent being the well-known historian and political scientist Charles A. Beard. Unlike the great majority of his professional colleagues, Beard resisted the Roosevelt administration's frank support of the Allies in their war against Germany before the bombing of Pearl Harbor. Beard interpreted the European war as without immediate significance to the vital interests of the United States. Beard thought that the American 'national interest', which most official and
unofficial supporters of Britain and France invoked in justification, was largely a
euphemism for the economic interests of American business and finance. After the
war, Beard continued to denounce Franklin Roosevelt for having put the United
States in such an international position in the late 1930s as to have made the
Japanese attack in 1941 very likely. Beard's heavily economic really anti-business
interpretation of the motives behind US foreign policy and action won few
supporters until the 1960s, when it began to be taken up in different forms by a
substantial number of young historians of American diplomacy. The issue around
which the revival turned was the question of how the 'Cold War' between the two
Second World War allies the United States and the Soviet Union had begun.
The generally accepted view was that it grew out of the Soviet Union's truculence
and lack of co-operation with the Western allies, particularly Great Britain and the
United States, in settling the many difficult questions that were left by the political
as well as the physical devastation of Europe in the course of destroying Hitler's
Germany.
Perhaps the first systematic effort to call that interpretation into question was
Beard economic explanation, but he did raise questions about the fairness of the Allies' dealings with the Soviet Union and that unfairness, he contended, threatened the kind of sound world order that Woodrow Wilson had sought to achieve after the First World War. Even before Fleming had published his critique, the ground had been laid for bringing the old Beardian interpretation into the discussion. A young diplomatic historian at the University of Wisconsin opened the new path with his Tragedy of American Diplomacy, published in 1959. Williams was not only an admirer of Beard's approach, but also a historian who focused that interpretation on the idea that at least from the end of the nineteenth century, US foreign policy had been shaped, if not dominated, by a search for markets abroad for the goods pouring out of the expanding American industrial economy. The chief American policy instrument for accomplishing that goal, according to Williams, was the 'Open Door', or open competition for international markets, a policy that had been proclaimed at the onset of the twentieth century and which became a centrepiece of US international economic policy thereafter. Both Williams's and Fleming's books appeared just as the United States was entering upon what American leaders of both political parties later came to see as a
world struggle to contain the influence of the Soviet Union and its ideology of communism. Not all Americans accepted the view that the United States was without fault or responsibility in bringing about that struggle. This was especially true of many historians who were already critical of many actions and policies of the American government and society as they examined the social and political history of their country. Again and again the point was made by Williams and by those who followed in his footsteps that the antagonists in the Cold War were socialist and capitalist countries. N. Gordon Levin's Woodrow Wilson and World Politics (1968) developed this theme for the years when the Soviet Union was being founded, as a conflict between Woodrow Wilson's and Vladimir Lenin's conception of world order. In short, there is a fairly obvious connection to be discerned between the changes in the social historiography we have already examined, and the emerging critical or revisionist historiography of American foreign policy influenced by William Appleman Williams. Both were critical of the distribution of power in American society, past and present.

Gar Alperovitz was not a student of Williams, but his book Atomic Diplomacy: Hiroshima and Potsdam (1965) made an argument similar to Williams's in contend
ing that nuclear bombs had been dropped on Japan principally to bring the war to
an end before the Soviets could enter in force against Japan, and to demonstrate to
the world, and to the Soviet Union in particular, the enormous power in the hands
of the United States as it prepared to shape the post-war world. Walter LeFeber's
America, Russia, and the Cold War 1945-1967, appearing in 1967, pressed a much
clearer economic or market-seeking interpretation of US diplomatic behaviour, that
left the motives of the Soviet Union as largely defensive and even fearful of the
dangers of capitalist intrusion into the socialist economy of eastern Europe. Joyce
Kolko and Gabriel Kolko, The Limits of Power: The World and United States Foreign
Policy, 1945-1954 (1972) provide the most extreme example among scholarly
efforts to follow single-mindedly an economic determinist interpretation of US
foreign policy. Although these revisionist interpretations were derived from
examination of many internal often once top secret documents of the United
States government, no United States historians had access to comparable docu
ments of the Soviet government. Nevertheless, the revisionist view of the Cold War
was widely accepted, though not in all particulars, as the widely praised book by
Daniel Yergin, Shattered Peace: The Origins of the Cold War and the National
Security State (1977), makes evident.

The generally critical approach to recent foreign policy, which the revisionists on

the Cold War began, has continued in studies on the

origins of the Korean War, as

exemplified in Bruce Cumings's two-volume Origins of the

Korean War (1983,

1990), and numerous monographs on the roots and policies

of the war in Vietnam.

The magnificent efflorescence of historical studies as a

result of the social and ideological changes in American society since the Second

World War does have a

little shadow over it. That is the difficulty of

integrating this diversity of subjects

and interpretations into a fresh and useful picture of the

American past. No

agreement has been reached on the meaning of the newly

revealed America.

Obviously it cannot be said that the United States is

simply a collection of diverse

groups that the new social history has brought so

forcefully and fruitfully to

everyone's attention. The old political history that once

provided a coherent theme,

a line of development to the American past, is clearly no

longer sufficient. Yet the

complexities spread before us by the new studies provide no

obvious theme or

pattern. Certainly one of the central purposes of national

history is to display to the

nation's people the story of who they are. That, however,

requires some focus, some
identification that is more than a patchwork of events and ideas. Most likely, in time

and in line with the changing historiography of the last thirty years, that synthesis,

that focus will emerge from the present, which, in the end, is the ultimate shaper of

history since most written history is composed largely of responses to questions the


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Like the labyrinth of King Minos, Latin America is big, complicated and easy to get lost in. The first task of any tour-guide is therefore to give the traveller some bearings. Latin America covers nearly 8 million square miles (more than twice the area of the United States, sixteen times that of the United Kingdom). Historically underpopulated, it has experienced a demographic explosion in the twentieth century and now contains a population approaching half a billion. This population is divided among twenty sovereign states and the 'self-governing commonwealth' of Puerto Rico. All these states save one (Cuba) have enjoyed independent existences since they emerged from the dissolving Spanish and Portuguese empires in the 1810s and 1820s. As such, they are older than most European states and almost all African states. Latin America is ethnically diverse: large Indian populations live in what once were the heartlands of the Aztec, Maya and Inca civilizations, in Middle America and the Andean highlands. Black slaves, brought in droves to the slave plantations of Brazil, the Caribbean, and some coastal regions of the Spanish American mainland, added to the region's rich ethnic and cultural blend. And the original Spanish and Portuguese immigrants of the colonial period conquistadores and clerics, merchants and officials were later followed by millions of the late
nineteenth and early twentieth-century migrants, who left southern Europe and came particularly to Argentina, Uruguay and southern Brazil to 'make America' (hacer America), thus creating Latin versions of the US 'melting pot'.

1 The labyrinth, as a metaphor, has a particular Latin American especially Mexican association: paz 1961.

2 Cuba did not gain its independence from Spain until 1898, following two massive wars of national liberation and Spain's defeat at the hands of the United States which promptly installed a quasi protectorate over the island. Ethnically mixed, Latin America has also experienced contrasting processes of economic development, class conflict and state-formation. Argentina, in 1914, ranked among the ten richest countries in the world. Haiti was and remains among the poorest. If the colony (c.1500-1800) was a period of relative political calm, independence unleashed the demons of instability. Bolivia, it has been calculated, experienced 185 revolutions between 1826 and 1903; 1848 a 'red year' in Bolivia as in Europe witnessed fifteen of them (Dunkerley 1992: 153). Mexico similarly suffered instability and territorial loss; yet, following the Mexican Revolution of 1910, the country acquired a 'revolutionary' government which achieved stable,
one-party, civilian rule for over two generations. Uruguay, the 'Switzerland of
South America', pioneered a precocious welfare state in the early twentieth century:
while neighbouring Paraguay, in contrast, languished under the personal
dictatorship of General Alfredo Stroessner for thirty-five years (1954-89).

Powerful political leaders of this kind, usually hung with the loose label of
'caudillos', 4 punctuate Latin American history and traditionally and excessively
dominate the region's historiography: Bolivar and San Martin, the architects of
South American independence; the renegade priests, Morelos and Hidalgo, who
established a tradition of Mexican popular insurgence in the 1810s which Villa and
Zapata would reprise a century later; Toussaint L'Ouverture, who helped forge the
first independent black republic from the ashes of the French slavocracy in Haiti;
Jose Marti, the Cuban nationalist leader, and Maximo Gomez, his black guerrilla
chief, who drove Spain from its last colonial possession the 'ever faithful'
(siempre fief) island of Cuba in the 1890s. These insurgent caudillos may be
contrasted with the hard-headed, state-building caudillos of the nineteenth century
Portales in Chile and Paez in Venezuela; Paraguay's Dr Francia; Rosas and Roca
in Argentina; Juarez and Diaz in Mexico. Finally, we encounter the 'populist,6
caudillos of the twentieth century, who captained new mass movements and
confronted an ideologically turbulent world in which socialism and fascism, liberal
capitalism and economic nationalism, contended for power: Calles and Cardenas in
Mexico; Argentina's Peron; Brazil's Vargas; Chile's Allende (the world's first
democratically elected Marxist president); the Cuban
president/dictator Fulgencio
Batista and his nemesis Fidel Castro; and their Nicaraguan counterparts, Somoza
and the Sandinistas, who set the stamp of history on their struggle by assuming the

3 Useful analyses of Argentina's subsequent fall from grace are: Ferns 1973 and Lewis 1990.

4 Roughly, caudillos were warlords, capable of mobilizing a clientelist following, and often dependent

5 It is sometimes worth making the distinction between 'history' (what happened) and
'historiography' (what historians have written about what happened). However, it would be rather
pedantic and long-winded to insist on this distinction throughout, so I have sometimes used 'history' to
denote 'historiography', particularly where no ambiguity exists.

6 'Populism' is another common but contentious term: it can denote a general political style or, more
often, a specific type of movement/regime/leader which, it is argued, flourished in Latin America
during the middle of this century i.e. from the Depression
to the 1970s. A useful symposium is Connif1982. 729

name of the great patriot and political martyr of the inter-war years, Augusto Cesar Sandino.

These, of course, are but a handful of examples drawn from a political universe remarkable for its diversity (hence the very term 'caudillo' may disguise huge contrasts). Furthermore, as I shall try to show, historians have rightly moved away from the 'Great Man' approach to history (there never was a 'Great Woman' theory, certainly not in macho Latin America); Carlyle's 'history [as] the biography of great men' (Carr 1964: 49) has given way to forms of local, regional, social, economic and cultural history, in which individuals appear as products as much as creators of their historical environments. This does not mean the annihilation of biography, but rather the interweaving of biography or that form of collective biography which is jawbreakingly labelled prosopography 7 with these other analytical approaches. The goal, as in the case of nineteenth-century Mexico, has been to avoid 'explaining Mexico in terms of Santa Anna rather than Santa Anna in terms of Mexico';8 that is, to locate key individuals within their political, cultural and socio-economic contexts, without entirely sacrificing their individuality. The
result, at national as well as regional and local level, has been a retreat from
simplistic stereotypes (providential national histories, crude teleologies, the
'distorting dichotomies' of colonynation, conservative/liberal, hero/villain) and a
renewed stress on diversity and variation. Contrasting political histories mirrored
or created? contrasting political cultures in Latin America. Colombia, it was once
said, was ruled by lawyers, Ecuador by priests and Venezuela by soldiers: a neat rule
of thumb, conveying a simple truth about contrasting national experiences, but one
that requires almost infinite qualification both by region (Guayaquil and coastal
Ecuador were not 'ruled by priests' in the way that Quito and the highlands were)
and by period (Venezuela, a bastion of militarism and authoritarianism up to the
1940s, has been consistently if at times a little precariously democratic since
1958).
Thus, despite its superficial unity a shared Iberian colonial background, a
powerful Catholic Church, a dominant linguistic tradition Latin America is
notable for its great internal variety. To claim to be a historian of Latin America
or to try to summarize historiographical trends for Latin America as a whole is to
7 That is, the compilation and analysis of data covering a range of individuals (usually elite
individuals) and their relationships political, factional, familial. A good example is Guerra 1985:

8 The phrase of Moises Gonzalez Navarro, quoted in Stevens 1992: 2.

9 Taylor 1985: 117. I acknowledge a considerable debt to this excellent article, particularly as regards

10 Although the dominance of Spanish and Portuguese affords the historian unusually wide access to

sources primary and secondary throughout the continent, it should not be forgotten that Indian

languages are widely spoken in Mexico and Central America (Nahuatl and Maya especially) and in the

Andean region (chiefly Aymara and Quechua); colonial historians and contemporary practitioners of

oral history have both used these to good effect. A classic example in which Nahuatl is used not just as

a tool of research but also as a means to calibrate social change is Lockhart 1991: 730

display a vaulting ambition, and to ride for a fall. Nowadays most 'Latin American

ists' are, first and foremost, historians of a particular country, increasingly of a

particular region, class or topic within a particular country, especially when it comes
to their 'primary', archival work. Some historians, driven by the urge to generalize

(a dying impulse, one sometimes feels) or by the need to supplement meagre

academic salaries, attempt grand syntheses, i.e. textbooks. But the latter do not

make although they may help break academic reputations,
which depend,

particularly at the outset, on narrow monographic work, based on archival research.

Hence, as I shall suggest in conclusion, a combination of professional command of the archives, coupled with a territorial imperative to defend one’s own patch, can lead to a certain narrowness of vision, and a resulting fragmentation of historical scholarship. It follows that any discussion of Latin American historiography where it’s at, where it’s going must try to encapsulate a field which, by virtue of sheer divergent detail and infinite variation, presents enormous problems.

In some particular areas for example, my own field of Mexican revolutionary studies it is difficult for the individual scholar to keep up with the exponential growth of articles, theses, monographs and the occasional synthetic work. In Mexico, as elsewhere in Latin America, archives have grown and improved. In Mexico City the old Lecumberri prison a Benthamite panopticon has been turned into an outstanding national archive, where, no doubt, scholars of the fashionable Foucaultian persuasion feel particularly at home. Agrarian reform programmes, by confiscating properties, have released estate archives into the public domain. Regional research centres have boosted the output of regional and
local history of which more anon. Today's novice historians
of Latin America,
while they may have to master a bewildering array of
published sources, at least face
a happier archival fate than that of previous generations,
whose primary work
sometime resembled a cross between an obstacle race and a
tribal rite de passage. (I
refer not just to the primitive or non-existent
photocopiers, but also, more
importantly, to inadequate catalogues, filthy conditions,
unpredictable hours and
eccentric archivists, such as the gentleman responsible
for the old Casa Amarilla
an ex-convent in Mexico City which once housed the labour
department papers
where researchers wore surgical masks and the archivist,
fond of taking potshots at
the pigeons fluttering in the rafters, served hot tacos de
paloma (pigeon sandwiches)
to hungry researchers at the end of a long morning.) 12

Primary sources are now more abundant, accessible,
catalogued, sometimes
microfilmed. The raw material of historical research has
therefore expanded and,
11 Bailey 1978 made the point nearly twenty years ago,
since which time the volume of historiography
has further mushroomed.

12 I am indebted to Barry Carr for this anecdote. The
'hands-on' knowledge and expertise of some
old-style archivists should not be disdained. 'Don Goyo',
as he was generally known, was for years the
best guide to the Guatemalan national archive in Guatemala
City. Conversely, gung-ho new archivists can foul up functioning systems with their unthinking innovations (e.g. at the ecclesiastical archive of Morelia, Michoacan, Mexico). 731

while twentieth-century sources may be most prolific (at least for the heyday of the written word, c.1900-50),13 the mixed blessing of abundance also affects the colonial period, when a bureaucratic administration churned out paper, particularly relating to administrative, fiscal and judicial matters. Where the archives are abundant, the historians assiduous and the techniques advanced, complex ‘professional’ debates have developed, concerning, for example, the fluctuating population of the Americas, race mixture in colonial New Spain (Mexico), Bourbon fiscal reforms and tax yield (e.g. Seed 1982; McGaa 1984; Ouweneel and Bijleveld 1989). By virtue of their complexity the engaged historian needs to understand colonial procedures as well as modem statistical techniques these debates are resistant to quick resumes, and may even prove impenetrable to the ‘lay’ historian.

I mention these points partly to stress that Latin American historiography is, like most other historiographies today, multifaceted or, to use a more negative term, fragmented. The drive towards professionalism, expertise and archival research fuelled by graduate programmes and productive of an
enormous volume of

historiography (good, bad and indifferent) makes synthesis
difficult and liable to

any amount of subjective bias. As a modem
(twentieth-century) historian, chiefly of

Mexico, I write with rather more authority about this
century than preceding

periods, and about Mexico rather than the rest of Latin
America. However, for an

overview of this kind to be of use, it must strive for
broad coverage, venturing into
different comers of the labyrinth, with all the risks that
that entails. I shall,
therefore, try to avoid an excessive focus on my own place
and period though it is

not, I think, personal bias to suggest that the
historiography of Mexico, both

colonial and modem, has certainly been the most voluminous
and probably been the

most penetrating in the last generation: Mexico, in other
words, leads the Latin

American pack as it tries to cut back the long lead
established by European and

North American historiographies. 14 And I shall attempt
some coverage of the

colony, despite my lack of expertise; not only because the
colony was the cradle of

modem Latin America,15 but also because colonial
historiography has, in some

areas, proved more innovative and sophisticated than its
modem counterpart

proof, were it needed, that historiography, like the past
itself, does not advance and
13 It is my informed guess that the combination of literacy, state-building and technology made the first half of this century a golden age for the production of written information, not least official documents. More recently, I suspect, the growth of non-written communication (telephone, radio, TV, e-mail) has resulted in a relative reduction in the quantity of useful written material which future historians may be able to consult.

14 It is worth stressing that, despite its rapid advance in recent years, Latin American historiography remains some way behind its European and US counterparts, in respect of volume, archival resources and number of practitioners. While this can make for an apparent lack of sophistication Latin American historians are to be found hacking clearings out of the jungle rather than cultivating neat market gardens it also imparts a certain raw, outdoor vigour to the enterprise.

15 Of course, pre-conquest Native American societies were also ‘cradles’ of colonial society, and colonial historians have often stressed the continuities which spanned the trauma of the Iberian conquest. However, it would break the back of an already overloaded llama if I attempted to cover pre-conquest history in this brief resume. 732 unfold in a neat, unilinear fashion. However, it would be self-defeating to try to cover everything, to cite every trend and drop every name. I shall therefore concentrate on two themes and a deficiency; en route through the labyrinth, I shall also touch upon without abstracting the eternal question
of periodization. 16

THREADS THROUGH THE LABYRINTH: REGIONS AND LOCALITIES

The two salient themes in recent Latin American historiography are closely intertwined and readily recognizable to those who have contemplated the leafy branches of Clio as they spread across other countries and continents: first, regional/local history (history 'from-the-periphery-in', as it were) and popular / subaltern history (history 'from-the-bottom-up', the history of los de abajo). 17

These, clearly, represent a reaction against earlier emphases which stressed nation states, national elites, political and military narratives, political and constitutional evolution (or retrogression) (Taylor 1985: 142). In part because these earlier works suggested but did not much investigate regional variation, political Balkanization and provincial resistance to centralization, recent historians have tended to quit the capital for the provinces, addressing questions of local or regional economic development, political organization and collective identity. Thus, the once bland façade of the colony or the independent nation-state has, on closer inspection, come to resemble a Baroque exterior, rich in detail, contrast and eccentricity. The process has been made possible by the growth of regional archives and research centres, by the better organization of national archives.
already mentioned, and by

the proliferation of graduate programmes, particularly in
the United States and

Latin America itself. Thus, a new generation of young
regional and local

historians has assumed the mantle once worn by venerable
'amateur' historians of

the patria chica, the chroniclers of local events and
customs. Sometimes these

newcomers have even acquired a touch of local chauvinism
('my patria chica right or

wrong') to go with it. As in Europe, they have often taken
the utility and rationality

16 I am aware of a third theme which may merit special
treatment: the Latin American state, in its

colonial and post-colonial forms. While this is an
enduring historiographical issue to which I refer in

passing it is less useful as an 'organizing concept' with
which to make sense of recent historical

research: for that reason, and because of pressure of
space, I have chosen to concentrate on two other

concepts, or approaches: (1) regions/localities and (2)
popular history.

17 'The underdogs', as in the title of Mariano Azuela's
classic (1915) novel of the Mexican

Revolution.

18 The output of Latin American history by Latin Americans
has been remarkable in recent years.

Furthermore, as Latin American universities and research
institutes have often in the face of severe

financial and political difficulties developed innovative
historical programmes, periodicals and career

structures, so the historiography produced within the
continent has come to conform to the US/

European model, the older bellelettrist tradition though far from dead, has given way to a more

'professional' approach, in which archival research, graduate degrees and specialist journals figure

language readership I have regrettably excluded most Spanish/Portuguese sources; hence Latin

American historians of Latin America are sadly underrepresented. 733 l r/ I

of regional or local history for granted: justifications of the unit of analysis (why

choose to study a state, province, district, municipality or community?) do not bulk

large; the very notion of 'regionalism' the tissue of relationships which ties people

together, politically, economically, culturally, in subnational units is rarely

explored, even though the centrality of the concept in contemporary historiography

(not to mention contemporary history) makes the 'region' almost as deserving of

conceptual clarification today as the 'nation' was in the past. 19

Given the volume of regional/local studies, any resume must be partial and

arbitrary. Colonial regional studies have illuminated the process of conquest and

settlement, particularly in the old heartlands of Mexico and Peru. The pioneering

work of Charles Gibson (1952; 1964) on the Indian communities of Tlaxcala and the

Valley of Mexico has paved the way for subsequent studies of colonial regions,
northern and mestizo as well as central/southern and Indian (Altman and Lockhart 1976). The richness of colonial archives has made possible sophisticated analyses of the structures of the colonial economy, including its mining centres at Guanajuato, Zacatecas (both Mexico) and Potosí (Upper Peru/Bolivia) (Brading 1971; Bakewell 1971; 1984); its mercantile networks (Oaxaca and Mexico City; New Granada; Buenos Aires); its commercial farming, particularly as practised by the entreprenurial Jesuits (Morelos and Guadalajara, Mexico; Cochabamba, Upper Peru/Bolivia; Quito and coastal Peru) (Martin 1985; Van Young 1981; Larson 1988; Cushner 1980; 1982); and a theme to which I will return its abiding, enduring, peasant communities (Oaxaca and Yucatan; Huamanga and Huarochiri, Peru) (Taylor 1972; Farriss 1984; Stern 1982; Spalding 1984). Colonial research has also helped us understand the tensions of late colonial society aggravated by the Bourbon administrative and fiscal reforms, already mentioned, and the brisk commercialization of the late eighteenth-century economy which in turn contributed to the independence struggles of the 1810s (Hamnett 1986; O’Phelan Godoy 1988).

This research, dominated by the regional approach, has made possible new but
still few syntheses, which try to integrate regional studies, weaving local, provincial and (proto)national stories together: John Tutino's ambitious overview of Mexican agrarian history; James Lockhart's (1992) masterly analysis of cultural and linguistic assimilation in colonial New Spain; Nancy Farriss's (1984) outstanding study of the Yucatec Maya and their long struggle for survival through three centuries of colonial rule. Given the nature of colonial society ordered, orderly,

19 Taylor 1985: 178, makes a similar point, asking why a diocese should be used as a unit of socio-economic analysis (save for the fact that it happens to contain a diocesan archive). For a useful discussion and examples, see Van Young 1992.

20 Hamnett 1971; Kicza 1983; Twinam 1983; Socolow 1978. Note the preponderance of studies on the Bourbon eighteenth century, especially post-1750; 'studies of merchants in the mid-colonial period', as Catherine Lugan (1986: 73) observes, 'are rare'.

21 Tutino 1986. It should be added that many of the Mexican regional studies which Tutino, for example, synthesizes were themselves stimulated by an earlier seminal synthesis which, though qualified by later research, remains a classic: Chevalier [1952] 1970. 734

bureaucratic and legalistic these studies are particularly good at describing methods of rule and modes of production over time (they can also, I shall later
suggest, probe deeper into popular 'mentalities' and beliefs). They can also bite off big chunks of time, giving a sense of and what historiographical analysis dare omit the phrase? the longue dude. 22 Eric Van Young (1981) charts Guadalajara's socio-economic evolution over a century and a half; Brooke Larson does the same for Cochabamba over 350 years. These are chronological chunks which few modern (nineteenth/twentieth-century) historian could bite off, without risk of severe indigestion or frustrating superficiality: in part because the modern archives are, in general, more voluminous (though not necessarily more revealing); and in part because, following independence, the tempo of political change quickens and the staccato story of events of l'histoire evenementielle interrupts the slower socio-economic cadences of colonial history. 23 Thus, as we leave the (relatively) orderly and bureaucratic colony,24 to enter the rowdy, caudillesque era of independence, the emphases shift. Regional and local studies still prevail, and with good reason: they alone can subvert, question and qualify the grand old simplicities of national history. But the cast and the plot are rather different, hence the historiography differs too. Bureaucratic actors viceroyos, audiencias, mercantilist monopolies give way to political animals:
caudillos and their motley followers; embryonic political parties (or their counter parts, the masonic lodges); regular armies, freelance financiers and cliques of local notables; and a Catholic Church which, no longer subject to royal patronage, now becomes politically as well as spiritually and socio-economically salient. Local and regional studies tend to contract in chronological scope and, particularly for the earlier nineteenth century, to admit the novel importance of politics. For some, this is largely a story of caudillos, clients and local cliques/camarillas: politics is a Namierite struggle for place and privilege, requiring a Namierite analysis of family ties and patronage network (Balmori et al. 1982, esp. chs 3 and 4; and on Brazil, see Lewin 1987 and Graham 1990). Others continental Europeans, as it happens prefer to stress a more genuine and precocious politicization, involving ideological 22 In fact, Braudel's longue durie is misquoted as much as quoted, including in this instance. I use it (as many seem to do) to denote long periods of usually slow, incremental socio-economic change, spanning a century or more. As I understand Braudel, however, such a span corresponds to the Braudelian 'secular trend', longue dure being reserved for yet longer phases of almost glacial, geographical change: Braudel 1984: 76-7, 620-1. 23 Of course, colonial history started with a bang the Spanish Conquest, which offers ample scope
for classic histoire ivinamentiel, as William Prescott demonstrated in the 1840s. Concrete proof that histoire ivinamentiel is alive and well and living in the British House of Lords is afforded by Hugh Thomas's readable but nitpicking narrative, The Conquest of Mexico (1993). A contrasting contemporary study, illustrating where historiography is going rather than coming from, is Gruzinski 1993, which discusses the conquest in terms of cultural domination and resistance.

24 The colony was not uniformly tranquil, of course; but revolts and riots were sporadic and, more important, often formed part of a broad spectrum of popular protest, litigation and petitioning; they were as much parts of, as threats to, the colonial system, at least until the later eighteenth century. See Taylor 1979 and Cope 1994: ch. 7. 735 debate, elections and participation; for them, the Spanish-American revolutions bear comparison with the French, for both serve as midwives of a new political culture (Guerra 1994). All agree implicitly if not explicitly on the central importance of the state, whose workings and rationale were transformed by independence. 25 But few agree as to the nature of this transformation. The strength or weakness, legitimacy or illegitimacy, class affiliation or relative autonomy of the post-colonial state are as difficult to evaluate in the case of Latin America as they are in the case of Africa; and the regional studies of the period remain too few and
scattered to admit of an imminent synthesis.

In economic terms, however, the break at independence is less marked. If the colonial mining and mercantile economy partly collapsed, this did not historians now stress result in a dramatic breakthrough to a vigorous free-market capitalism.

Foreign chiefly British investment was limited and often unsuccessful; the old notion that Latin America exchanged formal Iberian colonialism for informal British imperialism has been severely qualified (Platt 1980). Latin American markets remained shallow and politically unpredictable; recalcitrant regions such as the Argentine interior blocked the best-laid plans of enlightened urban reformers (Ferns 1960; Gootenberg 1991). Trade remained patchy, liberal reforms and foreign loans sank without trace in a quicksand of protectionism, subsistence and caudillo politics. Many (social and economic) historians would therefore see a certain unity in the 'Bourbon' period, c.1750-1850. Political revolution, for them, did not imply social or economic revolution; the lineaments of the colonial order merchant and artisan guilds, big haciendas, corporate peasant communities remained, stressed but surviving. 26

Thereafter and the timing would vary from place to place the tempo of economic change quickened, as Latin American countries
boosted exports, acquired

an infrastructure (ports, railways), and began to
participate more vigorously in the

world division of labour. Regional studies have offered
numerous and crucial

examples of this process of economic extraversion: wheat
farming and stockraising

in Argentina (Scobie 1964; Sabato 1990); sugar in
north-eastern Brazil, coastal Peru

and Cuba (Eisenberg 1974; Klaren 1977; Knight 1970);
henequen in Yucatan

Ooseph 1982: chs 1, 2; Wells 1985); silver, later tin, in
Bolivia (Langer 1989);
copper in the Peruvian highlands (Mallon 1983); rubber in
the Amazon basin

(Weinstein 1983); oil in Mexico and Venezuela (Brown 1993,
esp. chs 1,2; McBeth

25 As Taylor 1985: 144-5 points out, the question of the
colonial state remains contentious: was it a

'nearly omnipotent' Leviathan, relatively autonomous of
society, or, rather, 'weak and fragmented', a

'passive arena of competing interests'? As indicated above
(n. 16), I shall duck any direct discussion of

this issue. Interestingly, similar debates arise in the
context of twentieth-century Latin American states,
to the extent that colonial labels 'Habsburg', 'Bourbon'
can be pressed into service for the

construction of contemporary typologies: Knight 1993b:
42ff.

26 Taylor 1985: 122-3 entertains doubts about this by now
almost conventional periodization of

social-economic history in terms of a rough 'Bourbon'
century, c.1750-1850. For good examples, see
Jacobsen 1993: part I, and Andrien and Johnson 1994, the introduction to which discusses the question

of periodization. 736

1983); and coffee cultivated on small plots or big estates, by peasants, peons,

slaves and proletarians throughout the hemisphere, from Puerto Rico through

Guatemala, Venezuela, and Colombia down to the great coffeetropolis of Sao Paulo

in southern Brazil (Bergad 1983; McCreery 1994; Roseberry 1984; Legrand 1986;

Palacios 1980; Stein 1957; Holloway 1980; Dean 1976).

While not all socio-economic studies of the later nineteenth century what

Tulio Halperin (1993) terms the 'neo-colonial' era of Latin American history are

precisely regional in approach (some, for example, attempt broader national

analyses of export staples, such as Chilean copper; Monteon 1982), most in effect

are, and have to be. First, because patterns of socio-economic development tended

to be regionally specific: 'modernization' thus served to Balkanize, socio-economi

cally, rather than to homogenize it hardened divisions between, for example, the

porto metropolis of Buenos Aires and the Argentine interior; between the declining

Brazilian north-east and the dynamic south; between a buoyant, free-labour

northern Mexico, a more torpid 'traditional' centre and a coercive, plantocratic

south. 27 And, second, because the primary sources, though
ample, are often

intractable: nineteenth-century historians, lacking the ordered bureaucratic archives of the late colony, are driven to resort to the rich but recalcitrant archives of courts, lawyers, landed families, foreign companies, state/provincial governments and municipal authorities.

Nevertheless, great strides have been made and, as usually occurs when case studies multiply, the old certitudes are called into question. The infant Latin American states did not, as I mentioned, exchange formal Spanish or Portuguese rule for informal British rule: foreign commercial penetration was slow and partial: even in the late nineteenth century, as penetration accelerated and export markets became the motor of development, patterns of regional diversity remained.

It was not just that some regions benefited from trade and investment while others suffered from neglect (this may seem a banal statement, but it begs some thorny theoretical questions). 28 These studies also show how the nature of capitalist development varied significantly from place to place. In some regions, the market displayed its presumed affinity with free-wage labour, sucking migrant workers from peasant communities locally, nationally, and, of course, internationally. 29 But elsewhere it either reinforced 'traditional' forms of
peonage or created new forms of coerced labour. In Cuba and Brazil, slavery flourished until the supply of slaves was cut off by British action in the second half of the nineteenth century (Bethell 1970; Murray 1980). In the old Indian heartlands of Middle and Andean America,

27 Katz 1974 offers a good resume of the Mexican case.

28 Extreme versions of dependency theory suggest that any integration into capitalist world markets produces underdevelopment, while development can be achieved only by shunning such integration whether deliberately, by means of radical policies of economic nationalism, or fortuitously, because of external market failure, as during the 1930s: see Frank 1969. A perceptive critique of (simplistic) dependency theory is provided by Halperin 1982.

29 A useful symposium is Duncan and Rutledge 1977. 737 landlords resorted to the coercion of Indian and mestizo populations to secure labour for their enterprises. Guatemala's coffee boom involved dragooned labour and a hardening of ethnic divisions (McCreery 1994; Smith 1990). In Mexico, a coercive, plantocratic south confronted a more liberal, free-labour north (the parallel with the United States may be pursued: the Mexican Revolution saw the victory of the north and the irruption of reformist northern carpetbaggers into the recalcitrant south; Knight 1986: II, 236-51). Yet in Brazil it was the
declining sugar-producing north, unable to retain its dwindling slave population,

which pioneered abolition, in the face of opposition from that great bastion of slavery and modernity Sao Paulo (Conrad 1972; Toplin 1972). Highly divergent in its regional and rural impact, export-orientated economic development (desarrollo hacia afuera) did tend consistently to promote major cities Buenos Aires, Sao Paulo, Rio de Janeiro, Mexico City where a rich elite and growing literate middle class were the chief beneficiaries of economic growth, of urban infrastructure and cultural florescence. 30

Regional and local studies, while particularly revealing of patterns of socio economic development, also shed light on political trends and events. In most Latin American countries export-led growth facilitated the rise of stronger, more solvent states; and, into the twentieth century, as the continent experienced a series of external shocks the First World War, the Depression, the Second World War so the role of the state increased. 31 States intervened increasingly in economic life: regulating, taxing, even expropriating. Especially during and after the 1930s, 'inward-orientated' development (desarrollo hacia adentro) came to prevail over the older export-led strategy (see Thorp 1984). Regional studies therefore pay great
attention to the impact of national and international events on the provinces, and
to the latter's interaction with centralizing forces, both political and economic. Brazil's
Old Republic (1889-1930) a polity based on a delicate balance of states' rights and
interests is a classic example (Love 1971; Wirth 1977; Levine 1978; Topik 1987).

So too, in a different way, is the Mexican Revolution. Despite the existence of
some new syntheses (Knight 1986; Hart 1987), the thrust of 'revolutionary studies',
broadly defined, has been toward regional and local history. If Mexico was a
sprawling, variegated country before the Revolution 'Many Mexicos' in
Simpson's (1941) phrase, now a mantra for Mexicanists so there were many
revolutions, in respect of both causes, course and outcome. 32 Zapatista Morelos
30 Morse 1958 pioneered big-city history, which was further developed by Scobie 1974 and Needell
1987. Holloway 1993 stresses the class dimension of urbanization; Scobie 1988 reminds us that
urbanization was not confined to metropolises.

31 On the growth of the state, see Whitehead 1994. Knight 1993: 39-42 makes the obvious but
sometimes overlooked point that the 'strength of the state' is a vague concept which may involve
different and even contradictory criteria: e.g. on the one hand, the state's capacity to mould society and,
on the other, its durability, longevity and capacity for self-reproduction over time.
32 Brading 1980, and Benjamin and Wasserman 1990 are useful collections. Note also Carr 1980 and
the articles by Linda Hall and Paul Vanderwood in Mexican Studies/Estudios Mexicanos 3.2 (summer 1987). 738
differed from Villista Chihuahua; Sonora, cradle of the triumphant Sonoran
dynasty of the 1920s, was different yet again. 33 For Yucatan, in the south-east, the
revolution came 'from without', carried by northern carpetbaggers who descended
on the peninsula in 1915 Ooseph 1982). A decade later, the great Catholic uprising
known as the Cristiada Mexico's Vendee represented a regional repudiation of
revolutionary anticlericalism and centralization in the centre-west states of Jalisco,
Guanajuato and Michoacan. 34
Yet even these broadbrush regional distinctions require further spatial disaggrega
tion. In Michoacan, for example, clerical/Cristero communities existed cheek by jowl
with revolutionary/anticlerical counterparts. Local politics often revolved around
such historic enmities, which were successively redefined according to the political
terminology of the day: liberal and conservative in the nineteenth century; revolution
ary and Cristero in the 1910s and 1920s. 35 Hence the importance of genuine
microhistoria the history of individual communities, painstakingly reconstructed on
the basis of local archives and oral accounts which, in
the Mexican case, has been
pioneered by Luis Gonzalez (1983). Though some antediluvian historians of the
Revolution still cling to the crude notion of an
undifferentiated popular insurgency
(Hart 1987), most now recognize the infinite local variety of revolution. In some
communities, the Revolution was eagerly espoused; some had revolution thrust upon
them; in Luis Gonzalez's community, San Jose de Gracia, 1910 was not the annus
mirabilis of the Revolution, but rather the year when Halley's Comet blazed in the
night sky and the village eccentric tried unsuccessfully to soar from the roof of his
house, flapping wings made of straw matting.

THREADS THROUGH THE LABYRINTH: THE COMMON PEOPLE

Such centrifugal historiography the historian's flight from the centre to the
provinces has its social or class counterpart. Apart from fleeing the centre,
historians have tried to plumb the neglected depths of the societies they study.

'Centre-out' historiography is therefore complemented by 'bottom-up' historiogra
phy, the historiography, we might say, of los de abajo, of 'groups such as women,
servants, children, peasants, vagrants, and criminals' (Taylor 1985: 119; cf. Guha
and Spivak 1985) Latin America's equivalent of India's 'subaltern studies' school.

33 Womack 1969 is a classic of interpretative narrative: it can be supplemented by Warman 1980,
which builds on oral accounts and locates the Zapatista rebellion within a wider chronological
framework and Brunk 1985, a good recent biography. By way of comparison note Katz 1980 and
Aguilar Camin 1980: 59-75, 92-123. Katz’s pioneering work on Villismo will soon culminate in a
major study of the northern caudillo and his movement.

34 Meyer 1976 is a major revisionist study, a translation and abridgement of the author’s three
volume La Cristiada (1973).

35 The broad debate between 'traditional' and 'revisionist'
interpretations of the Cristero revolt has
as we might have hoped and expected begun to take into
account such local variations: see Jrade 1985
and Purcell 1994, which forms part of a very promising research project on local politics in 'counter
revolutionary' western Mexico. 739

Such a trend is apparent throughout the five centuries of post-conquest history.

The viceroys and prelates of the colony have ceded ground
to the Indian peasant

(especially the hard-drinking, rock-throwing, protesting Indian peasant), to the city

mob and the urban artisan, to the petty official and the rabble-rousing cura, the
counterpart of medieval Europe’s 'hedge-priest'. 36 The colonial archives have, in

particular, facilitated enquiries into Latin America’s subalterns; and they have

enabled historians to go beyond purely political or socio-economic categories the

straitjackets which so often confine 'popular' subjects and
to penetrate the minds

and morals of the common people. We now know about their drinking and
carousing (Taylor 1979); their resort to sporadic violence, in both city and
countryside;37 their formulation of a 'syncretic' religion which embraced aspects of
both Catholicism and Pre-Columbian religion, the messianic and millenarian beliefs
which coursed like subterranean streams beneath the baroque edifice of Tridentine
Catholicism. 38 If rebellions and even early industrial strikes have naturally
captured historians' imagination, increasing attention has also been paid to daily
life, to the normal and unexceptional, or to forms of 'resistance' which were
slippery, discreet and anonymous rather than overt and violent. 39

Such an approach, involving careful research into largely illiterate, frequently
evasive, social groups is not easy, and places particular demands upon both
historians and archives (Taylor 1985: 155; Van Young 1990). The latter, necessarily
the product of literate 'superordinate' classes, admit the subaltern only at particular
moments: during or after rebellions and popular protests; at court trials or
church investigations of immorality or blasphemy. (Menocchio, Ginzburg's heretic
miller, now has his Latin American counterparts.)40 None of these situations
encourages transparent revelation they are not, in Habermas’s term, ‘ideal speech situations’. On the contrary, arraigned plebeians may have every incentive to dissimulate. Nevertheless, the colonial bureaucracy, both lay and clerical, generated ample material which may be sensitively dissected. The same, alas, is not true of the often unstable governments of the nineteenth century; hence, when it comes to ‘history-from-below’, especially history which seeks to go beyond simple socio-economic categories, the post-independence period presents particular problems. Again, wars and revolts may offer brief moments of revelation, moments when the

36 On the role of the curas, see Taylor 1985: 149-52.

37 Taylor 1979 deals with the countryside; on the city, see Cope 1994: ch. 7. Valuable symposia, which span the colonial and national periods, are: Katz 1988 and Stern 1987.


39 The turn away from overt revolt to quotidian resistance is, of course, a global shift, influenced, in particular, by James Scott’s south-east Asian anthropological study, Weapons of the Weak, Everyday Forms of Peasant Resistance (1985). A concrete example, relating to the plantation peons of Yucatan, is Joseph 1994: 145. Brazilian slave studies (see n. 43 below) have taken a similar turn.

40 Van Young 1989 offers a foretaste of a major
forthcoming study of popular insurgency in Mexico, 1810-21, in which questions of mentality, heterodox religion and political dissent are broached, with court records supplying much of the empirical and Freudian psychology some of the analytical tools. 'public transcript' of popular deference and caution is torn up and subaltern groups speak by their deeds as well as their words with greater clarity and forthright ness. Thus, Florencia Mallon infers a vigorous proto-patriotism among peasants of the central sierra of Peru at the time of the War of the Pacific (1879-81): far from languishing in anti-national parochialism, Huancayo's peasants resisted the Chilean invader with greater commitment and resolution than Peru's 'national bourgeoisie' who, according to some theoretical scenarios, were the designated carriers of early nationalism (see Mallon 1983; 1987; Bonilla 1987). Peasant patriotic consciousness, Mallon (1995) argues, was not peculiar to Peru indeed, one may even talk of an emergent school of peasant-nationalist studies, involving, for example, Mexico and Nicaragua (Grossman 1992). Nor was it confined simply to questions of national identity: rather, it exemplified a broader crise de conscience among nineteenth-century popular groups who, Mallon (1995) argues, mobilized, politicked and according to today's fashionable jargon developed a 'national-democratic discourse' which
blended universal appeals with particular, local concerns.

Mallon's bold claims, some might say, outrun her empirical data. But she rightly stresses two related points: first, that popular groups even during the murky nineteenth century entertained ideas, theories and projects, they displayed ingenuity, autonomy and creativity; they were neither the inert primitives of conservative historiography, nor E. P. Thompson (1978) has reminded us the prisoners of grand historical structures, as structural Marxism suggest. In this respect, peasants as well as other popular groups are no longer equated to Pavlov's dogs, but rather to Aristotle's political animals, possessed of ideas, goals and knowledge, including knowledge of the world beyond the village. The study of elite cultural history and elite proto-nationalism is an established and still productive field;42 now, however, themes once confined to elite studies have invaded 'popular' history, and 'the people' are credited with agile brains as well as empty bellies. Of course, popular culture and consciousness differ from their elite counterparts. The latter can to a degree be culled from printed sources; for largely illiterate communities the transmission of ideas and symbols requires alternative approaches. Thus, historians have recently looked at symbolic violence

In particular, Latin American historians have discerned a kind of historic compromise linking popular groups with nineteenth-century liberalism (especially patriotic liberalism). Liberalism was not simply an ideology of free-trading.

41 Scott 1990. Joseph and Nugent 1994 is an interesting attempt to marry Scott’s thesis to Mexican revolutionary history.

42 Lafaye 1976 helped initiate a course of enquiry into (elite) proto-nationalism which has been carried to fruition by Brading 1991. Shumway 1993 offers an interesting but contentious analysis of Argentina’s foundation myths. Hale 1989 is another good example of recent elite intellectual/cultural history. 741

mercantile elites. This may seem banal to students of European or United States history, for whom the notion of popular liberalism is no oxymoron. But Latin American historians and social scientists more generally have consistently seen liberalism as imposed, elitist and anti-popular (while Catholicism and conservatism, conversely, have often been seen as popular and organic) (Burns 1980; Tutino 1986). The latter perspective is certainly not all wrong. But recent attempts to
grapple with popular politics and belief in the nineteenth and early twentieth centuries clearly demonstrate the vigour of popular, even peasant, liberalism, the product less of calm ratiocination than of pressing historical events and conflicts: the Chilean invasion of Peru (1879), the French invasion of Mexico in the 1860s, the US invasion of Nicaragua and pursuit of Sandino in the 1920s. Tristan Platt (1987: 285-6) sees the Indian peasantry of the Bolivian highlands linked to the liberal state by means of a political and symbolic pact, whereby the payment of tribute implied the recognition of community autonomy. But, in all these cases, wide local variations prevailed. If peasants the greater part of the Latin American population were more plugged into national politics than was previously thought, it is perhaps not surprising that they like more literate, more studied, elite groups should display important divisions and contrasting allegiances: thus, the more we study popular culture, the more rich and variegated it appears, the less valid seem the old stereotypes of bland uniformity (Knight 1994).

The rescue of 'subaltern classes' from historiographical neglect from the 'enormous condescension of posterity' (Thompson 1968: 13) is not simply a matter of Indian peasants or of proto-patriots. Brazil, for example, witnessed a series of
messianic movements, some of remarkable scale and duration: the most celebrated,

Canudos, was ruthlessly and gratuitously? repressed by the army in 1897 (Levine 1992). Although messianism affected other Latin American societies, it flourished most vigorously in backwoods Brazil, for reasons which historians hotly debate (Pessar 1981; Queiroz 1985; Diacon 1992). Brazil was also a prolific source of another controversial and characteristically Latin American phenomenon: banditry. Hence, Eric Hobsbaum's notion of 'social banditry' the bandit as Robin Hood and banditry as a form of surrogate social protest has been extensively tested in the Latin American context (Joseph 1990; Slatta 1991; Singelmann 1991; Birkbeck 1991; Joseph 1991). But Brazilian and, to a lesser extent, Cuban historiography has, necessarily, focused on a third issue, no less relevant for comparative analysis: the peculiar institution of slavery its rise and fall, economic and political rationale, its inner workings. The suggestive but questionable broad brush studies of Gilberto Freyre have therefore given way to a range of specialist approaches: studies of slave economics, some of distinctly cliometric sophistication; of abolition, both domestic and international; and, most significantly in recent years, of slave protest, culture and daily life. 43 The collapse of Brazilian slavery, we now
know, derived not just from the

43 See the useful survey in Schwartz 1992. Varieties of slave resistance are analysed in Reis and Silva 1989. 742

British naval tourniquet, internal economic contradictions or high-minded middle class abolitionism; the slaves themselves played a part, especially during the final debacles of the last slavocrat bastion, Sao Paulo (Conrad 1972: ch. 16; Toplin 1972).

Cuban and Peruvian slaves, too, played an important part in their own incremental emancipation (Blanchard 1992; Scott 1985). But slave resistance, in Brazil particularly, had punctuated the nineteenth century, sometimes assuming the form of overt protest (Reis 1993), sometimes deploying the covert 'weapons of the weak'. As the spectrum of 'resistance' expands, so historians of slavery have now begun to recapture subaltern experiences which are less violent and eye-catching. Court archives reveal the lives of female slaves and domestic servants; we are learning more about the daily live of slaves and freedmen; and biographers, traditionally enamoured of white male elites, have begun sources permitting to cast their net more widely (Lauderdale Graham 1988; Silva 1993 is a rare biography). The 'faces in the crowd' once an indistinct and anonymous mass have begun to acquire individual features.

Slavery ended in the later nineteenth century, although
forms of coerced labour

peonage (debt-slavery) and contract labour survived and even flourished well

into the twentieth century. But as we enter the latter, it is the growing urban

working class who command increasing attention. Until quite recently this

attention was excessively coloured by grand theory and crude assumptions. If, as I

have suggested, casual students of Latin America regarded the peasantry as

marginal, illiterate and parochial, they often paid disproportionate attention to the

(small) urban working class, seeking though not usually finding a revolution

ary vanguard, and pondering questions of somewhat bastard European

origin. Was the labour movement revolutionary? If not, why not? Did the Latin

American state especially the 'populist' state of the mid-twentieth century, the

state of Peron, Cardenas or Vargas co-opt the workers, thus drawing the sting

of revolution? Did the workers sell their revolutionary birthright for a mess of

populist pottage?

These are loaded, though not wholly pointless, questions, and they are still being

An interesting, though inconclusive, debate on the character of debt-peonage has developed,

involving roughly revisionists who would stress the rural workers' relative freedom and bargaining

cut as against traditionalists who would tend to equate

45 'Bastard' in the sense that they were probably illegitimate questions even in the original European context. Thus, Latin American labour historians have wrestled with the 'problem' of their continent's 'non-revolutionary' working class, assuming that any decent, self-respecting working class ought to be 'revolutionary'; yet the European not to mention the North American experience hardly confirms that assumption. A similar bastard Eurocentrism is evident in the study of revolutions, where European exemplars (e.g. France, 1789) are assumed to be rapid, root-and-branch transformations, whereas their Latin American counterparts (e.g. Mexico, 1910) are feeble compromises; again, the European yardstick is hopelessly crooked. A third and final example: the syncretic 'pagan/Christian' religions of Latin America are sometimes taken to be unusual hybrids, compared to the orthodox and consistent Catholicism of Europe which was itself, of course, a heterodox 'pagan/Christian' hybrid. 743

debated, more so by political scientists than by historians. 46 However, they encouraged an excessively top-down, macropolitical approach to labour history.

Workers were seen as political clients (or occasionally subversives), not as social, economic and cultural actors. They were also seen, too often, as essentially reactive, easily manipulated by conniving populist
caudillos, particularly by

caudillos who supposedly capitalized on the clientelist
tendencies of first-generation

urban workers who had recently migrated from the
'traditional' countryside to the

'modern' city. Political in its focus, Latin American
labour history concentrated on

organized as opposed to non-organized labour, on men rather
than women,

and on major unions and confederations, especially those
linked to left-wing

political parties, rather than on informal groups and
relationships. The only good

worker was a red worker; the union hall and the
Internationale counted for more

than the tavern and the tango. Some analyses even some good
ones sank into an

alphabet soup of impersonal acronyms. Epochal

Now, just as peasants and slaves have received more
sympathetic and

empathetic attention, so, too, labour historians are
disaggregating the acronyms,

skirting the simplistic old questions (were the workers
revolutionary and, if not,

why not?) and descending from the sindicato office to the
shopfloor even, in some

cases, to the bar, poolroom and football field. The result
is not to quote

Trevelyan's old description of social history 'history
with the politics left out', but

history in which politics is located within a social,
economic and cultural context, in

which unorganized as well as organized workers figure,
women as well as men, artisans as well as industrial workers, and in which rather more subtle questions of protest and accommodation, 'resistance and integration' are debated. This shift is not entirely confined to the twentieth century. The colony had its working class, notably in the mines: Mexico's first major strike, the subject of a recent study, dates back to 1766. Historians have also begun to investigate the key role of artisans in nineteenth-century urban politics and society, notably in Peru and Colombia; they have uncovered traditions of organization, protest and culture which long antedated the rise of industry and the 'social question' of the 1900s (Gootenberg 1989; Sowell 1992; note also Thomson 1989). But, given the resistance of written sources to detailed investigation of working-class culture, it has been the recent post-1930 period which has benefited most from oral history, from the unpacking of bland acronyms, and from the development of the 'new labour history'. Such approaches 46 Collier and Collier 1991 is a compendious study of state-labour relations in the twentieth century; but, from a labour-history perspective, some of the questions look a bit dated. Roxborough 1994 is similarly useful, but conventional. 47 Germani 1965 proposed this thesis for Argentina; Murmis and Portantiero 1971 challenged it, pretty successfully.
48 Barnard 1992 is one of the best chapters in a good symposium, but it seems unable to escape from the dance of the acronyms (e.g. p. 75 'the PS set aside its animosity toward the PCCh in order to collaborate with the ADCh parties and the CTCh').


Oral history, of course, is no passe-partout. It presents particular problems. Subaltern spokespeople, like their elite counterparts, have to employ a useful Mexicanism their own rollo, their own often unconsciously prepared script. 51 They may also be understandably wary of talking to inquisitive gringos. Additional problems not to say risks may attend this form of research, and they go rather beyond surgical masks and pigeon-potting archivists. Peter Winn (1986: viii) was pulled in by the Chilean military and, after three days' interrogation, warned: 'we have no proof that you have committed a crime exactly ... but talking with our workers, interviewing union leaders, all this is very suspicious. We do not want
anyone talking to our workers'; after which he was summarily expelled from the country. Nevertheless, when the risks and problems can be surmounted, oral history, complementing archival work, can provide a fuller picture of the 'lives of labour', one that is neither one-dimensionally political nor excessively coloured by crude Eurocentric suppositions.

I have left till last the biggest victim of posterity's condescension: women. (What is more, my brief and belated reference will seem to some like abject tokenism.) The traditional history of Latin America was, by and large, history with the women left out. Political historians could of course, plead that women usually played a minor role in politics, especially if 'politics' meant 'high' politics the politics of presidents, generals, caciques and caudillos. But this was not an absolute rule and, during the twentieth century, as universal suffrage and mass parties developed, the political role of women expanded (Lavrin 1994). Much more important, however, has been the shift in historiographical emphasis, as a result of which historians have begun to investigate women in colonial nunneries, in nineteenth-century metropole, in the coffee fields of Sao Paulo or in the ranks of Mexican revolutionary armies. If, initially, articulate feminist minorities attracted attention, the latter has
now switched to more numerous and socially significant? groups, hitherto the

victims of historiographical neglect: the prostitutes of Buenos Aires, the women

schoolteachers who sought to bring 'socialism' to the macho Mexican countryside in

50 A more traditional broad, structural and comparative approach is evident in Bergquist 1986.


51 This is even more true of elite scripts, which can reduce though not negate the usefulness of elite oral history: e.g. Wilkie and de Wilkie 1969.

52 Even here there are exceptions, most obviously Eva Peron; studies of Evita, however, are notable more for their quantity than their quality.

53 Lavrin 1986; Graham 1988; Stolcke 1988; Salas 1990, although the latter is rather stronger on myth than history. 745

the 1930s. 54 Furthermore, as these examples suggest, research into the role of women involves a broader enquiry into issues of gender, affecting men no less than women. In a continent and culture where witness the stereotype of the Latin American macho notions of patriarchy have been unusually strong, historians have set about unravelling 'gendered' structures of work and warfare, nationalism

The new labour history like the 'new peasant history' and the 'new local history' necessarily tends to be finely focused. Pioneering investigation cannot be done with a wide-angle lens. Occasionally, the fine focus leads to a certain myopia. The historian of a community becomes immersed in its every detail; the meticulous quest in the archives becomes an obsessive end in itself; the local historian even acquires a kind of petty local chauvinism a means, perhaps, to warn off interlopers from his/her chosen terrain. As a result we may lose not only the wood for the trees, but even the trees for the twigs. Fortunately, the better and more broad-minded practitioners keep an eye on the big picture. While remaining faithful to their case studies, they try to link them to grander debates they may, indeed, initiate grand debates. One the putative peasant patriotism of Florencia Mallon has been mentioned. Steve Stem (1988a) has drawn on his pioneering research on colonial Peruvian peasants to contest the sublime simplicities of Wallerstein's world-system theory. Eric Van Young (1990 and 1989) links his work on peasant insurgency to broader comparative questions and brings to bear
theoretical insights as diverse as psychoanalysis and geographical space theory:

Freud and von Thiinen connect in the halls of Clio. In the field of modem labour history, case studies have been located within wider debates about state-civil society relations; and, if these involve reprises of older questions (e.g. did the state co-opt the workers?), at least the questions are posed with greater sophistication and answered with greater empirical evidence than in the past. Of course, debates of this scope are rarely resolved: the character of the Mexican Revolution which in tum hinges a good deal on the nature of Mexican peasant life and politics remains a polemical issue; recent studies of Brazilian labour have generated vigorous debate concerning working-class attitudes towards government and the respective roles of class and gender in worker mobilization (Wolfe 1991a, 1991b; French 1991).

This is as it should be. Historiography does not involve a gradual, peaceful and consensual process of frontier expansion. Like the real frontier, the historiographic is jagged, irregular and sometimes violent. (There is also, we should note, a massive 'internal' frontier: i.e. large chunks of ignorance within areas we once thought securely settled, especially with regard to the period c.1820-70.) The

54 Guy 1989; Vaughan 1990 and 1994 (which appears in an excellent new symposium). A good
example of the more traditional political history, which pioneered the study of women, is Hanner 1980. 746

historians' advance is governed not only by the logic of the archives the onward

march of time, 55 the slow process of archival rescue and reorganization but also by

the prevalent questions of the day. As I have stressed, provincial, local and popular

history has been dramatically developed, partly in response to changing historio

graphical fashions that extend far beyond Latin America. Latin America has also

shared to a lesser degree in the more sophisticated 'cliometric' economic

history and in the cross-fertilization which has occurred in the social sciences more

generally. One obvious and very positive example is the fusion of history and

anthropology, brought about, on the one hand, by the historian's sharp focus on

small communities and their inner cultural and symbolic life, and, on the other, by

the anthropologist's departure from a static ('synchronic') structural functionalism

and growing interest in both historical ('diachronic') analysis and non-Indian

societies. Thus, anthropological concepts such as the 'closed corporate peasant

community' are now earnestly debated by historians of the Mexican Revolution

(here, as elsewhere, the powerful and generally beneficient influence of Eric Wolf is

evident);56 and studies are written be they monographs or
broader analyses in
which the 'history' cannot be disentangled from the
'anthropology' and, indeed, the
dual disciplinary inspiration is acknowledged: Paul
Friedrich's (1977; 1986)
arresting analysis of the revolutionary pueblo of Naranja;
Daniel Nugent's (1993)
comparable study of Namiquipa, in the Villista north; and
Antonio Garda de Leon's
(1985) monumental history of colonial and modern Chiapas,
to which recent events
have given an additional relevance.
The anthropological turn is matched by the switch to
semiotics, the postmod
ern? concern for decyphering texts and deconstructing
discourse. I speak of this
with both caution and ignorance. In one sense, historians
have always deconstructed
discourse (whether they did it well or badly is another
matter). Some, it is true,
may have placed an excessively positivistic faith in the
dicta of documents; but it is
nothing new to warn students about the need to
'interrogate' documents and to
display a due scepticism as regards their writers'
motives. E. H. Carr (1964: 22-7)
was, in this respect, a semiotician avant fa lettre. However, this is not to say that
historians work with nothing more than free-floating texts
which are capable of
infinitely variable interpretation, and behind which
'historical reality' remains
utterly elusive. The recognition of textual bias and
autonomy is compatible with a cautious commitment to an ‘objective’ historiography (meaning by that a historiography which strives for as close an approximation to ‘reality’ as can be achieved).

55 The criteria which determine when and which official documents are lodged in accessible archives vary from country to country, institution to institution and, to my knowledge, there is no comprehensive guide. Britain’s ‘Thirty Year Rule’ is unusually precise; in Latin America the rules vary, or are indeterminate. However, it usually takes at least a generation for documents to find their way to the archives and many for reasons relating to security, inefficiency and lack of resources never make it. At local and municipal level the attrition rate is probably much greater.

56 Wolf 1957; Vanderwood 1990. Note also the recent symposium edited by Schneider and Repp 1995 and Wolf 1982, which departed from the premise (p. ix) that ‘anthropology needed to discover history’. And this recognition is nothing new: old-timers like Max Weber (1970) and Jack Hexter (1971) said as much decades ago. Thus, just as M. Jourdain spoke prose for decades without realizing it, so many historians (I do not say all) have practised intelligent and sceptical discursive deconstruction if that is what we wish to call it without falling into the black hole of utter relativism.

Delivered in small doses, deconstructionism may sensitize
historians to textual nuances. An overdose, however, can lead to a surreal detachment from reality, as
texts sometimes remarkably few texts are subjected to merciless torture and,
like the witches of Salem, yield up the 'subtexts' which their interrogators want to
hear, morbid imagination triumphing over solid good sense. In this respect,
deconstructionism and the postmodernist turn of which it forms part revels in
multiple meanings, ambiguities and intellectual fancies: again, a useful antidote
were it needed to bullish positivism, but also a solvent of grand theory, or even
middle-range hypotheses. 57 Such an approach has its drawbacks, the defects of its
sceptical virtues. Bullish positivism and fey postmodernism may be poles apart, but
they share a common hostility to general theories and grand paradigms which, to
both schools, appear as delusive engines of intellectual authoritarianism. And it is
here that the historiography of Latin America again following global trends is
currently deficient.

Almost all the recent historiographical trends, mentioned above, conspire to
produce narrow, usually highly expert, but sometimes myopic history (Taylor 1985:
120). Rigorous professionalism, intimate acquaintance with the archive (s), immersion
in the text, even an admixture of anthropology all tend to
encourage narrow focus

and to deter broad comparison. The hypotheses generated by such work and

historiography depends on hypotheses, whether explicit or implicit 58 are liable to

be low-level: they are limited to a narrow range, spatially and temporally. They tell us

about historical processes in this place or that period, but rarely establish links across

space and time, and, if they invoke 'grand theory' the 'organizing concepts'

whereby the minute clutter of history can be usefully ordered they do so either

cursorily or dismissively.59 Thus, we are lead to believe, the grand paradigms which

57 By positivism I mean that approach to history which believes that historical enquiry can

approximate the rigour, precision and certitude of the natural sciences (for example, by means of

cliometrics sophisticated economic and statistical methods). Needless to say, this begs the question of

the rigour, precision and certitude of the natural sciences.

58 Hypotheses, concepts and assumptions about causality underlie even the most basic narrative

statement. A random example: 'The failure of the magicians to unsettle the Spaniards of course

disturbed Tenochtitlan' (Thomas 1993: 205, referring to Cortes's arrival in Mexico in 1519). This

simple statement, itself a hypothesis, raises questions about 'magicians', 'Spaniards' and 'Tenochtitlan':

collective nouns which, however superficially self-explanatory, in fact embody layers of meaning which

in turn depending on how they are understood determine the
meaning of the sentence.

59 A common pattern is for a work of history to invoke grand theory Marxism, modernization or dependency theory at the outset, and perhaps again in conclusion, while maintaining a rigorously empirical and atheoretical approach through the long middle. Theory and data do not engage; hence the theory cannot help organize the data, nor the data qualify the theory. 748

once stalked the savannahs of Latin American history modernization theory, structural Marxism, dependency have lumbered off to die; their bleached bones are to be found in dog-eared texts of the 1960s or in dismissive footnotes of the 1990s. Like all extinctions, this had its positive side: lumbering paradigms which could not withstand predatory criticism deserved to die. Thus, simplistic notions of national character, Hispanic-Catholic backwardness and racial stereotypes have been justifiably killed off (though note Dealy 1992). No historian worth his or her salt interprets Brazilian society in terms of Gilberto Freyre's tropical lasciviousness. The simple 'distorting dichotomies' into which historians once shoehorned a complex historical reality have been severely qualified, if not altogether discarded. 60 Few historians would now regard modernization theory as a passe-partout to historical understanding (although, I shall note, modernization theory has made a regrettable
comeback, thinly disguised, in today’s 'neo-liberal' discourse).

On the other hand, some extinctions were arguably premature. Dependency theory, which posited a Latin America detrimentally conditioned by its subjection to successive imperialist metropolises Spain, Britain, the United States was certainly simplistic and sometimes plain wrong. It exaggerated external stimuli, created simple dichotomies (metropolis/satellite), and depicted an almost timeless and undifferentiated capitalism (Frank 1969). But, despite severe disparagement (Platt 1980; and, by way of reply, Stern and Stein 1980); it retains some limited analytical utility. There are times and places such as Porfian Yucatan for which 'dependency', suitably interpreted, is usefully applicable Ooseph 1982: 41-65, 299). In what is probably the best general overview of post-independence Latin America, Tulio Halperin (1993: chs 4, 5; note also Taylor 1985: 123-4, 132) sees the period 1850-1930 as one which witnessed the emergence and maturity of a 'neocolonial order'.

Structural Marxism, which chose to see Latin American history in terms of unfolding modes of production,61 has also been eclipsed by a widespread and often unthinking dismissal of Marxism as a system of intellectual enquiry. (I do
not refer, of course, to Marxism, or communism, as a system of government, which is a different matter.) By dismissing or simply neglecting grand theory of this kind, historians tend to encourage an excessive fragmentation of their discipline, while leaving the difficult, but important, task of high-level hypothesizing to those sociologists, political scientists, journalists, instant pundits and self-appointed gurus whose indifferent grasp of history seems to encourage, rather than to deter, stratospheric generalizations (on Wallerstein, see Stern 1980b (reply to Wallerstein 1988); cf. Fukuyama 1992: 21-2,103-6). Even at lower levels of generalization where, if you like, 'middle-range hypotheses' enter in historians have probably too readily abdicated the field, too generously ceded it to non-historians. It has

60 The phrase is Taylor’s (1985: 117; and, for a useful set of examples, p. 169).

61 Examples would include Semo 1993 and Cardoso 1980. Mallon 1983 combines a fairly traditional structural Marxist framework with rich empirical data a somewhat unusual combination which also manages to avoid the intellectual schizophrenia mentioned in n. 59 above. 749 fallen to the Colliers political scientists to attempt the most ambitious recent synthesis of Latin American labour history.62 The application of Barrington Moore to Latin America and the interesting comparison with Balkan Europe which
Moore's scheme suggests has been undertaken, rather well, as it happens, by a sociologist, not a historian (Mouzelis 1986).

Thus we come to the recent period of political 'democratization' and neo-liberal economics. Historians have played no more than a bit-part in the booming industry of 'democracy' studies which has flourished in recent years. As a result, political scientists and others have played fast and loose with history, with relative impunity. They have hailed a brave new democratic world which is far from new and may not be so brave. Historians, in contrast, would stress recurrent cycles, rather than outright novelty, underlying structures of power rather than superficial transformations, local variations rather than bland national aggregations. They would query, perhaps, how 'new' the 'new social movements' of the 1980s really were, and whether the whipping boy of 'economic populism' really bears any relation to the classic 'populist' regimes of the 1930s. They would, in other words, subject today's fashionable assumptions to a sceptical and informed historical critique.

Unfortunately, they have rarely done so. The imperatives of the historical profession archival expertise, narrow research, specialist publication have militated against a broad encounter with current trends in social enquiry and policy.
making. As a result, the major transformation which Latin America has undergone in the last fifteen years of economic crisis, structural adjustment, neo-liberal experiment, and halting democratization has proceeded in an intellectual ambience of mild amnesia. Freewheeling social scientists economists first and political scientists second have dispensed advice and analysis; but, with a few notable exceptions, theirs has been a short-term, ahistorical, 'immediatist' perspective. The same is true of many of today's ruling technocrats in Latin America, who believe that the timeless logic of the market must transcend messy historical traditions, and that, where necessary, historiography can be painlessly rewritten to suit that logic. 65 They are not the first generation of technocrats to subscribe to such

62 Collier and Collier 1991. It is interesting to note that the most recent volumes in the monumental Cambridge History of Latin America, vol. VI, parts 1 and 2, entitled Latin America since 1930: Economy, Society and Politics, which deal with thematic issues (the state, organized labour, the Left, the military), are largely written by non-historians, who outnumber the contributing historians by four to one; one relevant factor is no doubt historians' diffidence about broad, contemporary syntheses; one consequence is a marked emphasis on formal organizations, salient events and political leadership (what might be called cupular history).
63 Linz 1978; O'Donnell 1986; Higley and Gunther 1992; Mainwaring et al. 1992 are among the major studies.

64 These happen to be two of my own bugbears: Knight 1990 and 1993a.

65 Hence Ernesto Zedillo, now President of Mexico, tried, as Minister of Education, to have the country’s school textbooks rewritten to accord more with today’s neo-liberal technocratic ethos: traditional (nationalist) heroes were excised or written down; the 1910 Revolution was deflated; the pre-revolutionary regime of Porfirio Diaz itself loosely liberal and technocratic was rehabilitated. Popular and pedagogic opinion was offended; in the ensuing brouhaha the new texts had to be withdrawn. 750 beliefs, although their collective amnesia prevents them from being aware that they are trading in second-hand goods. Hence the collective bewilderment which ensues when history rears its ugly head: when, now as in the past, financial crisis overwhelms the best-laid plans of economic wizards, or when popular revolt, redolent with historical allusion, rises up in the Lacandon forest of Chiapas to challenge the overweening presumptions of First World ‘modernity’. 66

My conclusion to this rapid and subjective resume is therefore ambivalent. In terms of its volume, sophistication and intrinsic interest, the historiography of Latin America is alive and kicking. New themes have been broached (gender, subaltern
studies) and new approaches have been tried (cliometrics, deconstructionism, 'anthrohistory', 'microhistory'). The 'condescension of posterity' has been corrected, though not eliminated; and the resulting 'democratization' of history has involved 'finding seats at the banquet of history for great gatherings of forgotten people' (Taylor 1985: 121). Historians of Latin America have been alive to historiographical trends and fashions elsewhere, particularly in Europe and the United States. (Needless to say, this has not been reciprocated: historiography is an industry in which core-periphery relations correspond to the good old, crude, dependency model.) But growing volume and expertise have often been accompanied by excessive focus and introspection; historians have tended to turn in on themselves, to pride themselves on their narrow professionalism, thus to cede the broader field of comment and comparison to non-historian social scientists. Historiography has therefore suffered from a certain intellectual constriction; and, perhaps more important, the social sciences have cut loose from their historical anchors. Yet, at a time of social, political and economic flux such as this, historical anchors are crucial: not to inhibit advance, but to ride out storms without coming out.

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Zapatista Army of National Liberation draws heavily on historical memory, invoking the revolutionary
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People know what they do; they frequently know why they do what they do; but what they don’t know is what they do does. (Michael Foucault, quoted in Dreyfus and Rabinow 1982: 187)


Disciplinarity is a curse to some, a blessing (and a crutch) to others. Defining and defending disciplinary boundaries is an exercise in identity politics. Travelling along boundaries, crossing boundaries, operating on the fringe, in the shadow of canons:

seeing from the outside in, the inside out: these are exercises in identity dynamics.

This chapter is not about a lot of things, of which three are perhaps most important. First, it is not about disciplines. That is, it is not an excursion into the relationship between history and anthropology, in the sense of an exercise in
epistemology. Issues arising in the philosophy of history are treated elsewhere in this volume (Chapter 30). As for practice, that is another matter. The whole issue of discipline(s) and disciplinarity is covered elsewhere, to which the reader is referred (Messer-Davidow et al. 1993). Second, the chapter does not list names qua names, (historian and anthropologist) and one practice (history and anthropology) over another. That has already been done on many occasions and, in the opinion of this author, has been certainly for the practice of history a distinctly unhelpful exercise. Finally, it is not prescriptive. It is not a guide as to how to do history with Eric Hirsch, Ludmilla Jordanova and Steve Smith read this chapter and made very many valuable suggestions and criticisms. To each of them I would like to express my thanks for their time and friendship. Michael Bentley gave me important pointers at the start and helped me bring the chapter into shape I thank him for that.

2 See the remarks by Ludmilla Jordanova in her review of Burke's book, 'Resisting reflexivity' History of the Human Sciences 5 (1992): 59-67. 783 anthropology. What the chapter is about and how it gets that message across is intimately related to this author's own standpoint. This may be summed up in the following short assertive statements.

1 Those historians who have gone fishing in
anthropological waters have been timid, reserved and pre-selective. They have made the cardinal sin of following an anthropologist, not anthropologists and certainly not anthropology.

2 Anthropology is a gateway. Anthropology and the writings of anthropologists offer boundary-crossers an enormous theoretical/abstract/problematized literature that opens onto other literatures. These literatures may appear bounded and mutually exclusive but that is an image of disciplinarity, no more. From the viewpoint of practice, however, the literatures are hybrid creations and they are as much part of the practice as are the practitioners.

3 Neither discipline stands/has stood still. In practice they are two-way avenues. Some historians worry about anthropology; some anthropologists worry about history. In both cases, knowledges are contingent.

The chapter is divided into three sections. The first looks at how some historians and anthropologists have attempted to make sense of each other's practices. The focus will be on an exchange in the Journal of Interdisciplinary History published in 1981 but the discussion will move both backwards and forwards in time from this date. The second section will explore the disciplinary hybrids, cultural history, historical anthropology, ethnographic history and others. The final section
concentrates on the issue of the right tools for the job. The chapter concludes with a
discussion of disciplinary intersections as evidenced by what has been called science,
technology and medicine studies. Some of the most perceptive insights in this area
of study offer ideas simultaneously to both history and anthropology.

BOUNDARY ISSUES AND THE RAPPROCHEMENT LITERATURE

Bernard Cohn (1980: 216) stated it clearly and unambiguously: 'history can become
more historical in becoming more anthropological, ... anthropology can become
more anthropological in becoming more historical'. This statement was made after a
playful but caustic rendering of nearly a quarter of a century of historical and
anthropological practice and definition, leading to what the author described as
'epistemological anarchy'. To save the situation, Cohn recommended a 'conjuncture
between history and anthropology' in the shape of an anthropological history.

The central problem for this reassertive hybrid would be the process of cultural
construction, constitution and representation. Moreover, it would restore the
common subject matter otherness while dispensing with the dualism of time
and space, the former assigned to history and the latter to anthropology. The choice
of place and time in which anthropological history could be studied would be
neither arbitrary nor a product of an overarching or metanarrative. Rather, the specification of place and time would emerge in the dialogue between question and subject. The units of study Cohn envisioned for the project of anthropological history included power, authority, exchange, systems of social classification, rituals, and the construction of time and space.

One interesting call, and one which I shall refer to later in this chapter, was for anthropological history to open up its black boxes, one of which contains the concept of an event. Cohn argued that a specific analytical field is not necessarily defined by what may be perceived as temporal unity. As examples, Cohn referred the reader to labels such as 'Age of Enlightenment' and 'Age of Revolution'. Events, he argued, are not of equal value. Some have structural consequences not apparent within the event itself. Only with hindsight can events be ranked in some scale of importance, for example. Some actions which, at the time they were taken, had specific contexts and explanations, could be thoroughly transformed from what Cohn termed a dependent to an independent variable. Though he did not provide an example of this phenomenon, the 'killing' of Captain Cook by the Hawaiians is a case in point: what was being killed in this 'killing' was different for the Hawaiians.
who performed it and the English who witnessed it the latter responded by setting in motion events that finally transformed the Hawaiian world (Sahlins 1985).

Cohn’s article appeared without citations. In the following year, he was invited to comment on the relationship between history and anthropology in a symposium on history in the 1980s (Cohn 1981). This time, Cohn provided a full scholarly arm many historians and anthropologists who had, over time, crossed disciplinary boundaries. This article did not, unsurprisingly given its timing, offer any substantive theoretical additions to the earlier article. It did, however, cast a wide, non-Eurocentric net, as far as the writing of history is concerned.

Readers of the 1981 issue of the Journal of Interdisciplinary History who had just finished reading Cohn’s contribution might have been forgiven for thinking that furthering relations between history and anthropology, between historians and anthropologists, was a matter of finding common and fertile epistemological ground.

But nothing could be further from the truth. Turn the page and there is a contribution to the debate by John Adams (1981).

Adams set his sights on how historians, particularly the ‘new breed’ of social historians of colonial America, had engaged with anthropology and anthropologists.
over the past decade and more, and his verdict was both disturbing and damning.

He chastized historians for dabbling with anthropology. He attacked them for misunderstanding the issue by thinking that anthropologists studied exoticism and that historians should do so as well. He almost seems to have lost patience when he reproached them for searching out social equivalents in their patch of the Balinese cockfight, Kula ring or potlatch. 3

All was not destructive criticism in Adams's account, however. A lifeline was thrown out at the end of the article. In trying to understand the nature of social relations, he advised historians to attend to the 'mundane, with its pettiness and


dissensions, as well as its cooperativeness', rather than dwelling on the exotic and, thereby, creating fantasies of Golden Ages (Adams 1981: 265). As for how historians should engage with anthropology, his advice was at once surprisingly conciliatory and possibly unattainable. 'If historians borrow from anthropology', he writes, 'it should be with the intention of developing the concepts borrowed and of making, in return, a contribution to anthropology [my italics], (ibid.). His parting shot
should also be recorded here, for it is an insight that intersects historians' practices
generally. 'History', Adams warns,
must become more reflexive about its goals and about the means it uses to realize its ends.

To anthropologists the lack of interest in theory among historians still seems great. But
borrowing concepts from another discipline does not hold out much promise either if the concepts are simply misused in a thoughtless way. (ibid.)

One can imagine a whole set of reactions to Adams's comments, from 'Right on!' to 'Mind your own business!'. Ironically, as an anthropologist, Adams did not let on that the theoretical turmoil in his own discipline, as heretofore seemingly invincible theoretical standpoints, networks and institutional positionings, was being prodded, tested and assaulted.

Next in turn in the section on Anthropology and History in the 1980s was a key witness to and practitioner of history with anthropology. While not directly responding to Adams, Natalie Davis outlined the historian's engagement with anthropology from the former's point of view. The anthropological literature, she implied, liberates historians from a metanarrative that had ranked past phenomena by importance, marginalizing some events (and types of events) and privileging others (Davis 1981). How this metanarrative was constructed and how events were
valorized she didn't say. What is important is that anthropology is useful, according to Davis, because of four features that the discipline brings to bear on its objects of study: 'close observation of living processes of social interaction; interesting ways of interpreting symbolic behavior; suggestions about how the parts of a social system fit together; and material from cultures very different from those which historians are used to studying' (ibid. 267).

For Davis, the key word is culture, about how it is constructed, how it is reproduced and how it is represented. To meet these objectives, she implored historians to 'slice into a culture through texts, pictures and artifacts' (ibid. 272).

Davis did not deal with the issue of exoticism as raised by Adams; instead she made out a case for using ethnographic texts as comparative material. In studying oral cultures in the European past, for example, she suggested that an understanding of present-day African language practices might help reveal to the historian 'how things were said in sixteenth-century villages' (ibid.).

The prescriptions appear to be delivered and, presumably, self-administered effortlessly. Before bringing her article to an end, Davis alerted historians to some possible pitfalls in drawing upon anthropology. The key comment is that
anthropology is not ... some kind of higher vision of social reality to which historians should convert, but a sister discipline with increasingly close ties to our own [my italics] ....

We should not only be borrowing from them with discernment, we should also be prepared to offer advice about their own work and about anthropological theory. (ibid. 274)

Right here lies the problem. Davis's article is richly referenced, taking in a wide range of historical and anthropological writings: not surprisingly, given her keen interest in the nature of culture, the key anthropological guides to expanding historical awareness for her are Clifford Geertz, Victor Turner, Roy Rappaport and Mary Douglas. 4 She asks historians to read these works, understand the arguments and evidence in ethnographies, make themselves aware of different schools of anthropological interpretation, integrate them into the historian's 'own vision of social organization': but not to 'import all the special reservations that anthropologists have about each other's work or all their infighting' (Davis 1981: 273).

Lying behind this seemingly commonsensical in other words, no longer controversial statement, is a deep and damaging contradiction. How can historians advise anthropologists about their theory without getting into the mess with them? Yes, anthropology is concerned with and anthropologists write about
culture, but that is a bit like saying that Hamlet is a
play about a Danish prince.

Before slicing into a culture, historians need to be able
to find their way into and

around cultures, to be able to pose cultural questions, to
understand how culture

constitutes itself and that of course includes its
objects, texts, etc. What Davis
calls infighting may, at one level, be nothing more than
departmental politics, but to

turn it into nothing more than this is insulting. Rather, historians need to

understand that the infighting is a reflection of
reflexivity about profound issues of

interpretation of culture and its meaning.
The choice by Davis and for that matter many historians
writing about culture

in history at the time of Geertz, Turner, etc. is not
surprising because these

anthropologists and their works came to prominence
precisely as a consequence of

special reservations and infighting. Their canonization and
that of their writings as

revered texts must be understood as the end of a process of
disciplinary argument.

Though to an outsider it may have the characteristics of
closure, closure itself is

how the victors to an argument declare they have won the
arguments, though, are

not closed. Not recognizing this has very serious
consequences for a broad

engagement of history and anthropology. Davis’s advice
that historians should stay
away from anthropologists was wrong. 

How many historians hold this view is, of course, unknown and beside the point, 

though Davis could not possibly be alone. One historian, in print at least, took issue 

with Davis. Darrett Rutman was invited to comment on the relationship between 

history and anthropology in a short collective exegesis on dialogue between the two 

disciplines (Rutman 1986). The piece is quite remarkable disturbing in several 
senses in that it was written in the form of an extended metaphor, in which 

4 Davis refers to several works by these authors including Geertz 1973; Turner 1967; Rappaport 1979; and Douglas 1966. 787 

Rutman recounts Clio's practices as a lover, both enticing and being seduced by 

disciplinary suitors, in which 'anthropology is only the latest object of her ardent' 

(ibid. 121). The sexual explicitness of the text, of the gender assumptions behind 

the actions are quite revealing. 5 They also resonate in quite interesting ways with 

those E. P. Thompson (1972) used earlier in his review of Keith Thomas’s Religion 

and the Decline of Magic and Alan Macfarlane's The Family Life of Ralph Josselin, a 

Seventeenth-century Clergyman, two books that were prominent in their explicit use 

of anthropological perspectives. 

It is very difficult to read Rutman's article without
becoming engaged in the
gender politics of his choice of metaphor. Nevertheless,
through his dubious
attributions of character to Clio that she is a shallow
figure, that she flirts widely
and that the satisfaction of her sexual activities are
primarily for herself he makes
the important argument that, far from avoiding the
reservations and infighting of
anthropologists, historians wishing to engage with
anthropology need to begin right
at this point, with the fundamentals (Rutman 1986: 122).
Conjuring up his
metaphor of choice, though now treating the sexuality of
the suitor ambiguously,
Rutman concludes by urging historians 'to enter
anthropology widely and deeply,
allowing its fundamentals to affect their doing of
history' (ibid. 122-3).

At the start of this chapter, I stated that I would not be
listing names qua
names. I would like to offer two reasons for this. First,
命名 particular
anthropologists has the effect of reinforcing privilege in
one place where such
privilege is likely to be contested in another place.
Second, naming is not quite the
same as engaging and on that I have already made some
comment. Take, for
example, the distinct infatuation that some historians
have had for some
anthropologists. Of course, I am referring here to
Clifford Geertz. I do not intend
to analyse why Geertz has had the impact he has had on some historians: others

have done this admirably (see e.g. Walters 1980 and Kertzer 1984). I just want to

note that one of the reasons for Geertz’s reception and elevation to a 'paradigm for

the entire discipline' for historians had to do precisely with historians' reservations

about each other's works; with infighting over what some believed was the

encroachment into history of the positivistic social sciences, especially economics

and quantitative analysis; and with a search for a saviour in the shape of a towering

figure from another discipline. 6

That Geertz is an 'obligatory point of passage' (Latour 1988: 43-9) for historians

is still being reinforced in a recent collection of essays by Peter Burke (1991a). One

of the reviewers of this collection makes this a central concern of her essay when she

writes: 'a Martian, sent to investigate the state of historical knowledge on the late

20th-century Earth, might be forgiven for thinking that anthropology was a

discipline with only one practitioner Clifford Geertz!' (Oordanova 1992: 62).

5 See Martin 1991 for a discussion of the constitutive functions of metaphors.

6 See the comments in Walters 1980, Kertzer 1984 and Kertzer 1986. Further insights into the

Geertzian influence can be gained from Darnton 1990, especially chapters 10 and IS; Clark 1983. For
anxiety over the social sciences, see Stone 1977 and 1979.

Geertz is not, of course, the first or last obligatory point of passage. He has,

however, made his own analysis of the relationship between the two disciplines

(Geertz 1990; see also Rosaldo 1990).

The position of Geertz takes us back to the issue of what was going on and had
gone on in anthropology when Cohn, Adams and Davis contributed to the debate

about anthropology and history in the 1980s. The relational issue is picked up by

the anthropologist Paul Rabinow. He writes:

There is a curious time lag as concepts move across disciplinary boundaries. The moment

when the historical profession is discovering cultural anthropology in the (unrepresenta
tive) person of Clifford Geertz is just the moment when Geertz is being questioned in

anthropology. (Rabinow 1986: 241-2)

It is not, however, just a one-way street. An anthropologist on history:

Just as we were turning to history for guidance, at the moment when our early paradigm

matic foundations were crumbling, many historians began to repay the compliment. Just

as we were inclined to see history as 'good' as if time might cure everything they

seemed to see ethnography as a panacea. This should have warned us that they were in as

much theoretical trouble as we were ourselves. (Comaroff and Comaroff 1992: 17)
A sense of what this statement means can be gained from an essay by Sherry Ortner (1984) surveying the debates within anthropology from the 1960s to the early 1980s. It is a crucial paper for historians wishing to engage with anthropology.

Ortner takes the reader on a voyage beginning with symbolic anthropology in which Geertz is located temporally and theoretically then to cultural ecology, structuralism, structural Marxism, political economy and finally to practice theory, drawing specifically on Pierre Bourdieu’s (1978) key work.

Ortner, to be sure, provides historians with a rich landscape of theoretical developments within anthropology together with a check-list of required readings.

For that alone, the essay is critical. But it also has a more subtle offering.

Specifically, and not accidentally, she turns to the question of history and its meaning for anthropologists in the final pages of her review. Reference is made to the historic turn in anthropology, or, as she terms it, the ‘move to diachrony’, evident certainly in the early 1980s, and associated at the time principally with Marshall Sahlins, Renato Rosaldo, Anthony Wallace and John Comaroff. While she applauds the historical turn, Ortner is careful to argue that the fundamental issues in anthropology thrown up by the theoretical insights of the previous decade
cannot be pursued simply by bolting on history-by which
she means a temporal

7 Keith Thomas, in an earlier discussion of history and
anthropology, implicitly placed Evans

Pritchard (and by implication British social anthropology)
in this position see Thomas 1963. See also

the comments in H. Geertz 1975.

8 Apart from Sahlins 1981, see Sahlins 1985, especially
chapters 2 and 5; Rosaldo 1980; Wallace 1996;

Comaroff 1982. For a survey of history and anthropology
from an anthropologist’s point of view see

Faubion 1993. Some earlier comments on these disciplines
can be found in Schapera 1962 and Smith 1962. 789

process which happens to people. Placing practice at the
centre of anthropological

corresponds, she argues, will also rebound on history, in the
sense of thinking of

history as being made through practice. 'A practice
approach’, she states,

attempts to see this making, whether in the past or
present, whether in the creation of

novelty or in the reproduction of the same old thing.
Rather than fetishizing history, a

practice approach offers, or at least promises, a model
that implicitly unifies both

historical and anthropological models. 9

What is so important about Ortner's statement is that it
offers a way forward

simultaneously for history and anthropology I will return
to this point later in the

chapter. Before leaving this section, it would be well to
draw attention to two
historians who have attempted to travel across much of the anthropological terrain covered by Sherry Ortner and who share many of the same sentiments. Both William Reddy (1992) and William Sewell (1992) have attempted to outline what a theory of change would have to be like if it were to intersect with the historically minded anthropologists at the same time as it cohered with the idea of social practice. Sewell's insistence on adopting an understanding of society which is at the same time multiple, contingent and fractured has enormous implications for how history is done; Reddy's analysis of the public sphere as being historically constituted and constitutive of society raises fundamental issues about the possibilities of a historical ethnography of the present.

HYBRIDS AND PARTIAL CONNECTIONS

The discussions about history and anthropology and the metaphors employed to describe the relationship need to be seen in the context of the creation and proliferation of hybrids: anthropological history, historical ethnography, cultural history, historical anthropology, ethnographic history, historical ethnography and cultural history. What these hybrids are or should be are, of course, negotiable and, not surprisingly, in view of the many different accounts of the history and anthropology relationship, ambivalent. To give no more than just a flavour of this, let us tune into two quite different imaginings of
historical anthropology.

The first is given by the historian Peter Burke (1987: 3-4) in the introduction to a set of essays on early modern Italy. Burke lists a number of features of historical anthropology that define its concerns and practice and which, according to the author, distinguish this discipline from social history. Historical anthropology rests on qualitative evidence and concentrates on local practices in small communities; the historical anthropologist seeks to uncover and understand social interaction particularly through apparently trivial actions in precisely the same terms as they were constructed locally, that is, following Geertz's 'thick description'; as for theory, historical anthropologists seek inspiration from figures such as Arnold van Gennep, Marcel Mauss, Clifford Geertz, Victor Turner and Pierre Bourdieu.

By contrast, the anthropologists Jean and John Comaroff offer a different historical anthropology, one that makes the version by Peter Burke appear parochial and theoretically naive. Theirs is a call for a theoretically principled historical anthropology. Interestingly, they draw some inspiration from cultural history (as practised by historians) and on the methods for uniting the disciplines, but argue
that neither amounts to a historical anthropology. In something that is very much
like a call to arms, they assert their historical anthropology to be ‘anti-empiricist,
anti-objectivist, anti-essentialist ... anti-statistical and anti-aggregative’ (Comaroff
1992: 20). The challenge for this historical anthropology is
to address the equations of structure and indeterminacy, of form and incoherence,
involved in tracking the movement of societies and peoples through time; to disinter the
endogenous historicity of local worlds, both perceptual and practical, in order to
understand better their place within the world historical processes of which they are a
part; and to rupture the basic tropes of Western historiography biography and event
by situating being and action, comparatively, within their diverse cultural contexts. (ibid. 27)
Whatever the topic under analysis, they argue, it should be approached in the same
way: ‘as meaningful practice, produced in the interplay of subject and object, of the
contingent and the contextual’ (ibid. 32).
It is not possible here to give more than just this short statement of historical
anthropology as advanced by Jean and John Comaroff. The entire richly layered and
nuanced argument needs to be treated with concentrated attention. Theirs has not
been the only call for such a hybrid. Marshall Sahlins (1993), Terence Turner
(1991) and Sherry Ortner (1989), to name but a few anthropologists, have thrown their lot in with others to argue for a historical ethnography. II

Historical practice that seeks to produce a historical ethnography explicitly is an exciting project. Its relationship with other forms of historical practice that focus on local matters, such as local history, microhistory and Alltagsgeschichte, would be useful to analyse. 12 Yet, as the anthropologist Renato Rosaldo (1986) has shown in a critique of Emmanuel Le Roy Ladurie's Montaillou, the claim of ethnographic authority is not unproblematical. Indeed, Rosaldo takes Ladurie to task for being too textualized, perhaps even contrived. As recent anthropological literature has shown, ethnography as an authoritative referent is problematical for anthropologists, and, by implication, should be so for historians (see also Clifford 1988, esp. ch. 1).

10 The insistence on the anti-statistical and the anti-aggregative has an interesting resonance in recent literature which seeks to deconstruct social science disciplines (and subdisciplines) which are positivism-driven. The literature is burgeoning. Some sense of what's going on can be gained from the following: Megill 1994; Greenhalgh 1996; Nelson 1996.

11 See also Schieffelin and Crittenden 1991; Gewertz 1983; Ortner 1995. For an interesting volume
covering areas of interest to both anthropologists and historians see Dirks et al. 1994.

1993; and Revel 1994. I discuss Alltagsgeschichte later on in this chapter (pp. 794-5). 791

Perhaps the charge frequently made by anthropologists (and others, including historians) that historians are not particularly interested in or do not understand theory, is one that will be made whenever historians attempt to participate in a hybrid adventure. Many historians would, without a moment’s hesitation, resist and rebut either characterization with vigour. Yet it is interesting that the hybrids with history and anthropology have been particularly exploited and analysed primarily by anthropologists. Rather than engage in the theoretical fracas, it would seem that history is more interested in producing its own in-house hybrids and then allowing the monster to go in search of prey.

This indeed seems to be the case with cultural history. I am fully aware that using this expression runs the risk of effacing a substantial literature that dates back more than a century (Burke 1991b). But as it is commonly used today, cultural history refers at least to a body of work that began to appear mostly during the 1970s.

History needs a sustained critique of cultural history; perhaps it will appear soon.

Whenever it does, it will need to engage with cultural
studies as it is and has been
developing. This is not the place to begin, though there
have already been
beginnings. The only point I wish to make here is that
cultural history as a
subdiscipline occupies two sites simultaneously: one of
these is cultural history qua
cultural history; the second is cultural history in the
sense of 'the cultural history of
X', where X can stand for anything.
The first of these brands of cultural history (re)emerged
in the 1980s as 'the new
cultural history'. Theoretically, as the editor of a
volume of papers locating the field
stated, the new cultural history drew inspiration from
anthropology and literary
criticism, superseding the sociology which framed issues in
social history until then
(Hunt 1989: 10-11). As for which anthropologist speaks the
theoretical arguments,
one should not be surprised to know that it was Clifford
Geertz, though Aletta
Biersack (1989) solicits theoretical advice from Marshall
Sahlins, signalling for the
reader that there may be more than one anthropologist
around. Historians meeting
the requirements for inclusion/identification as cultural
historians are those such as
Natalie Zemon Davis, Roger Chartier and Robert Damton,
though these are not
meant to be exclusive. Pierre Bourdieu and Michel Foucault
are the theorists at the
centre of discussion.
While the first kind of cultural history appears to be the core, there is the second kind, where cultural history operates at a site of practice. As I have suggested, the site can be almost anything. To give some sense of this, I list some-only a tiny proportion-of the topics that have been submitted for a cultural history analysis.

A good beginning as far as cultural studies is concerned is Grossberg et al. 1992.

François Furet has criticized the school of French history concerned with mentalitis precisely along these lines, arguing that the study of mentalitis is fragmentary, lacking in discipline, a factory of topics see Furet 1983. 792

In recent years there have been cultural histories of: 15 science and technology; 16 consumption; 17 landscape; 18 labour; 19 the body; 20 the senses (Camporesi 1994; Corbin 1995; 1986; Classen et al. 1994); disease (Delaporte 1986; Patterson 1987; Gilman 1995; Barnes 1995; Bates 1992; Rothman 1994); advertising (Lears 1994); popular culture/ 1 clothing; 22 colour; 23 food; 24 sugar (Mintz 1985); tobacco (Goodman 1993); drugs (Camporesi 1989; Goodman et al. 1995); the tele

In what follows, I have decided to present either review articles that, in themselves, guide the
reader through a wealth of material; or, in the absence of such valuable bibliographical works, I have chosen what I feel are representative works, preferably of recent vintage that include some overview of the field.

16 For a recent overview of literature, problems and insights see Dear 1995. For thoughts on cultural MacKenzie and Wajcman 1985. A recent exploration of the history of the computer that is framed by tropes familiar to cultural history is Edwards 1996. Anthropological perspectives on technology are neatly summarized in Pfaffenberger 1992. See also Pfaffenberger 1988. As for science, a review of anthropological concerns is available in Franklin 1995. On the intersection between cultural studies and science and technology studies see Rouse 1992. Excellent overviews of the burgeoning field of the anthropology of science and technology can be found in Hess 1995, and Hess and Layne 1992. Strict cultural histories of technology in a general, rather than particular, sense are rare. For interesting suggestions of how this might proceed and of the theoretical areas into which one might venture, see Scranton 1995.

17 Brewer and Porter 1993. A recent overview of how anthropologists (and some historians) have been treating consumption is given in Miller 1995.

18 Schama 1995. For the anthropologist’s perspective, see Hirsch and O’Hanlon 1995. Related to the interest in landscape is a burgeoning literature in the history of the environment, much of it bordering on, if not within, the cultural history mode. See, for example, ‘A round table: environmental history’ Journal of American History 76 (1990): 1007-1147; Cronon
Spatial history is also making a break for recognition see the special section 'Spatial history rethinking the idea of place', History Workshop Journal 39 (1995): 137-92. See also the quite remarkable work on perceptions and representations of the sea, a topic that cuts across other concerns of this mode of cultural history, particularly popular culture: Corbin 1994.

For an overview of the problems encountered in engaging with theoretical 'turns' in the last few decades, as far as labour history is concerned, see Berlanstein 1993.

Although it is ostensibly about 'social history', much of the debate recently aired about its future can be reconstructed as a debate about the cultural history of labour see Joyce 1995 and the response by Eley and Nield 1995.

An interesting study that investigates the body not only as a site of social practice but as an object of economic investment practice is Rabinbach 1990. A possible rich area for investigation is the history of the body at work, specifically, in what has been termed a workplace ecology, of the interactions between technology, the social relations of production, and the mediation of biology and manufacture. Some interesting ideas on this can be found in McEvoy 1995. For an anthropological perspective see Martin 1994 and Lock 1993.

See also the fascinating and very suggestive essay by Martin 1992.

21 Levine 1993. See also Levine 1992a and the responses to
it by Kelley 1992, Davis 1992 and Lears 1992. See also Levine's response: Levine 1992b. Though Levine's primary concern is American popular culture during the 1930s, what he has to say goes well beyond the United States at the time: comparative perspectives, from Europe and Africa are also on offer from Davis. See also Susman 1984.

A recent study that incorporates a wide range of representational tropes and which promises to be a model for work in other areas is Ross 1995. Another recent work, recalling an earlier generation of work on the study of the crowd in history and the work of historians such as Emmanuel Le Roy Ladurie, is Corbin 1992.

Phone; 25; Electricity and lighting (Nye 1990; Schivelbusch 1988); plastic (Meikle 1995); and the pencil (Petroski 1993).

In attempting to clear an area for historical investigation into which theoretical or methodological approaches from other disciplines can be folded, historians have pursued other kinds of hybrids and webs of partial connections, particularly the latter. The two that come most into view at the moment are Alltagsgeschichte and microhistory.

The latter has been a historical practice which has enjoined its adherents to reduce the scale of observation but nothing else. Teleology and ethnocentrism are two of its enemies. The event becomes a lens for gazing at an entire landscape of
processes, some within view and others in the distance. The important point is that
the reduction of scale is not a reduction to the particular (see Ginzburg 1993). It
shows possibilities and with it offers up the past as more contingent and less
monolithic than frequently portrayed by the macro-practitioners. One of the
frequent charges against micro history is that its attention to the object in the
reduced scale of vision precludes an engagement with the larger tapestry from
which the object has been approached. Interestingly, this problem has also been
raised by anthropologists, especially John and Jean Comaroff (see particularly J. L.
Comaroff 1982). John Comaroff has offered dialectics an internal dialectic, a set of
systematic contradictions of the local that shape and are shaped by historical
outcomes of complex arrangements, and a dialectic of articulation, the contradictory
principles that underlie the interaction between the local and its encompassing
context not only as a solution to the problem but, more importantly, as the unit of
study itself (ibid. 146). Natalie Davis has responded to this charge in an
interesting way by turning the problem on its head: namely, by arguing that
macro historians must accommodate the microhistorical disclosure. Microhistory, or to be more precise, those who practise or speak for those who
practise microhistory, frequently elide it with Alltagsgeschichte (see Davis 1990; also Medick 1994; 1987). Geoff Eley (1989) has recently shown that to define it as a uniform programme of study would be to obscure much of its lively diversity. Put plainly, and certainly overs imply, Alltagsgeschichte is the history of everyday life, in


23 Gage 1993; Pastoureau 1988; 1987; Medick 1995. Anthropologists have had much to say about colour. See, for example, Sahlins 1976. For a review of some of the issues, see Saunders 1995.

24 Camporesi 1993; Mennell 1985; Mintz 1993. See also Camporesi 1992. An overview from other perspectives can be found in Mennell et al. 1992.

25 Fischer 1992. See also the highly original and deeply interdisciplinary Ronel 1989.

26 See the point made in Medick 1995. A longer discourse on this point is made in Medick 1994.

27 One of the key attempts to write a local historical anthropology set within a world-systems approach is Wolf 1982. For a recent review of the problems see Abu-Lughod 1995. See also Appadurai 1990.

28 Davis 1990. For a recent and very interesting collection of essays that examine the encounter of different cultures in different historical contexts, see Schwartz 1994. 794 whatever circumstances the analyst chooses. Though it has a
specific German

pedigree, the formulation of this historical approach reaches beyond national boundaries and specificities. Eley's synthesis of the writings of Alf Ludtke, whom he takes as typifying the approach, makes this clear. In particular, he draws out five main methodological diktats: a history from below of cultural practice; an emphasis on subjectivity and experience; decentralization of analysis and interpretation; a new formulation of the big questions through the latter turn; and finally, the rendering of the political as an expression of the intersection of public and private.

(Eley 1989: 322-3).

If, as Eley argues, the theoretical debt (especially to anthropologists) is more implicit than explicit, nevertheless the intersection of Alltagsgeschichte and historical ethnography has been noted by others, including Ludtke himself. 30 One interesting question that Alltagsgeschichte raises is the possibility of finding an analytical route towards an understanding of social formations as historical agents involved in cultural practices. Here, there is another intersection, but one that has not been noticed: the increasing interest in the concept of hegemony, among those cultural historians and anthropologists who are committed to a political engagement. 31 As Eve Rosenhaft (1987: 105) has so clearly stated it:
'Hegemony' directs us more clearly than any other term in current use to examine 'culture' as an arena of class domination and negotiation, but it does not instruct us as to what we will find or even how to go about it. In this sense, Gramsci has bequeathed us not a philosopher's stone for the perfect fusion of history and anthropology, but a credo for combining methodological diversity with coherence of purpose in any social study.

RIGHT TOOLS FOR THE JOB: THE HISTORIAN AS BRICOLEUR AND CHIFFONIER

It is often said that historians eschew theory, preferring a world of facts. Who exactly says this is uncertain, but whatever the provenance, there seems to be a confusion over the word 'theory'. The question is: which theory? Certainly historians in their practice do not engage with the theory of history in the way that anthropologists, for example, engage with cultural and social theory. On the other

29 This turn resonates particularly with debates within labour history, especially those taking place in the United States. On experience and on the possibility of the historian 'knowing' this, see the important article by Scott 1991. An interesting discussion of the relationship between language and experience and of both with that of historical practice can be found in Reid 1993.

For some recent thoughts see Comaroff and Comaroff 1992: chs I and 10. See also Lears 1985. One anthropologist who has made the return to the political his de elEur is William Roseberry. See, especially, Roseberry 1996; 1992; and 1989a, especially chs 5 and 6. Nicole Polier and William Roseberry have provided a stinging critique of the representational turn in ethnography, as demonstrated particularly in Clifford and Marcus 1986, and a plea to return ethnographic knowledge to the rich contexts in which it is produced and consumed see Polier and Roseberry 1989.

Hand, it would be untrue to say that historians do not engage with social or linguistic theory. It is just that not all of them do all of the time. The other statement that is often made of historians is that they are eclectic in their choice of who or what theory to follow. Historians are best described as bricoleurs or chiffoniers (Wohlfarth 1986), pursuing their interests, picking up little theoretical titbits here and there, knowing this name and that theory; but essentially driven by the pragmatist's credo the right tools for the job.

Despite appearances, this credo is deeply problematic. What is the job? How does a tool become right? These are reflexive questions. Confronting them effaces the dangerous dualism of theory and practice that haunts the historical discipline. The object here is not to harangue but rather to provide signposts for the profitable
engagement of history with anthropology. To bring this chapter to an end, I want to
return to an earlier point that is, for the need of concerns that simultaneously call
forth an anthropological and historical response and to look briefly at the field of
science, technology and medicine studies where such simultaneity is, I think,
especially clear. 33

In a recent article concerned with the predicament of social history, particularly
with respect to its failure to engage with contemporary social and cultural theory,
the argument was made that the historian’s terrain is not only being invaded but
interpreted and offered to postmodernity by those not practising history themselves
(Goyce 1995: 73). In other words, not only is the past contested terrain, but the
terrain itself is contested with historians as the spectators. Patrick Joyce, the author
of the article, is thinking especially that postmodernism ‘must project a
"modernism" by means of which its own image can be made ... there is a tendency
to invent a modernism which acts as postmodernism’s founding concept’ (ibid.). In
other words, if historians hang around while ‘postmodernism’ is performing its
dastardly deed, they will find the past foreclosed; that is, the past will appear as an
assemblage of elements acting as one into which it will be difficult, if not impossible,
to enter. Gone too will be the cherished but often unstated principle that history is open-ended; or as Carolyn Steedman put it, the 'implicit understanding that things are not over, that the story isn't finished, can never be finished, for some new item of information may alter the account that has been given' (Steedman 1992: 614; see also Miller 1995). One might add: some new insight, theory, or even off-the-cuff remark.

This is not the place to debate history and postmodernism, but rather to add to

32 A whole volume of essays in science and technology studies (including historical ones) has been devoted to this very issue. See Clarke and Fujimura 1992.

33 A recent overview of the intersections of science, technology and medicine studies, though with little reference to historical material, is Traweek 1993. This article also surveys feminist perspectives and intersections, but for material relating to history see Jordanova 1993.

34 The question is where to begin in following the ongoing debate. A good place to start would be Stone 1991 followed immediately by Joyce and Kelly 1991, and Stone and Speigel 1992. Going around the problem from another direction, one can begin with Kirk 1994: and working backwards through the same journal. See also Eley 1993 and Appleby 1994. 796

Joyce's concern a more positive or (pro)active turn. Namely, that there are others, historians none of them, who are in the middle of the
skirmish about history, not on
the sidelines with the practitioners. They are allies. They
can help historians with
terrain problems. And what is more, and by very good
fortune, they are also in the
skirmishes in anthropology, in philosophy, in science and
technology studies, and
in gender studies. Historians should get to know these
voices.

Though I said at the start of the chapter that I would not
be mentioning names, I
would like to offer Marilyn Strathern, Bruno Latour, Donna
Haraway and Andrew
Pickering. 35 Instead of symbolic interpretative
anthropology and thick description,
these analysts offer partial connections, irreducibility,
hybrids and post-humanism.

As a historian, I offer them as tropes, as ways of thinking
about the past, about
historicizing the tropes themselves.

What appears to me as central to much of this work and
what should appeal to
historians is the primacy of practice, performance and
time. To take one example,
historians, and many others as well, choose, for instance,
between writing about
people or things. When the choice is people, things seldom
appear: when the choice
is things, then the problem of agency pops up, often solved
(or thought to be
solved) by resorting to the principle that things only have
meaning when used by
people. This is, of course, analytically asymmetrical, but
most historians would not give it a second thought. By contrast, Donna Haraway challenges with the following:

what counts as human and nonhuman is not given by definition, but only by relation, by engagement in situated, worldly encounters, where boundaries take shape and categories sediment .... Human and non-human, all entities take shape in encounters, in practices;

and the actors and partners in encounters are not all human, to say the least. Further,

many of these nonhuman partners and actors are not very natural, and certainly not original. And all humans are not the same. (Haraway 1994: 64-5)

In this passage, Haraway, it seems to me, has in one stroke offered historians an unmissable opportunity to historicize the entire package. Together with her repertoire of the webs of culture studies, feminist, multicultural and antiracist theory, Haraway clears a terrain for a 'new' new cultural history; a cultural history of categorization, of classification or, to put it another way, how we make sense of every thing. 36 Following a suggestion from Zygmunt Bauman (1991), this must include not only the anomalies but the practices devised to confront and manage them. Needless to say, an analysis of the management of anomalies must extend to

35 I only list here either the most recent with citations

36 Ludmilla Jordanova has recently made a similar argument concerning the use of the word reproduction in a non-problematized, presentist sense see Jordanova 1995. Jordanova's essay is a perfect example of how historians can put the brake on runaway presentism by working from the inside that is, in the historically specific out. 797

include non-human actors. Marilyn Strathern (1990 and 1992) does a similar job on the idea of an 'event'. By analysing, or as she puts it, decomposing the event,

Strathern confronts how time and identity are constructed through an encounter.

Event becomes an analytical category through which relations and definitions become theorized. Bruno Latour (e.g. 1988; 1993) has quite a bit to say about time,

about metaphors of time, and about chronology and its construction. But he also makes a powerful argument in favour of the irreducibility of phenomena and the attack on it by analysts. 'The smallest AIDS virus', he writes,

takes you from sex to the unconscious, then to Africa, tissue cultures, DNA and San Francisco, but the analysts, thinkers, journalists and decision-makers will slice the
delicate network traced by the virus for you into tidy compartments where you will find

only science, only economy, only social phenomena, only local news, only sentiment,

only sex. (Latour 1993: 2)

In all of his writings, Latour has insisted on following the actors wherever they may take us. This has two main objectives. The first is simply to follow the actors in the same way as a biographer might perform her/his task. The second is much more subtle; that is, to treat the traces left by the actors' movements as constituting the analytical field itself. Rather than prejudging the nature of the analytical project, the researcher allows the subject/object of the study to perform that function. 37

Ironically, this method is superbly suited to a historical ethnography, in which the choice of which actor (human or non-human) is entirely open, in which classifications and categories are not pre-selected. Recently George Marcus (1995) has advocated just such an approach in order to help anthropology out of its commitment to single-sited ethnography.

In a way, the chapter has now come full circle. Anthropology throws a very wide net for its theoretical catch. Getting in on this act allows the historian to be taken into an unexpected landscape. I have been talking about engaging with anthropol
ogy, in the key sense of following it as it spins its web. The patterns left behind it, but especially the nodes of greatest density, are what should interest the historian.

Slicing through the nodes will reveal areas of interand multidisciplinary practice, include those of history itself. As a model discipline, anthropology foregrounds interrelationships between things that appear to be unconnected. Historians should


The historian Thomas Hughes has done precisely this in his analysis of electrification, using Thomas Edison as his guide see Hughes 1983. See also Hughes 1986. 798


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(First published in Social Research 49 (1982): 1013-28.)


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ARCHAEOLOGY AND HISTORIOGRAPHY
Guy Halsall

Introduction

Archaeology is the study of the human past through its material remains, 1 yet even this straightforward definition produces problems. Are not manuscripts and books material remains? Are other objects, such as inscriptions or coins, which bear
writing, the province of the archaeologist or the
documentary historian? We might

hive these classes of material off into the disciplines of
epigraphy or numismatics,

but the problem remains, with, for example, graffiti on
pottery, and so on.

Acknowledging these blurred areas leaves us with an
admittedly problematic and

provisional, but nevertheless useful, definition of
archaeology as the study of the

unwritten, material records of the human past; it also, of
course, raises the question

of the interrelationship between the disciplines of
documentary history and

archaeology.

The two are separate disciplines, in that the types of
evidence dealt with, written

records on the one hand and excavated data on the other,
require, at a technical

level at least, the acquisition of different skills for
their evaluation and interpreta
tion. Nevertheless, as they are both concerned with the
human past, archaeology

and documentary history ought to be natural allies.
However, over recent decades

there has been something of an estrangement between the
two. In some areas of

historical writing, like the early medieval period, where
the volume of archaeological

cal work and the comparative absence of documentary data
highlight the problems

of integrating written and excavated evidence, there is a
disturbing lack of dialogue
between archaeologists and historians. To give one current example: within the

University of London a regular Wednesday-evening 'earlier medieval' seminar

meets at the Institute of Historical Research, while on Thursday evenings, a few

1 For introductions to archaeology, see Rahtz 1991; Greene 1995; Bahn and Renfrew 1991. Bahn 1989 is extremely entertaining and contains a surprising amount of serious information. 805

hundred yards away at the Institute of Archaeology, the 'Postgraduate Seminar in

Medieval Archaeology' convenes. Yet, in spite of the chronological, geographical

and thematic overlaps between the two seminars, the personnel in attendance are

almost entirely different. This lack of dialogue is particularly ironic as most schools

of archaeological theory currently stress the importance of the close interrelation

ship between history and archaeology.

This chapter will not be concerned with the methodologies of archaeological

fieldwork which, it is worth stating, deals with a much wider range of activities than

excavation alone, including aerial prospection, field-walking, and methods of site

evaluation and survey such as electronic 'geophysical' technology, enabling

subsurface remains to be charted to some extent without excavation. 2 It should also

be made clear that archaeological theory and practice do not stand as opposed
entities. The theory-bound nature of even the most apparently mundane aspects of archaeological fieldwork have been known for some time, even if there often remains a self-conscious opposition between 'dirt archaeologists' and 'theorists'. In recent years, the lack of practical integration of theory and fieldwork has preoccupied several leading archaeologists (see Hodder 1992; Carver 1993; Bradley 1993; Barrett 1995).

Nor is it the concern of this chapter to provide an introduction to specialist scientific aspects of archaeological enquiry such as metallurgical, ceramic or petrological analyses, scientific dating methods, palaeobotany, or the study of animal or human bone (see Aitken 1990). Instead the chapter will provide a guide, in rough chronological order, to the ways in which archaeologists have approached the past. The main focus will be that of the present writer's research, the immediately post-Roman period of the Middle Ages. The concept of an archaeological 'culture', 'certain types of remains pots, implements, ornaments, burial rites, house forms constantly recurring together' (Childe 1929: v-vi), emerged at the end of the nineteenth century. This led to narratives of the succession of such cultures within a particular area, or the
movement of 'cultures' from one area to another. This ensuing 'Culture-History' (not to be confused with cultural history!) is today something of a dirty term in

2 On excavation, the best introduction remains Barker 1993. For field walking, see Fasham, et al. 1980, and for 'geophysics', see Clark 1990.

3 For an excellent history of archaeology, the reader is referred to Trigger 1989. See also Renfrew and Bahn 1991: 17-40; Daniel 1967.

4 I reject absolutely the assertion that 'historical archaeology' means the archaeology of the modern world, recently made by Orser 1996: 26-8. Orser's view appears to be that 'history' only began when the New and Third Worlds came into contact with the Old. Although intended, laudably, to counter Eurocentrism, this formulation seems to me to do precisely the opposite. It also, of course, makes the title of his book tautological! Here, the tight schedule required for the production of this chapter has forced me to ignore historical archaeology outside Europe. This is a significant lacuna. Mea culpa. 806

archaeological circles. As examples of Culture-History in the historical period we might note the excavations of Sir Mortimer Wheeler which aimed at elucidating and illustrating the conquest of Britain by the Romans, best known at Maiden Castle.

Similarly we might cite the use made of archaeological material to track the invasion and settlement of the 'Angles, Saxons and Jutes', 5 and the similar plotting of the
migration of Franks into Gaul from the distribution of grave-types and their associated forms of artefact. Change in archaeological culture was usually interpreted as resulting from either 'diffusion' (the spread of cultural forms but not necessarily of people) or migration (where the appearance of new cultural forms in a region represented the immigration of new people 'bearing' that culture), or even invasion. It was within the Culture-History paradigm that most classical and biblical archaeology operated, attempting to link sites into a particular political-historical narrative. As one example of this, and of how it affected the publication of fieldwork, we might cite the example of the French Romanist Jean-Jacques Hatt, who reduced most of the sites upon which he worked to unilinear sequences of 'layers', each identified with a particular historical phase, and punctuated by burnt levels which he associated with historically attested 'sacks'. Culture-History, by presenting the possibility of tracing 'peoples' through time, and especially backwards from their first appearance in written records, lent itself to abuse and manipulation in the service of nationalist and racist beliefs, and soon acquired some fairly nasty connotations (Trigger 1989: 148-206, esp. 163-7). The Nazis made much use of the interpretations of Gustaf Kossinna (1858-1931), who
argued for the supremacy of the Aryan race and the Germanic origins of Indo-European civilization; whereas Stalin (until 1950) employed the ideas of Nikolai Marr (1865-1934), which, in total opposition to Kossinna's migrationism argued that material cultural and even linguistic changes represented internal socio-political change, thus allowing Slavic dominance in eastern Europe to be projected far back into prehistory. FUNCTIONALISM

In the 1930s and particularly after the Second World War, with the ensuing awareness of the horrific extent to which ideas of 'national characteristics' could be pushed, Culture-History fell out of vogue, to be replaced, gradually and far from universally, by functionalist archaeology.8 Influenced by structural-functionalist anthropology, such as the (admittedly diverse) work of Malinowski and Radcliffe Brown, this approach saw 'cultures' as functional systems, wherein internal components served a particular purpose for the maintenance of the whole (whether

5 Leeds 1913, which, as has recently been noted, used the concept of an archaeological culture over a decade before its more famous 'introduction' by V. G. Childe: Dark 1995, 5-6.

6 For critique, see James 1979.

7 Hatt's approach can be seen clearly at his excavations near St-Medard, Strasbourg: Matt 1953.
8 Culture-history in effect remained the dominant paradigm in medieval archaeology until the 1970s. 807

or not acting towards biological imperatives}. Each component of a culture, or social institution, could be explained in terms of its function, rather like the organs of the body. This led to environmental determinism; cultural components functioned in an adaptive way to cope with the natural environment, and could thus be explained in terms of the latter. To understand the function of a given element of the archaeological record, it needed to be interpreted in relation to other parts of that 'culture'. Thus developed the archaeological subdiscipline of environmental archaeology and the study of settlement patterns, increasingly using aerial archaeology, some practitioners of which had noticed the potential of aerial photographs during wartime service in the RAF.

Functionalist archaeology could encompass diverse views, such as the later writings of V. Gordon Childe (Childe 1936; 1949; 1951; 1956a; 1956b; 1958; on Childe himself, see Trigger 1980) and the earlier work of Grahame O. G. D. Clark (Clark 1932; 1939 (3rd edn of 1957 cited here); 1940; 1952; 1954). Whereas Childe's functionalist work began within an economically determinist perspective, he gradually became more interested in social organization, the contradictions inherent
in the 'functioning' of society, and eventually the ways in which social evolution was advanced or retarded by the use and control of knowledge. Clark, on the other hand, in opposition to Childe's Marxist views, saw functionalism as meaning that societies operated primarily to maintain stability and their continued existence in their specific environment. Childe's actual archaeological work remained somewhat limited and constrained within the field of typology, but Clark, most famously in his excavation of the Neolithic site of Star Carr in Yorkshire, was one of the pioneers of environmental archaeology.

Another influential outcome of the functionalist phase in western European archaeology was 'Hawkes's ladder' (Hawkes 1954), a hierarchy according to the difficulty with which archaeologists could investigate topics without recourse to historical texts or oral tradition (Clark, for example, made much use of folklore). Technology, according to Hawkes, was the easiest subject to examine archaeologically, followed by economics; socio-political organization was at best recoverable with difficulty, and religious belief more or less impossible to study. This provided some justification for functionalist archaeology's major concern with the environment, settlement patterns, technology and economy, the latter three of which were
seen as very heavily constrained by the first. Environmental archaeology within the functionalist perspective produced works of overtly environmentally, or ecologically, determinist character, such as that of Claudio Vita-Finzi and Eric Higgs. 

During this period, advances in scientific dating, most notably in carbon (C I4 )

dating, produced a huge revision of prehistoric chronology, which was dramatically lengthened. 9 For all its potential diversity, however, functionalist archaeology still relied, like Culture-Historical archaeology, on factors such as diffusion and migration for much of its explanation of change.

9 On which see Renfrew 1973. 808

THE NEW ARCHAEOLOGY (OR PROCESSUAL ARCHAEOLOGY)

In the 1960s archaeology significantly altered the way in which it conceived of itself as a discipline. These developments are usually bracketed together as 'the New Archaeology'. This was not the first time a 'New Archaeology' had been proposed (see e.g. Wissler 1917; Caldwell 1959), but the name has become fixed to the movement generally associated with the American archaeologist Lewis Binford. 10

Binford believed that archaeology should recover general aspects of human behaviour, and strive to be a science. Binford resolutely opposed what he conceived of as the 'particularist' tendencies of history (his view of which discipline was
decidedly bizarre and outmoded). Instead, archaeology should be concerned with looking for cross-cultural generalities, and the proposal, testing and establishment of general laws (nomothetic interpretations). Explanation was thus equated with prediction.

Binford conceived of society as a functioning system, responding adaptively to its natural environment. This led to a certain ecological or environmental determinism, and a view of social relations as essentially tending towards stasis. Again change was viewed as produced by agents external to the system. Such change could, however, only really be understood in relation to the effects it had on the workings of the system, so internal relationships had to be studied. This in turn meant that one had to study the whole of a social system, and all its remains. Binford was uninterested in diffusion, and the New Archaeological approach to change has been termed 'neo-evolutionism'. Although humans were capable of adapting to such things as environmental change (adaptation), the forces driving change were largely non-human.

The English archaeologist David (D. L.) Clarke came to similar conclusions as Binford at about the same time. However, Clarke, however, was influenced more by the
New Geography and less opposed to the use of history. Clarke, like Binford,

believed that archaeology needed to formulate its own technical terms to describe

and analyse its data more efficiently. This led to a growth of what traditional

archaeologists, and specialists in other disciplines, regarded as excessive jargon.

Reading his and Binford’s early work can be hard work, and the posthumous

second edition of Analytical Archaeology carried out by R. Chapman has even been

tered 'the Chapman translation'. One of Clarke's principal interests, arising

naturally from the approaches outlined, was in the spatial distribution and

10 Binford’s early work is collected in Binford 1972. Within this volume, his essays 'Archaeology as

anthropology' (originally American Anthropology 28 (1962: 217-25), 'Smudge-pits and hide smoking:

the use of analogy in archaeological reasoning' (originally American Antiquity 32 (1967): 1-12) and

'Some comments on historical versus processual archaeology' (originally South-Western Journal of

Anthropology 24 (1960): 267-75) are of particular importance. See also Binford 1983 and 1989.

11 As has often been noted, Clarke’s influential Analytical Archaeology (1968) appeared in its first

edition in the same year as Binford’s equally important edited volume (with S. R Binford), New

Perspectives in Archaeology (1968). Clarke died tragically early, and his essays are collected in Clarke

1972 and Clarke 1973. 809
relationships observed within the archaeological record, and the means by which such patterns could be tested for statistical relevance, predicted and explained.

This was followed up in the early work of his student, Ian Hodder (Clarke 1977; Hodder and Orton 1976).

New Archaeology rapidly, and unsurprisingly, became interested in 'systems theory' to study of the workings of cultural systems, using models derived from geography and biology. This allowed New Archaeologists, rather than seeing systems as simply reproducing themselves in a straightforward functional way, to examine the relationships between different elements of the system and seek processes which tended towards change, which they termed structure-elaborating, or morphogenetic, processes, as well as those which tended towards stability (homeostatic processes). This was accomplished through the use of feedback. When change occurred in one element of the system it would produce related change, adaptation or feedback in the others. Negative feedback by counteracting the initial change, would tend towards stability, whereas positive feedback, by amplifying it, would produce greater and irreversible change.

Systems theory produced two further developments. The first was the use of
Immanuel Wallerstein's (1974; see Champion 1989) notion of the world system, and

the second was the idea of 'peer-polity interaction', developed by Colin Renfrew (see Renfrew and Cherry 1986). Whereas the first developed ideas of 'core and periphery' in the interrelationship of different cultures, the second theory was designed to explain comparable changes in different, more or less equal, cultures. In a system of units of roughly equal socio-political and economic power or geographical extent (peer polities), changes in one unit would, through processes of emulation and competition (agents of which being warfare, competitive monumental architecture and so on), lead to comparable and more or less contemporary change in the other units.

New archaeologists were interested in long-term developments, such as the development of social organization, or the collapse of complex societies. These they perceived as processes which could be studied in general cross-cultural perspective (hence this approach is generally now referred to as processual archaeology). They made use of theories of the 'stages' of social development, such as the progression from band, through chiefdom, to state (see e.g. Sahlins and Service 1960; Service 1962; Fried 1967; Claessen and Skalnik 1978).
In order to test the hypotheses proposed and arrive at the desired general laws of human behaviour, various archaeologists proposed different means of relating archaeological remains to actual human behaviour. Binford argued that archaeologists tend to 'confirm the consequent', 12 by which he meant that since the archaeological record does not speak for itself, and thus we approach the archaeological record with particular views in mind (the 'feudal' nature of Maya economics, for example), any testing of these ideas against the data will simply tend to reaffirm them. Binford instead argued for the use of 'middle-range theories' from outside archaeology, which would demonstrate general aspects of human behaviour. These would then be used to find such regularities within the data, and act as a kind of bench-mark against which to judge interpretations. These external theories would come from, for example, ethnographic observation or the use of written records which reveal such general observations. These provide externally validated laws, and archaeological data can be examined with them in mind. Thus Binford stimulated 'middle-range research'. 13
With the same general goal in mind, Clarke (1973) defined five types or levels of theory in which archaeologists were engaged:

- Pre-depositional and depositional theory, dealing with the human and natural processes which led to the deposition and initial patterning of archaeological data;
- Post-depositional theory, concerned with what happened to this data once it was in the ground, how processes of decay and disturbance would affect the archaeological record;
- Retrieval theory, simply enough theorization of excavation, sampling procedures and so on; analytical theory, covering the treatment of the material recovered, its classification, experimental studies, etc. and interpretative theory, which involves the recovery of the original social, behavioural, environmental patterns and activities (bringing us full circle to the processes studied by pre-depositional theory). Clarke argued that archaeology had to develop adequately scientific theory to deal with all these levels. Otherwise, he argued, archaeology would remain 'an irresponsible art form' (Clarke 1973: 16).

In the United States, Michael Schiffer (1976) argued that archaeologists would have to understand certain processes involved in the formation of archaeological sites, which give the data their observable patterning. First, one identified
'correlates', relating artefacts and patterns to specific human activities, or behaviour

(so a certain patterning of material will be the correlate of gift exchange, for example). Then one had to study the ways in which this material record had been altered by two kinds of process. The first he called 'C-transforms', 'cultural transformation processes' or the means by which the archaeological record is formed in the course of normal human activities, loss patterns and so forth, but also things which would come under Clarke's heading of post-depositional theory, such as subsequent ploughing. N-transforms, on the other hand, 'non-cultural transformation processes', determined how the material record would be altered by its relationships with the natural environment. If archaeologists could reconstruct these transformation processes and eliminate them, they would arrive at a 'reality' of the material record, and the basis for its interpretation. Seen in the light of much of the functionalist archaeology of the mid-twentieth century, especially that of Grahame Clark, or similar work in the United States, much of the New Archaeology does not look particularly revolutionary, or even new. Binford was, however, reacting not so much to functionalist archaeologists

13 See Binford 1978, which produced comparative data on discard patterns around hearths, and so
on, for testing against palaeolithic evidence of hunter-gatherer communities. 811

such as Clark as to the Culture-Historical archaeology which predominated over

much of the United States. Clarke, as a student of Clark, proposed a way forward

from earlier functionalism and especially away from the text-dominated or art

historical archaeology which was still to be found all too often in Britain. New

Archaeology did add to earlier paradigms the concern with historical processes, the

quest for general laws, and the attempts to make archaeology a more rigorous and

scientific discipline. The latter concern brought the introduction of mathematical

formulas, computerized techniques of statistical evaluation and modelling, and

spatial analysis. It also produced a more formal concern with the processes of site

formation and the problems of archaeological sampling. These ideas together

produced the concept of the 'research design', which should be set up to evaluate

current knowledge of a subject, and establish sampling procedures at various levels,

theoretical frameworks and the questions to be asked, before any fieldwork was

undertaken.

New Archaeology was slow to catch on in historically documented periods,

although spatial analysis of the distribution of Roman towns was undertaken by
It took even longer to make an impact on medieval archaeology. In 1982, at the twenty-fifth anniversary conference of the Society for Medieval Archaeology, Philip Rahtz proposed a ‘New Medieval Archaeology’ (published as Rahtz 1983). In the same year, Richard Hodges published a lengthy paper which similarly argued for the application of the methods and theory of New Archaeology to medieval data. (Hodges 1982b).

Ironically, by 1982, when New Archaeology found its way on to the stage of medieval studies, it was already beginning to be rejected by a significant body of theoretical archaeologists. After the New Archaeology: Current Approaches is 'New Archaeology' always had its critics, and these were by no means all the traditionalists and reactionaries which writers like Binford liked to portray them as being. As early as 1974, an anthropologist, R. Adams, had, quite rightly, criticized some New Archaeologists for their 'behaviouristic gradualism and biological reductionism' (Adams 1974: 249), and for failing to recognize the sometimes decisive 'conscious diversity of actions' (ibid.). This provided a foretaste of what was to come in the 1980s. Ironically, Ian Hodder, pioneer of spatial analysis and student of David Clarke, spearheaded the reaction against the New Archaeology (Hodder 1982a; 1982b; 1986; 1992).
For major processual works in the historical period, we may cite Rondsborg (1980; 1991; Hodges 1982a).

For an unusually reader-friendly overview of current theoretical archaeological approaches to diverse subjects, the reader is referred to Dark (1995). A degree of caution is, however, necessary, as the conceptualization of various topics becomes increasingly vague as the book progresses. For example, the effective difference between his economic categories of 'feudalism' and 'render-systems' (pp. 139-40) will remain quite opaque to any medievalist.

A number of general criticisms of New Archaeology can be made. Prominent among these are its removal of the individual from history. Human beings simply responded in predictable ways to 'system needs', or to changes beyond their control;

social systems were essentially stable; change brought about by conflict within the 'system' was ignored. The issue of ideology was almost entirely absent from processual archaeology. New Archaeology was excessively functional; from components of a 'social system' down to individual artefact forms, everything was explained in terms of its function; there was little symbolism in New Archaeology, and that which there was saw symbolism, predictably, as functional. The idea that societies could be divided up into neatly compartmentalized interrelating 'boxes', is
also questionable. Gordon Childe had long before said that humans reacted only to
their perception of environmental needs, and this point was repeated in opposition to
the environmentally or ecologically determinist aspects of processual archaeology.

Middle-range theory and its analogues also faced a barrage of criticism. Schiffer's C
and N-transforms were attacked by Binford, but his own ideas of externally
verifiable general aspects of human behaviour were rightly condemned as being
every bit as theoretically contingent as any other aspect of archaeology. Further
more, the value of ethnographic analogies was called into question, particularly
where the cultures under study, rather than being in some pristine state, could be
shown to owe much of their character to their relationship to other types of society,
or to their contacts with the Western world. The anti-historicism of New
Archaeology also caused problems, and even processualists condemned refusals to
deal with written data in historic periods. Perhaps most importantly of all, the New
Archaeology was still only really interested in the lower rungs of 'Hawkes' ladder'.

Response to these criticisms of New Archaeology produced an alternative and
antithetical theoretical framework and a development within the processual paradigm.

The first of these is what is known as post-processual archaeology. Rather than being a
closely defined and more or less unified 'school' of thought, post-processual archaeology is essentially defined simply by its opposition to processual archaeology, in accordance with the points made above. The starting-point of post-processualism is that material culture is actively and meaningfully constituted. It creates as well as reflects social relationships, so that, for example, the particular costume deemed appropriate for a young woman does not simply reflect that social category (of 'young woman'), but also actively creates it, in that the mental image of someone of that age and gender will largely be conditioned by that costume which symbolizes it. People

16 For a lengthy statement, see Courbin 1988. Whilst Courbin’s criticisms of New Archaeology are entirely convincing, his subsequent proposal that archaeology return to a sole concern with the collection of facts is reactionary and theoretically untenable. See the review of his work by Moreland 1991b.

17 For the reactionary political undertones of this stance, see Trigger 1989: 312-15, 322-6.

18 Binford acknowledged the 'ideotechnic' aspect (note the term!) of artefacts; Binford 1965.

19 For the essential statements of post-processualism, see Hodder 1982a; 1982b; 1986; 1992; Shanks and Tilley 1987; 1991. 813

use material culture actively within social relations, so that material culture can be read
as a discourse. However, to understand this symbolism we need always to place it in
context. Not only might different symbols mean different things in different cultures,
but their meanings might change from one context to another even within the same
society. The often-quoted example of this would be the priest's costume, which is white
in some circumstances, symbolizing good: bad/white: black oppositions, yet black in
others. At the same time, however, post-processual archaeologists argued that objects
were polysemic (had more than one meaning) or even deliberately ambiguous, within
contexts as well as between them. One might be able to arrive at one meaning, perhaps
even the dominant meaning, by examining the context within which a material
cultural symbol was found, but one should always remember that other people might
have read the symbol in a quite different way.

These statements were also the product of a particular view of society and of
social change. Post-processualism reacted against the slow, long-term systemic
change envisaged by the New Archaeology, seeing social change instead as dynamic.

Social structure could be seen as a mental image of rules or norms, or appropriate
codes of behaviour, and of the ways in which material culture was used in these.

This kind of 'structure', while in some ways inhibiting action, nevertheless also
enables it, and individuals can play with these rules as well as within them. Social

structure thus never quite reproduces itself exactly. These theories drew heavily

upon the anthropologist Pierre Bourdieu's (1977) notion of the 'habitus', the

Marxist sociologist Anthony Giddens's (1984) concept of structuration and Noam

Chomsky's 'generative grammar'.

Some post-processualists, such as Hodder (1986: I; 1982a: 1-16, repro in 1992: 92-121), have been quite open in stressing the conscious 'steps backwards' involved

in post-processual archaeology, reviving the importance of historical context (and

thus even of Culture-History), and several of Gordon Childe's views. Many of their ideas were based initially upon a reading of structuralist thought, especially from

linguistics (e.g. Saussure) and anthropology (e.g. Levi-Strauss). The flaws in structuralist theory were rapidly taken on board, so that post-processualists quickly

became poststructuralists too. Given that processualists were also interested in structuralism (the comparisons between 'structure' and 'system' are obvious),

structuralism and the critiques of it provided the bridge from processualism to post-processualism. The influence of Marxist thought, particularly that of the 'Frankfurt school' (e.g. Althusser) was also to be seen in post-processualism, although the
Marx-influenced rather than Marxist theory behind much of post-processualism has led other Marxist archaeologists to describe it as 'ersatz Marxism' (Trigger 1989: 369). The other major source of theory drawn upon by post-processualists was that of literary criticism and 'critical theory'. In the early days of post-processual archaeology, thinkers such as Foucault, Barthes and Derrida came to be cited almost as often as archaeological writers. 20

20 For introductions to structuralist and post-structuralist writers, and applications of their thinking to material culture, see Tilley (ed.) 1990. 814

Post-processualists rejected absolutely the opposition between data and theory. Following work by Mark Leone (1978; 1982) they argued that interpretations of the past are entirely dependent upon the ideological standpoint and socio-political context of the researcher in question. This led to arguments for a 'plurality' of archaeologies, with each interpretation as valid as the others. This was an attempt to evade the elitism of academic archaeology, to make the subject more democratic, and to try to avoid imposing modern, Western capitalist 'pasts' on other societies.

At the same time, in line with some at least of these views, post-processualists proposed that archaeologists take an active political stance, to combat the
appropriation of the past by dominant ideologies. Archaeologists should consciously reflect on what they are doing, and use it actively to shape the present. Thus feminist archaeologists have been attracted by the post-processual approach. 21

Some processualists had come to some similar conclusions about the shortcomings of old-style processualism, a point which radical post-processualists often forgot. This led to the establishment of 'cognitive processualism', which reinstates symbolism, ideology, the active use of material culture, and internal conflict into the study of the past. 22 It is also far more open to the use of written sources than earlier processualist work. Nevertheless, it retains an obsession with long-term process and general laws. Some have claimed that that cognitive processualism represents a new consensus (Bahn and Renfrew 1991: 431-4), although post-processualists have denied both that this 'consensus' owes much to processualism or, indeed, that there is consensus at all (Hodder 1992: 147). The focusing on laws has led to a somewhat fruitless positivist search for laws of the mind. A further development mainly within processualist archaeology has been the (to a historian) rather belated rediscovery of Annales school history (Bintliff 1991a). One could say that the Annales school shares many features with processual archaeology, not least its
inability to cope with historical change. However, post-processualists have also taken up certain annaliste ideas about the longue durée and mentalités (Hodder 1987).

Needless to say, historians have been more than a little bemused by this, as they see it, reinvention of the wheel. 23 There still seem, in spite of Renfrew and Bahn’s claims for a consensus, to be major differences between cognitive processualists and even 'new' post-processualists. The term 'new' post-processualists is used because there were developments within this loosely defined body of theorists, or steps backwards from some of the extreme positions adopted in the mid to late 1980s. The principle line of attack was against the 'disabling relativism' of the 'hard-line' post-processualism of, for example, Shanks and Tilley. This attack was spearheaded by feminist archaeologists, who rightly saw that it was not feasible for post-processualists to argue that.


For an application to a historical period, see Gilchrist 1994.

22 The origins of this movement can be traced to Renfrew 1982. For cognitive processual studies in the historical period, see My tum 1991; Hedeager 1992; Hodges 1989.

archaeology be used actively in the present and at the same time claim that one reading of the past was as good as any other. If the latter was the case, then chauvinist readings of the past overtly aimed at the subjugation of women would have to be admitted as equally permissible as views of the past designed to empower women. How could archaeologists use their discipline to silence neo-Nazi abuse of the past, other than through the 'naked' use of academic power which post-processualists claimed to oppose? At the same time, the irony of lectures on the evils of elitism and the opening-up of the past to the masses delivered from the high table of Peterhouse, Cambridge, was apparent even to one of their Cambridge school proponents (Hodder 1991). The realization of the impossibility of maintaining both pluralism and a politically active stance led to a reassessment of the early post-processual insistence upon the inextricability of data and theory (see Graves 1991).

The difference between 'facts' and the body of data whence facts are selected, to paraphrase E. H. Carr (1987: 10ff.) was realized. Dark (1995: 36) has conveniently referred to this as the difference between data and evidence. Although the evidence we choose to study and cite in our research is entirely contingent upon the researcher, although, in archaeology, the way in which the
data is observed,
excavated, recorded and (above all) published is heavily
dependent upon the
theoretical stance of the excavator, and although the
formation of the data itself is
hardly unbiased, there nevertheless remains a body of data
out there, which exists
independently of our theoretical viewpoint, whether we
choose to study it or not. It
has frequently been possible to use data collected for one
purpose to study quite
different questions and arrive at conclusions entirely
alien to the original excava
tor. 24 It ought to be possible to accept or reject
hypotheses about the past according
to how well they 'fit' that body of data. For example, it
is no longer possible to
accept ideas linking particular types of weapons in graves
to particular social classes,
because closer examination of the data simply does not
support them (Samson 1987;
James 1989). If this were not the case, we would never be
surprised in our
encounters with history. So, as Hodder (1992: 175) has
most recently argued, we
are not locked in a hermeneutic vicious circle, but rather
our work represents a
'hermeneutic spiral', a dialogue with the past, where our
experience of the data
shapes our conceptions, as well as being structured by
them. There may be no 'right
answers', but we can establish that some answers are less
wrong than others.
Marxist approaches have to some extent stood to one side of the theoretical approaches outlined above. Marxism has its own theoretical basis and discourse, independent of the discipline of archaeology (see Chapter 36, this volume). It has also, however, been able to adapt in some way or other to each of these trends.

Childe was a Marxist who worked within a generally functionalist paradigm.

Although Marxist explanations of change were radically different from those of generally conservative processualists, there was something recognizable in pro cessualist 'stagist' views of social development for Marxists used to theorizing about sequences of modes of production. Structural functionalism or even systems theory could also be modified to represent contradictions within society, and especially the oppositions between infrastructure and superstructure.

Structuralism has been closely bound up with Marxist thinking and post-processual archaeology has very largely been proposed by theorists heavily dependent upon Marxist thought and, in some cases, self-consciously proclaiming their stance to be Marxist, although, as
noted, more traditional Marxists have rejected this. 25

Some overall remarks can be made about archaeological approaches to the past.

The first is that the sequence of fashionable paradigms is far more obvious, and

paradigmatic changes are accompanied by far more polemic and posturing than

appears to be the case in documentary history. Another is that for all the usual

disparagement of the previous theoretical fashion, most new approaches in fact owe

a substantial amount to their precursors. Referring to archaeology’s tendency to

borrow often obsolescent ideas from other disciplines Bintliff has written that

archaeological theory always 'reappears in clean but borrowed finery'. 26 This

borrowed finery is almost always worn over at least some of the garb of previous

theory. RELATIONSHIPS BETWEEN HISTORY AND ARCHAEOLOGY

There is an emerging consensus among archaeologists that closer links with

documentary history ought to be forged. Yet, for all these good intentions, there is

still far too little meaningful dialogue between historians and archaeologists. The

usual situation has been well summed up by Ian Morris:

[those] who rely mainly on texts and those who rely mainly on archaeological evidence

often act as if they were two sides in a competition, and one day an impartial observer will

judge whose evidence is the best and who wins the game. (Morris 1992: 200)
As instances of this 'competitive' attitude we may turn to work on the early medieval era:

It has been said that the spade cannot lie but it owes this merit in part to the fact that it cannot speak. (Grierson 1959: 129)

But for the first millennium the written sources are primarily chronicles, biographies, letters and poems. Few wills and charters survive before about 950, and virtually no records of village life. The historian is at an obvious disadvantage. With these personal


For Marxist archaeology applied to a historical period, see, recently, the two chapters by Steve Roskams in Christie and Loseby 1996.

26 Bintliff 1991b: 275 It should be said that Bintliffs 'discovery' of Annales puts him very much in the same category. 817

sources only at his disposal, it is not surprising that the result commonly amounts to what Colin Renfrew has termed the 'Dark Age myth', ... (Hodges 1989: 6f

Archaeology can help, at least in terms of the description of the material culture of the Scandinavian societies, but there are strict limits to the types of questions which it is competent to answer. (Collins 1991: 313)28

The roots of the problem lie in archaeology's development as an academic
discipline in western Europe. Culture-History, by simply attempting to flesh out,
illustrate or extend political historical narratives, and by being conducted, usually,
by researchers trained as historians, posed few problems for disciplinary relation
ships. History was dominant; archaeology was simply auxiliary or ancillary. These
attitudes are still to be found today; in the University of London's Institute of
Historical Research, archaeology is still classified under 'auxiliary sciences'. 29

Functionalist, by being essentially a prehistorian’s approach, and by concentrating
on topics such as environment and economy which were not readily confronted
through written sources, similarly did not rock the boat. The problems came with New Archaeology, which, as stated, tried consciously to
distance itself from history. Partly this was based upon a rather strange view of
documentary history; partly, too, it stemmed from an understandable and justifiable
desire by archaeologists to rid their discipline of the tag, 'handmaiden of history'
(Hume 1964). New Archaeological polemic, especially in the medieval period, did
not help interdisciplinary relations. Picking up on critical reassessments of certain
texts (most notably, Dumville 1977), some archaeologists decided that written
sources simply had nothing very interesting to say. It was unfortunate for their case
that, rather than having sufficient courage in their convictions, or faith in the explanatory power of their evidence, to produce alternative studies entirely drawn from the material data, they often fell back on the written sources whenever they ran into difficulties, especially with issues higher up 'Hawkes's ladder'. Moreover, the use made of such sources was frequently crude, and counter to historians' critical assessments. The perceived jargon of the New Archaeology heightened the

27 One wonders if Hodges has actually read many early medieval written sources!

28 Collins's total ignorance of archaeology is best illustrated by the astonishing 'clanger' dropped on n. 47 to p. 323 (note on p. 419), where he claims that no archaeological evidence has ever been found of Quay and Fishamble St. On the latter, see, for example, Wallace 1985.

29 Hobsbawm 1979: 249 implies that he thinks archaeology is an 'ancillary discipline to history'.

30 For example, R. Hodges's appeal to Bede's account of a political marriage to provide a context for archaeological evidence of cross-Channel contact. 'There is a firm historical foundation for these contacts, since Charibert I, who reigned at Paris from 561 to 567, gave his only daughter Bertha in marriage to Ethelbert of Kent' (Hodges 1982: 35). Bertha was not Charibert's only daughter; the marriage almost certainly took place in the 580s; thus Charibert did not 'give' her in marriage. See Wood 1983 for examination of how this forces a complete revision of Hodges's view of
Kentish Merovingian relations. 818

problems of dialogue (Wormald 1982). This is the context within which Morris's
'game' is played.

In trying to move forward we need to address traditional uses of archaeology by

historians. These usually fall into three categories:

Illustrative. Archaeological data, mainly artefacts, are used to provide illustra
tions of the appearance of people, or settlements and 'daily life' (see Collins

2 Justificatory. Usually crude and off-the-shelf use of archaeology is made to
'prove' history. Documents attest the existence of X and here it is, proved by

excavations at Y.

3 'Filling in the gaps'. Where documents do not tell us anything about a given

problem, it is valid to use archaeology to probe these areas.

These categories are also to be found in archaeological work. The third, in

particular, gives rise to what Ian Morris calls the 'zero-sum' view of archaeology,

that the importance of archaeology is inversely proportional to the number of

written documents. An explicit view of this statement can be found from the pen of

Grahame Clark (1957: 20-1). This is a view which must be abandoned by
historians and archaeologists if we are ever meaningfully to integrate written and material cultural evidence.

When we look more closely at previous attempts to use history and archaeology together, we usually see a three-stage process. In the first stage, an assumption drawn from written sources is used to structure the archaeological data, or the excavated data are themselves collected to answer a question drawn from the written sources. In the second phase, these neat correlations and illustrations are rapidly shown to be flawed, both by re-examining the archaeological sources and by more sophisticated consideration of the documents. As a result of the problems raised, in the third stage the idea that the two types of evidence can be used together at all is rejected. This can take the form of disparagement of one or other evidential form, and thus Morris's competitive interdisciplinary 'game'. Where this is not the case, it is simply assumed that the two types of evidence relate to wholly different things and thus cannot be used together. Sometimes this attitude is summed up in the epigram, 'archaeology tells you what people did, while history tells you what they thought', which of course implies that people did not usually think about what they were doing! The whole process is founded on the premises that one-to-one
correlations ought to exist, and that the two evidential forms ought to relate to

the same aspect of a given problem if they have anything at all to contribute to its

study.

31 See, for example, Hodges's comments on the failure (by 1982) of archaeology to find any

confirmation of the existence of the thriving emporium at York mentioned in written sources: 'At some

stage we have to accept the power of this kind of negative evidence' (Hodges 1982a: 74). This, needless
to say, is the opposite view to that of Collins 1991. Within a few years of the writing of these lines

excavations at Fishergate revealed the 'missing' town! 819

A concrete example may be seen in the examination of religion in post-Roman

Gaulish cemeteries. Written sources provided the idea that there was a dichotomy

between Christianity and paganism, especially in rural areas, in this period. Thus,

in archaeology, graves with grave-goods and those without were assumed to be the

graves of pagans and Christians respectively. Signs on artefacts were read as

religious indicators; west-east orientated burials and even burials in sarcophagi

were assumed to be of Christians. The flaws in these ideas were swiftly revealed.

The Church did not ban grave-goods; documentary and archaeological instances of

burial in church with grave-goods were cited; whether or not one was provided with

grave-goods was shown to be governed to some extent by age
and gender; general

anthropological work showed that grave-goods were not indicative of a particular view of the afterlife; west-east burial became common before widespread con

version to Christianity; the symbols on artefacts were rarely to be read in straight forward ways. Closer study of the documents even questioned the idea of this kind of religious opposition. As a result of this barrage of criticism, some archaeologists decided that this served as a 'cautionary tale' about using history and archaeology together. 32

The despairing view of integration now seems mainly to be held by historians. 33

Certainly the combative stance, and rather silly mistakes in the use of written sources, of New Archaeologists like Richard Hodges served to obscure in historians' eyes the very real advances and significant contributions to understanding which their work represented. Some historians still refuse to accept that archaeology has explanatory value. Many others see it as very difficult to produce independent hypotheses from archaeological evidence because that evidence does not take written form. Whilst it is true that the movement from 'data' to 'evidence' is, initially, more difficult, and requires more theoretical involvement (Dark 1995: 36), it is a long time since documentary historians drew their
interpretations purely from
the overt, face-value statements of their texts.

The movement away from New Archaeology produced attempts to bring about a
rapprochement between the disciplines. Yet although much sense has been spoken
about the need to reopen dialogue, break down disciplinary barriers and find
common theoretical approaches, no workable methodology has been proposed
which allows archaeology to retain an equal and independent explanatory voice, and
thus to move on from the 'zero-sum' view. In 1988 a
collection of studies of early
medieval Britain and Ireland was published (Driscoll and Nieke 1988) with the
expressed intention of examining the integration of historical and archaeological

32 P. Perin, 'La confrontation des donnees ecrites et archeologiques est toujours delicate et parfois il y
a peu de rencontres, comme j'ai pu les verifier, avec des autres, pour les questions de paganisme et de
christianisme' (personal communication, 14 June 1988).

33 Dumville 1989: 213. The comments of Cornell 1995: 26-30, esp. 29, seems to stem from a despair
at precisely the same sort of crass one-to-one correlations as are described above. The conclusion,
though, is unduly negative. 820

evidence from different theoretical perspectives. 34 Worthy though the aims were,
there was little methodological advance. Documentary evidence still set the agenda
and there was not much attempt to use the archaeology to question or refine the picture drawn from often problematic written sources. Partly the problem arose from an absence of papers by historians which might have served to question the applicability of these types of document to the periods, places and problems under study. The concluding essay by Driscoll (1988) begins with valuable and important comments about the value of interdisciplinary study and the historiography of historical archaeology. However, the case study intended to illustrate the potential of interdisciplinary work remains disappointing. The analysis is flawed by being structured around particular views of conversion to Christianity, the Pictish state and the expanding power of the latter, all drawn from late and problematic sources.

In the end the material remains, Pictish symbol stones, are used simply to illustrate these notions rather than to interrogate them.

More recently, the post-processual archaeologist John Moreland has written extensively on the relationships between history and archaeology. Moreland makes excellent points about how material culture may be read as text, and how texts may be seen as a kind of material culture. This permits us to use material culture and texts to examine the same kinds of power relationships and discourses. 35 However,
while Moreland's work provides a valid means of using material cultural studies with work on the use and form of texts and literacy, he does not offer a framework for the use of archaeology with the contents of documents, which is, after all, what most historians work with.

To progress, we need to take a step backwards. The problems in previous attempts to integrate historical and archaeological sources stem from two key issues. The first is a continuous hopping back and forth from one discipline to the other, using off-the-shelf interpretations from one to shape interpretations of the other. This is exacerbated by the ignorance of specialists in one discipline of the problems involved in the use of data in the other. This has resulted in the borrowing of 'facts' from unreliable documents, or simplistic use of archaeological data. We therefore need to do two things. We have to eliminate the cross-disciplinary comparisons and borrowings from all but the highest and most sophisticated levels of interpretation, and we have to rebuild the walls between disciplines, even if as low walls respected only in particular stages of analysis. In practice this means separating different categories of data within the disciplines (i.e. keeping settlement data separate from cemetery evidence, and the study of saints' lives distinct from that of charters) as
well as simply keeping written data distinct from excavated.

34 Such as Leslie Alcock’s culture-history, Chris Arnold’s processualism, Richard Hodges’s (by then)
cognitive processualism and Stephen Driscoll’s post-processualism.

35 Moreland 1991a. For more consideration of common theoretical approaches to written and
excavated material, see Tabacynzki 1993. The article’s excessively impenetrable style is not helped by
bad proof-reading.

36 See, e.g., B. S. Bachrach’s (1970) use of cemetery archaeology to evaluate the numbers of mounted
warriors. 821

If we see all our data in context and as meaningfully and contingently formed by
active individuals, analogous processes of enquiry are presented. These become
clearer still if we see all our evidence, whether written document, decorated
artefact, settlement or house plan or grave, as textual, conveying symbolic, coded
‘messages’ to an audience. The stages of enquiry can be set out as follows:

1 Frame questions.

2 Collect data.

3 Evaluate the data. This means assessing its reliability, to what extent archaeo
tological sites were reliably excavated or published and what potential features
were not recorded, or the extent to which a historical text is interpolated.

4 Examine the context of the data. This implies not only
the geographical and
chronological provenance of the material but also more
general questions of who
formed the data, why and for whom. Textual scholars need to
consider the
questions of genre; archaeologists analogous issues such
as what the data repre
sents: votive pit, midden, grave and so on. Archaeologists
also need to consider
such questions as how easy of access was this evidence,
how visible was it, and
for how long?

5 Establish patterning within the data. In documentary
terms this simply means
reading the document, and reconstructing internal semantic
patterns, ranges of
meaning, narrative strategies and so forth. For
archaeologists it means looking at
spatial distributions, patterns of correlation and so on.
In both cases it involves
reconstructing the message of the data, as opposed to the
context within which
that message was made.

6 Produce detailed conclusions from this body of data.
Alternative explanations
should be kept open.

This process is not purely linear. For example, stage 5
must sometimes precede
stage 4: sometimes we can only establish who created a
body of evidence, for whom,
and how visibly, once we have considered what the overt
message of the data is.
Moreover, the process is recursive, as ideas emerge during the investigation of the data at stages 4, 5 or 6; we often return to our data to examine aspects of it which did not at first seem important, or we collect more data.

Each body of data should be studied separately. Although at stage I we come at that evidence with preconceived ideas drawn from the usual bricolage of history and archaeology (including parallels from other periods and places), geography, anthropology, ethnography, linguistics, literature, sociology and philosophy, as well as personal experience, we ought to be asking what that particular type of data, when analysed on its own merits, can say in answer to those questions. I would also argue that questions at stage 1 be framed in as general a way as possible. Posing questions which are too specific, or too closely based upon ideas drawn from another discipline, can lead at best to the abandonment of the examination of potentially interesting sources of information, or, at worst, hammering the data to fit these preconceptions. During stages 2-6 we should rule out any interference from the study of other categories of data. It has been this kind of criss-crossing between the study of different kinds of material at these stages which has obscured the potential of so many sources.

We must always remember that different kinds of evidence
might have different

things to say about different aspects of particular
problems. This method allows us

to abandon questions which do not seem to be answered, or
answerable, from that

kind of data, to frame new ones. It also allows us to
determine those issues, or

aspects of questions, which that material does confront. In spite of a widespread

scepticism, the analysis of different bodies of data can realistically be kept distinct at

these stages. Just as, recently, students of hagiography have been able to study their

source material without trying to make it fit preconceptions drawn from work on

narrative histories, and as settlement archaeology (especially urban) has often been

studied in total separation from cemetery studies, it is quite feasible to study a body

of archaeological material without constantly framing one's questions or structuring

the data and its patterning according to preconceptions from documentary history.

The approach recommended here not only falls into line with the contextualism of

post-processual archaeology, but also chimes with more rigidly contextual studies of

written texts proposed recently by historians (see Fouracre 1990; Wood 1992).

Moreover, it permits each body of data to question, as well as to confirm or

complement, the others 37 because once we have studied these bodies of evidence
separately, we can and must merge the conclusions drawn from each (stage 6) at a higher level. 38 We ought to be able to see more clearly where genuine correlations do exist, but where, as is usually the case, such direct links are absent, it allows us to confront the questions of why they do not, by looking at the purposes served by the creation of each kind of data. It also allows meaningful integration to be made at a more sophisticated level, by looking at common purposes or mentalities involved in the creation of data. It should enable us to identify significant social change, which unidisciplinary studies have either ignored or dismissed as mere 'fashion'.39 This approach should also allow us to choose between alternative explanations of one body of data on the grounds of sophisticated study of the others, not by preconceived ideas of the supremacy of one kind of data, but by the production of hypotheses which explain all sets of data equally. If different kinds of evidence are approached in this way, via analogous stages of analysis, posing comparable kinds of question, and arriving at independent conclusions, fruitless old questions of whether history or archaeology is dominant, ancillary or auxiliary are sidestepped. This 'multidisciplinary' methodology does however require either that people be more specifically educated in more than one discipline or at least that disciplinary
37 I would argue that none of the post-processual examples of interdisciplinary integration cited approvingly by Moreland 1991a uses archaeology or material culture to provide a voice independent of the written sources. Some seem only to justify or illustrate the latter.

38 Thus whilst I agree with Richard Reece (1984) that archaeologists should keep their data separate from historical ideas, I do so only when considering preliminary stages of analysis.

39 For an attempt to put this methodology into practice, see Halsall 1995, although, like all who would be jack of all trades, the author suffers from being a master of none. For a subsequent rejection of the only neat, direct correlation identified in this work, see Halsall 1996: 18. 823

specialists be prepared to engage in the kind of dialogue which allows such conclusions, rather than vacuous one-to-one similarities or competitive attempts to prove one set of data better than the other. As far as future developments are concerned, it requires academics to educate their students in the possibilities of such dialogue, and to stop perpetuating the disparaging or confrontational states of mind discussed above.

If we can carry out this methodology, we will produce much more rounded views of the past. Binford (1977: 13) once wrote that historical archaeology would one day be at the forefront of archaeological theory. With a more thoughtful integrative
methodology this might at last be possible, but just as archaeologists ought to benefit from the existence of documentary evidence, historians ought to be prepared to accept the benefits of independent material cultural research, and not just for illustrative, justificatory or 'zero-sum' purposes. The recognition that archaeology has an equal and independent explanatory voice will allow a fuller understanding of all historical periods, from the Bronze Age Near East to the present day, by allowing us to challenge 'the primacy of the articulate and the articulated from the


15: 239-58.


Binford, L. R. (1965) 'Archaeological systematics and the study of culture process',


-(1977) 'Historical archaeology: is it historical or archaeological?', in L. Ferguson (ed.)

Historical Archaeology and the Importance of Material Things, Tucson.


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Comparative Studies in Archaeology, One Word, London.


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-(1991) 'To interpret is to act. The need for an interpretive archaeology', Scottish Archaeological Review 8: 8-13.


-and Orton, C. (1976) Spatial Analysis in Archaeology,
Cambridge.


Moreland, J. (1991a) 'Method and theory in medieval archaeology in the 1990s',
Archaeologia Medievale 18: 7-42.
25 Years of Medieval Archaeology, Sheffield.
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Cambridge.
Renfrew, C. (1973) Before Civilization, The Radiocarbon Revolution and Prehistoric Europe,
London.
Cambridge.


There is no single, linear, progressive history of Western art history; rather there are several parallel traditions of writing about art, some of them truly historical. Indeed, it is hard to separate the history of art history from the history of art theory:

the historiographic boundary in relation to art history is somewhat blurred.

Nevertheless, if we adopt the working definition that art history is a historical account of the existence and form of works of art and if we bear some important contextual points in mind, a historiographic analysis of art writing is possible.

The first necessary cognizance is of the common ground between writing about art and writing about other historical phenomena, for example, the adoption by art history of models used by historians to describe religion or diplomacy; second, an awareness of the links between historical writing and the current practice of art itself (Gaehagens 1990 and Belting 1987); third, a recognition of the actual state of
knowledge, in the senses of access or archaeology, for example, how much of an artist's work is known and available for study, what is the state of its conservation and restoration?

fourth, an awareness of the role of technology, to capture a memory of a work of art, by sketching or using a camera, to reproduce a work of art or overcome the difficulty of understanding colour or paint via a line engraving (Haskell 1987; 1976: 160ff.; Lloyd 1975); fifth, an appreciation of the political context for historical discussions about art, since its high symbolic potential, understood since antiquity, means that art's history is not written in ideologically neutral terms, for example, the eighteenth-century rationalists who referred to their own times when addressing the historical problem of art under absolutist tyranny, or the German-Jewish art historians of the 1930s who were obliged to rethink long-standing racialist assumptions about artistic development; sixth, an awareness of the role of taste, those shifting emphases towards and away from particular artists.
genres and objects, for example, the changing critical and historical fortunes of

various artists such as Piero della Francesca or Vermeer;

seventh, an understanding

defining the role of ideology in shaping an art historian's view

and writing, his or her

class, gender and national interests; and lastly, an

awareness of the use of language


Art history shares some of its terms with other histories,

especially concepts

used to describe the shapes of time. Repeated patterns

include a periodic, cyclical

model (London under George III as a new Periclean Athens,

Augustan Rome or

Medicean Florence), models of decline and fall, assumptions about progress,

biological models with stages of growth, maturity and
decay, and eras divided into

megaand sub-periods. 4 The Hegelian form of dialectical
development, a style

period in art as a statement (thesis), subsumed and
answered by another style

(antithesis) out of and between which a third style
(synthesis) appears, is no

longer fashionable, but was highly influential over

several generations. 5 Style

periods can be labelled in various telling ways: for

eexample, after place, race or
culture (Gothic, Early Christian); following negative
criticism (Archaic,

Mannerism); or consequent upon style rather than period
(Realism, Romanesque)
Gombrich has argued that all styles are in essence either classical or anti-classical (Gombrich 1966: 81-98). There are many tensions between these labelled concepts, for example, the work of an artist might represent a style counter to that prevailing in the mega-period. Is the Baroque a late corrupt phase, a version or the antithesis of the Renaissance? (Kurz 1963 and Dempsey 1977). In addition, these style-period labels are also anachronistic and undesirable in their application to non-Western art.

2 Friedlander 1969: 51: 'Racial mixture seems to be good for the intellect.'

3 Haskell 1976: 147 and Meltzoff 1942 on the rediscovery in the 1840s-1860s of Vermeer by Thon: (b. 1807). [Lord] Alexander Lindsay (1812-80) was one of the first to celebrate Piero della Francesca, see Lindsay 1847. Piero is much beloved of the English and regarded as rational and unpopish in northern Europe. For his changing reputation see Longhi 1963: 116-69 and Carrier 1987; for Lindsay see Brigstocke 1981. No one wrote much about Botticelli until the later nineteenth century when a taste developed for his art, see Haskell 1976: 51 and Levey 1960. For a recent attempt to chart changing fortunes, see Turner 1993.

4 For periodic cycles see Potts 1985 and Janson 1973: 331. The inevitability of decline was long recognized as a problem, see the comments of the Abbe Laugier (1753) 'the moment we arrive at
58 

perfection ... we are fallen again', see Herrmann 1962: 12 and Wulfflin [1888] 1964: Preface 'The subject of this study is the disintegration of the Renaissance'.

5 Wind 1963 is perhaps the best-known modern development of Hegelianism.

6 Andrea Palladio (1508-80), regarded in some quarters as an arch-classicist, lived in the Age of Mannerism. 829 FOUNDERS

In antiquity there was no self-standing body of literary discourse called art history.

In general, ancient histories of any subject tended to be based on the writers' observations of distinctions and categories, which could then be arranged in a developmental pattern. Given the valuable models offered by the political historians in the Roman canon Livy, Plutarch, Tacitus and the Roman tradition of writing historical accounts, we can only regret what has been lost in the field of art history. Recurrent themes in the ancient historiography of art include certain models for the shaping of historical time; the tendency for historians to take a point of view (Pliny stresses the Roman achievement, the triumph of naturalism and the end of Greek art) and utilize their personal observations; the inclination towards functional analyses of art, to ponder the status of its practitioners, national stereo types, artistic individuality, descriptions of style, classifications such as genres, and
relations between art and society. Ancient art history was, however, produced in a range of literary genres. Encyclopaedists surveyed and compiled data, some of it art historical, and the rhetoricians cited art objects. Rhetoric, the science of speech making, was so important that its principles and aims informed the total cultural experience of the educated ancient world and several Roman rhetoricians (Cicero, Quintilian) discussed the evolution of the style of visual art by analogy with verbal languages. The rhetoricians had a particular interest in style but could not really agree on the relative merits of early (simple) styles and later (complex) ones. The sublime work of Pheidias was given priority over the conquering of realism in the following century. Another group (Philostratus, Callistratus) used works of art as the objects of descriptive or ekphrastic writing giving a sense of the conceptual language applied to visual representation in the ancient world Games and Webb (1991).

A third group of ancient art-writers were practical people. The Greek concept of technē ('making') required the authors of treatises (Vitruvius and others), writing for practitioners and the patron class, to consider art and help solve technical problems by exploring historical accounts of the arts. Fourth, ancient aestheticians,
concerned with the perception of beauty, have left a substantial group of fragmentary writings which assume the components of art-historical thought. The most influential figures in Greek thought Socrates, Plato and Aristotle all had ideas about visual representation and their Roman successors were especially concerned with the problem of art objects as symbols of decay and excessive luxury. These thoughts often resolved into historical comparisons and so reveal something of the art-historical assumptions of the day. There was also a lively ancient literary tradition in biography, although artists did not always enter the canons of 'Great Men'. Artistic biographies only appeared when their subjects had extraordinary capabilities; they were exceptionally ingenious or witty or employed by the rich and powerful. Finally, there were guide-book writers such as Pausanias, a Greek author and traveller of the second century AD, whose text includes art-historical material as a matter of intriguing but secondary significance. Pliny appears to have made use of other topographical texts in his encyclopaedia, which is by some long measure the most substantial extant ancient art-historical source, and a survey of the Natural
History will disclose themes that recur throughout the history of Western art

The principle of Pliny's shaping of art-historical time is demonstrated in his treatment of the history of sculpture where he identifies a clearly determined starting-point a creation myth of sorts from which progress is slowly made (Pliny XXXIV. 54-65). Progress follows directly from the actions taken by a series of gifted artists who have individual skills and ingenuity to solve problems of representation in a series of shifts on the naturalistic scale towards the real.

Individual, competing, male artists are Pliny's main historical concern, following contemporary Roman histories and biographies by writers such as Plutarch and Suetonius. The Natural History might be encyclopaedic but it is also partial and subjective and Pliny's history of art has been enormously influential for its schemata, giving straightforward models of historical development. His simple patterns edit out controversy about the nature of representation and about the means of achieving it, for it is very clear from other sources that the ancient world witnessed a lively debate about the principles upon which representation should be based. Such questions as those about the relative merits of drawing and colouring have recurrent
throughout history and have had a profound effect on
critical historians. In

painting, Pliny's narrative moves from the simple to the
complex. The rigidity of

his schemes for the history of sculpture and painting is
clearly derived from his

Greek sources and noteworthy and familiar is the
assumption in the Natural History

and elsewhere that the art of the past a Golden Age is
superior to that of the

present. Pliny's explanation for decline is rooted in the
common Roman complaint

about the close links between decadence and luxury: a
certain softening of the

Roman character, too many fancy ideas, mostly imported, and
too many luxury
goods. Some writers argued that over-elaborate drawing and
excessive colour lay at

the root of Roman decline Pliny, Vitruvius and Dionysius of
Halicarnassus

though not everyone agreed with them (Plutarch).

Pliny uses literary sources rather than works of art
themselves, preferring to read

Varro's antiquarian tracts than to study the huge numbers
of Greek works available

in the imperial collections and elsewhere. The idea that
the history of art had to be

written with the author having engaged directly with the
objects is, in fact, a quite

recent one: right into the nineteenth century, the Word
controlled the Image in art's

history. Pliny's account of bronze statuary reflects a
widespread assumption in
Roman society that art was primarily a functional phenomenon, to serve the state


9 Pliny the Elder, Natural History: The Elder Pliny’s Chapters on the History of Art, translated by K. Jex-Blake with a commentary by E. Sellars, London, 1896. 831

and commemorate great figures and events. In the Natural History, individual artists are responsible for the development of art and the text is full of biographical elements celebrating the skills of artists and their capacities for problem-solving. This, despite the fact that his primary subject was the materials worked into art the intrinsic value of the bronze, even its transformation by skill.

The architectural theorist Vitruvius used history to address questions of style. 10 He explains three different forms of ancient architecture referred to since the sixteenth century as the Orders as historical and ethnic phenomena. The Ionian people wanted to improve on the kind of columns used in Doria so they embellished that form with a volute and made it taller. All Pliny’s colleagues used anecdotes to explain how new artistic forms evolved. An important consequence about these historical stories is that they gendered and enspecied styles, they imbued them with emotion and made them physically manifest. Such a history
politicizes works of art. Once a conceptual language for art was invented in antiquity, style itself became a historical tool. Criteria such as proportion, harmony, appropriateness and decorum, emotion and narrative could all be identified, measured and compared to register historical change. In Pliny's treatment of painters we are also shown categories of art and ways of prioritizing art and marking historical change unrelated either to materials or to authorship (Pliny XXXV. 117-18). The assertion that a decorative landscape fresco can never attain the reputation of an easel painting on a narrative theme or of a portrait ties in with Pliny's implicit admiration for expression in art. Amongst the ancient Greeks, he writes, painting became the exclusive art and practice of the freeborn, Aristeides was the first to paint the soul, and [give] expression to the soul of man ... and also the emotions' (Pliny XXXV. 98).

A final, influential theme in Pliny is that the standard of a society is matched by and (influences) the standard of its art (Pliny XXXV. 50). Some Romans were intensely suspicious of art as a foreign source of idleness and luxury associated especially with the decadence and effeminacy of conquered nations; some harked back to the legends of the earliest Roman state when
sculpted or painted representations of the gods had been forbidden as it was only thought proper to contemplate the divine through the intellect; others took a sceptical view of images, believing that looted booty in the form of works of art endangered the body politic (Livy reporting Cato in Pollitt 1966: 33). Amongst a few, there was an intellectual yearning for moments of quiet contemplation before the work of art (Pliny XXXVI).

Hardly any of these ideas can be found in Western medieval writing about art.

The artist, as an individual, was accorded a low social status and was therefore not worthy of biography. To medieval historians, the makers of art were usually the patrons, not the artists; educated ecclesiastics had little time for the skilled manipulation of the material world in comparison with the contemplation of the world of ideas. Science, in the sense of knowledge, was regarded as far superior to art in the sense of skill with the hand. The ancient conceptual vocabulary of stylistic
change and historical development was largely abandoned, thus discouraging the comparison of past art with that of the present. The medieval Christian world regarded itself as superior to the pagan past. We can trace some intellectual parallels with ancient attitudes towards art, but these were insufficient to supply the context that would nurture art history (Panofsky 1951). In biographies of the saints, art objects were considered for their material value but not for the way those materials were worked into art (Panofsky 1946). The least ambitious ancient kind of art historical writing did recur: the topographical guides to places and collections, for example, Mirabilia Urbis Romae ('Marvels of the City of Rome') was written and often rewritten for pilgrims. This treated the surviving ancient works of art as vehicles for allegorical commentary and in so doing negated the pagan aspect of antiquity for Christian pilgrims. Secular works of art were hardly written about and religious art was considered first and foremost in terms of function either as an aid to devotion or as a sign of grace. As the late eleventh-century monk wrote in his description of an abbey church at Dijon 'it seems to have been achieved through some mystic intuition being attributable rather to divine inspiration than to the experience of any master', a convention of approval that would have disappointed
the architect and mason responsible. RENAISSANCE
RE-FOUNDERS

Key changes in Italian Renaissance historiography, from the earliest biographers,
through Ghiberti (1378-1455) to Vasari (1511-74), followed the invention of
printing and the drawing of tighter parallels between visual art and verbal
communication (Cochrane 1981; Soussloff1990; Joost-Gaugier 1982; Norman 1983:
223). The very structures of Latin prose historians were adopted as models for
composition and style (Goldstein 1992). In line with Roman historiography,
Renaissance histories of art were primarily didactic, great artists became great men
and were treated like other great men, for example, generals, statesmen and poets. 12

Pliny's powerful causal links between change in art and the innovative and inspired
solutions of gifted individuals was undoubtedly an influential model, as was the
medieval tradition of saintly lives as exemplary primers for human behaviour. 13 The
best-known biographies of all, the Gospel accounts of the life and ministry of
Christ, were also influential. 14 In his Tuscan vernacular manuscript Commentaries,
an eccentric mixture of history (some a precis of Pliny) and technical advice,

12 See Vespasiano de' Bisticci's Lives of Illustrious Men, available in several modern editions

after 1839.
Pliny's MSS circulated widely and the text was printed over a dozen times by 1500.

See the opening paragraph of Vasari's 'Life of Michelangelo' and Barolsky 1990. 833

Ghiberti, a practising sculptor and metalworker, stressed that artists had to be learned ('instructed in all the Liberal arts') (von Schlosser 1912; Krautheimer 1969: 257; Gombrich 1966: 1-10). Working with the hands alone would deprive them of authority, although learning to draw was presented as a sign of civility and intellectual progress. Ghiberti described the period between antiquity and his own day as an age of dark iconoclasm, resulting from bigotry in the Church. As with all his contemporaries, he knew next to nothing about ancient statues and pictures; however, his scheme of renewal requires that the classical be assumed to be unsurpassable. But Ghiberti's ambitious, unfinished project was unmatched in Renaissance Italy or anywhere else.

The long Renaissance tradition of biography and scholarly and theoretical discussions of the paragone, or critical comparison, between poetry and painting evidences that intellectual advancement which slowly improved the social status of artists, who even became autobiographers (Cellini, Bandinelli), although it has saddled artists with a peculiar, gendered-male, 'outsider' heroism ever since. 15 Only
gradually did more scientific approaches to art-history writing, based on archival or archaeological evidence, enter Italian historiography. An important theme in the peninsula was regional rivalry, for art was politicized and local historians were disinclined to concede to their rivals. Vasari's great Lives is precisely not a compilation of factual material; nevertheless, many subsequent readers have had just those expectations. His politics the servant to an ambitious dynast and absolutist were immediately recognized and challenged by his followers, although the self-congratulatory, autobiographical element in the book took longer to identify. An important device was to use anecdote when historical material ran out, for example in the 'Life of Giorgione' whom Vasari rightly identifies as important to the history of style but poorly documented. He adopted an extremely subtle and even polemical critical position, for Vasari was not simply a hoarder or compiler but a real historian, with a point of view and capable of sifting evidence to support it. He used descriptive writing to reconstruct the form and style of the pictures discussed and worked within a historical model of revival after decline. He set out artistic periods and identified those periods' leading protagonists. Most of all, his history of art is also dedicated to honouring the
achievements of his prince,

hence the politicization of artistic renewal and standards
as based on Tuscany and
the virtual deification of Michelangelo (1475-1564).

The non-biographical thread in Renaissance historiography
had less of an impact.

The literary critics sometimes drew on their historical
knowledge of art and
Renaissance treatises also contain historical material, for
example Alberti’s influ
ential works on architecture, painting and sculpture
(Grendler 1969a; Zimmermann
1976; Morisani 1953). Palladio’s much-published Four Books
(1570) on architecture

15 See the proemi to Vasari’s Lives for a taste of the
paragone debate.

16 Amongst the substantial literature on Vasari several
texts stand out: Kallab 1908; Alpers 1960;
Belting 1987.

17 Haskell 1971. Little positive progress was made in
Giorgione studies until Morelli in 1880. 834
has a chapter on Roman archaeology that was influential for
several generations
afterwards and written as a result of time-consuming,
dangerous and demanding
scholarship. As with Ghiberti and others, Palladio assumes
a decline from ancient
standards into ‘barbarian abuses’, rescued by the
Renaissance enterprise of which he
is a part. The topographers also had to have some sense of
the historical develop
ment of art. This genre is typified by Francesco Sansovino
in Venice (1581), who
was extremely well connected, as part of a small learned circle within the Venetian
patron class and the son of the great sculptor-architect to the Serenissima, Jacopo
Sansovino (1486-1570) (Grendler 1969b). Early printed art history outside the
Italian peninsula is very patchy, although examples in all genres exist and have to be
understood against the context of political and religious tension in Renaissance and
Reformation Europe (Filipczak 1993). EARLY MODERN ART HISTORY
Art-historical literature from the mid-sixteenth to the late eighteenth century was
initially dynamized by a politicized reaction to Vasari's Tuscan-centric view of
artistic standards and progress. In Il Riposo (Florence, 1584), Borghini states an
opposing but equally biased point of view stressing the roles of Venetian artists and
a Venetian theory of art (arguing for the primacy of colouring) to counter the
Tuscan emphasis on drawing (Hope 1983-4; Sohn 1991). This polemical literature
typifies an explosion of historical writing about art across Europe, very little of
which is sufficiently well known. Many writers followed by generations of editors
produced collections of biographies which tended to include theoretical or polemical
prefaces and revealed the interests of their authors simply through their selection of
artists (Goldberg 1988). All European cultures and
languages saw exercises in this manner of writing and the production of a set of great artists became a matter of national pride. 18 For the increasingly paradigmatic art of Bologna there is Malvasia (1678; Perini 1986; 1988; 1990): north of the Alps the key work is the Schilderboeck by Karel van Mander, a scholar of the noble class (a parallel with Alberti here).19

Van Mander's history is more synthetic than Vasari's; it is fully cross-referenced and closely integrated but has no periodic model, no scheme of history and no single hero. As with some Italian forebears and French successors, van Mander pursues the analogy between painting and poetry (Dubos 1719). He uses the Vasarian device of a claimed antique inheritance and standard, but celebrates the particular northern creative achievements of landscape and portraiture. Each painter is treated as an

18 For France see Roger De Piles (1635-1709), Abrigi de la vie des peintm (Paris, 1699), for whom see Puttfarken 1985 and Descamps 1753-63. For Spain, Francisco Pacheco (?1564-1654), Arte de la pintura, su antiguedad y grandezas ... (Seville, 1649) and [Antonio 1 Palomino (1655-1726), El Museo Pictorico y Escala Optica, 3 vols (Madrid, 1715-24).

19 Schilder-boeck ... (1603-4), 2nd edn with supplementary biography of van Mander, 1616-18; newly edited by H. Miedema as Karel van Mander, Lives olthe Illustrious Netherlandish and German Painters (Doornspijk, 1994), the first of a projected
individual specialist (Vasari had described this as their each having a maniera or 'manner') operating within a particular niche.

All his successors had to confront the harsh logic of Vasari's model. If standards had peaked with Michelangelo, what might happen subsequently? Was decline inevitable or could art be redirected? Might standards be maintained? Bellori, following Baglione's book of lives and describing seventeenth-century art with unabashed partisanship, was highly manipulative in his historical presentation of the seventeenth century and set up Raphael and Poussin (a new Raphael) as alternative high points to Michelangelo (Bellori 1672; 1695; 1664; Baglione 1642). These arguments, presenting artists such as Michelangelo or Raphael as heirs to the ancients, were paralleled in other scholarly discourse by philosophers such as Francis Bacon and literary historians such as Perrault in France, protagonists in the Ancients versus Moderns debate (Felibien 1685-8; Perrault 1688). In the 1600s there was also an increasing interest in establishing a framework for writing a critical history of art by establishing the authenticity of the very materials themselves, an interest which became known as 'connoisseurship', the exercising of judgement to establish authenticity and the definitive aːuvre of
particular artists. The listing of pictures in collections, for example in the third part of von Sandrart, as a historical source has continued into our own day.20

In the 1700s, the subject markedly opens out. Walpole (1717-97) attempted a history of the unfashionable art of England and in his encompassing of all schools and artists he was matched in due course by Dezallier d’Argenville and Seroux d’Agincourt in France and by J. D. Fiorillo (1748-1821) in Germany. 21 For the theory of history, the most important eighteenth-century development was an increasingly ambitious description of stylistic change without recourse to analogy with literature and in terms of evolution rather than in Vasarian, modular terms.

Gradually, a broad range of art came to be considered, rather than the exemplary peaks and standard-setting yardsticks. During this period, serious books on Chinese temples, gardens or Gothic cloisters were published, as were more and more studies of classical monuments, although the established historiography was boosted by an increased reliance on new archaeology. In Enlightenment circles, there were deep rooted assumptions that 'under a minor prince, and amidst a struggle of religion, we are not likely to meet with much account of the arts' (Walpole 1762-71 (1862 edn): I, 135). The Risorgimento offered further encouragement to
this kind of speculation, one contemporary editor wondering whether national unity might return

Italian art to the standards established elsewhere in Europe (Kugler 1842). Amongst

20 ... Teutsche Academie ... , 8 vols (Nuremburg, 1768-75). The labelling of objects on the basis of

ownership is an important historical procedure, for example, the Farnese Hercules, the Apollo Belvedere.

21 Walpole 1762-71; Dezallier d'Argenville 1745-52; Seroux d’Agincourt 1823; Leopoldo Cicognara (1767-1834), Storia della scultura ... in Italiafino al secolo di Napoleone ... , 3 vols (Venice, 1813-18)

(with a 2nd edn in 7 vols, Prato, 1823-4); Fiorillo 1796-1808. Seroux was influenced by Gibbon's The History of the Decline and Fall of the Roman Empire (London, 1776-88), especially in the blame for decline as residing with the Byzantines and other 'eastern' Mediterranean cultural powers. 836

this ever-increasing published output, Cicognara's celebration of neo-classical taste signals the tendency of history-writing to parallel taste in the production of visual art. Systematic and enlightened accounts by Winckelmann (1717-68) of the style and development of ancient art greatly influenced historians, artists and an international, well-informed public. He imposed an objective of philosophical idealism on his historical judgements and set a new standard in the scholarly use of literary sources. 22 Winckelmann firmly established fixed
points and standards in a way unseen since Vasari, setting Greek art in a quadripartite periodic scheme: the ages of the archaic, the sublime, the beautiful and of imitation and his underlying cyclical conceptions link these stages to periods in the modern era, namely, the Renaissance before Raphael, Raphael to Michelangelo, Correggio to Guido Reni and the death of art from the Carracci to Maratti (e.g. Rousseau 1755). Winckelmann also engages in a rich homoerotic, discursive fantasy to explore the impact of a selection of key antique works, mostly male nudes. His explanation for their form lies partly in his account of their context, for example, in the practices of ancient religion, in the moral economy of the ancient Greeks and even in the workings of the weather, which allowed the naked body to be revealed in sport and exercise to the beneficial effect on watching artists. Modern scholarship on Winckelmann has shown that he had less genuine knowledge of Greek originals but a great deal of poetic inspiration. Like all eighteenth-century historians of art, he was deeply interested in taste and expected the study of past art to have an impact on current practice. Another key eighteenth-century figure was Luigi Lanzi (1732-1810), an abbe, curator and scholar who set out to revise Vasari’s account, reduce its gossip
and offer a comparatively democratic account of the whole
Italian achievement

(Lanzi 1792). He confirmed the pattern of schools of art
(see earlier in the early
seventeenth-century manuscript Considerazione of the papal
physician Giulio
Mancini) and was immediately influential amongst European
collectors who wanted
exemplary works of each class and genre. In Lanzi,
each School is divided into a
series of epochal divisions and he argued consistently
that all the regional arts
required intervention from academies to effect a revival
of standards. For Piemonte,
the first epoch covers 'The Dawn of Art and Progress to
the Sixteenth Century', the
second frames the seventeenth century and the establishment
of an academy, and
the third covers the court painter Beaumont and the
restoration of the academy in
the eighteenth century. Throughout this period, European
art history remained a
matter of national pride. Late in the 1700s, public
museums developed as sites for
the presentation of new art-historical taxonomies (see
McClellan 1994).

22 Winckelmann 1755; 1764. For Winckelmann see Potts 1994.

23 For climate as a determinant in early modern historical
writing see Cardy 1976: 73.

24 For schools see too Moyer [1752] 1966 and J. F. Christ
(1700-56), for whom see Kultermann

25 de Dominici 1742-63; Ratti 1766, for whom see Collu
During the nineteenth century important technical advances and changing institutional practices transformed the way that art history was illustrated and increased the range of art-historical publication (Haskell 1976). Connoisseurship, well established in the eighteenth century, became a dominant mode of art historical writing over the next 100 years. It is an approach to art history which assumes the primary evidence of the art historian to be the work of art itself, words being unable fully to describe a work's qualities (Pope-Hennessy 1980: 11-38; Offner [1927] 1972). In important ways, especially in attempts to reconstruct the artistic 'personality', connoisseurship depended on Freudian psychology, although this has often been refuted by its practitioners (Wollheim 1973: 177-200; Spector 1969): 'Connoisseurship is not a poor substitute for knowledge, but provides ... means by which our limited state of knowledge can be broadened' (Offner, quoted in Pope-Hennessy 1980: 35). Important contributions to this historical method were made in early nineteenth-century Germany by Waagen and others (Waagen ...
1842-5; 1837-9; see too Bickendorf 1985; Schroter 1990): Rumohr (1785-1843),

whose interests were curatorial the originality and authenticity of works made

extensive use of documents in the positivist manner (Rumohr 1832); Kugler was a

state employee and the author of an influential handbook on painting from late

antiquity to the present day (see n. 20 above); J. D. Passavant (1787-1861)

established a new tone by discussing art as a public act and responsibility, not as the

plaything of the private collector (Passavant 1833). Morelli's method is particularly

interesting: anatomy and the camera in use for the first time (Morelli 1897). The

reputation of Berenson's published output, often vivid and memorable as criticism

but now poorly regarded by historians, has been damaged by his commercial

interests. 27

With its origins in antiquarianism, documentary research developed apace in the

1800s, for example in the production (1878-83) of a definitive edition of Vasari's

Lives by Gaetano Milanesi, sub-director of the Florentine archives. 28 In various

ways since the nineteenth century, art historians have learned to deploy the

document in their arguments, led by Ranke and other professional 'positivists', and

a tension between such a documentary approach and connoisseurship has existed
since. Cavalcasselle's work in the 1850s depended on his own drawings, exhaustive archival work and the democratic treatment of any artists about whom information could be found (Crowe and Cavalcasselle 1864-6; 1877; Levi 1985; Crowe 1885).29

26 For the eighteenth-century origins of connoisseurship see Gibson-Wood 1984 and Llewellyn (forthcoming).

27 Key Berenson texts are: the essays later collected as Italian Painters of the Renaissance, which originally appeared separately in New York in 1894, 1896, 1897 and 1907; The Study and Criticism of Italian Art in three series (1901-16); The Arch of Constantine; or, the Decline of form (1954); on Berenson see Mostyn-Owen 1972; Samuels 1979 and Brown 1979.

28 For antiquarianism and the Enlightenment see Thornton 1963: 24-7.

29 J. A. Crowe (1825-96); G. Cavalcaselle (1817-97).838

Such enterprises tended to be flawed by a disinclination or inability to distinguish between second-hand accounts and genuine original documents. Access to sources of documents and exemplary works of art was patchy, for most of the art was in private ownership, although public access slowly increased as more museums and galleries opened and the first academic posts were filled in history of art. 30 There were also new non-empirical theories of art history, for example the materialism of Taine (1828-93), which showed his interests in the ethnic and climatic contexts
for picture-making and a system based on an analogy between art and natural science, especially botany. Any particularity is only a variation in terms of classification and therefore of limited significance, a methodological assumption precisely opposed to connoisseurship. The determinism of Burckhardt related particular phenomena to general trends (Burckhardt 1855; 1878; Gossman 1988).

Although he claimed otherwise, his famous book on the cultural history of the Renaissance represented a current of nineteenth-century thinking that was based on Hegelian metaphysics, a system sheltering two ideas, that art has a certain autonomy and that history was growing in its values. In such Kulturgeschichte, the changing styles of art became an index of the spirit of the age, the Zeitgeist. Other streams of art history continued to be didactic and several prominent English authors stressed religious teaching in relation to art history. For example, Anna Jameson had a Protestant audience in mind when she opened her survey of religious iconography with an observation that papist "'hero-worship' had become, since the Reformation, strange to us ... as if it were antecedent to the fall of Babylon’ (Jameson 1890: 1; see also Holcomb 1983: 171).

Others celebrated the value of art history to comment on contemporary mores.
William Roscoe

(1753-1831), an autodidact interested in the impact of patronage, made extensive use of engravings and literary sources rather than visit Italy itself (Roscoe 1795; Compton 1960). His Life of Lorenzo de’ Medici contains a political and moral subtext, that culture was morally and politically beneficial and the role of commerce was to fund it. Roscoe's study of Renaissance Florence analysed this system, arguing that Lorenzo 'il Magnifico' had supported a number of initiatives 'from which Europe derives its present advantages', such as printing, geographical discovery, the established principles of taste, etc.

DEVELOPMENTS FROM THE 1880s

Connoisseurship, developed initially to establish a taxonomy of Renaissance painting, has continued to evolve and has broadened its range in the twentieth century.

Historians of ancient art have turned their attention to Greek pots, arguments about which reveal essential conflicts in art-historical practice. The connoisseurs, conscious of the absence of any other Greek 'painting', have established artistic

30 Gustav Waagen was appointed professor at Berlin in 1844; for comment on a proposed expansion in provision the UK in the 1960s see the editorial to the Burlington Magazine 103 (May 1961).

31 For an interesting angle on Burckhardt see Riisen 1985.
personalities and traced hands while the social historians have emphasized trade and
argued that the pots were worth less than the goods they carried. The answer to the
standard art-historical question: why did a style develop from one point in art
historical time to another, is one of immense theoretical complexity and has
dominated much twentieth-century art-historical writing. The formalists set out to
answer it. Formalism, based on the assumption that art is a closed discourse, reliant
on intuition and revealing an inner meaning, was established by Wolfflin
(1864-1945) and his followers and has become a powerful theme in twentieth
century historiography. A characteristic English school of formalism, shaped by
German philosophical arguments about the impossibility of objectivity, linked both
to the taste for post-Impressionist painting and early Italian painting and the first
English translation of Wolfflin's *Classic Art* (1903), was exemplified by Roger Fry
(1866-1934) (Smart 1966; Maginnis 1996; Falkenheim 1980). Formalists are now
more rarely heard, whether complaining about 'naive historicism', condemning
oversimplifications of cause and effect, or continuing to celebrate the role of the
artist as gifted innovator, confronted by the critic. 33
Increasingly, questions of psychology have entered the art historian's purview.
Freud’s own historical essays tend to be treated with caution since he uses Leonardo’s pictures to offer an analysis of Leonardo’s brain; however, his influence has been enormous and is growing (Wollheim 1973: 202-19; Damisch 1971-2; Schapiro 1956). Psychoanalytic accounts of art-historical phenomena appear from the 1920s. Aby Warburg is an example of an innovative figure whose influence has vastly exceeded his published output. 34 Other German-speaking scholars developed a school of writing especially directed at explanations of expression: for example, Max Dvorak (1874-1921) whose key interest was art history as the history of the spirit (Kunstgeschichte als Geistesgeschichte, 1928), Wilhelm Worringer (1881-1965) and Alois Riegl (1858-1905). 35 Some early twentieth-century scholars interested in expression started to reassess unfashionable schools such as the northern and Spanish artists of the sixteenth century, like Grunewald and El Greco, who themselves worked in an expressive vein. 36 This art-historical method depends on the concept of a Kunstwollen, an era’s collective, psychic state with respect to artistic creation, and an inherently obscurantist term. Gombrich’s influential book Art and Illusion takes a more mechanical line and argues for perceptual explanations for artistic understanding. 37
32 The key Wilfflin texts are Renaissance und Barok (1888); Die Klassische Kunst (1899); and Kunstgeschichtliche Grundbegriffe (1915). For Wilfflin see Iversen 1981.

33 An exception is found in the 'Preface' to Freedberg 1983: 'what I say about these artists derives almost altogether from my confrontation with the visual substance of their art'.

34 The key collection of essays is in Gesammelte Schriften (1932), to be read with Gombrich 1970 and Ferretti 1990 (1st Italian edn, 1984).


The direct challenge to formalism came initially from iconography, the earliest versions of which took an approach analogous to the establishment of definitions in a dictionary, identifying the subject matter of works and objects by certain signs. 38 Panofsky later formulated a new theory of iconography, and an adjunct methodology 'iconology', as a historical approach that conceives of the visual arts as part of the 'universe of culture' including other creative manifestations of human enterprise. 39 Iconology effected 'synthetic intuition', a way of examining the mentality which governs the self-reflexive viewing of
humankind, in groups. The artistic experiences of groups of humans have also been the concern of social histories of art based on ideas about class and the established means of production.

A more recent variant of social history of art raised questions about gender, not only in the sense of the practitioner (male and female artists) but as a filter for understanding production and for understanding meaning, as a context for both seeing and making. Marx never completed his proposed description of art in its social setting, although his materialist followers have attempted an analysis, working in two distinct waves (Williams 1980: 48). First, an older German-speaking generation led by Antal, who published little: 'it is the content of art which clearly shows its connection with the outlook of different social groups for whom it was created', 40 and Hauser (1951; 1965)41 who promulgated a sociological law connecting and explaining both aesthetic pleasure and the popularity of certain works and artists.

For Hauser, the way art changes was a direct consequence of social conditions. The second wave comprised a younger group formed in the 1960s and 1970s, with T. J. Clark as its leading voice. 42 According to Marxist theory, art exists as a phenomenon of the superstructure inextricably linked and dependent upon the activities of the
economic and social base; art therefore shows the direct trace of ideology; however,

art is not merely its reflection, nor is it its expression merely as a passive recipient,

but as an active force.

The human sciences offered art history a set of methods understood in the Anglo Saxon world as relying on a melange of French thinkers such as Althusser, Barthes, Foucault, Lacan (none of them art historians), in opposition to Anglo-Saxon empiricism. Such approaches assume that the world, as perceived by humankind, is an artificial construction rather than a natural phenomenon and they extend this principle to all human creativity, establishing meaning through a process using inversion and differentiation, defining humankind not by thought but by language. 43

38 Cesare Ripa, Iconologia (Padua 1611 is a full early edition); B. de Montfaucon, L’Antiquite expliquee et representee en figures (1717), for whom see Haskell 1993: 131-5.


40 Antal 1966: 2, an argument further developed in Antal 1948: 4.
41 For Hauser see Werckmeister 1984 and Orwicz 1985.


43 For adaptations of the Lacanian method see Bryson 1983 and Leeks 1996. 841

Semioticians understood visual motifs by analogy with language as signs operating
within systems, the meanings of which require decoding and have to be understood
as conventions, their relations one to the other arbitrary, not fixed in nature. 44

These kinds of histories do not result in explanation in a conventional, historical
sense and from some quarters they are dismissed as 'mere formalism'. 45 EPILOGUE

Finally, current methodological and institutional difficulties and issues in art history
need to be signalled. Academic art history is now a major international concern and
describes a host of differing (sometimes conflicting) interests. There is a struggle
over the definition of the art historian's material as there is over what constitutes the
practice of art history itself. Many art historians have acquired new methodological
tools developed over the last decades without necessarily subscribing to all the
conditional theoretical apparatus. It is likely that additional significant advances will
be possible in empiricist art history resulting from technological development,
especially as manipulated by museum curators and conservators. Discoveries made
by these means are likely to transform the sciences of attribution, classification, dating and the eliciting of meaning. On the level of theory, two basic schools seem to be developing: one which explains the form of the object through its analysis within one of a number of historical contexts, as determinants of its meaning; the second which stresses, often from quite wildly different standpoints, the autonomy of the text itself. This second group criticizes cultural history for its interest in finding in art objects equivalents to the ideas already formed about the 'spirit of the age' or about the patron and, in turn, it tends to be criticized for its lack of objectivity. In turn, its opponents are condemned for their claim to empirical objectivity. Social anthropology, dealing with ritual and performance and social structures, has become more influential, matching moves made by the Annaliste historians who have moved from merely quantifying to an understanding of values and attitudes via a study of linguistics and anthropology (Bull 1988).

The most recent trend is towards multiculturalism, that is, a dissatisfaction with a scholarly canon comprising the art of the West. Resistance to this approach to art history is voiced through concerns that such a shift of focus, away from the statues of the ancients, the frescoes of the Renaissance and the oil paintings of Rembrandt
and the masterpieces of cubism towards tribal masks, sand-paintings and Chinese manuscripts, is vulnerable to mere tokenism. The counter-accusation that the maintenance of the Western canon is intellectually indefensible is formulated in the aftermath of feminism, which has raised central questions of the role of 'artists'. For art historians to raise questions about gender is part of an inherently complicating process, for it creates and encourages histories by challenging the notion of a single paradigm. Gender issues do not equate precisely with feminism, although some argue that the materials of traditional and radical art historical analysis are irretrievably polluted by patriarchy (Pollock and Parker 1981). In keeping with a general mood of postmodern pessimism, serious philosophical scepticism has been voiced that histories of art can be anything other than histories of the literature on art (Thompson 1993). Some of the historical approaches rehearsed in this chapter claim exclusivity and truth, as manifestations of an attitude to life, not methods to
be selected as if from a menu. However, there is no evidence that any of them can claim mastery; there is and never has been a single true art history, only a range of


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-[1888] (1964) Renaissance and Baroque [tr. of Wolfflin 1888].


V V.2 Approaches


-(1976) The Pleasure of the Text, London. 870


-(1989) 'Taking Darwin even more seriously', in K. Hahlweg and C. Hooker (eds) Issues in Evolutionary Epistemology, Albany. 871


-(1959) The Logic of Scientific Discovery, London. (First published as: Logik der Forschung, Vienna, 1935.)

In spite of its awkward title, its esoteric content and the modest size of its subscription list, the French journal Annales: Economies. Sociétis. Civilisations began to reach a global audience in the 1960s. It has become, since then, probably the world's most talked about and most influential scholarly journal devoted to historical studies. In Germany and in Britain, the Annales approach to history encountered incomprehension and resistance for some time, but the triumph of Annales in American universities is indisputable, and this in spite of the fact that many of its American admirers cannot actually read the French journal and must remain
content with anthologies of articles, in English translation, taken from Annales. 3

Many books by Annales historians are now available in translation. Among these books there are celebrated doctoral theses, huge and little read, such as Braudel's Mediterranean or Le Roy Ladurie's Peasants of Languedoc; some exceptionally imaginative monographs, such as Febvre's Religion of Rabelais, Bloch's Royal Touch or Schmitt's Holy Greyhound; and outstanding works of synthesis as well, intended for a non-specialist audience, such as Bloch's Feudal Society or Lopez's Birth of Europe. 4

It was only in the late 1960s that the reputation of the French historians, established earlier in much of Europe, especially in Italy, at last began to penetrate the English-speaking world. In 1968 I persuaded a major American commercial historians and provides judicious analyses of these. My own chapter focuses on the journal itself. It may be read as complementary to Burke's book.

2 Erbe 1979. Erbe is especially astute in describing German resistance to Annales in the Cold War years. Erbe's monograph may well be the most careful and accurate assessment of Annales in any

3 Among a number of such anthologies, I would cite Ferro 1972. Another is Burke 1973.

4 Burke 1990 provides an up-to-date selective listing of the major Annalessle books in English translation. 873

All three of these books happen to be enormous, but that is not the chief obstacle for the translator. It is their style, supremely attractive in the original, which is exceptionally difficult to render into English. Unlike the ordinary, garden-variety kind of French historical writing, which is not particularly inventive and which proceeds, as does most academic prose, by means of clichés whose equivalents are easy to locate in English or in German, the language created by Febvre and Braudel
is idiosyncratic and poetic, full of archaic turns of phrase and of technical words lifted straight from the sixteenth-century sources. The effect is extraordinary, but it does not translate. Even the French reader is easily baffled by the oceanic

murmur of those thousands of pages, exotic, authentic and exquisite though they are.

Not all the books associated with Annales, alas, are masterpieces: there is also a considerable proportion of rather dull academic productions and, increasingly, piles of textbooks, coffee-table books, ponderous volumes produced by committees and collections of less-than-scintillating occasional writings the chaff, the spin-off, which reaches our bookshops and libraries because of the Annales label, much as ordinary and inferior wares bearing a famous couturier's trademark reach the market-place. 5

First rate or not, these books can present a distorted picture of what the Annales experiment has been about. They tend to be written, almost exclusively, by Frenchmen whose concern is with pre-industrial France. The reason for this is that many of the weighty volumes which find their way into standard bibliographies were produced as doctoral theses. These overweight volumes, each of which
represents ten to twenty years of hard labour in a French archive, display few of the qualities called for in Annales. They are rarely comparative or experimental and their style tends to be touched with the usual rigor mortis demanded of academic theses. It is also true that most of these theses depend exclusively on French archival sources. Reading them, one could reasonably conclude, with Peter Burke (1990: 99, 5 An example among many: the multi-volume, multi-author Histoire des femmes en Occident, first prepared for an Italian publisher, then translated into French and eventually into English. The summer 1993 issue of Annales devotes much space to this consistently mediocre in-house product. 874 108), that the horizon of the Annales historians is limited to three centuries of French economic and social history.

Are the annalistes not interested in modern, contemporary or ancient history? Are they not curious about the world outside France? Outside Europe? You could not guess it from most of the books which make their way into English, but the truth is that Annales, from its beginnings, has always been ferociously, aggressively global in scope, both in its recruitment of contributors and in its choice of topics to pursue.

Since the 1930s the journal has made a practice of commissioning articles and review essays from first-rate scholars not necessarily associated with the history departments
of French universities: Americans, Italians, Poles, emigres without academic posts,
sociologists, anthropologists or colonial administrators. The editors made a practice
of inquiring into topics not usually of interest to the Sorbonne, topics such as the
collectivization of agriculture in the Soviet Union, the origins of railways, the
function of Islamic universities, historical demography or attitudes towards death.
Those were not subjects in which ordinary academic journals expressed much
interest, until recently, when even the stodgiest of them began to imitate Annales.
I do not think of Annales as an academic publication among others. The special
quality Annales possessed until recently is not easy to define. I remember my first
encounter, in 1957 or 1958, with the thick, well-groomed volumes on the shelves of
the reading room at the University of California at Berkeley, where I was an
undergraduate student. I wandered in from the sunlight and the eucalyptus groves
to pick up the latest issue of the journal, which had just acquired its peremptory
new cover design. I came away, somehow, for better or worse, having found my
vocation as a historian.

At the time, the journal was just beginning to attract attention in Cambridge,
New York, Chicago and Berkeley, although it had been in existence for more than
thirty years already and long before that, before 1914, the future editors, Lucien Febvre and Marc Bloch, were already saying and writing pretty much what they would say and write in later years. The spirit of Annales is that of the early years of the century. If there is a single book, a single work of original scholarship, which embodies, more effectively than any other, the qualities venerated by those who admire the Annales kind of history, this book is Lucien Febvre's Philippe II et la Franche Comte, which was published in 1912.

The journal wished into existence by Febvre and Bloch in the 1920s did change in some important ways after 1945. It was to change again at various junctures in its history, each new departure spelled out in memorable manifestos written by Febvre and eventually, after 1956, by his younger alter ego, Fernand Braudel. Still, anyone who undertakes to read, or at least to sample, the journal in its entirety, traversing some sixty years of continuous publication, is bound to be struck not so much by the editors' successive enthusiasms for new problems to be solved and new techniques to be applied, but rather by their rock-solid fidelity to the objectives defined by Febvre, with remarkable constancy, from the early years of the century up to the last year of his life. These objectives were reaffirmed by Braudel, right into
Some conclusions may be drawn from these observations. First, that the famous
esprit of the Annales team was in large part the reflection of one man's mind. Lucien
Febvre was seconded for some years by his younger colleague, Marc Bloch, and
Bloch's contribution to the journal in its early years was important. But it was
Febvre who wrote the editorials, it was his style and his role as impresario and talent
scout which dominated Annales from the start and continued to do so beyond the
glave.
Febvre was something of an anarchist. He rebelled, while still a student, against
the cultural conventions of the nineteenth century and embraced revolutionary
trends in art, music, psychology and literature. He proclaimed his distaste for
everything bourgeois by which he meant everything prudent, timid, lacking in
passion and associated with the Sorbonne. In this, and in his fascination with non
Western cultures the product, after all, of 'peoples so far removed from
everything bourgeois' (ces peuples si peu bourgeois) (Annales 1948: 388), he belonged
to the avant-garde. In harmony with the experimental painters, composers and
novelists of his youth, he rejected established ways of doing history and ridiculed
academic historians as laborious compilers of the obvious.
From the time when he was a student at the Ecole Normale, in 1902, to the time of his death, in 1956, he pursued his vision of what history as a discipline could become in the twentieth century if it could only be rescued from the mortuary atmosphere of the university. It was Febvre's contention that historians had reached a dead end, that the books they wrote were mostly concerned with trivial surface effects (histoire evenementielle) and that they ought instead to find ways of reaching deep down into the hidden forces which shape the destinies of human beings. To recount political events in chronological order seemed to Febvre as limited and pointless an activity as would be an attempt to understand landscapes without being aware of the geological forces which shaped them. His own Franche Comte was, among other things, an eloquent demonstration of the uses to which geology and geography could be put by historians.

Febvre's strategy for wresting historical research away from those he ridiculed as 'needle-point specialists' included alliances with geographers, sociologists and anthropologists, with students of comparative mythology, with economists and linguists. Not only were his friends in these fields more willing to support his heretical inclinations, but they were engaged in forms of inquiry which, by
definition, favoured the search for long-term, hidden realities. It is really remark
able that Febvre, given his objectives, did not fall prey to the attractions of

Marxism, to which some of his friends and associates did succumb. As early as

1920, however, Febvre set a course away from any kind of ready-made scenario for

explaining everything. 6

Annales was to steer away from ready-made formulas. It was not to espouse any
doctrines. It was not to become a 'school'. It was to remain, as one of Febvre's

6 In his inaugural lecture at the University of Strasbourg, published in the Revue de Synthèse


earliest and steadiest allies, the sociologist Georges Friedmann, put it, 'un chantier
d'hommes libres' (Annales 1957: 4). Once it had rejected the old ways of doing

history, the Annales group kept searching for an easily communicable way of

defining their experiment. Febvre knew how to ridicule the Old Guard, the 'losers

of 1870'. He saw what was wrong, what was missing, in the books he reviewed a

history of cities, for instance, whose author wrote about buildings and plans without

mentioning human beings. But how was he to explain the kind of history he

envisioned, since it did not as yet exist, since it was only an experiment in progress?
Any attempt to reduce the Annales approach to a formula is bound to proceed from a misunderstanding. Yes, the emphasis, in the 1930s, was said to reside in the pursuit of economic and social history, as the very title proclaimed: Annales d'histoire iconomique et sociale. But Febvre was quite clear in his own mind that 'economic and social history' was an expression vague enough to defy definition and therefore especially useful for his purposes. 7

The essence of what the annalistes were after was summed up most succinctly by the economic historian Ernest Labrousse: 'l'histoire est a faire,' that is, 'history has yet to be invented' (cited by Pierre Vilar, Annales 1973: 165). To invent a history suitable for the needs of the twentieth century, that has been the consistent programme pursued by the Annales group. This was the goal, proclaimed in an editorial manifesto of 1946 in which Febvre celebrated the resurrection of the journal after the difficulties of the war years: an experimental history quick to respond to the needs of the present.

That the needs of the present, rather than a university committee, should dictate the choice of problems to be investigated was a consciously heretical position, deeply at variance with academic practice. Febvre went so far as to declare that he
could not countenance the notion of scholarship for scholarship's sake. The Rankean or Panglossian hope, according to which each newly unearthed fact, like a modest brick, would join others, until the shape of the edifice revealed itself at last, struck Febvre as absurd. It made more sense to him that one should engage the services of an architect before employing bricklayers. Annales was not to publish simply whatever new research happened to come its way, as is the usual practice of academic journals. Instead, Annales was to commission research into problems of particular utility and act as a clearing house, matching problems requiring solution with appropriate specialists. The goal was that of a 'managed history', 'une histoire dirigee' (Febvre 1965: 55).

In some ways Annales may be considered a period piece. In recent years it has become more ordinary, more academic, but throughout its long life it was doing for historians what brash new quarterlies, art galleries, salons and cafes were doing for artists and poets in the 1920s: providing a home base for rebels who banded together, united in their opposition to official culture. Not that the contributors to Annales, in the 1930s, and even less so in later years, 7 Febvre 1965: 19: 'II n'y a pas, a proprement parler, d'histoire economique et sociale.'

8 'Et disons: l'erudition pour l'erudition, jamais,' cited
were readily identifiable as rebels. They were mostly seasoned and middle-aged university professors, quite a few of them members of the narrowest elite imaginable, that composed of graduates of the Ecole Normale Superieure. Annales was founded in the wake of Febvre's appointment to the most coveted post in French higher education, a chair at the College de France, and Febvre made sure, eventually, that his chosen successor to head Annales, Fernand Braudel, would inherit the chair at his retirement, just as Braudel himself helped Le Roy Ladurie inherit the post in turn.

If, in spite of the indisputable elite status of its leadership, Annales had a reputation as a radical journal, the merit belongs entirely to the novelty of the ideas expressed in its pages, not to some imagined obedience to a political agenda, let alone a political party, although it is true that Annales was clearly on the Left, in a general sort of way, as long as Febvre dominated its editorial policy. Especially disturbing to conservative academics was the present-minded ness of the journal's editors, that is, their clearly expressed desire to influence events.

The defining moment for the journal and for Febvre himself came on the eve of the Second World War, when Febvre, it seems, broke with his
publisher and almost ended the journal's life over the issue of the proper response to the triumph of the Nazi and fascist regimes (see Schottler 1991). Taking sides against fascism was relatively easy, even if it meant publishing out of your living room. But to do it without being anchored in the Marxist constituency required rare independence of mind.Febvre identified the emergence of totalitarian mass movements as the most critical issue to face thinking men. It was therefore the duty of Annales to explain this phenomenon. 'Right next door to us', he wrote, 'a world has ended. A new world has taken its place.' To explain and therefore to counter this menace, one would have to develop new conceptual tools to replace worn-out theories, including Marxism: 'The old keys do not turn in the new locks' (ibid. 75).

In his search for new keys, Febvre turned to Franz Borkenau, a Viennese political refugee newly arrived in Paris. Trained as a historian, Borkenau was also intimately acquainted with both the Nazi movement and the Soviet system. He had been, until recently, a Com intern agent, working out of the Soviet embassy in Berlin, with full responsibility for the German communist youth movement. He would soon become known for his lucid reporting on the Spanish Civil War and for
his authoritative analysis of totalitarian ideology. While in Paris, Borkenau served as
a consultant to Febvre, pointing out the family resemblances between the Nazi and
the Soviet regimes in a way which was simply unthinkable for the European Left.

Borkenau's contributions to Annales, for all that, were somewhat pedestrian in
character. It was Borkenau's wife, Lucie Varga, who turned out to be perfectly
suited to the task of explaining the appeal of the new totalitarian ideologies. In her
Febvre found the ideal partner. She was a medievalist by training and, among other
things, an ethnographer, a linguist and a mountain-climber by avocation. It was
Lucie Varga who fashioned the keys Febvre was looking for, in a series of articles in
Annales. In these essays, full of intelligence and not in the least mindful of academic
conventions, she reported on her fieldwork among Alpine villagers, both Austrian and Italian. She brought her experience as a student of medieval religious cults as
well as Freud, Marx and Malinowski to her task, which was to understand what it
was that prompted young people to discard the world-view of their parish priests in
favour of the gospel preached by the black-shirted or brown-shirted purveyors of a
different sort of salvation (ibid).

Lucie Varga's meteoric passage through the pages of Annales provides a concrete
example of what Febvre was aiming at. He started by identifying an urgent problem.

Then he cast about for the person most likely to bring a fresh solution to it. The

historian, he believed ought always to begin with a problem, not with a set of
documents which may or may not turn out to be significant. The Varga studies were

concerned with popular belief systems (mentalités). Febvre's own Religion of

Rabelais would pose the question: Was it possible, within the belief system of the
sixteenth century, to be an atheist? This study was undertaken, originally, in

collaboration with Lucie Varga. It may even be possible to detect the germ of

Fevvre's thesis, namely, that Rabelais was not an atheist, in Varga's dissertation

(ibid.).

After the defeat of the Nazi and fascist regimes, the most urgent problems facing

Europeans, in Febvre's analysis, were those created by centuries of rapacious

colonial expansion in Africa, Asia and South America. Annales now began to make

room systematically for articles and reports on the world outside Europe. A related

problem which preoccupied the editors in the 1950s was that of a population

explosion in the Third World, hence the journal's increasing interest in historical
demography.9 In those post-war years, under Febvre's leadership, the little heretical
quarterly once known only to the cognoscenti experienced an irresistible ascension.

Its size grew by leaps and bounds, it came out more frequently, the pool of its contributors widened, as did its subscription list and its influence. 10

Febvre was able to participate in the general build-up of new research institutes initiated by the French government. He was appointed president of the newly created department of social sciences, the Sixième Section, within the old Ecole Pratique des Hautes Etudes. He established a Centre for Historical Research, headed by Fernand Braudel, and a number of other Centres, not lavishly funded, but capable of providing a modest institutional base for the Annales group. Alliances were established with the National Centre for Scientific Research (CNRS) and the National Institute for Population Studies (INED), among others. Annales now had a budget. Many of the historians and social scientists affiliated with Annales had opportunities now for permanent civil service appointments as researchers in the new institutes.

The Sixième Section was to grow prodigiously, especially under Braudel’s 9 The interest in the Third World was initiated by Febvre in the 1930s and institutionalized under Braudel’s editorship in a regular and increasingly important rubric under the heading of Ie monde sau
Europe.

10 Erbe 1979: 49. In 1946, 400 pages per volume; by 1958, 800; by 1960, 1,200 and six issues yearly; by 1970, close to 1,800 pages. 879

shrewd management, threatening to swallow the other Sections, until it was

eventually established as a separate institute, the Ecole des Hautes Etudes en Sciences Sociales, with its very own imposing glass-and-steel headquarters building on the Boulevard Raspail, the Maison des Sciences Humaines.

The resources were in place. It was time to move from prototype to production.

It is at this point that controversy arises among those who pay close attention to the history of the Annales experiment. There are those who prefer to remain loyal to the original Annales (Annales premiere maniere) and those who prefer Annales in its mature, fully developed stage, when it had become a highly publicized behemoth spewing out new journals and monograph series. This is when the annalistes began to achieve star-status. One now has to consult specialized and frequently updated bibliographies just to keep track of books and articles published each year about Annales, in several languages. A good illustration of the special place occupied by Annales in contemporary French culture is the reception accorded in the daily press, recently, to a serious monograph devoted to the work of Lucie Varga
The author, Peter Schottler, intimated, in passing, that Lucien Febvre had been in love with Dr Varga, in the 1930s. The daily press, papers such as Liberation, saw fit to devote space, under lurid titles (Lucien et Lucie) to a putative romance involving two middle-aged historians sixty years earlier. Clearly, Annales is news in France.

Whether one is pleased or irritated by all the publicity surrounding Annales, the question remains: Did Annates deliver? Did it change the way history is written, when it acquired the means to influence historians on a large scale, when it moved on from provocation and exhortation to become the centre of a vast publishing empire? Personally, I preferred the original, combative stage, when the journal was full of possibilities and excitement and before it became the official mouthpiece of an entrenched bureaucracy. Braudel, too, at the end of his career, felt that way. But for all that, I am not quite as ready to close the books on Annales as Peter Burke appears to be. I shall review the criticisms which have been directed at Annates since the 1960s before going on to describe the genuine achievements which, in my view, still make Annales stand out.

Perhaps the most commonly heard criticism has been that Annates adopted a
naive positivist stance, claiming, in effect, that only those things which can be counted are worth studying. This is certainly a valid criticism. There was a time, in the 1960s and 1970s, when Annates appeared to focus almost exclusively on topics which lent themselves to quantification, the study of historical demography (for example), of grain prices, of the tonnage passing through Atlantic ports. Counting on a large scale was a technique applied also to less obviously suitable problems, the measuring of religious fervour, for instance, on the basis of very large numbers of testaments, or the study of literacy or book production.

There is nothing wrong with all this, and, in almost every instance, the tireless number-crunchers could rightly claim that they were only carrying out orders, II Burke 1990: 106-7: 'the movement is effectively over' and 'this is rather like writing an obituary'.

Fevre's marching orders, which did, indeed, specify team work, the search for long-term trends and the systematic use of evidence such as testaments to get at the history of collective mentalities. The problem with the way the late founders' orders were carried out is that it was done mechanically. As Franco Venturi pointed out in passing, rather gently, the annalistes of the 1960s, in their enthusiasm for 'scientific history', went to absurd lengths, 'using a cyclotron to crack a nut' (Venturi 1971:
9-10) and issuing theoretical pronouncements of a comical nature: 'from a scientific point of view, the only social history is quantitative history..12 It is hard to escape the feeling that the normaliens who found permanent employment in the Sixième Section, after an apprenticeship in the Communist cells of the Ecole Normale, were conditioned to become doctrinaire followers and, as such, were the worst possible choice for carrying out the experiments envisioned by the founders.

It is worth noting in this connection that Annales is capable of self-criticism, on occasion. The fiftieth anniversary issue (1979) confesses, rather candidly, that the journal had been concentrating on methods for their own sake, in a routine fashion, while losing sight of its original mission. The editors were candid enough to admit that the journal was no longer what it had once been, that it was no longer dedicated to improving the world, as it had been under Febvre's direction, that the annalistes, no longer outsiders and critics, were securely integrated now and part of the academic establishment. This self-criticism did not go far enough, to be sure. What needed to be said is that if Febvre or Bloch, by some miracle, were given the opportunity to scan their journal's pages now, they would die laughing at the fractured jargon which passes
for French and the evident mediocrity of some of the contributors. 13 Some may wish to attribute the mechanical counting and the mechanical prose to the take-over of Annales by a new generation of salon-Marxists (Burke 1990: 24) but I do not believe that it was ideology alone which caused the Annales experiment to falter. I think that the problem is an institutional one, far too complex to describe in detail here, but, grosso modo, having to do with a new generation of normaliens who turned out to be civil servants first and historians last. When Annales had no paid staff and no offices and no more than 300 subscribers, the articles were signed Henri Pirenne, Marc Bloch, Georges Dumézil, Mircea Eliade, Roger Dion, Jacques Berque, Louis Gernet, Edouard Perroy in a word, signed by giants, by scholars with towering reputations who endowed the struggling journal with the capital of their own well-earned reputations. As Annales was becoming the thriving house publication of a vast research institute, much of the journal's content was produced by staff members who, in some cases, had not even published a doctoral thesis, let alone a significant 12 Adeline Daumard and François Furet in Annales 1959: 676: 'scientifiquement parlant, il n'est d'histoire sociale que quantitative'.

13 Case in point: Lucette Valensi, who was to join the editorial board eventually, writes
'l'explicitation du non-evenementiel suivant une perspective de longue duree.' In her pseudo-language, words like 'evenementiel' and phrases such as 'longue duree' are transformed into 'scientific' concepts.

Not surprisingly, she also speaks without inhibition of 'I' ecole des Annales' (Annales 1974: 1309).

work of scholarship. They began at the bottom, in the house that Braudel built, the Maison des Sciences Humaines, and moved, from cellar to attic, so to speak, working their way up through fierce office intrigues. They owed their entire reputations to their connection with Annales.

In spite of these changes, the price of success, one might say, Annales still retains a good deal of its original appeal. This is because of the extraordinary sense of continuity, of filial piety, of sectarian loyalty which holds the Annales group together. 'Les Annales continuent' was the title of Braudel's editorial, in 1957, implying that there was at least some question whether the journal could bring itself to continue to exist in the wake of Febvre's death, even though the vie matirielle of the journal was more secure than ever at this point. The continued spiritual presence of the founders is obvious in every issue of Annales. Bloch and Febvre are invoked at every turn, they are commemorated at every opportunity, and so are their peers and apostles. The editors' desire to change the journal's perspective as
little as possible is clearly expressed. Braudel's sense of his apostolic mission never faltered and his authority remained firm until the events of May 1968 (and the patron's temporary absence in Chicago) provided the opportunity for a little palace revolution which led to some sharing of authority and a Directoire style of governance which has since led to apocalyptic complaints about the empire's crumbling: l'histoire en miettes.

Actually, one need only study the composition of the editorial board over the years to see that Febvre's grip was hardly weakened, even after 1968. Twenty-five years after his death the board was still dominated by his closest comrades, some of whom had been in place since the 1930s. The choice of topics for research retained much of the old spirit. The 1960 volume, for instance, included studies of Yoruba farming practices, of Bosnian gold mines, of North African camels in the Roman period, of the Chilean economy in the eighteenth century, of technical experiments with mechanical harvesters in antiquity. In the same volume Jacques Le Goff published a first sketch of his study of medieval conceptions of time, Claude Levi-Strauss reflected on history and anthropology, Lucien Goldmann wrote about Chagall, Robert Mandrou on the concept of the baroque, Alexandre Koyre on
Newton, Galileo and Plato, Roland Barthes on Racine: altogether scintillating company and a roster which could have been chosen by Febvre. Perhaps it was,
since most issues of Annales are painstakingly planned, sometimes years ahead of their deadlines.
Ten years later the 1970 volume remains just as rich and suggestive. It certainly was not planned by Febvre, but it was the creation of an editorial board unchanged since 1957. Etruscan city-planning, Indian gods, contemporary French schools, Peruvian cathedrals, rural Algeria, Sudanese gold, Bedouins, fairy-tales, probability theory, Freud, the history of climate, the physiology of the brain, the student movement of May 1968: another Febvrian cocktail with the power to intoxicate. What other publication dared to combine so many disciplines, continents and cultures? And in so rigorous and disciplined a way?
It may be true that some of the current annalistes lack both imagination and truly first-rate scholarly credentials, not to mention a sense of style, but even so, the journal as a collective enterprise continues to experiment, to invite experiment, to encourage collaboration between historians and social scientists, to engage the historical imagination in every conceivable way. It may be the case that the editors
were more inclined to take risks, twenty years ago, by publishing, for instance, a wild but suggestive essay by a central Asian folklorist on the shamanistic significance of the Trojan Horse legend, or by publishing, under the rubric of 'comparative iconometry', an attempt to prove that the basic rules of artistic representation were invented in the Stone Age and would not change thereafter (see Annales 1963). One had the sense, in those years, that Annales was always ahead of everyone else, the first to make death, sexuality or climate objects of systematic historical study, for instance. Of late, Annales is more likely to feature articles on topics already discussed exhaustively in print elsewhere. That seemed to be the case when the journal hosted a conference on 'Vichy and the Jews' and published the proceedings in a special issue, with a somewhat defensive introduction (1993). For that matter, ten years earlier, in 1983, Fernand Braudel had already reached the sobering conclusion that Annales had given up on its original mission. In a letter dated 23 February of that year he wrote to explain why he had removed himself from all editorial functions at Annales in 1972, even though his name remained on the cover. His successors had disappointed him. They had blunted and diverted the journal's ancient course. For Braudel, Annales had lost its raison d'etre when it ceased being
an avant-garde journal: 'une revue d’avant-garde, donc de risque, et probablement herétique'. The new Annales was trendy, following fashion rather than leading it.

At the same time, the editors were in danger of departing from the philosophical perspective which had been 'essential since the journal's creation', namely the ambition to write total or global history by reconstituting 'all the social realities' which are in the background of any historical problem. (From an unpublished letter in the possession of Mrs Braudel.) It would be difficult to take issue with Braudel's authoritative assessment. The journal is certainly no longer an avant-garde quarterly, no longer self-consciously provocative and heretical. And it does seem to have given up on some of the more ambitious goals set by Febvre early in the century. One can point to articles, even to entire issues, already twenty years ago, which seem less than inspiring, but the basic ingredients of the Annales tradition remained present, in some sense.

At the most visible level this tradition manifests itself in the self-conscious engineering of each issue. It will contain, usually, two or three substantial études, finished articles based on new and usually exhaustive research. A separate category of travaux en cours reports on work in progress. Permanent rubrics such as notes...
critiques and comptes-rendus cast a wide, knowing and
highly critical glance at what is
being published in selected domains. The reviewing is
coherent and follows
thematic lines. Unlike those journals which simply review
new books sent to the
editor those books which happen to fall within its
specialized area of competence,
medieval history, say, or Middle Eastern Studies Annales
assumes competence
over absolutely everything. Of necessity, the journal's
reviewing policy has to be highly selective. A topic is chosen and the critic or
critics go on to review both books
and articles in whatever languages may be relevant.

For instance, tucked away in the category of Notes Breves
et Prises de Position, in
the 1965 volume, we find a characteristic series of
critical notes held together by the
topic 'Germany and its Neighbours after 1914'. In six
closely packed pages the
reviewers romp through an article in the Historische
Zeitschrift, another in the East
German Zeitschrift fur GeschichtwissenschaJt, a third in
the Ceskoslovensky Casopis
Historicky (on the failure of Austrian social democracy).
The reviewers take up an
East German essay on the nature of the Nazi regime and
blow it to smithereens: 'the
author asks who was behind the Nazi system of control and repression. He replies,
simply, big capital.' The next item is a book published by
the Oxford University
Press in 1963, Jiřen Gehl's Austria, Germany and the Anschluss. 'This work by a conscientious beginner will be of very little use to specialists' is the verdict and the reviewer never forgets that he is writing from a certain perspective, that of a Febvrian hostility, not to diplomatic history as such, but to diplomatic history as a lifeless routine in which ministers, ambassadors and other public figures 'spring up and disappear from the stage like the marionettes of old-fashioned diplomatic history'. Speaking of another article in the Ceskoslovensky Casopis Historicky, the reviewer notes that the article's author waits until the last paragraph to ask the question which ought to have served as his guiding theme from the start.

What is striking about the reviewing policy at Annales, and this has been so since the beginning, when Febvre and Bloch did most of the reviewing, is that the judgements expressed, often rather brutally, about the published work of other historians are part and parcel of the Annales philosophy. The reviews often start by defining a problem, explain why the topic is an important one and then go on to do considerably more than bring a book to the readers' attention: they criticize and teach, always ready to show in what sense the book or article under review falls short of meeting the criteria for what historical research
and historical writing ought
to be, according to the editors of Annales and their
friends and sympathizers. This
pedagogical aspect of Annales criticism can of course be
very irritating if you happen
to be on the receiving end. Sharp reviews were a speciality
of Marc Bloch's and the
tradition was kept alive in the journal, although that
quality too has become
attenuated in recent years. 14
The spirit of the early Annales was reverently maintained
in the Braudel years,
although the journal was constantly being refashioned to
remain open to new
developments, new opportunities, new interests. Established
rubrics such as Le
Monde sauf l'Europe, Debats et Combats, Enquetes en Cours
were joined by newer
ones such as Frontieres Nouvelles, Domaines Contemporains,
Inter-Sciences, Temps
Present, Mentalites. Side by side with categories meant to
be more or less perma
rent, increasingly, the editors put together special
issues on topics such as Famille
14 For an example of Bloch's sarcasm, see, for instance,
his review of Louis Battifol's La Fortune de
Richelieu. Bloch writes: 'La fortune de Richelieu: beau
sujet, assurement. Ayons le courage d'ajouter:
apres l'étude de M. Louis Battifol, presque autant
qu'auparavant, sujet tout neuf' (Annales 1938: 459). 804
THE ANN ALES EXPERIMENT
et Societe, Histoire et Psychanalyse, Histoire et
Sexualite, Histoire et Environnement or
Histoire Non-Ecrite for instance.

'Pour une histoire anthropologique', in the 1974 volume, is a good example of the effort made by the editors to provide a reasoned foundation for cross-disciplinary explorations. I write 'reasoned foundation' so as to avoid the word 'theory'. The founders had no use for theory. As the sociologist Georges Friedmann was to recall,

in 1953, on the occasion of Lucien Febvre's 75th birthday, it was their healthy contempt for theory, in the 1930s, which made the Annales group so attractive to those who had their fill of theorizing in the Marxist camp. Instead of the 'ratiocinations, the polemics, the scholastic disputes, the suspicion, the endless critiques in the absence of any attempts at originality', they found, in Febvre's camp, 'a spirit of research and cooperation'. 'We were in need of a team spirit,' wrote Friedmann, 'and we wanted no part of the esprit de systeme which prevailed among sociologists' (Annales 1957: 4).

Friedmann himself, Charles Moraze and Fernand Braudel constituted the core of the editorial board throughout most of the journal's history. They acted as a Board of Censors, keeping a watchful eye on the enthusiasms of the younger generation and, occasionally, they exposed the fragility of these constructs. Moraze and Friedmann, among others, prevented Annales from becoming a
school, weighing

in, from time to time, in editorial essays of a
philosophical kind and providing a
discrete corrective move of the tiller when the vessel
showed signs of drifting into
the Sargasso Sea of theory.

A fine example of this kind of intervention is Charles
Moraze's essay on
'L'Histoire et l'unite des sciences de l'homme' in the 1968
volume. Sensing the
danger Annales was in of becoming an object of idolatry
and wishing to puncture
excessive claims made for the uniqueness of the Annales
experiment, Moraze
explained that 'the greatness of cities, their decadence...
the poison and the
vicissitudes called forth by prosperity, the ways in which
anxiety can produce both
vanity and wisdom, such considerations are to be found in
Herodotus.' Lest
under-educated technocrats mistake Febvre's call for
'economies, societes,
civilisations' for a new formula ready to be patented,
Moraze, who knew the
sources of Febvre's thought, his ame de papier, wisely
reminds his readers that 'no
history worthy of the name was ever written in which the
author neglected to
consider societies, economies and psychologies'. In a
word: the historian who
writes for Annales belongs to a tradition of philosophical
history which goes back
to the very beginning of rational thought. The social
sciences, writes Moraze, are

'anchored in the encounters between history and philosophy' (Annales 1968: 233).

This is a formulation which would have made sense to Montaigne, four centuries ago, but it may be less meaningful to those who wish to believe that history is a science only recently developed in the laboratories of the Boulevard Raspail. The mental habits fostered by academic Marxism are not easily compatible with the humanist outlook of Moraze, Friedmann, Braudel, Febvre or Bloch. This, it seems to me, is the chief source of tension between one generation of annalistes and the next. It was the special merit of Fernand Braudel to bridge the gap between the cultivated and cosmopolitan Old Guard and the smooth technicians under his command.

Braudel was the keeper of the flame. He observed the directives laid down early in the century for the good management of the journal and of its adjunct activities: research seminars, conferences, monograph series. He preserved the spirit of those early years, making sure that history, a living history, as Febvre always insisted, reigned supreme within the family of the social sciences, fraternizing with the other disciplines, borrowing their methods and perspectives, but remaining in charge.

The qualities which made Braudel so effective were those of
an apostle, of the man

who had absorbed the teachings of his master so completely
that the spirit of Lucien

Febvre lived on in Fernand Braudel. He made an art of his
down-to-earth, Third

Republic, up-from-the-ranks style, dismissing the species
homo academicus in

residence at the Sorbonne in pungent, unprintable,
one-word judgements. He

welcomed young disciples in a manner both lordly and
informal, sweeping them up

in his embrace and addressing them with an avuncular tu.
He reminisced at every

opportunity, evoking his long and close friendship with
Febvre, sealed in the course

of a slow passage across the South Atlantic, aboard the
steamship Campana, in

November 1937. His published tributes to Febvre,
especially the luminous essay

which, under the title of 'Presence de Lucien Febvre',
serves as an introduction to

the Festschrift of 1953, are the most authentic guides to
the spirit of Annates.

Reading those pages, more than once, and listening to
Braudel talk he talked

like a book, his lectures, stenographically recorded, could
go straight to the printer

it was clear that his words preserved the style of a
bygone era, the elegant, slightly

archaic style of the French intellectual circa 1930.
Listening to Fernand Braudel one

could sometimes hear Lucien Febvre in the background, not
Febvre alone, but the
sound of that belle époque, already a historical memory, but alive, still, by special dispensation. Braudel understood that the substance of the Annates achievement was not to be found in the adoption of clever techniques, but in the personal credo Febvre shared with Bloch: a passion for scholarship framed by an equally deep involvement in the affairs of this world and close friendships with other 'esprits risqueurs et originaux'.

'To divide one's life into two parts: to assign one part to one's work, performed without love, and to reserve the other for one's deepest needs: that is abominable,' wrote Febvre, explaining his passion for his metier, which he could not separate from his deepest convictions. 'I am very much alone. I am working,' he wrote on more than one occasion. Ceaseless work, enforced solitude, the condition, in a word of the scholar's life. But when he was not at his desk, you would find Professor Febvre in brilliant company, motoring across France in a venerable Bugatti driven by Leon Werth and part-owned by Saint-Exupery. Braudel's tribute reminds us of the large and exceptionally diverse and talented crowd of Febvre's friends, those to whom he remained 'romantically loyal' (Braudel1953: I, 1-16).

This was more than a group of colleagues. It was a good sample of the French
intelligentsia, including poets and politicians, psychologists and philosophers,
brought together in part by their remarkable natural abilities which had catapulted many of them into the Ecole Normale in their youth, regardless of their social origins and, in part, also, by a common attitude towards the grave political issues of their time. It was this attitude, opposed to all chauvinisms and deeply involved in the defence of the common man, in the spirit of Jules Michelet and of the anarchist Elisee Reclus both of whom figure prominently in the construction of Febvre's ame de papier it was this, and the sheer intelligence of their writing which passed like an electric current to reach young historians marooned in provincial universities as far away, spiritually, as Sardinia. These are the qualities, perhaps muted now, which explain the triumph of the Annales experiment in a world recovering from the totalitarian nightmares which came so close to destroying the souls of western Europeans and still hovered over the East. Watching Fernand Braudel preside over the annual meeting of economic historians sponsored by the Communist municipality of the Italian city of Prato, watching the Communist Prince Doria, then deputy mayor of Genoa, standing elbow to elbow with a Braudel protege from Warsaw, the medievalist Bronislaw
Geremiek, who was on the way to prison and a leadership role in the Solidarity movement, one sensed that the vita contemplativa was never far removed from the vita activa in these quarters.

It was the great merit of the Annales group to enlarge the horizon of French historians, notoriously monolingual, by publishing the work of foreigners, both in the journal itself and in the various monograph series directed by Febvre, by Braudel and others after him. Italian, Polish or American historians in tune with the Parisian group became part of the French intellectual scene. Their books were published in translation, while French historians connected with Annales found their way into foreign journals, routinely, and the lists of publishing houses such as the Cambridge or Chicago university presses swelled with the names of French authors.

In the last twenty years, even while the tone of Annales has changed, and while the annalistes have come in for some tart criticism, the influence of the French group has grown to the point where Annales tends to assume pride of place in introductory methods courses in American graduate schools and even conventional textbooks include sections on mentalitis. Social history with an anthropological twist in the Annales manner has become one of the most highly prized genres of historical
writing. This is no ephemeral fashion. That is the point made in these pages: that

the Annales experiment is with us for good. It has been in tune with our century.

Whatever reservations one may harbour vis-a-vis this or that aspect of Annales, it

remains true that a complete set of the journal is an indispensable tool for

demonstrating the variety of twentieth-century historical practice and for showing

how closely historians and social scientists mirror the tensions and the changes of


Stanford.

Erbe, M. n 979) Zur neueren französischen Sozialgeschichte, Darmstadt.


New York.


The value and extent of Marx’s influence on modern historiography are rarely denied, even by those who reject his economics, politics and philosophy. Yet the precise nature of Marx’s impact on later Marxist historians or on historians in general is rather more difficult to specify. Too often Marx’s theory of history (‘historical materialism’) is reduced to a general emphasis on the importance of class struggle or on the role of the ‘economic factor’. It is impossible in the space available here to provide a comprehensive survey of Marxist historiography, which would virtually amount to writing a history of the world. Instead, this chapter examines Marx’s major claims about social structure and historical change, explores how and to what extent Marxist historical writing differs from orthodox historiography, and offers a general assessment of the Marxist approach. MARX AND ENGELS’S ‘HISTORICAL MATERIALISM’

One problem in specifying Marx’s influence on later historians is that Marx and Engels themselves employed a number of different historical approaches and offered a variety of specific historical interpretations which were by no means necessarily mutually compatible. In general, as Fleischer has shown, Marx and Engels worked within at least three overarching historical outlooks: the anthropogenetic, the
pragmatological and the nomological. In their early works, Marx and Engels saw

history in Hegelian, or 'anthropogenetic' terms. Here history is seen as the

overarching, dialectical progression through which humanity comes to its full self

realization, passing through a necessary negative phase of self-alienation and social


2 Hobsbawm 1972. As Marxists have been keen to point out, 'economic determinism' is not a sin

which has been confined to Marxist historians: Hii111968: 21; Thompson 1971: 78; Genovese 1972: 319.

3 On the contradictory nature of Marx and Engels's legacy, see Gouldner 1980. 889

atomization before achieving a fully human, free and rational community.4 In their

works of the mid-1840s, such as The Holy Family, The Condition of the Working

Class in England and The German Ideology, Marx and Engels then shifted to a

'pragmatological' outlook, one more in line with orthodox notions of historical

agency. Here the anthropogenetic conception of social development as a logical

unfolding towards some particular goal is replaced by a view in which history is seen

as 'the outcome, more blind than the result of any tendency to a specific goal, of the

actions of individuals and of groups impelled by their needs in the situations in

which they find themselves'.5 Jon Elster has even claimed
that the works of this
period are characterized by a methodological individualism
(the belief that all social
phenomena are explicable 'in ways that only involve
individuals', their properties,
goals, beliefs and actions), although it should be stressed
that Marx and Engels
always insisted that 'real living individuals' were themselves the products of 'given
historical conditions and relations'. 6 Finally, whilst never explicitly abandoning the
pragmatological outlook, Marx and Engels's later works, such as Capital (1867) and
Anti-Duhring (1878), also adopted a 'nomological'
perspective in which historical
development is seen as analogous to a natural process taking place in accordance
with 'inner hidden laws' which it is the task of the historian to uncover. 7 Here the
emphasis on human agency of the pragmatological outlook is replaced with a
structuralist approach which sees the development of the economic formation of
society as 'a process of natural history' and in which individuals are presented as
'the personifications of economic categories, the bearers of particular class-relations
and interests'. 8 This outlook was generalized by later Marxists into the philosophi
4 Marx and Engels 1975-6, II: 476, III: 172-4, 395, 419-42, 463-72, 475-6, 485, 491-2, 499;
5 Fleischer 1975: 13; see also Marx and Engels 1975-6, IV: 93, 298, 503; 1982: 12; V: 36-7, 39-41.


This emphasis on history as a law-bound process was the basis of Soviet-Marxist historiography, see Acton 1990: chs 2 and 3.

8 Marx 1976, II: 92; Althusser and Balibar 1975: 180; Burris 1987. The debate between Miliband and Poulantzas on how to account for the class nature of the capitalist state provides a classic instance of the
clash between the pragmatological and the structuralist concepts of agency. See Miliband 1973;

pragmatological emphasis on agency formed the basis for E. P. Thompson’s attack on structuralist

cal system of 'dialectical materialism', but it should be stressed that such dialectical

materialism was neither chronologically nor logically prior to the empirical social

tory developed by Marx and Engels in the mid-1840s: historical materialism is

not an 'application' of dialectical materialism. 9

Even in their analyses of particular historical conjunctures, it was inevitable,

given that they were writing over a long period of time and in a variety of historical

circumstances and literary genres, that Marx and Engels would produce a range of

differing and even contradictory historical interpretations. There is, for instance, a

contrast between the account of the transition from feudalism to capitalism offered

in works such as The German Ideology (1845-6) and the Communist Manifesto

(1848), where Marx and Engels focus on the rise of towns, trade and an urban

bourgeoisie, and the increasing emphasis on the transformation of agriculture and

the expropriation of the peasant producers contained in the Grundrisse (1857-8)

and Capital. 10

Yet, despite the ambiguous and often contradictory legacy bequeathed to later

Marxists by Marx and Engels, and even though, as we shall
see, Marxist historians often disagree violently with each other about specific issues, it is possible to identify a distinctive school of Marxist historiography. If, as E. P. Thompson (1978: 236) argued, the methodology and epistemology of historical materialism do not differ from the orthodox historical procedure of formulating hypotheses which can be tested against empirical evidence, Marxist historians can be distinguished from their non-Marxist colleagues in terms of their common vocabulary and concepts, and their shared body of interests, questions, hypotheses and historical emphases.

In The German Ideology (co-written with Engels in 1845-6) and the 'Preface' to A Contribution to the Critique of Political Economy (1859), Marx set out a comprehensive account of social structure and of historical change. For Marx, all social life is based upon the material production necessary to satisfy humanity's subsistence needs. This process involves the transformation of specific raw materials by means of particular instruments of production, human labour-power, and scientific and technological knowledge within a given specific technical division of labour, i.e. by means of society's 'productive forces'. "A particular level of development of these productive forces forms the basis for specific 'relations
of production', i.e. relations between people (as in the case of the class relation between an employer and his employees), or between people and the productive forces (as in the case of the

9 Stalin 1951: 5; Dorpalen 1985: 35-45; Cornforth 1968: 126; Mejkle: 1985: 1; Croce 1981: 3, 7-9;


11 Marx and Engels 1975-6, V: 41-2; Marx 1971: 19-23; Marx 1976-81, I: 204-90; 1974: 560,

699; McMurtry 1978: 55; G. A. Cohen 1978: 32. 891 employer's ownership of a factory). These relations of production, or property
relations, determine people's access to the productive forces (for instance, the wage
labourer's access to the productive forces is dependent upon being employed by the
capitalist) and to the products of the labour process (in the form of wages in the
labourer's case, or as the ownership of those products which are to be sold for profit
Although Marx's own terminology was by no means consistent, Marxists usually refer to a combination of specific relations of production with a specific level (or levels) of development of the productive forces as a 'mode of production', each mode of production being defined by its relations of production (feudal, capitalist, etc.).

For Marxists, the class relations of modes of production based on private property (as opposed to the communal property of primitive communism) are necessarily 'exploitative' since they involve the appropriation of specific forms of 'surplus labour' from the producers by a class of non-producers, as in the feudal landlord's appropriation of rent from his peasants. Such exploitation inevitably generates class conflict as the producers seek to limit the level of exploitation and the non-producers seek to maximize it.

Marx and Engels frequently claimed that society's relations of production 'corresponded' to the level of development reached by its productive forces, a claim which they illustrated for each stage of historical development, from primitive communism, through the ancient, Asiatic, feudal and capitalist modes of production. As society acquires new productive forces, a transhistorical tendency which Marx and Engels largely took for granted is eventually reached.

13 Marx 1976-81, I: 313, 324-5, 345-8, III: 763-5, 917-50; Marx and Engels 1975, V: 409. The concept of exploitation has been the subject of a highly abstract debate amongst Marxist theoreticians. See Roemer 1982, Lukes 1985 and M. Cohen et al. 1980. For bibliographical guidance see Geras 1985 and Carling 1991: chs 5 and 6. If, as Croce and Roemer have emphasized, the notion of exploitation implicitly involves the concept of some alternative, non-exploitative social arrangement, the concept of 'exploitation' as possessing an objective, measurable existence becomes a rather more problematical idea than Marx and Engels themselves realized. See Croce 1981: 127; Roemer 1989; Dalton 1974 (and see the subsequent debate, in American Anthropologist vols 77-9); Rigby 1992a: 214-19.


where its relations of production lag behind its developing productive forces and become fetters upon them. In order that the productive forces may continue to develop, society's antiquated relations of production are cast aside and new relations of production brought into being, a process accompanied by social revolution, such as the bourgeois revolutions which marked the triumph of the capitalist class over the feudal aristocracy. 22

If society's productive forces form the foundation for its relations of production, then, in turn, these relations of production form the 'economic base' for society's legal, political and ideological 'superstructure'. 23 The state and forms of social consciousness are 'determined' or 'created' by its relations of production or, more broadly, by its mode of production. The state and ideology are thus said to 'spring from', 'correspond to', 'reflect', 'echo' or 'express' social relations. 24 More
specifically, the state usually serves to defend the power and common interests of the propertied/s whilst specific forms of social consciousness are determined by the interests and position of particular social classes. 26 Marx and Engels thus offered a three-tier model of social structure (the productive forces; the relations of production; and the political and ideological superstructure), and provided a 'functional explanation' of the relations between these three levels. A functional explanation is one which accounts for the existence of some particular arrangement or process in terms of its beneficial effects for something else, as when the long neck of the giraffe is explained by its advantages for the giraffe's survival and reproduction or the rain-dance performed by the Hopi Indians is explained by its tendency to promote social cohesion. 27 For Marx, society's relations of production are functionally explained by the development of the productive forces: 'in order that' society will not be deprived of the benefits of the growth of the productive forces, relations of production corresponding to the new level of development of social productivity have to be brought into existence. (Marx 1973a: 197; 1971: 21). Similarly, Marxism offers a functional explanation of 22 Marx and Engels 1975-6, V: 33-4, 52, 74, 82; VI: 212, 33; Marx 1973a: 106-7; Marx 1974b:
The metaphor of base and superstructure is also implicit in Marx and Engels's critiques of the Hegelian 'inversion' of human consciousness and social activity. See, for example, Marx and Engels 1975-6, V: 30, 36, 61, 107-9,126,159.

Marx 1971: 20-1; Marx and Engels 1975-6, V: 53, 55, 57, 89, 329, 355-6, 373. The metaphor of base and superstructure is also implicit in Marx and Engels's critiques of the Hegelian 'inversion' of human consciousness and social activity. See, for example, Marx and Engels 1975-6, V: 30, 36, 61, 107-9,126,159.


25 Marx and Engels 1975-6, V: 52,90,92,329,355-6,359,361; Engels 1968: 160; Marx 1972a:
28,30,32,35,81,102,121-3; Marx 1972a: 28, 30, 32, 35, 81,102,121-3; Marx 1973a: 137; Marx

25 Marx 1971: 20-1; Marx 1973a: 95,100,109; Marx and Engels 1975-6, V: 36-7,74,159,183,
250,438,462; Marx 1974b: 540; Marx and Engels 1975: 401; Engels n.d.: 23; Marx 1972b: 37-8, 40;
1972a: 33, 48.


the political and ideological superstructure in terms of its benefits for society's class relations which the state and ideology help to stabilize and to legitimize. 28 MARXIST MSTORIOGRAPHY
In accordance with the theoretical legacy bequeathed to them by Marx and Engels,

Marxist historians have tended to concentrate their attention on a number of key issues: on tracing the growth or lack of it of society's productive forces; on characterising particular societies in terms of their dominant relations of production; on exploring the extent and nature of class conflict; on explaining the crises of particular modes of production and the transitions between them; and on establishing the relationship between class relations on the one hand and political power and social ideologies on the other.

Of all of Marx's historical theories, it is his claim for the social primacy of the productive forces which, despite its defence by Marxist theoreticians from Kautsky and Plekhanov, through Lenin, Trotsky, Bukharin and Stalin; to Cohen, Loone, Shaw, Callinicos and Sayer,9 has proved least fruitful for later Marxist historians. 30 This is not surprising, since even within Marx and Engels's own works there is a contrast between their general, programmatic statements, such as Marx's '1859 Preface', and their actual analyses of specific historical periods. The former tend to emphasize the primacy of society's inexorably developing productive forces in explaining historical change; the latter recognized that, under
the impact of particular relations of production, society's productive forces could stagnate or even regress and thus laid a far greater stress on the role of class relations and class struggle in bringing about social change. 31 Indeed, certain Marxists, unwilling to saddle Marx with views which they themselves cannot accept, have even attempted to deny that Marx ever claimed a social primacy for the productive forces in the first place. 32

Far from emphasizing an inexorable tendency for the productive forces to expand and develop, Marxist historians have taken to heart Marx's warning that it is wrong.

29 Kautsky n.d.: 120-37, 144-5, 161-71; 1988: xxxviii-ix, 227; Plekhanov 1972: 123-33, 147,
Bukharin 1969: 120, 134, 140,249,257; Stalin 1951: 33-56; Cohen 1978: ch. 6; 1988: chs I, 5,6,8,

30 For the attempt by the archaeologist V. Gordon Childe to apply this approach to historical development, see Chi Ide 1947, esp. chs 2 and 7; 1941: 6; 1954: 23-6; Green 1981, esp. 78-83.

31 Marx 1971: 20-1; Miller 1991; Adamson 1980; Rigby 1987: 28-55, 144-60; Katz 1989: 3-4,
173-83; Lekas 1988: 105-6, 130, 153 and ch. 9, passim. For an attempt to reconcile these two approaches, see Miller 1981; Miller 1984.
McCarthy (1978: 24), though not a Marxist himself, also rejects the 'technological determinist' interpretation of Marx. 894

to apply to all modes of production the laws of development specific to capitalist society. Unlike capitalism, where there is a powerful, indeed historically unprecedented tendency for the productive forces to develop, all pre-capitalist modes of production were, as Marx himself argued, inherently conservative. 33 In practice, it is the control which society's relations of production exercise over its productive forces and the class struggles which result from particular relations of production, rather than the autonomously developing productive forces, which enjoy pride of place in Marxist accounts of change and crisis within, and the transitions between, particular modes of production. 34

Thus, for Walbank, it was precisely the failure of the productive forces to develop which underlay the decline of the Roman Empire. The level of the productive forces in the late Empire was essentially the same as it had been in the Greek world. Yet, from the second century AD, an Empire on the defensive faced the mounting costs of defending its frontiers, paying its bureaucracy, feeding Rome and so on. Once the Empire ceased to expand, the army was
no longer a source of profit to the state but a burden which had to be supported by the population: the inevitable result of the Pax Romana was legalized extortion. In the short term, as under modern fascism, rulers such as Diocletian (284-305) turned to increased state regulation and control in an attempt to maintain a social system which was in crisis. In the long term, this ever more top-heavy political superstructure, with no adequate economic base of its own, was doomed to failure. The key to this failure of the productive forces to develop was the prevalence of relations of production based upon slavery, which deprived slaves of the incentive to innovate, induced a contempt for all forms of labour amongst the propertied and reduced much of the population to the edge of subsistence, thereby diminishing total demand and limiting the possibilities of economies of scale (Walbank 1946: 22; 1969: 40-80, 109-10; Engels 1968: 145). Thus, despite the obligation of historians in Stalinist regimes to explain the end of the Roman Empire by the universal law of the expansion of the productive forces (Oliva 1962: 171ff.; Gandy 1979: 29), most Marxist analyses of the ancient world have stressed the failure of the productive forces to develop and have given a historically specific explanation of such
stagnation or regression in terms of society's relations of production (see also Konstan 1975: 149).

Robert Brenner's account of the crisis of feudalism also rejects any inherent tendency of the productive forces to develop and presents feudal social relations as a powerful brake on the growth of social productivity. For Brenner, feudal relations of production between peasants and lords inhibited agricultural innovation and thus generated the tendency towards over-population, declining living standards and demographic crisis which Malthusian or neo-Ricardian historians see as typical of feudalism. Unlike capitalism, which not only permits but, through competition on the market, positively encourages productive advance, feudalism offered little stimulus to investment or innovation. On the one hand, peasants lacked the resources or incentives to innovate. On the other,
landlords, with the extra economic coercive powers of serfdom and the manor available to them, were able to increase their share of the social product by enlarging their share of total production, through raising rents, tallages and entry-fines, rather than through increases in productive investment. In Marx's terms they resorted to 'absolute' rather than 'relative' surplus labour. Similarly, for Genovese, the 'immanent contradictions' of the slave-economy of the American South meant that it too was destined for crisis, its low-level of labour productivity, lack of capital formation, limited home market and the restrictions which it imposed on the vitality of the mercantile and industrial bourgeoisie all retarded economic development and paved the way for political secession and, eventually, for military defeat (Genovese 1965: 3, 8-9, 43-61, 158).

Marxist historians still see a tendency towards productive advance as characteristic of capitalism, but they have abandoned such expansion as a universal historical law and have emphasized instead the need to identify the historically specific tendencies and laws of pre-capitalist modes of production. As Perry Anderson (1977: 204) put it, far from vigorous forces of production bursting triumphantly through retrograde forces of production,
'forces of production typically stall and recede within the existing relations of production. ... The relations of production generally change prior to the forces of production in the epoch of transition and not vice versa'. What remains of Marx's claims is the idea of each mode of production being bound for crisis through its own inherent tendencies and, in particular, of social crisis as emerging from the clash of productive forces and the relations of production. It is such internal problems and endogenous causal factors, rather than external forces, which create social crisis and transition. The Roman Empire, for instance, did not collapse because of the barbarian invasions but rather from its own internal contradictions. Paul Sweezy was, therefore, rather unusual amongst Marxists when he sought an explanation of the dissolution of feudalism and the transition to capitalism in 


37 Wood 1995: 122-40. It was such fettering which, for Hobbsbaum (1965), underlay the 'general crisis' of the seventeenth century, 'the last phase' of the transition from feudalism to capitalism.

38 J. S. Cohen 1978: 30-1; Wright et al. 1992: 57-8; Marx and Engels 1975-6, V: 32, 83-5; Dockes 1982: 159. 896

'causes external to the system', such as the rise of towns and trade. 39 Most Marxists have preferred to find some 'internal' prime mover of transition, such as the inefficiency of the feudal mode and its inherent tendency towards crisis, or the effects of feudal class struggle. 40

Having abandoned Marx's claims for the primacy of the productive forces,

Marxists have naturally paid little attention to Marx's claim that societies should be classified in terms of their characteristic productive forces and have concentrated, as Marx himself suggested, on distinguishing societies in terms of their relations of production (Marx 1976-81: I, 206, 325; II, 120). This is not to say that Marxist historians have necessarily agreed with Marx and Engels or with each other on the class-character of specific societies. 41 The very existence of an 'Asiatic' mode of production, where the state enjoys a monopoly of land so that the peasant producers hand over surplus labour in the form of tax, has proved extremely controversial. 42 In the early 1930s, the concept of the Asiatic mode was
removed from the theoretical
canon of Soviet Marxism whilst more recently Hindess and
Hirst have denounced it

as theoretically incoherent. 43 For Godelier, the Asiatic
mode represents a form of

the transition from primitive communism to class society,
whereas Marx himself

was happy to apply the term to Mogul India, a society which
Godelier characterizes

as a form of feudalism. Certainly, since defenders of this
concept have seen societies

in Africa or pre-Columban America as possessing 'Asiatic'
relations of production,

and since a state monopoly of land was by no means to be
found in all pre-industrial

Asian societies, 'Asiatic' may not be the best term for
this mode of production


The class relations and dominant mode of production of the
Ancient world have

also proved a controversial issue amongst Marxist
historians. Traditionally, despite

Marxists have placed a great emphasis on slavery as the
class basis of the ancient

world and, as in Walbank's analysis, as the chief obstacle
to productive advance. 44

Brenner 1977: 38-53; Brenner 1976:

31-2. Paradoxically, those who reject towns and trade as
the prime movers of the transition to
capitalism also tend to deny their external status with
regard to feudalism. See Merrington 1976; Hilton

1985: ch. 13; Hilton 1992, esp chs 1, 2; Hibbert 1978:
91-104. For attempts to reconcile Sweezy’s emphasis on the role of towns and trade in the transition to capitalism with those who stress the integral role of towns within feudal society, see Katzenelson 1992: 161-3, 175-91 and Torras 1980.

40 Callinicos 1995: 116-25 argues that even inter-societal conflicts, such as military competition, can be related to the pattern of society’s internal economic development and class relations.

41 The social character of the Soviet Union and other state-socialist societies proved to be a particularly contentious issue. See Bellis 1979 for a survey of views.


43 Dunn 1982; Hindess and Hirst 1975: ch. 4.

44 Marx and Engels 1975-6, V: 32-3, 84, 89; Marx 1976-81, III: 449-50; Engels 1968: 145-7; Walbank 1946: 24-7; 1969: 42-7, 104; Anderson 1977: 22. 897 Wood (1981) has challenged this approach on the grounds that the bulk of the population in the ancient world were peasant producers and independent craftsmen rather than slaves (see also Hilton 1977: 10; Marx 1976-81: III, 942). However, De Ste Croix, in an ambitious attempt to show the utility of Marxism for the study of
the ancient world, argues that the key issue in characterizing the class nature of the ancient world is not simply the occupations of the majority of the population but rather that of the dominant form of surplus labour which provided the income of the propertied class. 45 His argument faces two main problems. First, it is by no means clear that slaves were the major source of surplus labour in antiquity; the prevalence of chattel slavery, even in ancient Athens, seems to have been rather limited in time and place. 46 Second, as Hindess and Hirst have argued, there is no need for us to identify an economic unity underlying antiquity. The cultural unity of the ancient world was perfectly compatible with a variety of relations of production, ranging from slavery and serfdom to the appropriation of surplus labour by right of citizenship. 47

Feudal social relations have proved less controversial. 48 Nevertheless, Marxists have been divided over whether or not to accept Marx’s claim that under feudalism the peasant producers possessed the means of production, and thus of their own subsistence, which meant that surplus labour in the form of rent could only be extracted by extra-economic means, by the landlords' legal, political and coercive powers embodied in serfdom and the manor. In this perspective, serfdom and extra
economic coercion become defining features of the feudal mode. Other Marxists, by contrast, have argued that, since landed property assumes the monopoly of certain people over certain parts of the globe and the exclusion of others, the peasants’ payment of rent should be seen as a result of their separation from the means of production and that servitude, rather than constituting a universally defining feature of feudalism, requires a historically specific explanation.

Marx’s account of capitalism in terms of the labour theory of value has come in for much criticism, even from those who are sympathetic to his general outlook.

45 De Ste Croix 1975: 16 (the whole of the 1975 issue of Arethusa is devoted to the question of Marxism and the ancient world); De Ste Croix 1984: 107; 1981, esp. 52, 54, 113, 173, 179. For appreciative reviews, see Browning 1981; Anderson 1983; Brunt 1982. Bois argues that, in the sense that slavery was the dominant relation of exploitation, if not the major form of production, ‘Frankish society remained a slave-based society’ until the end of the tenth century, although these were slaves settled on holdings: Bois 1992: 19-24, 157-8. Cf. this work, chapter 6.


47 Hindess and Hirst 1975: 85-6; 1977: 40-1. Such cultural unity on the basis of diverse economic bases would, of course, pose problems for the metaphor of
base and superstructure.

48 For a brilliant account of the short and long-term dynamics of feudalism, see Kula 1976.


50 Hindess and Hirst 1975: 236-7; Martin 1983: 16-17. For an empirical emphasis on the importance of free peasants in medieval England, see Kosminsky 1956: 92-4 and Hilton 1967; 140-3. Barg 1991 shows that many freeholders were non-peasants who must often have sub-let their land.


What Marxists have retained from Marx is his distinction between the 'manufacturing' period, which for Marx prevailed from the mid-sixteenth to the late eighteenth century, and the era of industrial capitalism, which followed. In the former, production remains on a handicraft basis but independent artisans are replaced by a number of wage-labourers concentrated in a single workshop. At first, workers still produce an entire product, although now under the supervision of a single capitalist (the 'formal' subsumption of labour to capital), although there is eventually a tendency for the concentration of production to be accompanied by the intensification of the division of labour by process. This 'real' subsumption of labour to capital reaches its extreme form with
mechanization and the reduction of
the worker to being an 'appendage of the machine'. In other
words, a specific mode
of production is defined by its invariant relations of
production, but such relations
may be compatible with a variety of forms of productive
forces (Marx 1976-81: I,
429, 445-8, 453, 456, 400-1, 492, 590). More recent
Marxists have emphasized
that in the era of pre-industrial capitalism, the 'putting
out' of raw materials to rural
workers, so-called 'proto-industrialization', was more
common than centralized
manufacturing. Furthermore, industrialization proper is
now seen as a much more
recent development. Only with the age of railway
construction, from the mid
nineteenth century, did industrialization spread beyond
textiles, the area of its
initial breakthrough in the period 1700-1800. Even after
1850, mechanization,
particularly in low-wage economies, made only slow
progress. 53
For Marxists, all of these modes of production are based on
the appropriation of
surplus labour by the propertied class, a process which
inevitably generates class
conflict as the propertied come into conflict with the
producers. De Ste Croix (1981:
44) usefully reminds us that class struggle is not merely
the product of the actions of
slaves, peasants and workers, but that measures taken by
the ruling class in its own
interests are also forms of class struggle: the employer's
lock-out is as much an
instance of class conflict as the workers' strike.
Nevertheless, in practice, Marxist
historiography has tended to concentrate its attention on
popular social movements
and forms of unrest. For the medieval period, Hilton
Razi (1979; 1983) criticize those historians who have seen
feudal social relations in
terms of consensus and argue that class conflict was the
inevitable consequence of
medieval social relations in both town and country.
Similarly, Christopher Hill
(1974: 181) has argued that class hostility was a 'simple
fact' of the social world of
sixteenth and seventeenth-century England. Even in the
American South, which
lacked the slave rebellions found in Brazil and the
Caribbean and where the slaves
have often been seen as brutalized or bribed into
submission, the overseers and
plantation-owners did not have absolute power, since the
Southern slaves did
exhibit an 'impressive solidarity and collective resistance
to their masters'. The
slave-owners' 'paternalism' did not just mean obedience on
the part of the slaves,
52 Kriedte et al. 1981: I-II. For critiques, see Hudson
1981; Coleman 1983.
but involved a negotiated set of practices which had to take account of the slaves' ability to frustrate their masters' wishes. 54

Naturally, much Marxist historiography has been concerned with the emergence of the labour movement under modern capitalism. On the one hand,

Marxists have been keen to show that the historical role which Marx and Engels ascribed to the proletariat was not sheer wishful thinking. Thus John Foster identified the existence of a revolutionary consciousness amongst the Oldham working class of the 1830s and 1840s, as the community's 'revolutionary vanguard' guided the workers from a trade-union defence of standards of living to a realization that what was needed was a 'total change of the social system'. 55

Likewise, Tim Mason (1981) argued that even after the Nazis had smashed the German labour movement, the actions of the working class, or the threat of such action, could force employers to give way and, even after the outbreak of war, disrupted economic activity. On the other hand, given the absence of revolution in the advanced capitalist countries, Marxists have sought some factor to explain the proletariat's failure to carry out its world-historic role, the emergence of a 'labour aristocracy', which 'implanted accommodationist responses to capitalism and
subsequently transmitted them to a broader class movement', being a favourite candidate for this factor. 56

In general, however, the Marxist historiography of popular struggle has been frankly celebratory, aiming, as E. P. Thompson famously put it, to rescue the 'obsolete' hand-loom weaver, the 'utopian' artisan and the 'deluded' follower of Joanna Southcott 'from the enormous condescension of posterity'. 57 Thus for Rodney Hilton, modern values of equality, liberty and freedom are a contribution to world history not of the bourgeoisie, but have their origins in peasant resistance to feudal subordination. 58 George Rude has criticized those historians who see the 'crowd', the typical form of popular protest in the eighteenth and early nineteenth centuries, as an irrational mob made up of the socially marginal. The 'Swing' rioters in England or the Parisians who stormed the Bastille turn out to have been mainly


For critiques of the labour aristocracy thesis, see Moorhouse 1978 and Musson 1976.

For the roots of this approach in a tradition of radical 'people's history' and of Communist Party populism, see Samuel 1980: 37-9 and Schwarz 1982: 55-6, 71. For an early version of this approach, see Morton 1938: whose achievement is praised in Kaye 1992: ch. 5. Marxist historians have thus tended to neglect popular political forms of which they disapprove, such as patriotism, even where patriotism provided a language of radical political opposition. See Cunningham 1981: 8-9.

respectable labourers and craftsmen of settled abode and fixed occupation. 59

Similarly, for Thompson, the food riots of the eighteenth century were not simply an excuse for crime or an instinctive reaction to hunger but constituted 'a highly complex form of direct political action, disciplined and with clear objectives' in their defence of a 'moral economy of the poor' against the emerging political economy of the market place (Thompson 1971: 76-9, 131-6).

Nevertheless, the Marxist interest in class struggle is not simply the product of a commitment to a 'history from below' and a political belief in the value of the experience and struggles of the mass of the population. Rather, Marxists have
argued that class struggle is not only of interest to us today, but that it was also of
decisive importance in determining social change in the past. Thus Brian Manning
argues that whilst most accounts of the English Civil War have been dominated by
the aristocracy and gentry, it was in fact the 'middling sort', who expressed their
class consciousness through Puritanism, whose grievances and actions were 'the
main force behind events'. It was, for instance, fear of social protest and popular
movements which was decisive in splitting England's ruling elite in the period
1640-2, allowing Charles I to overcome his political isolation and to create a
Royalist party, and thus making civil war a practical possibility. More broadly,
Marxist historians such as Hilton (1969: 32-43, 57), Brenner (1976: 47-75)
and Martin (1983: 56-7; see also Dyer 1981: 194; Rigby 1995a: 124-44) use the
class perspective to criticize the dominant historical orthodoxy which explains pre
industrial social change in terms of the rise and fall of population. Instead, they
argue that it was the varying outcomes of the struggles between peasants and lords
that determined which path of social and economic development was taken in
particular regions of Europe in the late medieval and early modern periods: serfdom
in eastern Europe, an independent peasantry and absolutism in France, agrarian
capitalism in England (Lis and Soly 1982: 97-104).

A classic instance of Marxist claims for the epochal significance of class struggle is Marx and Engels's interpretation of events such as the English Civil War and the French Revolution as 'bourgeois revolutions', as movements through which, with their destruction of feudal property relations, the bourgeoisie created a new social order (see refs. on p. 893). Marxist historians have attempted to defend Marx and Engels's views on such movements. Christopher Hill, for instance, argued that the English Civil War was not just a constitutional conflict or a religious squabble but was a class war, in which the despotism of Charles I was defended by the reactionary forces of the established church and feudal landlords. Parliament defeated the king because it could appeal to the enthusiastic support of the trading and industrial classes in town and countryside, to the progressive gentry, and to wider masses...
of the population whenever
they were able by free discussion to understand what the
struggle was really about.

In a society that was still essentially feudal, revolution
was essential in order to pave
the way for the full development of capitalism. 61
Historians such as Lefebvre,
Soboul, Rude and Hobsbaum have offered a similar
interpretation of the French
Revolution as 'a conflict of social classes', as the
product of the mismatch between
the traditional social, political and legal pre-eminence
of the aristocracy and clergy
and the new reality of the economic power of the
bourgeoisie. It was the latter, a
'rising new social force' based on the expansion of
industry, commerce and finance,
whose interests were met by the revolution, even if, as
Engels argued, it was the
radicalism of the popular movements of artisans and
peasants which drove the
revolution forward to the complete destruction of
feudalism. 62 By contrast, it was
the weakness of the Russian bourgeoisie, its reliance on
the tsarist state and the
importance of foreign capital in economic development,
combined with a highly
concentrated industrial working class, which meant that the
Russian bourgeoisie
would not lead its own revolution and that this revolution
would not restrict itself to
bourgeois aims but would become permanent and proceed
immediately towards
socialist goals. 63

The Marxist fascination with the great historical revolutions has inevitably been accompanied by an interest in the social 'superstructure' of politics, the state and ideology. Indeed, despite the traditional Marxist claims for the determining role of society's mode of production, many of the most eminent Marxist historians, such as Christopher Hill, are better known for their accounts of religious and political change than for their original contributions to economic history. 64 Certainly, given this interest in society's 'superstructure', it would be wrong to portray Marxist historiography as simply 'history from below'. Hill, after all, has not only produced sympathetic analyses of the Diggers and the other radical sects of the English Civil War, but also a biography of Oliver Cromwell and a study of the economic problems of the Anglican Church and of how its attempts to solve such problems contributed...
As we have seen, Marx and Engels described the state and social consciousness as a superstructure which corresponds to society's economic base. In general they saw the state as the instrument of the propertied, the means by which the economically dominant class became the politically dominant class (see refs. on p. 893). Marx and Engels did, however, also argue that there were particular periods when the state could attain a certain degree of independence from the propertied, ruling class.

This was particularly the case in those periods where two rival classes cancelled each
other out, as in the absolutist states of early modern Europe whose rulers used the emergent bourgeoisie as a counterweight against the feudal nobility. Nevertheless, even though the state enjoys an abnormal social autonomy during such periods, such autonomy is itself socially determined. 65

That the state is 'a class state, the state of the "ruling class"' is a commonplace of Marxist social theory and historiography (Miliband 1979: 74; Therborn 1980: 132).

Thus, whilst constitutional and legal historians have placed great emphasis on the development of public authority in medieval England compared with the continued importance of private jurisdictions on the Continent, Hilton argues that it would be wrong to see this as a sign of the state's social neutrality. In England, a politically sophisticated baronage did not seek local independence from the Crown but rather sought to control the Crown through the royal council and Parliament whilst, at a local level, royal officials and justices were drawn from the gentry and nobility and many local courts were seigneurially controlled. The enforcement of law and order 'has never been a purely neutral act of government, especially when the power to do so is held exclusively in the hands of one social class'. What was important 'was not so much what the law was, as who administered it, and in whose interests' (Hilton
For Hill, the Tudor and early Stuart state was 'the main support of the propertied class' confronted with the threat of popular disorder (Hill 1974a: 186-7; 1976: 118-21). In a similar vein, Anderson even questioned Marx and Engels's claims for the autonomy of the absolutist states of early modern western Europe. Such states were not the product of the class-balance between aristocracy and bourgeoisie, but rather provided a 'political carapace' for a nobility which, with the decline of serfdom, had lost much of its local influence and so was increasingly reliant on the centralized extraction of surplus from the peasantry in the form of taxation.

Despite this emphasis on the state as a class state, E. P. Thompson has warned...
against the temptation to see politics and the law in simple instrumental terms

as a conspiracy of the rich. To be sure, the law functioned as the central

legitimating ideology in eighteenth-century England and, more practically,

reinforced contemporary class relations to the advantage of the ruling class, as

shown by Thompson's own study of the Black Act of 1723, which, in response to

forest disturbances in southern England, created more than fifty new capital

offences. But, ironically, in order to serve such functions, the law did have 'to

display an independence from gross manipulation'. In doing so, it helped to

encourage the idea of the 'free-born Englishman' who enjoyed an equality before

the law and a protection from absolutism, a conception which became a central

part of the rhetoric of plebeian radicalism (Thompson 1975: 158-69; see also

Hay 1975).

Just as Marxists have approached the state and law in class terms, so they have

interpreted particular forms of social consciousness, from religious ideologies to

political programmes and economic theories, in terms of the needs, interests and

experiences of particular social groups. As Marx and Engels put it, there is no such

thing as the history of ideas but only the history of the socially specific individuals
who produce such ideas. Marxists have therefore criticized those historians who see a religious ideology such as Puritanism as 'an obstinately religious phenomenon'

adopted according to personal 'taste and choice' (see Collinson 1982: 241; 1983: 5-6). Instead, they have sought to identify the social basis and class-specific appeal of particular theologies. Protestantism, particularly in its Calvinist form, is seen as the expression of the interests of the bourgeoisie which 'arose when it did because it was the religion most suited to stimulating capitalist enterprise and enforcing labour discipline'. Hill has argued that the social explanation of religious beliefs does not mean that such beliefs are simply a cynical cloak for vested interests. Protestantism was, after all, a system of thought for which men were willing to kill and be killed.

Nevertheless,

to understand Puritanism we must understand the needs, hopes, fears and aspirations of the godly artisans, yeomen, gentlemen and ministers and their wives, who gave their support to its doctrines .... It seemed to point the way to heaven because it helped them to live on earth. 69

Despite Marx's use of the metaphor of ideas as 'reflections' of social being, the Marxist theory of ideology does not mean that ideas are simply a passive product of
67 Marx and Engels 1975-6, V: 36-7, 154, 183, 250; Marx 1972b: 37.


69 Hill 1940: 44-5; 1958: 21; 1974a: 82, 89; 1969: 131, 142, 145, 494-5. Fromm, in contrast to Hill, argues that whilst Protestantism was a response to the social anxieties produced by the rise of capitalism, it was largely a conservative response and that, as Weber claimed, Protestantism’s stimulus to capitalism was an unintended consequence of its doctrines. See Fromm 1975: 53, 62, 68, 74, 78, 86-8; Weber 1976: 90, 904.

70 On the contrary, Hill argues that Puritanism was an active historical force, facilitating the transition from a society where poverty was no longer seen as a holy state but as a sign of wickedness and where the sin of avarice had become the virtue of thrift. Similarly, the upheaval of the English Revolution ‘could not take place’ without the ideals and new systems of thought needed if men were to risk their lives for the creation of a new order. For Hill, not only the individualistic faith of Calvinism but also the science of Sir Francis Bacon, the history of Sir Walter Ralegh and interpretation of law offered by Sir Edward Coke helped to pave the way for revolution (Hill 1958: 215; 1972b: 1-3).

The Marxist approach to ideology does not require that the intellectual
'representatives' of particular classes should themselves be members of that class

(Hauser 1971: 137-8), although Marxists have, on occasion, taken this view of particular ideological outlooks (Howkins 1977: 158-9). It is rather that particular forms of ideology will appeal to specific groups and that such groups will interpret intellectual, religious or cultural traditions in their own, socially specific ways. Thus the Puritan insistence on inner faith proved useful to a range of social groups in their resistance to a variety of forms of traditional authority, to the rebellious gentry of Scotland or Hungary as well as to the English middle class; it could be used to justify social order and discipline as well as forming the basis of a radical individualism. Similarly, concepts such as God, Antichrist and the 'Norman Yoke' meant very different things to the different social groups of early modern England. 71

In this perspective, even works of art can be seen as the expressions of particular forms of social being. Lucien Goldmann, for instance, offered a sociology of literature in which literary texts are seen as the embodiments of particular 'world visions' which, in turn, are the expressions of the interests and position of specific social groups, of which economic classes are the most important. Thus the philosophy of Pascal and the theatre of Racine express the tragic view of life of a
particular group at a particular time: the Noblesse de Robe of the seventeenth

century. 72 For Hilton, the ballads celebrating the deeds of Robin Hood, the ultimate

'social bandit' who engaged in a guerrilla struggle for justice on behalf of the poor

and oppressed, expressed the social aspirations of the English peasantry in the century and a half of endemic agrarian discontent which preceded the revolt of 1381. 73 Even the poems of Andrew Marvell, which at first sight seem to bear little relation to the age in which Marvell lived, can be better appreciated when seen in

70 Kaye 1988: 37. The inheritance of the problematical metaphor of reflection combined with an awareness of the positive historical role of ideas has even led Marxists to posit the existence of 'active reflections'. See Dobb 1951: 4; John 1953: 4.


the political context of his time, not to mention the works of an active participant in

the English Revolution such as John Milton (Hill 1958: 324-5; 1977: 4).

At times, Marx's own comments seem to imply that ideologies are simply
cynical deceptions designed to defend particular vested interests (Marx 1972b: 37-8). Yet elsewhere, Marx himself denied that this was the case. As he said of

the French petit-bourgeoisie of the mid-nineteenth century, classes tend to believe

that the social conditions which are favourable to themselves are those which are

most suitable for society as a whole. Nevertheless, Marx and Engels did believe

that the propertied classes would propagate ideologies which help to justify and

maintain their power and privileges: feudal social relations were presented as

ordained by divine will, or capitalism as the expression of human nature or of

'inviolable naturallaws' Marx and Engels even went so far as to claim that 'the

ruling ideas of each age have ever been the idea of the ruling class' (Marx and

Engels 1975-6, V: 59), although this claim can be interpreted in two different

ways. First, in its weaker form, it simply means that the ideas of the ruling class

constitute the official ideas of the age, rather than that such ideas were widely

adopted within society. After all, Marx and Engels were highly critical of those
thinkers who believed that 'the rule of a certain class is only the rule of certain ideas' (Marx and Engels 1975-6, V: 52, 60-1, 292; 1982: 78). Thus, although the social doctrine of the essential harmony and mutual interdependence of the social orders was an intellectual commonplace throughout the Middle Ages, Hilton argues that the peasants' willingness to resist feudal exploitation was generally unaffected by priestly exhortation and justifications of 'the existing order in terms of celestially sanctioned harmony'. Indeed, as Marx and Engels themselves argued, it was precisely in those periods when the ruling class was threatened, as in the later medieval period, that the ideology of the ruling class was most forcefully expressed. 76

Second, in its less-qualified interpretation, Marx and Engels's claim that the ruling ideas of the age are the ideas of the ruling class can be used as an explanation of the failure of the producing classes to rise up against their exploiters. In this perspective, it is the existence of a hegemonic, dominant ideology which forestalls the social conflict which would otherwise result. Ideology thus functions to conceal social contradictions in the interests of the dominant class. 77 As Althusser (1971: 139) put it, no ruling class can hold power over a long period purely by means of the
Repressive State Apparatus 'without at the same time exercising its hegemony over
and in the State Ideological Apparatuses'. His views had been anticipated, as Hill
points out, by Francis Bacon, for whom there would be perpetual social disaffection

74 Marx and Engels 1975-6, V: 290, 410-14; Marx 1972b: 40. Hill argues that the view of ideas as
either irrelevant to history or forms of hypocrisy and rationalization owes more to the historians of the

Namier school than to the works of Marx. See Hill 1972: 3 and Brenner 1993: 645.

75 Engels n.d.: 353; Marx and Engels 1970: 100-2; 1975-6, V: 154; Engels 1965: 41-2; Marx 1974b:
85-7; 1973a: 105-6.


77 Abercrombie et al. 1980: ch. 1; Larrain 1979: 60-3, 210. 906

'except you keep men in by preaching as well as the law doth by punishing,.78 In the

Middle Ages, it was the Church which functioned as the main ideological state

apparatus, its theology sanctifying and justifying feudal society, and subordinating

the individual to the social system (Althusser 1971: 143-4; Gurevich 1985: 10,
299-301). Likewise, in Tudor and early Stuart England,
it was the duty of the church to soften the bitterness of class hatred, to keep the lower

orders peaceful and subordinate, to stress the religious considerations which united a
hierarchical society against the economic facts which so visibly divided it, to console the
desperate (Hill 1974: 109)
even if it was not always successful in this task. When E. P. Thompson (1972: 663)
maintained that the growing radicalism of the English population meant that by
1816 'the English people were held down by force', Perry Anderson (1980: 37-8)
replied that military repression, in itself, was insufficient to maintain the English
ancien régime. The emergence of a counter-revolutionary nationalism, fostered by
twenty years of war against the French Revolution and its successor regimes, which
replaced religion as the dominant form of ideological discourse, was also crucial for
social stability. THE MARXIST TRADITION: AN ASSESSMENT
The influence of Marxism has been crucial in reminding historians, often caught up
in the minutiae of local studies and the difficulties of their sources, of the need to
study long-term social change, to examine social crisis and conflict not just
functional reproduction, to be aware of the historical impact of class struggle, and
to see the political institutions and forms of social consciousness in their broader
social context. Nevertheless, despite its many positive contributions to the
development of modern historiography, Marxism can be criticized on a number of
fronts. One line of critique of Marxist historiography is
to launch an empirical
attack on its specific historical claims. For example, the
Marxist interpretation of
the English Civil War and the French Revolution as
bourgeois revolutions has come
in for much criticism. It is by no means clear that, in
Charles I's England, a feudal
aristocracy, hostile to capitalism, was confronted by a
rising bourgeoisie. On the
contrary, Marxist historians themselves, such as Brenner
and Neale, have
emphasized that the internal transformation of the English
landed classes was the
key to the transition to agrarian capitalism. 79 In
France, 'revisionist' historians have
questioned whether the aristocracy and bourgeoisie of the
eighteenth century
constituted economic classes with clearly opposed class
interests and have denied
that the Revolution led to a decisive breakthrough in
French economic development
in either the short or the long term. 80 As a result, many
Marxists have abandoned or
78 Hill 1969: 111-12; Bergesen 1993; see also Genovese
general survey, see Hughes 1991: 3.
80 For a survey of the debate see Blanning 1987 and Lewis
1993. 907
modified the traditional Marxist account of the bourgeois
revolutions. Hill now
interprets the Civil War as a bourgeois revolution not in
the sense that it was led by
the capitalists against feudal aristocrats but because 'its outcome was the establishment of conditions far more favourable to the development of capitalism than those which had prevailed before 1640'. Brenner doubts even this claim, arguing that, rather than constituting the decisive turning-point in the transition to capitalism, the Revolution was the political reflection of the fact that society was already capitalist (Brenner 1978; 1989: 296-304). Marxists such as Regine Robin and Althusser have raised similar doubts about the existence of an inherent class conflict between nobility and bourgeoisie in eighteenth-century France. The difficulty of identifying a capitalist bourgeoisie in pre-revolutionary France has even led Comninel to admit that the validity of the traditional Marxist interpretation of the French Revolution has been 'exploded'. More generally, Wood (1991: 160) argues that the concept of bourgeois revolution 'conceals as much as it reveals ... the formula tells us little about the causes of these revolutions or about the social forces that brought them about'. Increasingly the Marxist emphasis is on the longue durée of the transition to capitalism rather than the supposed breakthrough of the bourgeois revolution. Yet such empirical critiques are unlikely to shake the foundations of Marxist
historiography. After all, Marxist historians have often disagreed amongst themselves about particular empirical issues. Opponents of historical materialism have thus been obliged to come up with a broader methodological critique of Marx’s social theory. Four main theoretical issues have emerged in recent years: the legitimacy of functional explanation; the limited nature of the Marxist conception of social being; the 'interpenetration' of base and superstructure; and the problem of pluralism, i.e. of the 'interaction' of the so-called base and superstructure, each of which will be discussed in turn below.

First, as we have seen, the main claims of Marx’s social theory, that the level of the productive forces explains the nature of society’s relations of production, and that the nature of society’s relations of production determines the nature of its bourgeois revolution, 'even though nobody intended it to be' (Hobsbaum 1990: 8; see also Morton 1978: 4). Callinicos thus defines bourgeois revolutions as ones which 'promote capitalism' rather than ones which are consciously made by capitalists. The French Revolution was, he believes, 'carried through under bourgeois leadership' but it is, in general, 'exceptional for the capitalist class to play the leading role in bourgeois revolutions' (Callinicos 1989: 122-5). Mooers also attempts to rehabilitate the
notion of the French Revolution as a bourgeois revolution in terms of its beneficial consequences for capitalism even though his own analysis shows that the Revolution was a very mixed blessing in terms of the development of agrarian capitalism (Mooers 1981: 2-3, 61, 64-72, 176).

82 Grenon and Robin 1976, cited in Blanning 1987: 16; Althusser 1972: 99-106; Comminell1987: 3, 19-20, 100, 195, 203, 205; Price 1993: 82. The orthodox view is defended by McGarr 1989. If even France is no longer seen as having experienced a classic bourgeois revolution, it follows that the specific course of modern German history can no longer be explained in terms of its failure to undergo such a revolution. See Blackbourn and Eley 1984: 7-21, 39-43, 51-9, 167-76, 287-8.

83 Corrigan and Sayer 1985: 85-6; Brenner 1993: 648-9; Blanning 1987: 16. 908 political and ideological superstructure, are both instances of 'functional explanation'.84 For certain writers, functional explanations, whilst the norm in the theory of biological evolution, are invalid in the social sciences, a claim which would automatically invalidate historical materialism. 85 Nevertheless, as is shown by the example of the Chicago School's analysis of the market in terms very similar to those used by biologists to explain natural evolution, functional explanations are not, per se, invalid in the social sciences (Elster 1979: 31-2; G. A. Cohen 1978: 287-8).

Their invocation in any particular instance can thus only be assessed empirically,
and historical materialism is not inherently invalidated by their use. 86

Second, even if we accept that social being can be defined independently of social consciousness, so that the former can be said to determine the latter, Marxism has been criticized for its equation of social being with class-position. Sociologists in the Weberian tradition have thus argued that economic class is simply one possible ground of 'social exclusion' and that other forms of exclusion, such as race, gender, status and order, which are by no means reducible to class inequalities, can be just as important. There are thus a number of forms and grounds of social power (economic, political and ideological), none of which can be assumed to have an automatic, universal or necessary social primacy.87 It is not that Marxists have neglected such non-class inequalities. On the contrary, Marx and Engels themselves distinguished the estates and orders of pre-capitalist societies from the economic classes of capitalism (Marx and Engels 1975, V: 69, 73, 89-90; Godelier 1988: 245-52), whilst Marxist social theorists have produced a number of studies of non-class inequalities, particularly those of gender. 88 The problem is rather that, as feminist historians and sociologists have argued, Marxists have tended to offer a

84 G. A. Cohen 1978: chs 9,10; 1980: 129-30; 1982a: 30;
85 Giddens 1981: 17,215; 1979a: 7,110-17,211-14; 1979b: 17,25; Elster 1979: ch. 1; Halfpenny 1983. Halfpenny's article is also included in Wetherly 1992, along with a number of other articles on functional explanation and Marxism. For functional explanation and the biological sciences, see Frankfort and Poole 1966-7. A particular problem is that whereas the theory of evolution specifies the 'feedback mechanism' (random genetic variation and the survival of the fittest) which enables us to explain the evolutionary development of a particular species in terms of its functional effects, social scientists (Marxist or otherwise) have no equivalent, universally valid feedback mechanism. As a result, it is easier to explain why functional social arrangements persist than why they appear in the first place.


1979a; 1979b; 1982: 100-2; Runciman 1969; 1974: 55-1; 1989: 2-24; Mann 1986; Giddens 1981. For Marxist responses, see Mackenzie 1980: 582-4; Barbalet 1982; Wright 1983; Wickham 1988a (on Mann); 1991 (on Runciman).

functional explanation of patriarchal social relations in terms of their benefits for the reproduction of particular modes of production. 89 Patriarchy is thus viewed as secondary and derivative from society's mode of production rather than being presented as an autonomous form of social inequality in its own right,90 or even, as some would prefer to see it, as in-built into society's mode of production as one of its defining features. 91

Third, much criticism of historical materialism has centred on the issue of the interpenetration of base and superstructure, i.e on whether social being can be defined separately from (and thus presented as the basis of) social consciousness, politics and legal relations. 92 As we have seen, Marx's model of social structure assumes that the state, law and forms of social consciousness 'correspond' to the form taken by its relations of production. Critics such as Acton, Plamenatz, Leff and Lukes have countered such claims with the argument that so-called
'superstructural' phenomena such as politics and ideas do not merely reflect society's economic base or just interact with it; they are actually a constitutive part of society's economic 'base'. But if the distinction between base and superstructure is untenable, it is illegitimate to derive the latter from the former: one cannot say that x produces y if y is actually a part of x in the first place. 93 Furthermore, if it is impossible to locate some pure economic level of society, separate from politics, law and forms of social consciousness, it follows that the concept of the economic base is simply an analytical abstraction. To abstract a concept from reality, and then to invert this process and present this abstraction as the basis of reality, would seem to be a classic instance of the procedure which Marx and Engels themselves rightly condemned as 'idealist' when resorted to by the Hegelians. Ironically, what presents itself as the most materialist analysis of society turns out to be, with true dialectical irony, its exact opposite: pure idealism. 94

In reply, certain Marxists have attempted to offer a defence of the


90 De Beauvoir 1974: 87; Firestone 1979: 15; Millett 1985: 38; Delephry 1984: 38-9, 74-5;


Graham usefully distinguishes two separate readings of the Acton-Plamenatz position. The first is that base and superstructure are conceptually inseparable; the second is that even if the two can be distinguished, they coexist and interact, which undermines the claim for priority of the so-called base (see the discussion of pluralism, pp. 911-15 below): Graham 1992: 52-4.

'traditionalist' Marxist distinction between the economic base and the political and ideological superstructure. However, such traditionalist defences of the
distinction between base and superstructure generally seem less than convincing,
even to those within the Marxist tradition. After all, Marx and later Marxists have explicitly accepted the existence of such interpenetration, as when they have presented the relations of production of the Asiatic mode of production and of feudal and ancient society as constituted by 'extra-economic' coercion and have seen the state itself as a key extractor of surplus labour in a number of pre-capitalist societies. Indeed, some Marxists have gone so far as to argue that far from the conception of society as an 'organic totality', in which the social parts are interdependent and mutually dependent, constituting a challenge to Marxism, it is historical materialism itself which (at least on its strongest reading) offers precisely this model of social structure. The notion of society as an organic totality is, in certain respects, an attractive one (provided that it does not dissolve all analytical distinctions and causal claims). The problem with such holism, at least from a Marxist perspective, is simply that it is rather difficult to see what is specifically Marxist about it.

Perhaps a more useful response to the problem of interpenetration of base and superstructure is Godelier's reformulation of Marx's metaphor of base and
superstructure into a claim for the primacy of society's relations of production,

conceived in their broadest sense, over those aspects of politics, the law and

ideology which are not constitutive elements of class relations. Society's base thus

includes those aspects of law and politics, such as the medieval landlords' manorial

powers, which are defining elements of contemporary class relations. It is such

broadly defined relations of production which constitute the base for those residual

elements of the law, politics and ideology which make up the social superstructure.

Base and superstructure are no longer seen as separate institutions but are instead

defined by their different Junctions (Godelier 1978; 1988: chs 3-6; Wood 1981: 79).

Fourth, and finally, if Godelier's approach provides a response to those critics

who raised the problem of the interpenetration of base and superstructure,


97 Marx 1976-81; III, 926-7; Lekas 1988: 3, 81,153, ch. 8; Hilton 1985: 123; 1984: 85-6; Gottlieb


Wickham 1988c: 76.

98 Wickham 1978: 72; 1984: 9, 20, 27-8; Brenner 1976: 68-9; Anderson 1979: ch. 1; De Ste Croix
For those who prefer the metaphor of society as a totality or an organic totality, as opposed to base and superstructure, see Rader 1979: ch. 2; Jay 1984; Gramsci 1977: 377; Jakubowski 1976: 102-3;


historical materialism faces even greater difficulties when confronted with the far more straightforward issue of the interaction of base and superstructure. As early as

The German Ideology, Marx and Engels had referred to the 'reciprocal action' of the productive forces, class relations, politics and ideology but, since they described society's superstructure as the 'expression' or 'reflection' of its economic base, it was easy for their critics to accuse them of presenting the economic factor as the only determining one and of ignoring the active historical role played by politics and ideas. From the time of Engels's famous letters on historical

materialism of the 1890s, Marxists have thus been obliged to reject the charge of
economic reductionism and to acknowledge the 'dialectical interaction' which
takes place between base and superstructure (whether
defined in the traditional
sense or that of Godelier). 101

The problem is how to acknowledge an awareness of the active role of politics and

ideas without abandoning the primacy of society's mode of production which is,
after all, the claim which gives Marxism its distinctiveness as a theory of the social

world and of history. This dilemma can be seen in the structuralist version of

historical materialism offered by Louis Althusser. Ironically, whilst Althusser's

theory been attacked by E. P. Thompson for its economic reductionism, it would be

truer to say that Althusser's theory actually founders (at least as a form of Marxism)
on its recognition of the complex interaction involved in historical explanation. Far

from reducing society to its mode of production, Althusser redefined the mode of

production to include economic, political and ideological levels (or practices), each

of which is 'relatively autonomous' and possesses its own chronology of develop

ment. Instead of positing a one-way determination of politics and ideology by

economics, Althusser argues that specific relations of production may presuppose

elements of the legal, political and ideological 'superstructure' as a condition of their
existence. Many historians and social theorists would be inclined to accept such a view. It is simply that, once more, it is rather difficult to see what is distinctively Marxist about it. In other words, the problem of reductionism cannot be solved simply by invoking the concept of the 'relative autonomy' of the state and ideology (even if qualified by a determination by the economic 'in the last

Marx and Engels 1975-6, V: 40, 52-3; Marx and Engels 1975: 390-402, 433-5, 441-3;
Loone 1992: 164-5. Engels's letters are still appealed to today by Marxists keen to reject the
222,227-8.

Althusser and Balibar 1975: 97,100, 104-5,
177-8,183,187,220-4; Althusser 1977: 96-101,113; Gordy 1983. Althusser is presented as an anti
reductionist in Blackburn and Jones 1972: 369-74; Hall 1977; Bennett 1979: 40-1; Anderson 1980:
66-77; Hirst 1985: 22-3; Milner, 1981: 8. There is now a massive literature on Althusser, for which
Elliot 1987 is a useful starting-point. For critiques of Althusser, see Clarke 1980; Geras 1978;

Miliband 1983: 56-62; Bennett 1979: 40-1; Althusser and Balibar 1975: 100-1; Eagleton 1991:
Neither is it a solution simply to abandon the metaphor of base and superstructure, perhaps by blaming it on Engels rather than on Marx himself. It is not the metaphor of base and superstructure which is the problem but rather the idea which it seeks to express, i.e. the claim for a hierarchy of social elements or causal asymmetries which gave Marxism its specificity and separate identity as a form of social theory.

Thus, in rejecting reductionism, Marxist theorists constantly slip towards an implicit pluralism by which Marxism dies the death of a thousand qualifications.

This tendency is even more pronounced in the complex historical analyses offered by Marxist historians. For instance, Brenner's account of why the eastern European peasantry was enserfed in the late medieval and early modern periods, when the peasantry in the West was winning its freedom, rejects explanations in terms of the population change and offers instead an analysis which is explicitly based on a Marxist claim for the primacy of class struggle. He argues that it was the strength of the western European peasant community which allowed it to resist the seigneurial offensive of the late medieval period and thus to win its freedom, whereas the weakness of the peasant
communities in the East meant that they were unable to counter the landlords' pressure, thus opening the way to serfdom. The problem is that when he comes to explain why the peasant community was weaker in the East than in the West, Brenner lists a host of factors which cannot be reduced to expressions of class structure or class struggle, such as the absence of common land in the East, the prevalence there of individualistic methods of farming rather than of highly evolved common-field systems, the small size of eastern villages, the lack of villages of divided lordship, the effects of political conquest and the emergence of particular state-forms. The outcome of class struggle thus ceases to be simply an explanation and itself becomes something to be explained in terms of a variety of factors. 107 There is a comparable pluralism at work in Brenner's account of how strong peasant property and the absolutist state developed in early modern France 'in mutual dependence upon one another',

104 Marx and Engels 1975: 393-6, 399, 401-2, 441-2; Kautsky 1988: xlii, 3-4, 227, 232-3. As Althusser famously put it: 'From the first moment to the last, the lonely hour of the last instance never comes' (Althusser 1966: 113). However, Althusser developed his own reading of determination in the last instance by the economic level which he saw as assigning the other social levels their specific
effectivity. Thus, in the feudal mode of production, the economic level 'determines' that the political level should be 'dominant' (Althusser and Balibar 1975: 97,177-8,220-4). For a critique, see Hindess and Hirst 1977: 55-6.


107 Brenner 1976: 57-60; 1982: 72-6. Guy Bois complains about Brenner's implicit pluralism in 'Against the neo-Malthusian orthodoxy' (1978: 67). 913 which suggests that absolutism was more than simply the 'expression' of social change, as Brenner also claims, but was itself an active agent in bringing such change about. (Brenner 1976: 71; 1982: 81).

Nor is such pluralism confined to the works of Brenner. Rather, it can be seen in Corrigan and Sayer's (1985: 85) explanation of why modern capitalism first triumphed in England in terms of 'the singularity of English state formation and state forms' and in Genovese's (1972: 322-3) attempt to square the circle by
claiming that the social superstructure is 'generated' by
the base of the mode of
production but that it also develops according to a logic
of its own and reacts back
upon the base. Similarly, Parker, in an essay explicitly
intended to defend the
metaphor of base and superstructure, argues that the motor
of historical change in
the early modern period was not to be found in class
struggle or in any aspect of the
economy, 'but in the activities of the state', in
particular, the rise of the absolutist
state under the pressure of warfare and religious
antagonisms. In practice, such
accounts present us with a multiplicity of interacting
forces, an 'infinite variety of
local factors', which together bring about a particular
historical outcome, a
picture of history which is familiar from non-Marxist
historiography and from
Weberian sociology, but which sits uneasily with the
Marxist claims for the
primacy of the economic base (even when redefined in
Godelier's terms). As
Kitching put it, commenting on the high quality of Marxist
historiography:
'Engaging in a professional practice which is more
sophisticated than its theorization
is in fact very likely to coexist with a trained inability
to either recognize or express
that sophistication formally or explicitly'.

The threat which such pluralism poses for Marxism cannot be
avoided merely
by changing historical materialism from a claim for the
primacy of a narrowly
defined 'economic' level to a more broadly conceived 'class
determination'. Nor
is explanatory pluralism only implicit in the Marxist
tradition which emphasizes

Western Europe of Anderson's account of the rise of
Eastern absolutism in terms of international rivalry
also the discussion of absolutism, p. 903

above.

This variety of local factors can be
seen in the treatment of the role of 'great
men' in history, where the emphasis of Marxist historians
on the indispensable role of particular
individuals provides a contrast with Engels's (untestable)
claim that 'if Napoleon had been lacking,
another would have filled the place' (Marx and Engels 1975:
442; Deutscher 1954: vii; Rodinson 1973:
ix-x, 290).


point about the pluralist-Marxist accounts of
the making of the German working class offered by Jürgen
Kocka and Hartmut Zewahr.

Wood 1995: 175; Genovese 1969: ix, 19, 103; 1972:
323-4; Kaye 1979: 415-19; Clarke 1979:
144; Williams 1979: 118; Kaye 1984: 232-41; Thompson 1972:
9-11; 1978: 85, 298-9. When class
becomes an economic, social, political, psychological and
cultural phenomenon (Genovese 1972: 323-4), there is a danger that this concept 'turns into a synonym for the social structure itself, occasionally masquerading as one of its principal parts' (Parkin 1979a: 8). 914

the primary role of the relations of production and of class struggle rather than of the productive forces (McLennan 1980: 39-40; 1989: 70-7). Rather, pluralism is an insoluble problem for any brand of Marxism which rejects reductionism and which seeks to explain historical change in terms of the interaction of a variety of historical forces. 113 As philosophers in the tradition of John Stuart Mill have argued, it is impossible to claim an objective explanatory primacy for any of the multiple factors which bring about a particular event. Causes have an objective existence in the real world, but which we choose to emphasize and which we take as given will largely depend upon our own subjective purposes, upon the knowledge which we think we can assume on the part of our audience, or on some new piece of the historical jigsaw which we have identified and to which we wish to draw attention. 114 In this perspective, it is not just the Marxist claim for the primacy of the economic which is doomed but any attempt to ascribe objective primacy in historical explanation. In other words, whatever our explicit theory
may be, we cannot help, in practice, but be pluralists. It is precisely this fact

which allows the piece of the historical jigsaw discovered by Marxist historiography to be so easily subsumed into orthodox history. As the high quality of Marxist historical writing suggests, Marxists have easily avoided the Scylla of reductionism, upon which its critics have usually seen it as foundering. Yet this danger has only been avoided at the expense of being drawn into the Charybdis of pluralism. I have argued here that there is no way in which Marxism can successfully navigate.


Adamson, O. et al. (1976) 'Women's oppression under capitalism', Revolutionary Communist 5.


113 Johnston 1986: 8, 50, 66-7, 69, 80-1, 122. Since 'reductionism' is a term of abuse which no one applies to themselves, this means, in effect, all versions of Marxism. After all, even Stalin's Dialectical and Historical Materialism emphasized the reciprocal influence of the social superstructure on the economic base and argued that, far from denying the role of the state and ideology in history, Marxism 'stresses the important role and significance of these factors in the life of society' (Stalin
1951: 26-97).


41-50: Hart and Honore 1985: xxxiii, i-22, 26, 33-7; Ryle 1963: 50, 88-9, 113-14; Runciman

1983: 193; Gorovitz 1965: 701-2; Veyne 1984: 91-2, 101; Dretske 1972; Gardiner 1961: 10-11,

99-112; Dray 1957: 90-101; Putnam 1979: 41-4; 1983:

211-15; Garfinkel 1981: 3-5, 21-34,

138-45, 156-74; Anderson et al. 1986: 171; Brodbeck 1962:

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A recent number of the French publication Autrement entitled 'Passes recomposes', edited by Jean Boutier and Dominique Julia, was concerned to trace changes in the writing of history and the mirier of the historian over the past thirty years. Many of the contributors attested to the unease or disquiet they sensed in the discipline.

Some spoke of the passing of the great and global vision: the emergence of micro history concentrating on a single episode, the art of thinking small or of history in a raindrop: the concomitant shrinkage of grand history and the constrained vision of a new generation of historians who were experts on the events of a few years over a few square miles of territory or masters of a single text. Some drew contrasts between those countries where history in the hands of the commercial publishers
ensured it reached a broad spectrum with the ivory-tower approach of the British,

American and to a degree French academics concerned more to impress each other

than to share historical experience. Others brooded upon the triumph of theory in

manifold form. The emergence of the great gurus of our time, for example

Foucault, who sought to distinguish the process of domination, of power relation

ships within Western society, some arising from the growth of capitalism, others

from religious change, a process which manifested itself in institutions like the

prison and the lunatic asylum and criminal processes attacking sexual deviance,

carried history deeper into the realms of the story of social control. Another

influential figure in changing historical styles was the anthropologist Clifford Geertz

who in the 1970s urged the examination of ritual and the symbolism encoded in the

forms it took and spoke of 'thick description', a narrative form which took account

of these complexities, as a means to understanding a culture. And then there was

linguistic theory, the triumph of discourse and of deconstruction of Derrida,

Saussure and Barthes and the advent of the new historicism (meaning the

conversion of the historical document or 'text' into a semantic exercise concerned to

demonstrate the meaning of the language used within it),
which caused history to take a linguistic turn (Bowana 1981). The focus of the historian (supposing such a generic personage exists) could be said to have shifted away from the recovery and interpretation of hard evidence to theories of representation, to symbolism and collective memory (itself the subject of a plethora of theories and studies about the selection of what to remember about the past) as a means to understanding culture.

The search for 'identity' (national, regional, sexual), and the definition of alterity, insiders and outsiders to a culture, have, in the view of some, changed history beyond recognition. If we add to this the relative ignorance of new generations about the content of the Bible and lack of understanding about religious belief (religion is just a form of social control), the demise of Marxist analysis which, whatever one's political persuasion, produced some historical classics and some active debate, then the belief that history is not quite what it was and the question 'what is history?' takes one away from the familiar landscape of past generations.

The road from Braudel's Material Civilisation (1981) to Thomas Laqueur's Making Sex: Body and Gender From the Greeks to Freud (1990) or Keith Baker's Inventing the French Revolution (1990) is by any reckoning quite an odyssey.
What have these changes got to do with women's history? The short answer is quite a lot. Indeed, viewed in an international context to take in American and European historiography as well as British, what has happened to the history of women has reflected the twists and turns, the bifurcations and the culs-de-sac of the entire discipline (or should one say, of the category history?). If for no other reason than that, it is worth reflecting upon its evolution; its different emphases in different national contexts; its strengths and its weaknesses; the debates waged within it and of course the questions: 'has it arrived as an acknowledged part of the discipline?'

and, most speculative of all, 'where is it going?'

The concern with the history of women developed in a significant way from the late 1960s. It grew in relatively propitious circumstances in certain senses and in other senses circumstances of suspicion and some hostility. The discipline was perhaps in these years led by social history, and social history was to be the hospitable ground in which the history of women was initially nurtured. The Annales school had spearheaded a preoccupation with demography, and demography in turn a concern with the history of the family. The major study by Lawrence Stone The Family, Sex and Marriage in Early Modern England (English edition,
1976) and the work by Edward Shorter on The Making of the Modern Family
(English edition, 1976), which argued for the birth of affection in Europe as coincident with a new sexual liberty generated by the Industrial Revolution which took the young out of traditional rural communities, were important opportunities for the first debates on where women were located in family and community. Keith Thomas had already published an essay on attitudes to women in ‘The double standard’ (Journal of the History of Ideas (1959) which for me remains one of the classics of women’s history) and Religion and the Decline of Magic (1971) which brought out the complexities of the witch-as-woman question. Witchcraft and heresy were in fact two of the issues in which the importance of understanding relationships between, and attitudes to, the sexes first became apparent. However, the American civil rights movement, the drive for equality which lay behind the 930 women’s movement and the anti-establishment ethos of the 1960s lay behind the impulse for further enquiry. The initial intent, whether among American women academics or young women scholars in Britain, was very modest: to give women a past, to put them in the record. Without a past, as Simone de Beauvoir said, no individual could have
dignity. For those anxious to develop the field, it was, in the first instance,
necessary to gather the evidence, and the first evidence to hand related to
demography. It was possible to determine a life course, dependent on class and with
some national specificities, and to reply to most of the questions which Virginia
Woolf had asked in the 1920s about women's lives in her A Room o/One's Own:
What one wants, I thought and why does not some brilliant student at Neunham or
Girton supply it? is a mass of information; at what age did she marry; how many
children had she as a rule; what was her house like; had she a room to herself; did she do
the cooking; would she be likely to have a servant?
Knowing age at marriage, the average number of children, the odds on dying in
childbed and on being a widow, and how class, geography and period modified the
picture contributed to some appreciation of an average life, but one that was rather
static. It did not take women out of the home or give them roles other than that of
reproductive vessel. As well as this kind of question about, as it were, the logistics of
life, another set intruded designed to show women as actors. Joan Gadol Kelly's
question 'Did women have a Renaissance?' (and by implication a Reformation, an
Enlightenment, a special experience of a particular war or of fascism) demanded a
reconsideration of events with a view to uncovering a specific female experience.

Even if the answer was 'no', it demanded an explanation to justify a missing presence. If 'yes', then their slice of the action needed consideration both in respect of those events in which they participated and in respect of what this showed about their lives. A number of women historians followed Linda Kerber and talked about this kind of history as 'refocussing the vision', meaning reviewing an event from the point of view of the involvement of groups of women. To what extent were women actors, positively as opposed to passively involved or drawn into unfolding events?

Natalie Zemon Davis's work on the Protestant women of Lyons in the sixteenth century in Society and Culture in Early Modern France (1979) is a fine early example of this kind of approach.

In some instances it was a matter of re-reading evidence accumulated for quite other purposes and discovering what had been left out. I remember quite graphically being part of this re-reading process. In the autumn of 1968 I was asked by the history society of Balliol College, Oxford to make a contribution to a series of talks on 'Revolutions' (in those days seen as necessary, desirable and arguably fun)

with a piece on women in the French Revolution. The series included several big
names Christopher Hill, George Rude and Edward Thompson to set beside

my relative obscurity as the author of a monograph on a provincial town during the Revolution, but I sensed the audience was very receptive. News that I had given such a lecture sparked off endless invitations of a similar nature. The essence of the piece was the result of reflecting upon the moments when certain groups of women (whom I did not suppose to be the same or to speak for all women) were actors in political disturbances and what caused them to be so. When the piece was published in Past and Present, I was inundated with letters and it was important in the formation of transatlantic contacts with Natalie Davis, Joan Scott and Louise Tilly.

The first was launching with Jill Conway one of the first courses on the history of had. That was how the first steps were taken: we networked with those we had never even met.

Ab initio, significant differences marked out the American situation from that of other countries. The American university is market led (although there can be a very considerable lag between demand and supply). The American degree courses are modular and the notion of a wide chronological coverage at the undergraduate level in a very specialized subject course has little or no purchase. It is possible for
students majoring in a certain field to elect individual courses in other fields.

Multidisciplinarity rules. To possess an island of knowledge in a sea of ignorance is of less concern than in the European universities. Boasting a broad spectrum of higher education institutions and significant numbers of faculty women prepared to give time and energy to the development not only of women's history but of women's studies (a multidisciplinary concept), action and development of the field was on the agenda. Specialist centres were developed (like the Pembroke Center at Brown), and the Schlesinger Library of Radcliffe College for the History of Women in America, which has no European parallel, expanded considerably. It is easy for Europeans to be dazzled by the achievements of American women scholars and to underestimate the intensity of some of the battles which had to be fought. However, certainly the American structure gave scope for rapid development in a multidisciplinary context in contrast to, for example, the French experience, which has generated three specialist chairs in women's history, and the English model which has no positions at all in women's history but an increasing number of MA courses in the field.

In the 1980s, it became increasingly common to talk not of 'women's history' but
of 'gender history', and what this means has been the subject of much debate. One factor lying behind this development was the fairly obvious realization that one cannot talk about women without talking about men. Simone de Beauvoir in The Second Sex (trans. 1954) pronounced (as had, it transpired, a whole series of feminists avant /a lettre stretching back to Christine de Pisan, and even certain male philosophers like Poulain de la Barre and John Stuart Mill) that women were made, not born. They, as well as men, were a cultural construction, but the cultural construction of women had left them weak and essentially outsiders in a society largely organized for the benefit of men. She mustered an impressive range of thinkers from the Greeks to Freud via Genesis, Saint Paul and Karl Marx in support of the thesis. What this meant was that every child was subjected to an elaborate and complex process of acculturation provided by family, church, community and tradition, as well as by institutions such as the law court or the labour market. The result was that the tabula rasa of the infant mind was inscribed with notions of the appropriate, and in addition to the biological difference which was the spurious justification for the process, every little girl was given a part to play, a life role as supporting actor in which her intelligence was not given free rein.
The historic process of conditioning rested on a biblical foundation text (Genesis) in which woman was made subservient to man and placed in a state of obedience:

her transgression (the Eve legend) meant that she must sit in silence in church. To

the Bible, medical thought stretching back to the Greeks, and possibly before,

added that her different physical form (weaker, colder and wetter than the male masterpiece) fitted her for the home but not for the more active life of warfare or politics: this weaker physical form and restricted period in the labour market meant

that she could be paid less etc. Nature had fitted her to be a reproductive object, a second-class citizen, someone without a history, whose growth was stunted to prevent her realizing her capacities.

The development of ideas and theories about gender had a great deal of importance for burgeoning women's studies departments because these notions were a co-ordinating concept bringing together a variety of subjects to form a multidisciplinary package, since they were of relevance not just to an interpretation of history but also areas in sociology, psychology, anthropology, legal studies, and perhaps above all in literature. Literary studies in fact in the 1960s began to dominate women's studies and gender analysis became one of the tools of decon
struction, the decoding of literary and historical texts to reveal the intent and mentalities of the writer taken to represent the culture.

The notion of a gender system constructed by the Church, the law, medical thought, economic ideas and tradition and perpetuated in the family had considerable effects upon historical writing. First, this system could be worked out in detail, be it from the point of view of the good-conduct literature of the Reformation, studied for example by Kathleen Davies (1981) or Steven Ozment (1985); from the point of view of early anatomical treatises: or the law could be unravelled to show how arbitrary views on the nature or the two sexes influenced the construction of property, marriage and even criminal law (British law, for example, until the 1790s treated the murder of a wife by a husband as homicide but the other way round as treason). The sauce society deemed appropriate for the goose could thus be distinguished from that deemed appropriate for the gander. Again, of course this was retrieval, but a form of retrieval which enhanced the understanding of mentalities even if it did not tell us the degree to which people built their lives on prescription.

To look at the possibilities available to each sex at a given time provided the
dynamic of a classic work in women's history of the 1980s, Natalie Zemon Davis's

The Return of Martin Guerre (1983), which used the evidence provided by a trial of

an alleged impostor, who replaced at bed, board and in the community an aberrant

husband who had gone off to war and had stayed away with whatever intent, but

then chose to return and claim his place. The trial was used to reveal the expecta

tions and possibilities a a given village society made available to either sex. This was 933

both 'thick description' and microhistory: thinking small and interpreting the

evidence by reference to notions of gender so as to give the study broader import

The work was a huge success even outside purely academic circles.

For some historians the concentration on the construction of both men and

women promised men a slice of the action in a dynamic branch of social history. It

became possible to talk of 'a crisis of masculinity' which accounted for male

behaviour towards women at certain periods, such as when women threatened to

intrude upon terrain deemed appropriate only for men, as they did during the

suffrage movements or when they attempted to enter jobs deemed male (the 'when

threatened, attack' principle). Or, the definition of rigid gender roles and sexual

behaviour clearly had application to the history of sexuality and gay studies (other
growth areas). Non-conformity to society's norms produced victims of both sexes.

Gender was clearly a part of 'cultural studies', another collective in steady development. It certainly lent edge to Black Studies as they evolved in post-civil rights America. For the black woman clearly experienced a double oppression, first from the white establishment which converted her body into a mechanism for the replication of slaves, and second from black male partners. For some feminist scholars the emphasis on gender was an act of appropriation which again threatened to reduce women to a subsidiary role. For others the positng of a gender system which could be unravelled like a thread in a complex web and used to interpret any given document (text) and hence be made to reflect on the culture that produced it offered considerable possibilities. History generally was taking a 'linguistic turn' and women's history should join the movement and embrace the techniques of deconstruction. Indeed, it could be argued that already women's history was a pioneer in the field. One of the most reproduced essays ever written, Carol Smith-Rosenburg's 'The Female World of Love and Ritual' (1986) examined a correspondence between friends and relatives in nineteenth-century America, and picked out forms of expression and ways of conveying affection and
bonding, a female use of language no less. However, more crucial to gender history was the language men used about women. Michel Foucault had accentuated the power of language in the assertion of dominance by a particular group though his main concern was class struggle and the definition of deviance. But surely male dominance of women was also reflected in language which could then be the key to cultural analysis?

A preoccupation with power relations was an early characteristic of American women's historiography. Starting from the assumption that the historical record is that of an ongoing (and even in some views intensifying) onslaught upon women's power, two types of women were elected for early attention in furtherance of the thesis. The first type was the witch. Certainly most witches were women, but the claim was made that they were 'strong' women, healers and midwives, figures of authority in their communities. Closer analysis revealed that few fell into the healer / midwife category, but most were frightened poor old women trying to fend for themselves in harsh agrarian societies by cursing those who refused them assistance.

Why these should be singled out for persecution of course needs explanation, but it can hardly be given in terms of their power. Moreover their incidence is far from
even across Europe and that too needs explanation. Furthermore, in some countries most were denounced by women. The second figure involved in early power debates was the midwife who, it was alleged, was progressively ousted from her job by the male midwife. A recent collection of essays made by Hilary Marland ranging over all the countries of western Europe has revealed how hasty such a judgement was and that what was at stake, in a very few countries and in the cities, was the upper end of the business. In Catholic Europe the Church kept the business firmly in the hands of women and for the bulk of the population, cheapness was the determinant. These early essays on gender power struggles lacked the sophisticated theories later formulated by some American feminist historians whose work fell most under the influence of French deconstructionist theory (which has had a much longer reign in the United States than in France itself). For this group, of which Joan Scott (1988) has become the chief exponent, the business of the feminist historian is to uncover the strategies of male dominance in the past or the mechanisms whereby patriarchy replicated itself. Social history tout court in this view is not enough: the approach is locked in economic determinism and description so that gender is no more than one among a number of devices for describing a
given society. Instead, it

should be a serious analytical tool concentrating on the
forms of expression

(discourse) employed to secure male hegemony.

Such a view has not gone uncontested. Louise Tilly
challenged the interpretation

as minimizing the achievement of survival strategies
adopted by women in the past

which made it possible for the working class to survive. For her, Scott’s view of the

social constraints on women was overweighted and left little scope for initiative. Her

criticism will find sympathetic resonances in Italy where women’s history at the

present time seems particularly concerned to recreate a qualitative appreciation of

the experiences of women which evades the rhetoric of oppression. The history of

the convent, for example, is being rewritten so that it emerges as the possible centre

of artistic musical and literary endeavour. (Hildegard of Bingen, currently in the

music charts, has many counterparts.) The conversion of all women into eternal

victims is not always how they themselves interpreted their lives and is hence to

distort their vision. Is it defensible to graft on to the past the anger of the present?

Perhaps more dangerously, an exercise which involves writing up an event by

reference to a specific ‘meta’-framework (an ideology?) without an in-depth

knowledge of the socio-economic framework of reference can
only give a very partial rendering, a gross reduction. It is possible that no historian can ever convey all the story, but surely the aim should be as much of it as possible?

Where has British historiography chosen to locate itself? The impetus to develop women’s history in Britain in the late 1960s and 1970s came from the left-wing historical tradition (sometimes referred to as the Thompson school, although Thompson was no significant protagonist of this field of enquiry, and indeed most of this school could at the beginning fairly be called gender blind, though his inspiration has led others further). However, women’s history found an important organ in the History Workshop Journal, which proclaimed itself the journal of 935 socialist and feminist historians. Its overt aim was to examine the interplay between gender and class. In some respects it drew on an older legacy; from the beginning of the twentieth century a number of young historians were shaped at the London School of Economics by Professor Lilian Knowles. They included Alice Clark who, though not a professional scholar as such, had received an impeccable training in the use of archival sources. Her Working Life of Women in the Seventeenth Century (1919), reprinted in two new editions (1982, 1992), has subsequently formed the
point of reference for an active debate on the chronology of women's history and on the effects of capitalism on women's lives. Whether or not one agrees with Clark's conclusions about these issues, her reflection on how changes in the economy could transform certain areas of women's lives promoted active questioning. But the new generation of the 1970s went much further. Barbara Taylor's Eve and the New Jerusalem: Socialism and Feminism in the Nineteenth Century (1983) was a highly influential and sophisticated analysis of women in Owenite socialism which constitutes now a classic study in women's historiography. It made apparent that the sexes were not one in their notions of an ideal society. Pioneering work by Sally Alexander on London women's working lives and by Anna Davin on motherhood and nationalism/imperialism, to cite merely two examples, opened up issues in which economic, social and political considerations accentuated the differing experiences of women and men. Tribute should be paid to this generation of historians, whose work came to fruition at the point when British universities were shedding staff. Most of them knew disconcerting discontinuity in their working lives and many found a home in part-time work in the Open University (which fairly early on showed a great sensitivity to women's history) or the then polytech
nics, where teaching loads were heavy and research time limited. Their relationship
to the formal academic establishment was therefore tenuous.

Some of the most adventurous steps in the forward movement
of women’s history

in Britain were then taken in unpropitious circumstances
and it was not until the

universities in the 1980s became more market orientated in
particular in search of

money from overseas students for MA courses that the
British higher-education

establishment in general adopted a more genial attitude
towards women, gender,
culture and those forms of history which did not follow
traditional paths but which

attracted the American student, as well as the British
student who could raise the

money for the fees. The foundation of MA courses in
women’s history in York, in

Sussex, at Royal Holloway College and elsewhere promised
the training of new

research students. The adoption of modularity (courses
which extend over only two

or three academic terms) and a more ‘American’ approach to
the structure of higher

education has also meant the introduction of greater
interdisciplinarity, already

conspicuous in what are now called the new universities,
and with these new kinds

of degree courses came less concern with chronological
coverage or what some

would consider mainstream history. The plus side of change
is the greater
propensity for more experimental and innovative courses: the downside is the lack of breadth and the reduced propensity of historians to think comparatively or to have 'flags' that is, an overall concept of the key issues of a period. The risk then is that gender analysis floats in a sea of contextual ignorance which detracts from its significance and exposes it to criticism.

How far has British historiography reflected the 'linguistic turn', the concern with textual analysis, or with power relationships, the rejection of the socio-economic and the sovereignty of class on the grounds that economic determinism clouds the significance of gender? These are not the same questions. The first question could be answered 'quite significantly'. Some superb work in this direction has been done, for example, by Lyndal Roper (1994) on German witch trials or by Laura Gowing (1993) on slander suits in the seventeenth century in the context of an in-depth study of London courts. In this study Gowing makes a special point of underscoring the extent to which women define women: the wife attacks the adulterous partner of her aberrant husband in almost biblical terms (Eve as temptress) and not the husband himself, or at least she does not do so publicly. The abandonment of the socio-economic is not as evident in Britain. Indeed, some very
remarkable socio-economic work has been done and is in progress on women in the labour market and their role in industrial expansion in the nineteenth and twentieth centuries. The examination by British scholars of property law relating to women, of their differential treatment and presence in criminal courts, of women in medical thought and practice, are examples of growth sectors. There is, however, a significant representation of the new historicism, textual analysis by reference to gender theory where multidisciplinarity prevails, but it is far from dominating British history. The strength of the discipline in England has always been its solid empirical base and in the higher branches of academic history I do not see this disturbed.

What then has been achieved? First, there is a woman's past, variously defined, there for the taking. If history gives identity, this is important. Second, in many fields of historical enquiry some consciousness of the differing experience of women and men is apparent. Who, for example would now write a history of the Industrial Revolution without a consideration of the cheapness and ubiquitous availability of women's labour? Who would approach the question of religious change over the last four centuries without taking into account the differing responses of women and
men? Who would confront the development of welfare systems in Europe without considering why they were designed largely to protect women and which women legislators had in mind. Who would call into question that fascism in both Germany and Italy were underpinned by female endorsement, which was active not passive?

In these areas and with this kind of enquiry it is very clear that women’s history has taken a great deal by sap rather than storm.

If, however, it is obvious that the historical discipline has been stretched to comprehend a whole series of new considerations, of which gender is but one, can one claim a transformation of the entire discipline as a result? Certainly there has been much change, but a great deal of it has been to add additional courses to the curriculum, a case of adding (and stirring) women to the record to make a slightly different mixture. If gender was a more pervasive issue in analysis, its presence within the problematic of 'grand' history would be more apparent. Instead, for example of examining the history of the suffrage from 1832 to 1928 as a series of gradualistic inclusions, it would be debated in terms of ongoing exclusions. Or, to give another kind of example, instead of looking at what groups of women did during the French Revolution and making a few radicals speak for all women and
the words of one deputy (Amar, to whose lot it fell to close the women’s clubs)

constitute the discourse of all men, we would examine how the entire fate of the

Revolution depended in the last analysis on its reception in the localities and that

opposition by rural women as well as by men, though in very different ways,

undermined republicanism both in the privacy of the home and in more public

matters where it hurt (such as the non-payment of taxes, military desertion or the

resurgence of Catholicism, etc.). In other words, the differing experience of men

and women would determine the approach.

Where is women’s history or gender history going? One might avoid answering

this impossible question by saying that there are plenty of unresolved debates.

There are profound dissensions on the chronology of women’s experience, such as

how much continuity and how much change there was in their lives over the

centuries, the dichotomies of public and private spheres and whether such hard and

fast lines can effectively be drawn in the lives of women and men. There should be

more debate on the historical plausibility of generalizing about women (and men) at

all. However, the answer to the question where women’s history is going will surely

lie with the next generation of historians. The undergraduates of western Europe
while interested in social and above all in cultural history and, cognisant of gendered identities, do not necessarily think in the same terms as did their mothers. The late Marie Brive, one of the three women who held a chair in history of women in France, observed very openly at a Brussels conference in 1991 that the history of feminism as such was a matter of indifference to her undergraduates at Toulouse.

Michelle Perrot, at a conference in Rome two years ago, admitted that she could not explain the overwhelming success in Italy and France of the Histoire des femmes (5 vols, 1992), given the lack of interest in feminism among her students. 'Are they buying it for their mothers?' she asked.

I was recently struck by the observations on a book list of gender-history titles made by a group of female Oxford undergraduates, legatees of some privileges hard won by their predecessors and very confident in their ability to compete with men in whatever the free market economy may offer. As far as they were concerned, male dominance in the past goes without saying. There is no need to keep on tediously inventing the wheel. They have, in their terminology, been there and done (registered) that. However, they were far from disinterested in issues affecting women in history. They acknowledged the importance of recognizing and debating
issues such as the constant recourse of fundamentalist religious movements and right-wing political movements, whether sixteenth-century Protestantism or John Major on lone mothers, to the control of women's bodies. They could see how nationalist movements exploited women's reproductive capacities and the role of mother of citizens. (Claudia Koonz's Mothers of the Fatherland (1986) secured quite wide approval.) They were quite conscious of the difference between themselves and earlier generations. If biology means little now, it must have done so before contraception and abortion, and so on. History came through to me not only as a foreign country but as something of a cautionary tale. They were suspicious of woman as a category, because they were conscious of considerable differences within the sex. Women should have a history, but it should also reflect their relationships with other women as well as with men and the wider community. Women are clearly bearers of tradition, shapers of the next generation and so colluders in the construction of gender roles. Many may have been victims, but not all were innocent.

At the age of 20, the future as well as the past is a foreign country. These privileged young women live in interesting times. Fin-de-siècle western Europe has a
birth-rate that will not ensure population replacement, and unemployment, precarious employment and 'intermittent employment' threaten even the most highly qualified of both sexes. Women's history has reflected the preoccupations of its times and has always been political in the sense of drawing attention to a female predicament, variously defined. The pattern for women of high levels of education now is postponed parenthood while they find and establish a career or no parenthood at all (graphically described in France as la greve des ventres). If they opt out for maternity leaves, they fall behind irretrievably in the career path and some may regard this as a risk they do not want to run. In short they can compete in the market, but they do so at a certain cost. Or they can decide to run the risks of giving up. They have a 'choice'. For other women in part-time jobs lacking pension and other entitlements (the lot of 60 per cent of the British female labour force) and those in a context of marital precariousness the fin-de-siècle looks very gloomy. There will, in short, be plenty of issues to keep a woman or gender question alive.

Brooding on this on a cold December afternoon, I went into Blackwell's bookshop where the Christmas rush was in full swing. On a table towards the front of the shop lay Marina Warner's From the Beast to the Blonde (a
study of the interweaving

of gender roles in fairy stories and hagiography), Claire
Tomalin's Mrs Jordan

Profession (the story of the actress who gave birth to ten
sons by a future king of

England and who was, when he ascended the throne,
unceremoniously abandoned
to obscurity so much for the much-vaunted power of the
royal mistress) and

Stella Tillyard's Aristocrats (the story of the four
daughters of the Duke of

Richmond, who were certainly not victims of social
restraints or slaves to

convention, virtue and modesty). All were impeccably
researched, highly readable

and professional works intended to reach audiences beyond
the bounds of academe.

Certainly women's history could not, in these hands, be
charged with an ivory
tower approach and certainly history was alive, well and
profitable. Equally surely, I

thought, if I have to be a prophetess, the increasing
cohorts of women in higher

education can only enlarge the market if, as and when they
command indepen
dent incomes.

Both inside and outside academe, then, and whether or not
feminism has suffered

the demise of the 'grand narratives' which have been part
of the Western intellectual

tradition, women and gender in the past will have some part
in the historiography of 939

the twenty-first century. My very modest proposal for the
future is that the

historical assumptions regarding the two sexes should be

incorporated in some way

into the school curriculum so that the emergent
eighteen-year-old has as part of his

or her intellectual baggage a cognizance of exclusions and

arbitrary categorizations

attendant upon sexual identity in the past and consequently

the significance and


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In 1981 William H. McNeill delivered as the annual Prothero Lecture of the Royal Historical Society in London 'A Defence of World History'
(McNeill 1982). It seems almost as unlikely that McNeill might have been the recipient of that distinguished invitation more than a few years earlier as that he would have felt it necessary to mount such a defence, even to the Royal Historical Society, more than a few years later. This was a moment at which 'world history', conceived as a distinctive mode of study, not merely as a statement of comprehensive content, was making what in retrospect has been a sudden and rapid movement from the margins to somewhere quite near the centre of historians' concerns. The volume, variety and quality of published work on world history, and the respect with which it is treated, have increased greatly since then. Courses in world history are being established, professional associations formed, journals founded, graduate training inaugurated (see e.g. Vadney 1990; Curtin 1991), textbooks and readings published on an increasingly lavish scale, though at the time of writing no established academic post has been dedicated to the subject in the United Kingdom, and only a handful in the United States. In 1980 McNeill was still almost alone in the Anglo-American historical mainstream (and not everybody would have endorsed that description of him cf. Allardyce 1990) in championing its cause both by precept and example,
though in 1976 a substantial and elegant history of the world by a highly regarded Oxford historian, John Roberts, had not only achieved considerable commercial success but commanded the (sometimes reluctant) admiration of his professional colleagues. Roberts was probably the first British professional historian of standing to attempt, and certainly the first to publish, a history of the world embracing the entire globe from the evolution of man to the present day and addressed to an adult readership.

McNeill argued that world history was no different from any other kind of history in its goals and methods. It was based on the systematic and critical examination of documents and other empirical sources, subject to the same rules of evidence and of logic, using the same procedures and judged by the same criteria.

The problems of scale which arose from treating questions over much larger stretches of time and space than historians had been used to were problems of...
degree, not of kind, and did not differentiate world history in any fundamental way from other forms of historical enquiry. The frank positivism with which the case was argued risked introducing its own distortion to a world history founded on it, but it was well calculated to appeal to the audience to which it was addressed, and gained credibility from Roberts's as well as from McNeill's own work. In retrospect it does not seem accidental or inappropriate that McNeill should have argued that case at that time. World history is history, no less and no more, he might have said, echoing the famous peroration of J. B. Bury's Cambridge inaugural lecture in 1902 (Bury [1903] 1957: 223). Bury's declaration that 'history is a science, no less and no more' has won few plaudits. It seemed to endorse the possibility and desirability of establishing laws of historical development all the more unacceptable when repudiation of the claim that history provided a scientific foundation for Marxism was high on the Western intellectual agenda for most of the twentieth century.

More immediately influential was the celebrated assault by G. M. Trevelyan (1913), which had overtones of the broader conflict between French positivism and German Idealism (Barraclough 1991: 5 n. 9) but became particularly associated with an argument, trivial but persistent in the English manner, about the relationship
between history and literature, and whether historians should be grey figures in lab coats, or prophets and men of letters. In a sense Trevelyman’s was the more pertinent criticism, for Bury’s main concern was to vindicate history’s intellectual respectability. It was, he maintained, a rational discipline in its own right, with its own justification, goals and methods, which had completed its emergence from the penumbra of other divisions of knowledge in which it had been embedded since antiquity rhetoric and theology, law, metaphysics and literature.

At the end of the twentieth century, world history finds itself in a similar situation. Its novelty lies not in the attempt to see the human past as a whole but in the claim that scientific history provides the appropriate methodology and interpretative framework for doing so. Until modern times such projects belonged to the provinces of religion and metaphysics from which history has separated itself so slowly and precariously over the last two or three hundred years and in many parts of the world still not at all. Indeed, much of the resistance to world history among professional historians has arisen precisely from the fear that the attempt to grapple with questions too large to be tackled by means of the critical appraisal de

2 Cf. the comments on McNeill 1963 'the first genuine world
history ever written' of his Chicago

novo of the relevant primary sources, which they imagine to
be their habitual

procedure, might lead to a resurgence of the grandiose and
sinister speculative

structures that they associate pre-eminently with the names
of Spengler and

Toynbee.

The alternative of seeking to grasp the workings of human
society as a whole, and

therefore to relate the various aspects of social and
cultural activity to each other,

has been equally distrusted as belonging to the province of
economics and sociology,

and similarly tainted with deterministic and
anti-historical overtones. Until the

1960s scholarly history the history that was taught in the
history departments of

the leading universities of Europe and America and written
by their professors

was firmly rooted in the achievements of the historical
movement of the nineteenth
century, and still dominated by its outlook as well as its
methods. Its chief business

was with the origins and development of the state, the
distribution and exercise of

power within it, including the culture and values of the
governing classes ('the

political nation' in Namier's widely used phrase), and the
relations of states with

each other. The parameters of the undergraduate
curriculum, and of much the
greater part of research as well, were very largely those which had been shaped by Ranke and his pupils for continental Europe, and by Stubbs and his followers for Oxford, and through Oxford the British Empire. The agreement of the two traditions on the essentials of content if not always of method was variously but effectively demonstrated in North America. Everywhere the history of the nation and what were held to be its predecessors, usually from around the end of antiquity until the end of the First or of the Second World War, formed the core of historical teaching. This was complemented by selections from the general history of Europe, or (more recently) of the European empires, and by courses which stood in their various traditions for some essential core of European intellectual culture political thought from Aristotle to Mill or Marx in the United Kingdom, Western civilization from Plato to Einstein in the United States and so on.

By and large, historians did not regard the parts of history which fell outside these limits as, for practical purposes, part of their field. Economic and social history were left to dedicated departments or sections in faculties of social sciences, from which 'historians' generally stood aloof even when formally affiliated to them. 3

The ancient world was the province of classicists and
archaeologists; Africa, the

pre-Columbian Americas and the Pacific of archaeologists

and anthropologists (cf. Trevor-Roper 1965: 9); and Asia of multidisciplinary
departments of area studies,

dominated by the necessity of mastering its classical

languages, and largely

exhausted by the effort of doing so. The same divisions

were reflected in pro

fessional associations, journals and even library
classifications. Ignorance and

3 'History: includes the main historical divisions, such as

medieval, early modern and modern history;

British, European and world history; cultural, scientific,
diplomatic and local history. It does not

include Economic and Social History' (Higher Education

Funding Council for England circular RAE96

2/94 June 1994}). Notice also the use of the term 'world

history' in the time-honoured sense of 'neither

British nor European'. 943

parochialism had their usual results. Voluminous research

on the history of Europe

and North America conventionally defined was conducted

with high technical

accomplishment and often presented with panache, but

remained hidebound, sterile

and incurably parochial. The rest, though occasionally

illuminated by a Tawney or a

Finlay, a Hodgson or a Needham, seldom rose above the

conceptual limitations of

positivism, or exceeded in imaginative range what could be

achieved through the

philological scrutiny of texts and chronological narrative
based on its results.

There had always been critics of these limitations among professional historians,
of course, including some of the greatest, but they were marginalized not only by
academic conservatism but by the association of the approaches and techniques they
advocated with political liberalism, and especially with Marxism. Only in France,
where the historians connected with Annales and the Sixième section of the Ecole
des Hautes Etudes under the leadership of Lucien Febvre and Fernand Braudel
became immensely (though by no means uncontroversially) influential after 1945,
was the hegemony of the Rankean tradition effectively challenged. The annalistes
did not identify themselves as world historians, or advocate world history as such,
but they contributed variously and enormously to the developments from which it
has emerged, and especially to acknowledging the arbitrariness of the chronological,
regional and disciplinary boundaries which defined traditional historiography.
Calls for a 'new history' did not begin with the founders of Annales. They had
been vigorously articulated at least since Voltaire's Essai sur les mœurs (1751), and as
commonly ignored, especially after the canonization of the political and diplomatic
agenda in the nineteenth century. That does not mean that they have been without
influence, any more than political hostility and intellectual indifference to Marxism

or to annalisme prevented either movement from having a substantial and pervasive influence. World history itself owes a great deal to both. Although the teleological character of the Marxist analysis unquestionably tended to reinforce both Eurocentrism and the stereotyping of 'Asia', its intellectual structure and political sympathies committed it from the outset not only to include non-European societies indeed, all societies in its scheme of things, but to do so without a culturally or racially based assumption of European superiority or centrality. Even apart from its direct concern with such issues as the transitions from feudalism to capitalism, and from capitalism to imperialism, which necessarily carried discussion beyond the traditional limits of European history, the best Marxist writing was always distinguished by a breadth of vision not approached by alternative methodologies.

Similarly, the annaliste search for 'total history', though not for world history as such, has not only broken down disciplinary boundaries but stimulated work on a great variety of topics, including for example communications and long-distance trade, technology, social structures and mentalities, which are crucial for any coherent approach to world history. Any number of Annales itself, or of the many
journals founded in its footsteps (among which Past and Present is outstanding),

will illustrate the point.

With these exceptions, neither of them mainstream in the anglophone world, the

emergence of world history since the 1970s has owed very little to developments 944

within the discipline of history itself, but a great deal to developments of which

history cannot, in the end, help taking account. Its principal intellectual debts are to

archaeology, whose findings even the most conservative historians have found hard

to ignore (though the converse has not invariably been the case), and to social

anthropology. These disciplines have not only brushed aside the chronological and

geographical limitations by which historians were customarily confined, but

shattered the fundamental axiom of all previous historical study, that documentary

evidence provides the only, or at least the best, rational basis for apprehending the

past. On the contrary, social anthropology has drummed home the lesson that

historical 'sources' and history itself are the products of culture, and cultures are

socially constructed and reconstructed over time to serve the present needs and

purposes of the groups to which they belong (Cohen 1987; Fentress and Wickham

1992). Archaeologists have repeatedly demonstrated the power and potential of the
comparative method, the essential intellectual foundation not only of world history, but of all history that is not mere antiquarianism. Historians continue to treat the fruits of that method as purveyed by economists, sociologists and political scientists with deep distrust. Nevertheless, they have found it more and more difficult to despise the succession of increasingly substantial and sophisticated comparative studies of long-term political and social change which has flowed from this tradition. (Major examples include: Eisenstadt 1963; Moore 1966; Bendix 1978; Skocpol 1979; Mann 1988.)

If historians in the developed world were becoming more open-minded in the 1960s and 1970s by no means an incontrovertible proposition a more profound reason lay in the marked weakening of the professional consensus represented by the continuous study of national history and 'Western civilization' upon which the undergraduate teaching of the subject had been founded (Allardycce 1982 and, more broadly, Novick 1988: 311-14, 415-68). This was everywhere expressed by a fragmentation of the curriculum, which was reinforced by a growing demand for more 'professional' meaning more specialized teaching. Individual courses became more varied and often more imaginative, reflecting the wider interests and
growing technical sophistication of those who taught them, but the curriculum as a whole became less coherent with the disappearance or dilution of compulsory elements and survey courses: professionalism was eating its children. Meanwhile, outside the classroom the urgency of establishing an adequate historical understanding both of every part of the world and of the world as a whole became increasingly obvious, as did the collapse of every previous basis for doing so. In those decades not only nationalism and religion, but positivism, Idealism, orientalism and Marxism finally lost (in the Western world, but not only in the Western world) whatever intellectual authority remained to them, though all have retained their devotees. If there is a single reason for the rapid advance of world history in the last twenty years or so it is that no alternative paradigm commands sufficient credibility to satisfy an obvious and urgent need to understand the world we live in. The dangers inherent in that fact are no less evident than the opportunities. It has not always been obvious what world history is the history of. Every advance of the idea and every new statement of its claims has tended to enlarge the subject matter which it embraces. When history was conceived as comprising, in effect, politics, war and diplomacy, with occasional
leavening by high culture, the
area within which it was deemed to have taken place at any particular epoch was correspondingly bounded. As a popular and respected textbook on World History since J 870 put it in 1953: It was natural and proper until recent times to regard the history of the world as consisting of separate accounts of each continent. Except for periodic eruptions of Europeans into North Africa or America the development of each continent was a separate story. These irruptions might, in themselves, have drastic and far-reaching consequences, as did the barbarian invasions of Europe, or the Moslem invasions of Spain .... But even the total effect of these great movements was not to create a permanent and constant interaction between developments in each continent. Such an effect is the creation of only the last two centuries of modern history: only they constituted truly 'world' history. 4 Even upon the criterion enunciated here, the existence of direct contacts leading to 'permanent and constant interaction between developments in each continent', the continents themselves being conceived as essentially self-contained what has been called 'a world of socio-cultural billiard balls coursing on a global billiard table' (Wolf 1982: 17) the domain of world history is enormously enlarged as soon as the 'significant' contacts and developments are deemed to
extend beyond the fields

des of war and diplomacy. It was, indeed, upon the same
principle, if somewhat less

exactingly applied, that McNeill constructed his history
of the human community

largely upon the formation and diffusion of religions and
cultures which shaped his

essential building blocks, the 'civilizations'. In this he
owed something to the

influence of Toynbee, and much more to a habit of European
thought stretching

back at least to Hegel (though one might say to Orosius),
which sits comfortably

enough beside the traditional preoccupation of mainstream
history with elites and

their activities. This approach also had the merit of
being able to accommodate

without conceptual difficulty the enormous extension of
the historical vision in time

and space brought about by archaeology, which has also
employed the civilization as

its principal unit of account. In the process, however, the
necessary (though also in

some influential cases ideologically motivated) foundation
of archaeology in material

life has extended the definition of 'civilization' beyond
the merely aesthetic or

spiritual evaluation of dominant cultures, and thus not
only added significantly to

the current agenda, but extended it yet further in
content. Exclusive preoccupation

with 'the great civilizations' has been undermined and a
very large and far from
completed number of previously unknown societies and civilizations added to the
list of those which must eventually be comprehended in our account of the human
past and not only the remote past, for archaeology's concern with the non-literate
brings within our grasp great swathes of the history of societies of every kind,

history', or more commonly 'European and world history' at least as used in the United Kingdom in the
post-war period, almost invariably referred to courses which ran from 1870 or later. 946

including the urban, the industrial and even the post-industrial, which were
formerly unperceived and unconsidered. The progress of archaeology therefore not
only represents an almost unimaginable potential for new knowledge, but gives
powerful reinforcement to the view that the central preoccupation of human history

Great strides have been made in the last twenty years, though much of what we
describe as world history is still the history of the Eurasian continent, of which a
coherent and unified, though of course incomplete, account, with a common
chronology, could now be written at least for the period since about 800 BCE when
the adoption of cavalry by the peoples of central Asia placed them in a dynamic
relationship with the sedentary civilizations both of China and the Middle East, if not since around 2500-2000 BCE, when some would argue that the more or less simultaneous appearance of citied civilizations in China, South-east Asia, the Indus valley, Mesopotamia and the eastern Mediterranean reflected common participation in continent-wide patterns of exchange. The vicissitudes of those civilizations and their successors until around 1500 CE show, of course, enormous differences, and included long periods when, as it used to be thought, direct and indirect contacts between them more or less ceased. But we can now see, for example, not only that the decline of the classical civilizations in the early centuries CE, which seemed quite recently to herald 'an age of diverging traditions', was the product of a single set of circumstances, but that the emergence in their wake of societies in which wealth and settlement were more firmly rooted in the countryside, power more localized, and religion more personal and more or less fiercely monotheistic (for all that the differences in degree were so great as to produce radically different consequences in different parts of the landmass), nevertheless also represented responses to circumstance which had a great deal in common. The appearance at this time of a new world civilization, that of Islam, whose expansion,
continuing until c.1600 CE,

quickly placed it in direct contact with all the other
cited regions, and generated

exchanges whose repercussions affected the most remote
regions of the entire Afro

Eurasian land mass and the seaways which connected and
surrounded it, provides a

natural and appropriate vantage point from which the
history of the whole may be

written (cf. Hodgson 1993: 97 -121), up to the time when
the expansion of

European commerce, the rise of the Atlantic economy and the
emergence of

industrial capitalism placed the north-western extremity at
the centre of world

events. It may be noted that one implication of all this
is that a common periodiz

ation for world history is now emerging rapidly; its
desirability and usefulness are,

of course, no more dependent on incontrovertibility than
those of any other


It will be immediately apparent that such a history of the
Eurasian continent

includes that of North Africa from the outset, and that a
similar integration of the

history of sub-Saharan Africa lies in the not very distant
future. Although current

The phrase is Roberts's (1976: Table of Contents), but the
view is widely shared. 947 IsI

5 b

attempts to begin that process at a very early (that is,
pre-Roman) date have
commanded little agreement (e.g. that of Bernal 1987) we already know enough to be perfectly certain that the history (including the sub-Saharan history) of the continent is bound up with that of Eurasia from the earliest times. The lesson of the fact that the long habit of dismissing the possibility of such an integrated history rested upon an absurdly arbitrary equation of the writing of history with the availability of documentary sources for direct human contacts is that it would be quite wrong to discount the possibility of including Australia, New Zealand, the islands of the Pacific and the Americas in an integrated account of world history from very remote times. Beyond the ever more ancient and further-flung contacts between relatively developed societies which are almost daily revealed by archaeology, anthropology has demonstrated that even very 'primitive' cultures are not stable or self-contained entities. Change within them, for example in the balance between a tribe and its hunting grounds, could generate vast and far-reaching cultural, economic and eventually political transformations, while conversely such changes in that delicate equilibrium might easily be triggered by commercial or other activities at very great distances. 6 The point is no less powerfully made by the growing field of ecological history, whose observation of
the dramatic and
devastating transmission of cultural disturbance between
peoples quite unaware of
one another's existence transforms the entire surface of
the globe into a millpond
whose remotest beach will eventually register the intrusion
of a single pebble but
where the manner and effect of the ripples will depend not
only on the shape and
mass of the intruder, and of the beach, but also on
currents and eddies driven by the
breezes of the moment, and steered and directed by hidden
currents, depths and
shallows that have accumulated across the previous
centuries (Simmons 1993;

Once set upon this path, in other words, there is no
resting place. World history
is total history, regionally and chronologically as well as
in respect of its range of
subject matter. Even if we resist in practical terms and it
is hard to suggest upon
what principle we might do so the argument of David
Christian (1991) that we
must extend our vision from the Big Bang to the present,7
the vast enlargement of
the area which must be embraced by historical studies is
clearly permanent and
inescapable. World history need not, and indeed must not,
aim to be encyclopedic
that can be left to encyclopedias. On the other hand, it
cannot exclude anything
from its purview a priori, as Swiss history or military
history might do. It can only
be defined as a perspective, one of many in which any
particular event, structure or
6 See for example Wolf 1982; Hodges 1988. From a somewhat
different aspect Barfield 1989 proposes
a dramatically revised understanding of the relations
between nomadic and sedentary peoples in
traditional Eurasia.
7 Cf. Clark 1977: I: If we seek to understand the process
by which man has emerged to civilization
through his capacity to adjust to almost every environment
encountered on earth and even momentarily
on the moon, we need to take some account of his evolution
as an organism, and to recognise that both
his biological and his social evolution have been
accomplished in the context of a changing physical
setting.' 948
process may be viewed. Thus, the cholera epidemic which
killed 10,000 people in
Hamburg in 1892, after other European cities had learned to
prevent it (Evans
1987), may be seen as part of social history, urban
history, or the history of
medicine or sanitation; of the history of Hamburg, or of
Germany, or of the world
and it will be treated differently according to the
perspective. But world history is
also a perspective and the only perspective in which all
events, structures and
processes must ultimately be capable of being viewed. It
is, historically speaking,
the set of all sets. The familiar argument of the
classroom that sheer practicality
demands some restriction of subject matter deserves no
more time and patience than

would be accorded a chemist or a physicist who sought to
defend the arbitrary

exclusion of inorganic chemistry or thermodynamics from
the curriculum on the

ground that doing so would make it easier to master. Of
course it is always necessary
to select. But a rational and sound selection for any
purpose, including pedagogy,
can only be made on the basis of a systematic assessment
of the whole.

This objective is, in some respects, very much more
attainable than it seemed
even twenty or thirty years ago. There has been an immense
increase, qualitatively
and quantitatively, of published work on almost every part
of the world, and on
almost every period and aspect of history. The force of
habit is not to be underesti
mated. National history, and after that the history of the
developed world in recent
times, still dominate both teaching curricula and the time
and resources dedicated
to research. Politicians still command far more attention
from historians than
peasants, and men than women. But the surrender of
intellectual monopoly by the
guardians of tradition (despite regular and
well-publicized rearguard actions from
champions of the old hat and the new right) has released
non-traditional areas and
approaches from their ghettos. There has been not only a
great increase in the
quantity of historical work on areas other than Europe and
North America, and on
non-traditional subjects, but a much greater increase in
quality. What used to be the
backwoods of historical method, seemingly abandoned to
cultivation by scissors and
paste, are now at the frontiers of innovation, as the
historiography of the Indian
subcontinent, or of the Islamic world or traditional
China, will furnish striking
testimony. 8
These developments in themselves are the province of other
chapters, but one of
their consequences is to foster the creation of a common
agenda in world history.
The enormous field of peasant studies, in which central
interests of both Marxist
and annaliste historiography converge with those of
anthropology, offers an
exuberantly fertile example of the ways in which
fundamental research of high

8 At the textbook level compare Kulke and Rothermund 1986
with the comparatively recent and
widely used narratives of Wolpert 1977 or Spear 1966.
(Vol. I, by Romila Thapar (1966) is far
superior). The volumes of The New Cambridge History of
India, ed. Gordon Johnson, since 1988,
though regrettably treating only the last 500 years,
exemplify modern historical writing at its best, as do
Lapidus 1988 and Hourani 1991. For a valuable survey and
discussion of the current state of Islamic
together with a brilliant synthesis of its conclusions and
quality feeds and is fed upon theoretical discussion at successively general levels.

Geertz's (1963) investigation of the impact of Dutch colonization on wet rice cultivation in Java, Stein's (1980) analysis of the agrarian foundations of political power in medieval south India or Smith's (1959) account of the economics of the Tokugawa village not only stimulate continuing research and controversy in their own and other regions (e.g. Ludden 1985; Scott 1976; Popkin 1979) but are taken up in general discussion at increasingly general and theoretical levels, while one of its leading exponents, the anthropologist Eric Wolf, is the author of an outstanding essay in modern world history, Europe and the People without History. The range and variety of questions now being treated with similar vigour and sophistication obviously includes all those which have traditionally been central to social theory,

such as the political identity and role of the city (Humphreys 1991: 228-38; Lapidus 1969; Hourani and Stern 1970; Blake 1991; Skinner 1977; Rowe 1984; 1989), the structure and activity of merchant communities, the formation and dynamics of governmental and cultural elites and a wide range of associated issues including the history of science and technology, family structures and gender.
roles, 13 and social deviance and marginality. Despite the abundance of opportunity offered by subjects like pilgrimage, temples and their functions, or guilt systems (Christian 1972; Turner and Turner 1978; Stein 1960; Elvin 1984), there has been surprisingly little comparative (or comparatively inspired) work in the areas of religious history other than those stressed by Weber; perhaps religion continues to be seen as belonging to the province of civilizational and diffusionist studies in which it has always figured so prominently (cf. Bentley 1993).

The integration of much of this work into broader comparative and thematic studies has been in recent years a regular, stimulating and often dramatically fruitful process. Several examples have already been mentioned; others include the elegant and thoughtful essays of Philip Curtin (1984; 1989; 1990) on cross-cultural trade, Geoffrey Parker's (1988) lively discussion of the early modern 'military revolution', or the powerful reinterpretation by C. A. Bayly (1988; 1989) and others of the foundations of British imperialism, which also raises afresh the old but vital question of how far European capitalist imperialism halted the political and 9 e.g. Moore 1966, Skocpol 1979. It is hardly necessary to emphasize what an arbitrary selection from what an enormous field this is; see, e.g., Shanin 1989.
II The subject is far too large for systematic reference. For recent work on important aspects of these questions see, for example, Humphreys 1991: 187-208; Richards 1993; Fairbank 1992: 443-9; Powis 1984. Randomly chosen but contrasting and important studies (as in the following notes) include Waldron 1990; Huang 1981; Mottahedeh 1985; Bayly 1983.


13 Another topic on which any attempt to list recent work would be futile, but see for examples Casey 1989; Ahmed 1992; Ebrey 1991.

14 e.g. the very fine and explicitly comparative studies of Do Is 1992; Kuhn 1990.

15 Hansen 1989 raises issues which might be much more widely explored in comparative contexts. Economic progress of other advanced societies of the Old World, and where otherwise they were headed (e.g. Elvin 1973; Sivin 1980; Perlin 1983). The list might be endless. Most historical research, however brilliant and progressive, is not conceived in the context of world history, or primarily intended to contribute to it.

World history cannot ignore it on that account. It is and must remain dependent for its data on work which is planned, undertaken and executed in a more limited perspective. A primary task of world historians, therefore, is continuously to integrate the conclusions of research and debate in numerous fields into a series of larger perspectives, of which some, though not all, will
aspire to be total perspectives. One of their important needs and uses will be to encourage the identification of fresh problems for investigation, and the development of common methodologies and agenda across fields so distant that their cultivators are scarcely aware of one another's existence. One of their important educational functions will be to sustain a shared awareness among specialists of the advances and innovations which are taking place in areas distant from their own. If historians are to have a common historical culture, in other words, and if history is to be a coherent intellectual discipline, the place of world history in the formation of historians will become increasingly important.

The writing of world history tout court has been less obviously successful. It is not self-evident what such a history should comprise, or how it can be written. It is in the nature of historians to limit their field: they can hardly do otherwise. Yet the random accumulation of regional and topical studies, however distinguished, will not of itself sustain a synthesis which is coherently conceived, consistently grounded, and chronologically and regionally balanced. The collective solution, so congenial to the ideal of the objective and disinterested pursuit of knowledge, is a chimera. Acton and his successors as editors of The
Cambridge Modern History and its progeny have demonstrated all too successfully that his conception of 'a Universal History which is distinct from the combined history of all countries, is not a rope of sand but a continuous development, and not a burden on the memory but an illumination of the soul'\textsuperscript{16} is unattainable by his chosen method of collaboration. The ill-assorted though occasionally very distinguished legions of the Cambridge Histories, which have now colonized almost the entire globe, have performed a far more useful service for much of it than they ever did for Europe by providing an elementary narrative where there was none before, and by persuading many fine scholars to commit themselves to print in a relatively accessible form. Nobody could accuse them of constituting a consistent, coherent or even memorable view of the human past, as the publishers in effect confessed when they decided to issue the second edition of the Cambridge Ancient History chapter by chapter.\textsuperscript{17}

The History of Mankind (1963) undertaken by UNESCO after the Second World War (largely on the initiative of a quintessential Cantabrigian, though not a

\textsuperscript{16} Lord Acton, 'Letter to the Contributors to the Cambridge Modern History', in Stern 1957: 249.

\textsuperscript{17} The experiment was attempted with Vol. 1 (1968), but does not appear to have been a resounding
success. The New Cambridge History of India (above, n. 8) is, of course, an exception to these remarks. 951

historian, Julian Huxley) was planned on the same principle, for much the same reasons and with even less success. 18 More might be expected of series in which each volume is written by a single author. Of these the most ambitious, following the model of the splendid Evolution de l’humanité launched in 1920 under the editorship of Henri Berr, which gave birth to acknowledged masterpieces in several fields, provided a grand conspectus of the discoveries of history and archaeology, but made little or no attempt at a systematic overview, even in the arrangement of their volumes. On the other hand, those which have explicitly offered a ’history of the world’ have usually been organized as sets of national histories. We have yet to see a real equivalent for world history of the multivolume textbook Histories of England and of Europe which began to appear regularly at the end of the last century, one of the more humdrum but none the less staple harvests of professionalization. 19

Current attempts to produce a coherent synthesis of world history rely, for the most part, on one of two approaches, describing and comparing the development either of ’civilizations’ or of ’world systems’. That the decades since McNeill’s The Rise of the West and Roberts’s History of the World have not seen those works
surpassed in either sweep or quality is in part because
the approach by 'civilizations'

which they embody has seemed increasingly problematical.
Nevertheless, it

continues to provide the unit of much comparative and
synthetic discussion. There

is, for such purposes, great utility in the possibility
which the 'civilization' provides,

not only of limiting the number of variables with which it
is necessary to deal, but of

associating political, social and cultural systems in, as
it were, the same package

so long as there is a sufficient degree of consensus that
the packages in question do

in fact constitute coherent entities of the kind that the
argument requires.

Unfortunately, such consensus is almost by definition
undermined by the

relentlessly differentiating accumulation of knowledge.

'Civilizations' have offered a unit of discourse for world
history not only because

of their apparent unity but because they have never
appeared to be self-contained.

At the root of their formation, as McNeill (1982: 77-9)
insisted, it is always

necessary to postulate (within whatever theoretical
parameters) a widening of

human contacts and a meeting of cultures, whether by means
of the diffusion of

religious visions, military or agricultural technologies,
or patterns of trade.

Consequently, civilizations also provide a convenient
currency for the main
alternative approach to synthesis, that through the history of trading networks

which it is now fashionable to speak of as 'world systems'. In its purest form, with a

hyphen, the world-system is the invention of Immanuel Wallerstein (1974; 1980;

Accounts of the project are contained in appendices to each volume. The UNESCO General

History of Africa (Berkeley, 1981) is constructed on the same principle.

The plan for the Blackwell History of the World, which will provide a comprehensive series of this kind, includes volumes defined both by regional and by global parameters.

Notable recent exceptions include Gellner 1988 and Stavrianos 1990, which describe respectively successive dominant forms of cognitive and of social organization, and Hicks 1969. All three titles are unhelpful, since only the last is organized as a continuous narrative, and a singularly lucid one. 952

1989) under the inspiration of Fernand Braudel, and in the great tradition of jargon (and because it has been translated from German) means almost the opposite of what it says not a trading system which extends across the world, but, on the contrary, one which being self-sufficient constitutes a world of its own. Wallerstein’s argument, reduced to caricature, is that the 'capitalist world-system' which was formed in the centuries after 1500 differed from its predecessors precisely in that it did come to dominate the entire world economy, but as 'a
kind of social system the
world had not really known before ... an economic, but not
a political entity’

(Wallerstein 1974: 15-16) whose profits were ultimately
devoted to supporting the
institutions and exponents of international capitalism
instead of a bureaucratic
superstructure like those of the traditional empires.
Wallerstein’s conception has
been immensely controversial and immensely fruitful,21 and
has been widely
applied, with varying degrees of fidelity, both to
particular societies and periods and
as the basis of general interpretations of world history.
22 Particularly pertinent to the
present discussion is the persuasive and well-documented
contention of Janet Abu
Lughod (1989) that the trading networks of Asia and Europe
and almost as good
a case might have been made for adding sub-Saharan Africa
were sufficiently
integrated in the century or so before the Black Death to
be regarded as a
functioning world economy, without a corresponding
political hegemony. The
sweep and vigour of the argument developed over many years
and publications by
Andre Gunder Frank and Barry Gills that neither the
accumulation of capital nor
the construction of world-embracing trading networks was
peculiarly a European,
or indeed a modern activity at all promises to stimulate
considerable progress
towards the construction of a new synthesis, though it is
difficult not to sympathize

with Wallerstein's plea that in the process the concept of
the world-system has

become so general as to lose much of its usefulness
('World system versus world

systems: a critique' in Gunder Frank and Gills 1993).

In its original form Wallerstein's capitalist world-system
represents an especially

powerful approach to one of the crucial and most
intractable sets of issues in the

writing of world history, those associated with the causes
and consequences of the

emergence of industrial capitalism in Europe and North
America. 23 A fascination

with Western hegemony, frankly expressed in the title of
one of the most successful

synthetic discussions of recent years, E. L. Jones's The
European Miracle (1981; 2nd

edn, with discussion of reviews etc., 1987)24 is itself one
of the most obvious

consequences of the hegemony which is exercised over the
discussion of world

history by Western historians. Not in spite but because of
the centrality of the

grateful to Gills and Frank for copies of their work.

22 The most influential is that of Braudel 1982-5. On
Wallerstein (whose second volume is dedicated
to Braudel) and world-economies see ibid III, 45-91.

23 For a wide-ranging and useful set of recent discussions,
see Baechler et al. 1988.

24 Jones takes vigorous issue with himself in the more
ambitious and more original Growth Recurring:
Economic Change in World History (1988), which seems to have attracted much less attention, perhaps because its publisher was slow to provide a paperback edition. 953

question to so many aspects of social and economic history and theory, we can make no attempt here to consider it from any other point of view.

Triumphalism is out of fashion, and the tendency is much stronger to lament than to admire the manifestations and consequences of the European and North American impacts on the rest of the world over the last two or three centuries. 25

Conversely, Eurocentrism is now sustained not by conviction or complacency, or even by the weight of inertia contained within the traditional hegemony of European and American historiography, so much as by the fact that it is deeply embedded in both of the main forms currently available for the writing of synthetic world history, the comparative study of civilizations and world-system theory. The classical social theory in which both are rooted is itself largely a product of industrial capitalism, and largely designed to explain it. The assumption of European exceptionalism is shared by Max Weber and Karl Marx, however differently they valued it, and by virtually every influential thinker of their own and subsequent generations. Consequently, it has made little difference
Whether discussion is conducted in a spirit of complacency or apology: even where 'Europe', 'Islam', 'India' and 'China' have not been employed as the units of comparison, the absence or presence of the 'European city', the 'European family', the 'European merchant', 'European rationality' in government and in science and so on have continued until very recently indeed to dominate the agenda of almost all cross-cultural comparison. 26

The theoretically limitless scope of world history and the actual abundance of material pose in intensified form a very old dilemma ~ how to maintain scientific respectability without fragmentation, and achieve synthesis at an acceptable scholarly level. Conversely, nothing has done more to give world history a bad name than its malleability to the uses of the fanatic, the false prophet and the propagandist, and at the workaday level of classroom and study no practical question is more urgent or more intractable than how to make a rational and manageable selection of topics for attention from the enormous wealth of information that stretches back into the remotest times. The Eurocentric character of both the theory and the historiography which is based on it might seem to deny the possibility of a rational, humane and dispassionate world history. Happily it is the
result of circumstance
rather than logical necessity. Almost all the recent works
referred to in the
preceding paragraphs will supply examples of increasing
sensitivity and sophisti
cation in detecting the culturally conditioned models and
expectations which have
pervaded so much of the historian's vocabulary (for a
recent example, see Inden
25 Nevertheless, the importance and continuing influence of
Rostow 1960, as reflected for example by
Landes 1969 or North and Thomas 1973, must not be
underrated.
26 It is not the responsibility of this chapter to trace
the origins and nature of the general changes in
epistemological and historical thought which have
identified the tradition referred to in this paragraph
as a discourse of imperialism, but we must note the impact
on historians of Edward Said, Orientalism
(1978); it is too soon to estimate whether his The Culture
of Imperialism (1993) will be similarly
influential. For a pertinent and invigorating defence of
European rationality, however, see Gellner
1992. 954
1990). At a more systematic and a more cerebral level W. G.
Runciman has
provided a comparative account of social evolution of
extraordinary range and
power which, when absorbed, may go far towards relieving us
of our dependence on
Max Weber. 27 In one of the most remarkable of recent
historical works, Asia before
Europe, K. N. Chaudhuri has presented a strikingly original
solution to the dilemma

presented by the selection of problems and information, based on mathematical set

theory, which provides a culture-free basis for defining identities, and one which,

in his hands, has supported brilliant and evocative historical reconstruction. 28

The task of synthesis is greatly magnified by the prospect of an expansion of

historical knowledge which will dwarf all its predecessors. The birth of 'scientific

history' in the nineteenth century and of the forms and ways in which it was written

was the product of both liberalism and nationalism, of the information explosion

produced by the opening of the archives of Europe's chanceries, and of the

enormous growth of recordand information-creating activity of every kind which

was inseparable from the emergence of modern society itself. In the same way the

questions and methods of world history in the twenty-first century, as well as its

conclusions, will be heavily shaped by analogous movements of state-formation

and state-destruction across the world, especially since 1945, on one side, and a

massive potential for the creation and preservation of historical materials inherent in

the extension of governmental and corporate activity, of the diffusion of literacy and

the construction of complex societies, and of the information revolution, on the
other. The mundane tasks of listing, cataloguing and chronicling, securing intellectual and physical access both to materials and to audiences, the spade-work of the pioneer archivists, librarians and committee-men of the nineteenth century, will continue to demand the greatest part of both the physical and the intellectual resources available to history for a long time to come. It is only necessary to compare the extent to which Essex, say, or the Auvergne, has been pillaged for monographs on each of the last ten centuries or so, and to consider that (like our nineteenth century predecessors) we are only beginning to exploit the resources that exist for the investigation of Hunan, Awadh or Benin and those are examples from some of the longest and best-researched regions of the non-European world to grasp that writing the history of the greatest part of the world has barely begun. Indeed the conditions for writing it have barely begun to be established. In most parts of the world, history remains subordinate to the demands of political power and religious doctrine. Even where that is not the case (as in India and Japan, and parts of South-east Asia, Africa and Latin America) limitation of resources and a

27 W. G. Runciman, A Treatise on Social Theory: II Substantive Social Theory (1989). The title, which is not calculated to seduce historians, is one of the few parts of the book that they can afford to
ignore.

28 Chaudhuri 1990, with further methodological discussion in Chaudhuri 1993. Braudel, whose influence on Chaudhuri was very great, occasionally uses the language of set theory, notably and effectively in the concluding chapter of Capitalism and Civilization, vol. II, ‘Society: "A Set of Sets'" (pp. 458-599), but as far as I know he did not propose or elaborate it as a general basis for historical method. I have discussed Chaudhuri's thesis at greater length in Moore 1993. 955

natural preoccupation with national and regional issues tends to keep historians isolated from each other, and to slow the entry of new contributors to the discussion of world history itself, which continues to be dominated by Europeans and Americans. The collapse of the Soviet Empire seems likely for the time being to narrow rather than expand the meaning of 'European' in that context. Much as in the nineteenth century, the opening of the records has been seen as both a consequence and a condition of liberation, and establishing the historical record is regarded on all sides as an essential weapon against tyranny. Wherever in the world old regimes and old dominations are giving way to new ones through blood or ballot box, wherever legitimacy is challenged or to be established, the battle for the past will be waged as passionately and as destructively as the battle for the future of
which it is part. The freeing of history from myth and rhetoric of which we have spoken is itself an essential element of the modernization of society. There is no reason to suppose that it will proceed any less tortuously, brutally or remorselessly than the broader process of which it is part. The methodology of modern history, like that of modern science, is a product of the West, and liable to all the suspicion, justified and unjustified, which that implies. It is also, like modern science, slowly establishing a common discourse for a rational and humane cosmopolis. In that sense, and to that extent, Bury's much-derided optimism remains defensible.

Meanwhile, unevenness both in the depth and variety of documentation and in the quality and agenda of scholarship will remain the greatest single limitation on the development and standing of world history. The extension of its base beyond the dominant cultures of the literate civilizations will be a very slow business, and world history itself will continue to be, with the exceptions mentioned above, to far


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Archivists have until recently rarely perceived the need to defend the preservation of documents for historical scholarship. The profession emerged from the antiquarian antecedents into a scientific subject. Manuscripts were to the humanities what observable natural phenomena were to the sciences. Like natural phenomena they were open to different interpretations; but their central position in Geschichtswissenschaft was taken as axiomatic.

Throughout Europe in the late eighteenth century the discovery and rescue of archives became an important preoccupation of scholars and later of government. A question of national prestige. Archives of nations subsumed within larger states were considered vital in the preservation or creation of distinct cultural identities, particularly where such nations had a common language. At first attention concentrated on medieval records, moving quickly to the modern period in an effort to preserve the papers of individuals and powers which had participated in...
the great events that had culminated in the defeat of Napoleon. Until the twentieth
century the focus remained on great events, great men and
government. National
archives were established to preserve the bewildering mass
of records produced by
modern bureaucracies. Archive commissions busily
investigated the contents of
cathedrals, monastic houses, corporate institutions and the
homes of great families.
Some owners of important collections of manuscripts
published elaborate
catalogues, transcripts and sometimes facsimiles of their
holdings. As the German
concept of Geschichte spread in the late nineteenth century
and as history became
an accepted component of a revised university curriculum, it was to these
manuscripts that the new profession of academic historians
turned both for their
own research and for that of their students engaged in
advanced study. The care of
manuscripts and their interpretation as objects, itself
became a separate profession
with its own skills in palaeography and diplomatique that
demanded considerable
scholarly expertise.
In the early twentieth century historical interests began
to change reflecting a 960
transformation in attitudes and perceptions throughout
European and North
American society. Liberal ideas informed by Hegelian
Idealism and socialist
aspirations informed by Marx shifted attention towards the function of the individual in society and by extension to the economy. Understanding of societies and economies required the collection of a more extensive and bulky range of archives, particularly records relating to individuals. In much of Europe since the early sixteenth century churches and government had collected information about individuals for whole variety of purposes baptism, catechism, confirmation, marriage, burial, military service, censuses, admission to poorhouses or prison, and so on. At the same time as government became more formal and extensive, there was a need to raise regular and greater taxes both locally and nationally on individuals and on trade. At various times taxation demanded the accurate recording of property, income, estates at death, imports, exports, and the production and sale of certain goods (for example alcoholic drinks and salt). In addition many regimes themselves became engaged directly in trade and industry, which left behind large accumulations of paper. Apart from all these routine records, the rapid growth of modern states in the nineteenth century had caused major social and economic problems that had been addressed by governments through special inquiries that collected huge volumes of statistical observations.
The use by historians of such transactional records of government posed new
problems for archivists; not simply in terms of their bulk. Their interpretation
demanded new skills far removed from the training or experience of many existing
custodians. What circumstances had led to the creation and modification of the
records? How reliable were they? Although they clearly contained a great deal of
information about people and economic activity, they would be used for purposes
for which they had not been created. Correspondence about a battle, a building or a
political event was written for that purpose; a customs account or an entry for a
catechumen in a register were demonstrably not recorded to answer the questions of
later generations about transatlantic trade or about patterns of migration in early
modem Scandinavia. In seeking answers to these questions historians and archivists
looked for sources that would complement and contextualize transactions. The
search was extended to include the records of property and enterprises owned by
great families and institutions, the papers of lesser families and 'ordinary' people,
and the archives of businesses. Many of these were to be found amongst the vast
collections of legal evidence preserved in local and central courts, which even today
are often inaccessible. Others were held by notaries and solicitors, and by families
and businesses themselves.

To accommodate the mountain of records that were turning up in Europe, North America and elsewhere in the world, archive provision became more plural as embryo repositories or manuscript departments were set up by regional and local governments, by universities, by museums, and by businesses and institutions. This led to tensions. National repositories that had made a virtue of collecting records of 'national significance' now found themselves under academic pressure to accept modem records that would under existing criteria not have been preserved and in 961 competition for new accessions. Speaking in Paris at the first International Congress on Archives in 1950 Sir Hilary Jenkinson declared:

De plus, on peut ajouter qu'il y a encore beaucoup à faire pour faire comprendre au public ce fait que les archives modernes doivent être aussi importantes que les archives anciennes et qu'en fait, un jour elles seront elles-mêmes anciennes. Le problème est également d'arriver à disposer de ces archives. Vous avez avec sujet-là la question du lieu ou l'on peut mettre de grands dépots d'archives. Il faut savoir si ces archives sont considérées comme étant d'un intérêt national véritable ou si, au contraire, elles ne sont que d'un intérêt local.

This statement, redolent of the Anglo-Saxon approach to
in stark contrast to the aims and ambitions of the recently established Annales group in the Ecole Pratique des Hautes Etudes in Paris, which knew no such distinction. They were determined to move away from the narrative history of great events towards the rigorous, often statistical, analysis of the rich sources (particularly the mass of transactional records) now available to explore the past from differing local and national perspectives. This was to be done not by the lone scholar working year in and year out in a great library, but by research teams (iquipes) of historians often working in different parts of the country and later different countries on related themes designed to inform common questions. Inevitably such iquipes had to be interdisciplinary to include statisticians, sociologists, economists and shortly computer programmers. Even today, with a few notable exceptions, such an approach is foreign to Anglo-Saxon scholarship and it is hard unless you have been a member of such an iquipe to explain its intellectual excitement.

In the post-war years universities expanded rapidly, recruiting staff and encouraging research across all disciplines. Demand for staff with postgraduate qualifications led to the exploitation of archives on an unprecedented scale, financed
for the most part by public funds. The establishment of new universities reinforced the development of local and specialist archives, which in turn encouraged the emergence of centres of research dedicated to specific themes, topics or methodologies. Some universities, particularly those in North America with generous benefactions, began buying manuscripts in Europe and even in India and the Far East in support of research centres. Although national archives shared in this growth, they increasingly became part albeit as a senior partner of a plural system. Amongst many historians the narrative tradition persisted; but a large number were influenced by the social science method if not the teamwork approach of the annalistes. Consequently they wished to experiment with new types of evidence: oral tradition, recorded memories and images. Some of these had been collected since the mid-nineteenth century; but often for different purposes, such as the recording of unwritten languages, folk music and dance. After the Second World War historians were able to collect such material in far greater quantities using the new technologies, and they shifted the emphasis towards contemporary events such as the experience of the Holocaust or the resistance movement in Austria to the AnschlufJ. Although archivists, most of whom, if they had been
trained at all, had been trained in narrative schools and in traditional paper technology, found the new methodologies difficult to comprehend, they were willing to collect records in their support, because most such material was to be found locally and therefore helped local archive centres to develop. Little or no attempt was made to discriminate between collections all records were worth keeping and if historians did not use them today they would tomorrow or in a hundred years' time.

There were other less direct but equally important changes in scholarly approaches to history, which stemmed in part from the annaliste method and in part from a growing interest in the development of science and technology. From at least the mid-nineteenth century there had been a fascination with industrial and commercial success, which produced such best-selling works as Samuel Smiles's Lives of the Engineers, 1861-2. In the wake of the massive industrial changes after the Second World War, the most obvious expression of these interests was in what became known as 'industrial archaeology', a concern to record and preserve the buildings and artefacts of the motors of economic change. This was accompanied by a recognition that the study of buildings and of urban and rural landscapes was
critical to the interpretation of the past. Interest extended far beyond the efforts to

preserve the great houses of Europe and colonial America, which had started in the

late nineteenth century. Landscapes were also not to be preserved just because they

were picturesque, but because they were of historical or natural interest. Architects

and planners, particularly those involved in the massive reconstruction of bomb

damaged Europe, wanted to know what buildings and landscapes looked like so they

could be faked. They naturally turned to archives in search of drawings and

photographs. Industrial archaeologists, most of whom worked in loose teams,

hunted for historic maps, photographs and technical drawings. Archaeologists,

responding to concerns with trade and the 'ordinary', extended their activities into

the more recent past where supporting documentary and oral evidence was more

plentiful. By the 1990s with the fashion for ideas of, sustainability' and conservation

these approaches to the past had won a large following. For the archivist they

presented a further range of difficulties. Collections of both architectural and

technical drawings are bulky and difficult to interpret without specialist knowledge,

as industrial historians discovered. From the late nineteenth century they were also

often made on special materials that require expensive
conservation. Likewise early photographs were mostly taken on glass negatives and few archivists or archival conservators have any specialist knowledge in this area.

The transformation of history from a cottage craft into an industry was accompanied by an equally rapid growth of 'local' and 'family' history. In its beginnings history as a scientific discipline owed much to antiquarian scholars, who rescued, transcribed and published documents, wrote learned articles, and helped teach the subject in universities when it was first introduced. As their profession became established, academic historians affected to disparage antiquarians and yet the antiquarian tradition survives everywhere in Europe and North America in a welter of local and specialist record and history publications. Until the mid-twentieth century antiquarian pursuits were on the whole the recreation of professional people with scholarly backgrounds clergymen, doctors, lawyers and the gentry. Mass education, the availability locally of freely accessible archives and the increase in early retirement were to change this. From the 1950s 'history' was colonized by enthusiasts with well-defined interests, their parish, their family, their house, a locality, ships, postage stamps, railways, locomotives, beer, gardens and so on all with long pedigrees in scholarly and popular
literature. At their worst these
enthusiasts are mere collectors of 'interesting' facts; at their best the enormous and
highly specialized literature which they produce makes a vital contribution to
debates and furthers research. Critically for the archivist, the enthusiast depends for
sources on the records that began to be collected in such quantities to sustain
developments in economic and social history. Family historians use registers of
baptisms, catechumens, marriages, burials and probates very heavily. They now
form the large constituency of users in national, regional, local and even most
specialist repositories. University archives as far apart as Graz and Glasgow receive
far more enquiries about former students than requests from scholars and students.

Local history was also exploited by teachers and archivists to make the school
curriculum seem more relevant. It was argued that children could understand the
First World War better if they could relate events to Great Grandpa Otto or Bill
than if they simply learned the progress and outcome of events. Packs of original
documents were prepared for use in the classroom and 'imaginative' projects
devised drawing on a range of evidence the local landscape, buildings, museum
holdings, memories as well as documents. These initiatives, designed to illuminate
general historical themes, were a prelude to much more radical changes in the curriculum with an emphasis on discovery or student-centred learning. Across the curriculum schoolchildren were encouraged to undertake projects (the bane of parents' lives) that would demonstrate both personal response and detective (research) skills. Schoolchildren have become in the 1990s research students without the necessary methodological skills or framework of historical reference.

The undergraduate syllabus has been similarly transformed with greater emphasis on dissertations and the exploration of historical data. As a result archivists have found themselves having to provide advice and guidance to pupils and students in search of the novel rather than the attainable.

This explosion in the use of archives by enthusiasts and students changed completely the outlook of most record offices. The close links that had existed with the scholarly community broke, and even national record offices became driven by the needs of enthusiasts, who in the post-war liberal world expected access to be free to all documents, whether public or private.

Deterioration in relation with academic historians was more pronounced in some countries than in others. In the English-speaking world and in the Nordic countries this was partly of the archivists'
own doing. In an effort to differentiate themselves from historians, they evolved an

ethos in which they provided the documents (often with an extensive critical

apparatus covering questions of provenance, style and authenticity) for the historian
to interpret within the context of wider historical debates. This distancing of the

archival profession from academic history was confirmed by the development of procedures needed to manage non-current modern records before the selection of

'歷史ically interesting' material and transfer to archive repositories. Records

management has now developed in many parts of the world into a separate

profession from that of archives. In Germanand French-speaking countries and in

most of central Europe, a different tradition of the scholar-archivist persisted. In

these countries archivists are often very well qualified both as academic historians

and as archivists; but even they have not been immune to the demands of the

enthusiasts and the pressures of the information managers.

In all countries archivists have steadily arrogated to themselves the right to be the

final arbiters of what records are deemed to be 'historically interesting' and therefore

worthy of long-term preservation. Since they have to negotiate the budgets for their

services this is understandable; but it leaves them very exposed to criticism from
scholars that important material that will inform new
debates and disciplines is

being discarded. Indeed some specialist archives owe their
origin to an academic

conviction that records were either unnecessarily and
wantonly being destroyed or

simply being overlooked by existing provision. Academics
are uneasy that selection

criteria and cataloguing priorities reflect either current
use in record-office search

rooms by family historians and enthusiasts or economic
expediency, rather than the

long-term needs of the discipline. For their part
archivists are concerned that many

historians in the social science or annaliste tradition
will combine with family

historians to force the retention of huge series of
personal transactional data whose

very bulk precludes investigation. They can point to large
collections on their

shelves that they have been persuaded to accept, but are
rarely used. The sheer bulk

of modern records and the fact that procedures are in place
to ensure that they are

considered for long-term preservation demands selectivity.
Neither historians nor

archivists have engaged in any meaningful debate about how
suitable criteria can be

agreed. For personal data (with the exception of records
of births, marriages and
deaths) statistically sound sampling techniques must be
employed. Both archivists

and historians, particularly those trained in the narrative
tradition, are deeply
suspicious of such statistical methods, preferring the retention of records of whole populations. Other types of records pose problems of significance and relevance where decisions on retention have of necessity to be subjective. They could undoubtedly be improved if efforts were made to re-establish the dialogue between historians and archivists.

Some historians and archivists believe that decisions about selection will gradually cease as more and more records are held in machine-readable form. This is very questionable and in any case only applies to records created in the future in systems where long-term management is considered important at the outset. For example, most personal records held on computers are derived from forms; and such sets of data can only be considered to be archivally complete if they include all the information on the form. If items are excluded, then the dataset cannot by most criteria be considered to be the original record. Scrutiny of datasets often reveals that entries have been 'normalized': in other words variant spellings have been standardized and mistakes rectified. To some later historians the fact that someone could not spell or write well may be significant. Certain sets of personal data are very bulky, for example hospital case notes, criminal records and tax files. In these
cases it is improbable that all the data will in the foreseeable future be held in machine-readable form. It is likely therefore that the archivist's task will be complicated rather than simplified by the existence of large datasets of personal records, which are already being kept in specialist (usually national) data archives.

The archivist will have to draw statistically sound samples from the datasets to determine which of the actual files are to be preserved in the long term, while at the same time preserving the dataset to provide the overall context.

Narrative sources in machine-readable form present a far more complex problem. The IT (Information Technology) industry has stolen the terminology of the archivist and records manager notably the 'archive' and the 'file'; but not their meanings. In the IT world 'archive' does not imply selection criteria or permanent preservation, and 'file' does not equate to the organized contents of a physical file.

However, in approaching machine-readable text the archivist and historian have to forget these meanings and develop new frameworks of reference. There is no doubt that most narrative documents throughout the world are being produced on computers, and software, and that mark-up protocols are being developed to allow information. This would suggest that the physical file of
records as we know it may disappear. Paper filing, as all historians and archivists know, is wasteful, involving much duplication as documents inevitably relate to more than one subject or individual. In any case, filing is expensive in labour and space. Instead, marked-up documents will be held in text bases that can be searched freely and easily transported to other platforms. The danger for the archivist and historian in this new world is that a great deal will be lost unless adequate management controls are imposed. Crucially, what may be missing from the documents themselves are the drafts and redrafts, which clog paper files but show clearly how a policy or decisions evolved and who was responsible. Even where drafts are retained it may be difficult to attribute changes in thinking to individuals. In addition, where confidential documents only exist in machine readable form, destruction is all too easy. The historian will be able to explore such text bases much more easily than paper-based records and employ sophisticated content analysis techniques, but will be left with formidable problems of bulk in writing narrative history. The archivist will need to learn a further range of computational skills. New technology may solve some of the problems of bulk and selection in the future, but it will do little to reduce existing
accumulations of paper. Scanning typewritten records is both expensive and problematic, and the scanning of hand written records is even more costly and uncertain. This is not say that scanning will not be used to create text bases, it will; but not for any significant proportion of current holdings. Computers, however, have long been recognized by both academic historians and enthusiasts as providing the means of unlocking information held in long runs of bulky observation data. Historians from the 1960s have been engaged in building datasets and more recently text bases to address specific historical questions, often employing teams of researchers to abstract and code data.

There is an active methodological debate about data-entry techniques, which has much in common with earlier discussions about the relative merits of calendaring and cataloguing should the dataset be an accurate transcription of the original, should it be normalized, should it be coded and should it include data not needed for the current project? In the early days when only numeric processing was possible these questions more or less answered themselves. With the advent of alpha numeric processing in the 1980s and new generations of software they have to be addressed from the outset. Invariably a database, particularly a relational database
where data is held in more than one table, is only an
approximation of the original
data, even if efforts have been made to respect the
integrity of the source, for the
simple reason that entries need to be enriched and
normalized to permit analysis.

For example, at a simple level a place name needs to be
supplemented in the same
or another table with the name of a parish, a locality, a
region and even a country.

Likewise occupations need to be coded and decisions taken
about multiple job
descriptions very common until recent times—do they
represent two or three
separate jobs or should they be counted as a half or a
third, and was one job more
important than another? In both data and text bases there
is a temptation to combine
data from other sources, just as there was in the critical
apparatus that supported
calendars. If the identity of an individual in a source is
known, why not add the
extra information? Archivists will assume that data and
text bases, like transcripts,
while they enhance access are no substitute for the
originals. In any event the cost of
their creation will prevent only but a small fraction of
existing paper based holdings
ever being available in machine-readable form.

Data and text bases will continue to be constructed by
academics, enthusiasts and
perhaps more importantly by archivists themselves as
guides and catalogues to their
holdings. National, local and specialist archives have been using computers for nearly as long as academics; in some cases research databases double as archive catalogues. Throughout the world the standard and depth of archive catalogues is very variable. Whatever their level, they mostly require elaborate indexing by names, places and sometimes subject or theme. As all historians and most archivists know, paper-based catalogues and indexes, especially those in large national repositories, are frustrating to use, largely because they are full of internal inconsistencies. This is not necessarily the fault of the archivist and often reflects changes in procedures to take account of perceived developments in the historical sciences. Where funds allow, efforts are being made to convert catalogues into machine-readable format, either as databases or text bases, in order to make the information they contain much more accessible and to save the expense of elaborate indexing. Such initiatives are not without dilemmas. Existing catalogues often lack an adequate critical apparatus explaining provenance and background. To sustain thematic enquiry they need to be supported by thesauri and subject indexes.

Moreover, many catalogues are old, placing emphasis on the historical concerns of the period when they were produced. None the less, making catalogues available in
this way can only be welcome, providing users understand their weaknesses. 967

Searches using even the most sophisticated software can be guaranteed not to locate all the material that might be of interest. This is most obvious in information contained within an individual 'piece' for instance a ledger, a letterbook or a diary but will also be the case where individual items within a group are not separately catalogued.

The very existence of on-line catalogues, research data and text bases, and machine-readable archives, is already generating demands for access. This in turn poses problems for the archivist of validation, documentation, standardization and dissemination. Some of these are familiar, others derive from the new technology.

Until recently, data and text bases were the responsibility of specialist national data archives, which set standards, compiled documentation and had the skills to move data between different platforms. Such national data archives will continue to have an important role, particularly in the maintenance of national datasets such as those containing population information. However, as compatibility between different software and machine platforms becomes less of an issue, as the Internet develops and the volume of machine-readable records explodes, so more and more data will
be held locally, following rather more rapidly the
development of archive services

themselves. Remote access over the Internet in the
immediate future will be
differential, as some institutions in some countries have
free connections to the
Internet while others face high subscription rates. In the
long term the experience of
other IT developments suggests that costs will fall; but
almost certainly existing
free provision will be replaced with charges.

Undoubtedly, over the next century the 'superhighway' will
greatly enhance
access to historical records, many of which will be in
machine-readable form for the
contemporary period. The historian, working in St
Petersburg, will be able to
analyse data, text and image bases held in Washington or in
Delhi with little
difficulty, using powerful tools. In this scenario some
argue that the archivist will
become simply an information provider, just another byway
on a superhighway

jammed with data overload. The development of the
historical and archive
professions suggests that this is fanciful nonsense. It
remains to be seen if global
access to information will create convergence in any
practices; it could just as easily
lead to greater divergence as nations and regions seek to
assert their identities. For

all the claims made for text and image analysis using
emerging mark-up tools like
SGML and HTML, it is difficult to conceive what they can offer beyond content analysis that will allow documents or paintings to be ascribed with far greater certainty through the identification of images with similar characteristics. The historian will still have to interact with sources and there will be far more of them.

The archivist will still have to select, organize, explain and reference; but perhaps not catalogue (in the traditional sense) machine-readable collections. It is difficult to believe that the global archive accessed down the superhighway represents anything more than a quantitative rather than a qualitative change in approaches to the past.

Historians of all complexions and interests will continue to be concerned about times before the advent of computers. There will still be vast accumulations of paper archives and it would be safe to assume that such traditional holdings will continue to grow, particularly in countries unable to afford mass access to the new technology. In the developed world all that can be hoped for is that the superhighway will continue to improve access to these collections through better on-line finding aids. New data, text and image bases will be created, but they will never represent anything more than a fraction of the totality of holdings. Although it will be possible to obtain copies of documents over the
superhighway relatively cheaply,

this will be no substitute for visiting archives. Experience suggests that far from

reducing visits, on-line information to whatever depth of cataloguing increases both

the number of visits and the volume of productions. For most, visiting an archive

will continue to be straightforward, as the majority of users will remain predomi

nantly local. However good the on-line catalogues, visitors to search rooms will still

need assistance with palaeography, the meanings of words in the past, dialect

spellings and so on. The archivist will also have to provide help and guidance to the

historians surfing the Internet through navigational aids on the superhighway itself

and by direct brokerage. Current thinking amongst archivists and rare book

librarians suggests that this brokerage activity (helping readers locate other sources

relevant to their research) will become more important as IT reduces the number of

routine tasks.

There is nothing to suggest that historians will be less interested in using

archives. In the written cultures that have dominated European, Middle Eastern

and Eastern cultures for over two millennia, it would be strange if it were otherwise.

The definition of what constitutes an archive will continue to change and extend to

include new types of evidence, but it is already accepted
that the term is simply a
useful metaphor for a certain type of historical evidence
which together with others
contributes towards a holistic understanding of the past.
As has happened already
with images and oral recordings, these new types of
evidence will present the
archivist with new problems of description and
conservation; but on past experience
these will not be insurmountable. There will also be a
continued blurring of the
boundaries between printed literature and archives as more
material is disseminated
in machine-readable form. This is likely to present more
of a problem to librarians
than to archivists, because of the rigid nature of
international bibliographical
reference procedures. Already some librarians are demanding
more flexible systems
akin to those used by archivists in response to their
changing role in informa
tion management. Where big developments can be expected is
in the
complementary use of physical objects held in museum
reserve collections, which
are being made much more accessible by the use of new
technology. This in turn
will transform museums back into repositories, making them
more like archives.
The massive expansion in archive services throughout Europe
and North
America in the last fifty years has been largely funded by
government, either local
or national. In recent years cuts in government expenditure, coupled with the
challenge to the liberal attitudes that characterized the post-war world, have
squeezed funding at a time when demand has been growing rapidly. Costs have also
been rising, partly through increased use but also through the introduction of new
technology, which for all its advantages is expensive. Participating in the global 969
archive will add even further costs. Archivists have on the whole resisted charging
for access. They have used the management of the modern paper records of their
employers as one means of cross-subsidizing historical services. With the growth of
machine-readable data managed by information professionals this will become less
possible. Many archives have generated income through the sale of copies of
documents, plans and photographs to enthusiasts. Some, following the example of
libraries and museums, have established associations of friends. Others now charge
depositors and undertake paid research work for users, particularly for the
ubiquitous family historians. Most solicit donations and many seek independent
grants for specific cataloguing projects. As government funding is reduced, so
pressures to raise more external income increase.

The concept of charging is not simply a response to reduced budgets, it is
bound up with notions about the free market and transparency of transactions. It is argued that if an archive provides a records management service this should be properly costed and charged for. If enthusiasts have to pay for other recreations, then why should they not pay to use archives? Schools and universities buy services, why should they not also buy archive and library services? Are archives in one country to be freely available to bona fide scholars from another? Concepts of purchaser and provider relationships pervade democratic countries whose citizens' freedoms have in part been assured by rights of free access, to a greater or lesser degree, to records of government. Whether they are aware of it or not, this presents historians with a serious dilemma. Arguments for free access to the records of government based on democratic rights will be no more successful than arguments that in democracies the state has responsibilities for social welfare. In some countries it has for long been accepted that access to certain government records of family transactions have a price, which on the whole individuals (usually family historians) are willing to pay. The danger for academic history is that the enthusiasts, who contribute a large part of the external income, will drive the market. Already, collecting policies and cataloguing priorities are being
influenced by their needs. Most enthusiasts only want access to very specific transactional information, grandfather’s case-notes in a mental home, a drawing of a ship or a photograph of their house. Such information is always contained within larger collections, which would set it in its context. As a result large parts of collections appear to be little used in comparison to the very heavy usage of these transactional records. Against a background of financial stringency the archivist will have difficulty in justifying the decision to keep little-used records for the benefit of future academic research. Those historians, who argue beguilingly that archives are the tools of a dead discipline, do not help. Professional historians have to accept that charging for services will come and find ways of securing the necessary budgets, but that in a sense is the least important aspect of the problems. They must seek to re-establish the relationship with the archive profession, if for nothing else than to claim their intellectual rights over their sources, particularly those that are little used by the enthusiast. They will have to avoid entering into an unholy alliance with enthusiasts to demand the retention of all records of personal transactions, whether in paper or machine readable format. There is nothing to suggest that, despite the savings in space, there
will be any significant reduction in costs when all records are in machine-readable form. IT equipment is expensive and records need continual maintenance.

Historians will need to recognize that selection and cataloguing criteria can never be completely objective they will always reflect contemporary interests and methodologies. They should also respect the heroic efforts that archivists have made to meet the demands of new approaches to the past by accepting huge accumulations of modern records and by widening their definitions to include information held in a variety of different formats. The historians with a social science perspective need to understand that they benefit from enthusiasts’ use of transactional records and that they must help archivists (most of whom are unfamiliar with statistical technique) to explain that sampling will provide surrogate information. All historians need to remind their audience that contextualizing transactions makes for more interesting research and can contribute to wider debates about the past.

In rebuilding their bridges, both academic historians and archivists should look back at the scholarly foundations of Geschichtswissenschaft. Skills had to be acquired before sources could be approached let alone analysed, in exactly the same way that the scientist had to learn experimental
method in the laboratory.

Today, as all archivists will testify, students and even established scholars approach sources without the necessary methodological skills. There are many reasons for this the growth in the range of sources available requires historians and archivists to acquire more skills from a variety of different disciplines, all of which themselves are becoming more specialized. To understand financial records the historian needs to master not only accounting techniques, but also the legal framework in which they were created. In using any transactional records the historian has to discover the purpose for which they were produced and know at least something about the method of collection, to avoid errors of interpretation. For example, with census records it is essential to know when boundaries were altered and when changes were made in enumerators' instructions. To interpret historical photographs it is necessary to know something about the development of the technology for example, that in the late nineteenth and twentieth centuries it was only possible to take pictures on sunny days in the summer and only sunny or snowy days in the winter. There remain the persistent wider questions of representatives, not just of samples drawn from larger groups, but also of
collections as a whole. Are the papers of one particular family, firm or institution representative of whole populations? The answer, as it has always been, will be subjective, based on intuition. With the growth of quantitative technique such intuition will require more justification in terms of whole populations. This is a problem that bedevils the study of enterprise where large archive collections come from firms with long histories, whose lifespans are demonstrably unrepresentative.

Can a firm with a 100-year history be taken as representative of an industry where the life expectancy of the typical firm was less than ten years? The teaching of historical method has become unfashionable. Archivists find themselves, through no fault of their own, providing such instruction. At the practical level of palaeography and traditional diplomatique they have up to now been well qualified; but at another level, particularly in social science methods, they themselves lack the necessary interpretative and analytical skills. As more students are encouraged to use primary evidence in project work, more attention needs to be given to method, if only to remind historians of the importance of respecting the integrity of the source.

Hand in hand with the teaching of method must go the development of
diplomatique of modern documents, which has been much neglected. Little research has been undertaken into modern record keeping systems, for example the development of large central information clearing houses in the public and private sector from the mid-nineteenth century and the widespread introduction of the file after the First World War. There is almost no literature on the development of modern histories. The high-tech historian accessing the global archive will more than ever need diplomatique skills to understand the legal, political, social and cultural contexts in which data was collected and the meanings of categories and entries.

Difference of meaning of the same words used in different languages or even of different regions within a country will not disappear, nor will differences in practice. Historians, more than ever before, will need to be reminded of the pitfalls of not comparing like with like. With the shift in emphasis away from long review courses towards a student-centred approach there are indications that more emphasis is being placed on method and technique.

All academic historians in the developed world by the beginning of the twenty-first century will, whether they like it or not, be connected to the superhighway and will increasingly be able to access from their desks information about the 'objects' or
the 'objects' themselves that inform their particular discipline. They will be able to capture information held at a distance, even in archives in different countries. They may have to pay for these services, but will their approach to their sources change in any qualitative way? However much material is available on the Internet, it will only represent a narrow fraction of the sources available. Historians will still need to consult manuscripts in archive repositories, and because the finding aids will be more widely available, probably in greater quantities than ever before. In writing history, which will remain a narrative art, the historian will still have to interact with the sources and even where sophisticated analytical tools are employed to make judgements to inform a larger thesis. Statistical analysis can only indicate probability. It does not replace the working historian addressing data which, for all their apparent completeness, are never comprehensive. Undoubtedly the historian and the archivist will need to learn new techniques; but this has always been the case. German philosophers attributed such an important place to Geschichte because they believed it represented a culmination of the sciences. In the future the range of techniques may demand teamwork, but it is difficult to believe that the historical process will be very different. Those who argue otherwise themselves
misunderstand the nature of research in the sciences. What will be different will be not only the speed with which sources can be located but also analysed and the greater range of sources that the historian can bring to bear on an issue. This will not mean that the writing of history will be any less time-consuming or intellectually rigorous; it will in all probability be more difficult to see the thesis for the data. Where there will certainly be difficulties is in defending the conservation of so many records in paper or machine-readable form to support a discipline whose centrality to the human sciences can no longer be taken for granted.