

4 The Sociological Study of History: Institutions and Social Development

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Classical sociology arguably is historical sociology. We may debate the relationship in Western sociology since the Second World War between the empirical-analytical, the hermeneutic and the historical-institutional threads in sociology as a discipline. Yet in the preceding few generations, a concern with the development of human societies – particularly but not exclusively the institutions of ‘industrial’, ‘capitalist’ or ‘Western’ societies, call them what you will – was *central* to the work of those we recognise as the ‘founding fathers’ of sociology. This is obviously true of the Holy Trinity of the sociologists’ pantheon, even if Durkheim expressed the concern in a slightly different way from Marx and Weber (see Bellah, 1959). It is also true of the dozen or so principal apostles who keep them company in the pantheon – such as Comte, de Tocqueville, Spencer, Toennies and Mannheim. These people framed most of the central problems of the discipline, and framed them originally in an historical or developmental way.

In the post-war period, however, the main bulk of sociological research has *not* been of an historical or developmental kind. These decades have been marked by what Wittfogel (1957) called ‘developmental agnosticism’ and Elias (1987b) ‘the retreat of sociologists into the present’. This tendency may in part be the outcome of sociologists’ wish to measure their ‘achievement’ against the utilitarian yardstick of usefulness in rectifying the ills of contemporary society. I think there are deeper reasons, and perhaps this is a question itself in need of sociological investigation. But several intellectual influences are worth mentioning, including those of anthropology, of cross-section data and variable analysis, and of philosophers of science – Sir Karl Popper notable among them.

Anthropology, especially in Britain, has always enjoyed higher prestige than its parvenu cousin, sociology, partly because of its intimate association with colonial administration. The rise within

anthropology after the First World War of the functionalist approach associated especially with Malinowski and Radcliffe-Brown had a strong but markedly delayed impact in sociology. Functionalism was at its peak in sociology during the two decades after the Second World War when Talcott Parsons – who had spent a year at the London School of Economics under Malinowski in the 1920s – dominated American sociology, and American sociology dominated the world. In anthropology, functionalism had begun as a methodological rule-of-thumb in field work: it represented a reaction against the tendency of Victorian social evolutionary anthropologists to resort to ‘conjectural history’ in seeking to explain the customs of preliterate societies, when for the most part any firm evidence about the past of such societies was entirely lacking. Seeking synchronic relationships between patterns which could actually be observed in the field made better sense for anthropologists. Why the same ahistorical approach should have had such appeal to sociologists studying societies blessed with abundant records of their own past development gives more pause for thought.

A prevalent relativism helped compound the ahistorical bias of anthropology. Ethnographic relativism, which also began as a methodological precept essential to understanding the modes of life and thought of unfamiliar cultures, was transformed in the hands of such distinguished anthropologists as Evans-Pritchard (1937) into an epistemological relativism, any questioning of which was liable to bring charges as severe as ‘racism’ down on one’s head. Rightly rejecting the evaluation of societies as ‘superior’ and ‘inferior’, most anthropologists also moved away from the more basic notion of sequential orders of development, even multilinear ones. Admittedly, it was never so unacceptable to despise one’s own ancestors as to despise someone else’s, yet something of this seems to have spilled over into sociology and hastened the retreat from the past. For their part, sociologists were haunted by the ghost of Herbert Spencer and other Victorians who, in attempting to put their own society and its recent transformation in the perspective of the history of humanity as a whole, actually succeeded in putting the whole history of humanity in the perspective of their own society. ‘Who now reads Herbert Spencer?’ was the rhetorical question with which Talcott Parsons (quoting Crane Brinton) opened his immensely influential *The Structure of Social Action* (1937).

The influence of atemporal functionalism might have been less had it not coincided with the rapid development of cross-section survey methods and techniques of variable analysis. As members of the

Frankfurt School often pointed out in the debates arising out of their collaboration with Paul Lazarsfeld, survey methods implicitly carried with them an assumption that social reality could be explained from the properties of constituent atoms – individual people whose opinions apparently welled up spontaneously from sources deep inside each separate one of them – as represented in a snapshot taken at one moment in time (Adorno, 1976; Pollock, 1976).

Functionalism, though not cross-section data and variable analysis, was in retreat in sociology across the world by the late 1960s. The developmental agnosticism which it had helped sustain was then, however, to receive strong reinforcement through the influence (among a minority of sociologists anyway) of French structuralism. Inspired by the shift in linguistics since Saussure from diachronic to synchronic investigations, Claude Lévi-Strauss sought the supposed eternal unchanging properties of the human mind underneath the surface flux and diversity. Something of this can be seen echoed in the radically ahistorical interests of groups like the ethnomethodologists. But, more important, in the hands of Lévi-Strauss and Roland Barthes, all history becomes myth. The prevailing relativism, or ultimately solipsism, is once again evident.¹

Finally, what of Sir Karl Popper? Whether he had a powerful independent influence, or whether his views simply resonated with the currents already described, is an open question. At any rate, his books *The Open Society and Its Enemies* (1945) and *The Poverty of Historicism* (1957) had, in Britain at least, a great impact on sociologists. Whatever may have been Popper's actual intention, my own generation of undergraduates – the generation of the early mid-1960s – somehow picked up the idea that it was neither academically nor politically acceptable to explain the present characteristics of society by any reference to the past.

Historical sociology, of course, never disappeared entirely from the scene. One of the earliest signs of a forthcoming upsurge came indeed from the very power-centre of the functionalist hegemony. Three papers published in the *American Sociological Review* in 1964 by Talcott Parsons, Robert Bellah and S. N. Eisenstadt were the product of a seminar at Harvard on the subject of social evolution. That Parsons and his colleagues should have thought in terms of evolution is not surprising. Evolutionary assumptions were implicit in functionalism all along: how else to explain the selection for survival of only the best-integrated social systems? There were, however, few sociologists on whom the irony was lost. Question: 'Who now reads Herbert

Spencer?' Answer: 'Talcott Parsons does'. And indeed, Parsons's own theorising about social evolution (1966, 1971) represented no advance on Spencer. In fact, he mainly quarried history to fill in his AGIL boxes.

More recently, since about the mid-1970s, there has been a very marked revival in more scholarly historical work within sociology. Indeed, during the Noordwijk conference, Ansgar Weymann remarked that it was now the retreat of sociologists into the past that required explanation, and John Eldridge expressed his puzzlement at many sociologists' fascination with (for example) the behaviour of French peasants on Shrove Tuesday in one small town in the sixteenth century (Ladurie, 1979). That could easily be explained away: historians' ethnographic case-studies of the past can have the same fascination and value as anthropologists' ethnographic case-studies of exotic tribes in present or recent times. But sociologists' renewed interest in the past seems more substantial than that.

It would be agreeable to believe that this was due entirely to a spontaneous recognition of the intellectual necessity of developmental theories in sociology. It may not, however, be wholly unconnected with the fact that in an age when research funds are in short supply, studying historical sources is much cheaper than gathering and processing large quantities of survey data. Nor is it, I think, unrelated to the fine flowering of Marxist or neo-Marxist scholarship in several disciplines, for Marxists – defying Popper's attempted knock-out blow against them – strove to keep alive developmental perspectives when they were most in eclipse in sociology at large.

At any rate, the revival has occurred. But what has it achieved? How can its achievement be assessed?

IS HISTORY BUNK?

The achievements of historical sociology seem to me to be peculiarly vulnerable to underestimation against utilitarian yardsticks, more vulnerable than either history or sociology in general. Few of the politicians who (in an ironic echo of the student generation of 1968) scream for 'relevance' would actually dare to declare that 'history is bunk'. Henry Ford was a philistine – everyone knows that. History is an old-established discipline in the academy, and historians have always enjoyed high prestige, power and popularity. Some of their books are quite widely read for enjoyment and entertainment. Many

politicians read history at university, and a few even continue to practise the craft in their spare time or in retirement. Besides, historians – or most of them – cultivate the mystique of the archival sources, of sticking close to the ‘facts’ and the documents. Historical sociologists – or the boldest of them, as we shall see in a moment – take more risks. They stand back, and build more ambitious models of longer-term processes, in which the leap of interpretation is, if not greater, at least more explicit and acknowledged than in the most conventional kind of historical work. Historical sociologists often refuse to be confined to a single specialised period, and show no shame in relying sometimes on secondary sources. To conventional historians they are not respectable.

On the other hand, historical sociology for the most part relinquishes the sociologist’s standard defence of pretending to be practically useful. Sociology is a late entrant to the academy, and therefore stands low down in the academic pecking order. In general, however, sociologists do not have too much difficulty in convincing some of the people some of the time that their work is directly or not too indirectly useful in improving social conditions, refining policy, or whatever. But this defence rests heavily on the present-centredness of the research in question, and is therefore much less easy to deploy around the activities of historical sociologists. Like so many marginal people, the latter end up having the worst of both worlds.

The picture I have drawn must now be modified slightly, to take account of the fact that some of those who practise historical sociology, and draw upon themselves the scepticism of conventional historians, are themselves historians by disciplinary affiliation.

Any comprehensive survey of major contributions to historical sociology since the Second World War would have to discuss, at least, the works of Reinhard Bendix (1978), S. N. Eisenstadt (1963), Norbert Elias (1978b, 1982, 1983), Barrington Moore (1966), Charles Tilly (1976, 1984) and Immanuel Wallerstein (1974, 1980) from among sociologists. But it would also have to include the works of Perry Anderson (1974a, 1974b), Phillipe Ariès (1962), Fernand Braudel (1976, 1983), E. L. Jones (1981), William McNeill (1977, 1982), E. P. Thompson (1963), Eugen Weber (1976) and Karl Wittfogel (1957) from among historians. It would also have to mention groups of scholars of diverse disciplinary origins, like the Cambridge Group for Historical Demography (notably Peter Laslett, E. A. Wrigley and M. Schofield). The list is no doubt arbitrary, and probably should include many more works published during the last ten years.² There is, in any

case, nowhere near sufficient space to survey them all here. They are, besides, very diverse in content, approach and scope. What they principally have in common, and what helps make them major contributions, is that they are more ambitious in the synthesis they attempt than is common among the works either of most historians or most sociologists. They have an ambitious concern with, in Tilly’s phrase (1984), ‘big structures, large processes, huge comparisons’. Most of them, one way or another, would attract the wrath of guardians of historiographical orthodoxy such as, for instance, Sir Geoffrey Elton. Why should that be so?

THE SOCIOLOGY OF THE PAST VERSUS STRUCTURED PROCESSES OF DEVELOPMENT

To clarify the matter, it is useful to draw a distinction between two types of historical sociology. The first may be called simply the ‘sociology of the past’. In this kind of research, the historical sociologist – whether a sociologist or an historian by affiliation – uses sociological concepts and theories to investigate groups of people living in some specific society at a specific period in the past. This kind of research does not differ fundamentally from research into groups of people living at the present: it is merely that documents form a larger part of the evidence than they would generally do in research into the present, because the usual apparatus of questionnaires and interviews is, shall we say, confronted with practical obstacles. As against this ‘sociology of the past’, other sociologists seek to discern and explain longer-term *structured processes* of development.

The distinction is not hard and fast. For example, Norbert Elias’s *The Court Society* (1983) – among his works the most widely admired by historians – deals with a relatively closely defined place and period (France and its royal court in the century before the Revolution), but his underlying concern is with more general processes of development. Still less is the distinction one of academic worth. Many of the finest examples of historical sociology are instances of the ‘sociology of the past’. To put it perhaps over-simply, their impulse is comparative rather than developmental.

The scale on which comparison is pursued or invited varies enormously. At one end of the scale, two widely admired books may be mentioned: Kai T. Erikson’s *Wayward Puritans* (1966) and Leonore Davidoff’s *The Best Circles* (1973), studies respectively of

deviance in colonial New England and of behaviour in nineteenth-century London high society, are excellent examples of the sociology of the past. Their value, however, seems to me not at all dependent on the fact that they are studies of the *past*: they are in effect contributions to our understanding of deviant behaviour and of endogamous status-groups irrespective of time. The arrow of time may also be reversed for comparative purposes: a present-day study may prove stimulating when studying the past. For instance, Stan Cohen's modern classic, *Folk Devils and Moral Panics* (1972) – a study of the battles of Mods and Rockers and of popular reaction to them – might usefully be read by historians labouring at the large body of research on witch crazes in the past.

Also instances of the sociology of the past, but on a more macrosociological scale, are books like Barrington Moore's *Social Origins of Dictatorship and Democracy* (1966) and Theda Skocpol's *States and Social Revolutions* (1979). They explicitly set out to study comparable episodes at different periods in different societies. Taking a small number of episodes they attempt to generalise to similar situations – past, present, and future. Yet they do not advance any theory of social development over the longer term.

Most macroscopic of all is the case where the scholar seeks to recreate a past society in the round. Braudel's *The Mediterranean and the Mediterranean World in the Age of Philip II* (1976) is the classic instance. Great events are set by Braudel in the largely changing material context of the *longue durée*, like tiny figures of classical antiquity in a Claude Lorraine landscape. The magnificent result is yet distantly reminiscent of functionalist sociology.

The sociology of the past, then, takes many forms. In all the best examples, studies of people in the past serve, in Harold Garfinkel's phrase, as 'aids to a sluggish imagination' when studying present-day people (and they do so, to my mind, far better than any of Garfinkel's 'demonstrations'). Their value is appreciated by non-historical sociologists and also by conventional historians who will admire their professional use of sources; but they do not set out primarily to build developmental models of structured processes of change. That characteristic they share with the work of such social-scientifically – and especially anthropologically – informed historians as Peter Burke (1974, 1978, 1987) or Robert Darnton (1984).

We are left, then, with only a minority of authors whose sociological interest in the past centres on the construction of models of long-term developmental processes.

The minority includes Immanuel Wallerstein, two volumes of whose *The Modern World-System* have so far appeared (1974, 1980). The study may from one angle be seen as a massive attempt at an historical disproof of John Stuart Mill's timeless 'law of comparative advantage', showing how initially small inequalities in ties of interdependence between societies and economies have been magnified over time to produce the massive differences between today's First, Second and Third Worlds. On a similarly ambitious scale the economic historian E. L. Jones, in *The European Miracle* (1981) and especially in *Growth Recurring* (1988), undermines the sense of Europe's uniqueness by demonstrating that it was more the result of chance that it escaped the disasters which afflicted other major civilisations. Related themes on the scale of world history are tackled by the anthropologist Eric Wolf in *Europe and the People Without History* (1982) and by William McNeill in *The Rise of the West* (1963). McNeill's later books, *Plagues and Peoples* (1977), a history of disease and its impact on human society, and *The Pursuit of Power* (1982), on technology and armed force since AD 1000, are no less ambitious.

The minority also includes Norbert Elias. He is still best known for *The Civilising Process*, first published in 1939 but virtually unknown until the 1970s and 1980s. In this extraordinarily complex two-volume work, Elias advances a theory of state-formation based on a study of Western Europe during the last millennium, and links that to the changes in psychological makeup undergone by individuals as they become gradually more subject in the course of generations to the social constraint imposed by the monopolisation of the means of violence by the state apparatus. In his later writings, Elias has continued to extend the theory in scope to mankind as a whole (1984, 1987b).

Finally, the minority includes many Marxist scholars. I will cite only one instance here: Perry Anderson's massive comparative and developmental study, begun in *Passages from Antiquity to Feudalism* (1974a) and *Lineages of the Absolutist State* (1974b), and as yet incomplete.³

The notion of theories of long-term processes of social development remains suspect for different reasons, both to many sociologists and many historians.

Many sociologists remained wedded to the ideal of nomothetic-deductive explanation. If one's theory cannot be expressed in the form of propositions or axioms from which the observed course of events can be logically deduced, it is not 'scientific'. The condition is, of course, rarely attained in historical studies (nor, if truth be known, is it

very common in present-centred sociological studies). Elias, for one, argues (1987a) that the nomothetic-deductive model of explanation is based on an outmoded image of classical physics, that the biological sciences (and now even modern cosmology) do not in practice employ nomothetic-deductive theories but rather 'process-theories' which are more adequate to the character of their subject-matter, and *a fortiori* to the social sciences. Nevertheless, the model of nomothetic-deductive explanation has enormous emotional appeal, and the hunting of the snark continues. Joseph Needham, indeed, suggested it was a cultural trait which could be traced back to the Greeks. 'Although historians of science are never tired of hymning the services of Euclidean deductive geometry to the Western world,' he wrote, it was arguable

that Europe had had more geometry than was good for it. Of course, geometry was an essential basis for modern science, but it did have the bad effect of inducing too ready a belief in abstract timeless axiomatic propositions of all sorts self-evident, and too willing an acceptance of rigid logical and theological formulations . . . China, however, was algebraic and 'Babylonian', not geometrical and 'Greek', . . . [tending] to be practical and approximate rather than theoretical and absolute, and men did not feel obliged to formulate such timeless axiomatic propositions. (Needham, 1965, pp. 46–7n.)

Theories of long-term processes of social development remain suspect to most – not all – professional historians as a prime source of 'mere speculation'. But the table can be turned on the conventional historians. Elias, for instance, asks why the shelf-life of works of historical scholarship is typically so short (1983, p. 4ff). Why is it that history seems to have to be written anew in every generation? Can it be *only* the continuous discovery of new historical facts? Leopold von Ranke was aware of the problem a century and a half ago:

History is always being rewritten . . . Each period takes it over and stamps it with its dominant slant of thought. Praise and blame are apportioned accordingly. All this drags on until the matter itself becomes unrecognisable. Then nothing can help except a return to the original evidence. But would we study it at all without the impulse of the present? . . . Is a completely true history possible? (Ranke, quoted by Elias, 1983, p. 4)

Ranke's own solution was to base history on the most meticulous and scholarly study of the documentary evidence. But while his work marked an enormous advance in historical scholarship, his solution was not enough in itself. For while the methods he pioneered lead to the accumulation of well-established discrete details, they do not in themselves yield any systematic or verifiable framework of reference against which the details can be set. The details may be well-established yet the *connections between them* are often left to arbitrary interpretation and speculation:

The constraints of the tradition of historiography that allows the individual historian very great latitude for personal hermeneutics in the establishment of narrative connections between carefully researched sources finds expression, among other ways, in a conscious renunciation of theory . . . Yet the proud renunciation of theory in this form of history writing leaves the door wide open for the formation of historical myths of all kinds. (Elias, 1977, p. 137)

It is all too easy for narrative historians – and the case for narrative as the central activity of historians is still made (Stone, 1983; cf. Hobsbawm, 1980) – to represent history as a jumble of 'unique' events, an unstructured sequence of the actions of particular people. The importance of writers such as we have mentioned is that they help to show that long-term, unplanned but explainable processes form the infrastructure of any such apparently random and structureless juxtaposition of people and events. The claim that historians study unique, unrepeatable sequences of events implies that 'uniqueness' is an inherent property of the objects or events studied. It is not: it is a property conferred on them by the low level of abstraction of the frame of reference with which such historians operate.

The claim of 'uniqueness' is linked, too, to the writing of history in a highly voluntaristic manner – that is, in terms of the motives and actions of apparently 'free' actors. The introduction of concepts at a higher level of abstraction, and especially longer-term process theories, is often seen by historians as bringing with it a quite opposite image of 'determinism'. But such absolutes as 'free' and 'determined' are metaphysical notions: in the real world, all human beings have always been interdependent with other human beings in various degrees and patterns. Voluntaristic studies of people's motives are valuable and essential, but process-theories – used consciously or unconsciously by most of those interested in long-term processes – aim to show how historical processes unfold through sequences of inter-

locking levers of accident and design, the unintended consequences of intentional actions forming the unintended conditions of subsequent intentional actions.

This links with a persisting suspicion, both among academics and among (non-Marxist) politicians, that any theory of long-term developmental processes is necessarily bound up with a belief in inevitable 'progress' and with predictions of an 'inevitable' future society – the legacy of Popper as much as anyone. The work of Wallerstein and of McNeill (*The Pursuit of Power* more than *Plagues and Peoples*) ought to be enough to show that developmental theories certainly do not always depict history in diachronic Panglossian terms as an ineluctable movement towards greater welfare. Elias too is always careful to stress that greater civilisation, in his technical sense, does not necessarily mean greater happiness: there are progressions, in his view, but no progress.

As for the 'inevitability' of social development and predictions of the future, Philip Abrams (1982, pp. 145–6) rightly singles out for praise Elias's discussion of the problem (Elias, 1978a, pp. 158–74). It rests on the basic distinction between necessary and sufficient conditions of a social change, or, as Abrams puts it, 'between that which is inevitable in the sense that all conditions necessary for its existence have been met and that which is inevitable in the sense that all other possibilities have been ruled out'. The historian and sociologist may frequently be able to demonstrate the former, but rarely if ever can possess the knowledge necessary to prove the latter. In other words, historical sociology may well be able to establish a sequential order, to demonstrate that an earlier pattern of social organisation (feudalism, for example) was a necessary precursor of the emergence of a later pattern (capitalism, shall we say), if the former in every case contained all the conditions necessary for the latter to come into existence. But that is not at all the same thing as being able to prophesy that the earlier pattern must inevitably give rise to the later; from the viewpoint of an earlier pattern, several alternative outcomes are usually possible. Popper sought to discredit 'historical prophecy', but any reasonably ambitious historical retrodiction seems to have been unnecessarily damaged by his argument too.⁴

CONCLUSION: WHAT IS ACHIEVED?

So far, I have perhaps sidestepped the question of what is achieved by the sociological study of the past. What use is it? It is possible to cite

instances where the sociological investigation of the past has proved useful in the present in the most utilitarian ways. One example is the work of Eric Dunning and his colleagues on football hooliganism (1988). The very achievement of demonstrating that hooligan violence was not something which appeared abruptly in the 1960s, but had long historical roots and had been socially structured and channelled over time, had direct implications for the kinds of policies which would and would not succeed in controlling it. I myself have also found that my own purely historical sociological study of the development of culinary taste and appetite in England and France (Mennell, 1985) proved useful in designing current research to assist doctors in giving dietary advice to their patients.

It would, however, be hazardous to seek to justify historical sociology on utilitarian grounds. Any utilitarian, practical value is almost always incidental. But it does often serve to improve the human means of orientation. Perhaps the most practically useful result of the investigation of long-term developmental processes is to lend scale and perspective to the present. In this, its significance may spread beyond narrowly academic interests into influencing how people at large and even policy-makers interpret events they see before them at the present day. Only two examples will have to suffice by way of conclusion. The work of Wallerstein and of Jones could, for instance, eventually seep through into popular and political perceptions of the countries of the Third World. As recently as the 1960s, sociological interpretations were dominated by the 'culture of backwardness' approach associated with authors like Hoselitz (1960), and Almond and Coleman (1960). Using conceptual schemes derived from Talcott Parsons and previously bowdlerized from Max Weber, they seriously discussed the 'value-orientations' of 'societies' in isolation, usually diagnosing the need for an intravenous transfusion of the Protestant Ethic (Eisenstadt, 1968). No one could now take that seriously who has read Wallerstein, Jones, and related writers' accounts of the long-term structured processes in the course of which economic, political and cultural gulfs gradually widened in the context of increasingly unequal power-balances. Nevertheless, it is probably true that people at large in the rich industrial countries, and probably most politicians also, still see the situation in terms closer to the earlier, static, sociological view.

A second instance is the widespread belief that contemporary society is more insecure and more violent perhaps than ever before. To have read Wittfogel, McNeill and Elias – among others – adds

perspective and makes one sceptical of any such claim.⁵ Yet again, the popular view is extraordinarily hard to dislodge, if only because people who live in inner cities and feel insecure find it hard to believe that things could ever have been worse than they are for them. Sociologists, however, should never allow themselves to be too sanguine about their chances of reforming the perceptions of people in society at large. Even our own social scientific colleagues sometimes find it hard to accept research findings: for instance, Sir Edmund Leach (1986) poured scorn on Eric Dunning's painstaking documentary investigation of football hooliganism in the past – it was easy, he claimed, when you already knew what you would find – and preferred to rely on his own personal recollection of rowdiness after the Boat Race in the 1930s. Historical sociologists *can* hope to influence the way people see the world, shaking them out of old perceptions and into new. But it is a slow business. It, too, in its own right, may be a long-term, structured process of social development.

Notes

1. Scepticism about the reliability of historical sources is of course perfectly healthy. During the Noordwijk conference, Robin Williams expressed just such a scepticism about one recent fashion in history and sociology: oral history. He gave us an example – Studs Terkel's book *The Good War* (1984), compiled from the recollections of American veterans. He pointed out that Americans did not speak of the Second World War as 'the good war' until after the Vietnam War, when hindsight revised their perceptions retrospectively.
2. A conspicuous absentee is the philosopher Michel Foucault, whose works have sometimes been read as historical sociology. They certainly contain many nuggets of striking fact and insight. As a *theorist*, however, he seems to me to be confused and incoherent; it is notable how fast Foucault shares fell on the intellectual stock market after his death.
3. Harvey J. Kaye, *The British Marxist Historians* (1984), provides an extremely useful discussion of the work of Maurice Dobb, Rodney Hilton, Christopher Hilton, Eric Hobsbawm and E. P. Thompson.
4. For a critique of Popper and defence of developmental sociology, see Dunning (1977).
5. For quantitative evidence on long-term trends in interpersonal violence, see Gurr (1981) and Stone (1983).

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