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From Social History to the History of Society

THIS ESSAY is an attempt to observe and analyze, not to state a personal credo or to express (except where this is clearly stated) the author's preferences and value judgments. I say this at the outset in order to distinguish this essay from others which are defenses of or pleas for the kind of history practiced by their authors—as it happens social history does not need either at the moment—but also to avoid two misunderstandings especially common in discussions heavily charged with ideology. All discussions about social history are.

The first is the tendency for readers to identify authors with the views they write about, unless they disclaim this identification in the clearest terms and sometimes even when they do so. The second is the tendency to confuse the ideological or political motivations of research, or its utilization, with its scientific value. Where ideological intention or bias produces triviality or error, as is often the case in the human sciences, we may happily condemn motivation, method, and result. However, life would be a great deal simpler if our understanding of history were advanced exclusively by those with whom we are in agreement or in sympathy on all public and even private matters. Social history is at present in fashion. None of those who practice it would care to be seen keeping ideological company with all those who come under the same historical heading. Nevertheless, what is more important than to define one's attitude is to discover where social history stands today after two decades of unsystematic if copious development, and whither it might go.

I

The term social history has always been difficult to define, and until recently there has been no great pressure to define it, for it has lacked the institutional and professional vested interests which

normally insist on precise demarcations. Broadly speaking, until the present vogue of the subject—or at least of the name—it was in the past used in three sometimes overlapping senses. First, it referred to the history of the poor or lower classes, and more specifically to the history of the movements of the poor ("social movements"). The term could be even more specialized, referring essentially to the history of labor and socialist ideas and organizations. For obvious reasons this link between social history and the history of social protest or socialist movements has remained strong. A number of social historians have been attracted to the subject because they were radicals or socialists and as such interested in subjects of great sentimental relevance to them.[1]

Second, the term was used to refer to works on a variety of human activities difficult to classify except in such terms as "manners, customs, everyday life." This was, perhaps for linguistic reasons, a largely Anglo-Saxon usage, since the English language lacks suitable terms for what the Germans who wrote about similar subjects—often also in a rather superficial and journalistic manner—called *Kultur-* or *Sittengeschichte*. This kind of social history was not particularly oriented toward the lower classes—indeed rather the opposite—though the more politically radical practitioners tended to pay attention to them. It formed the unspoken basis of what may be called the residual view of social history, which was put forward by the late G. M. Trevelyan in his *English Social History* (London, 1944) as "history with the politics left out." It requires no comment.

The third meaning of the term was certainly the most common and for our purposes the most relevant: "social" was used in combination with "economic history." Indeed, outside the Anglo-Saxon world, the title of the typical specialist journal in this field before the Second World War always (I think) bracketed the two words, as in the *Vierteljahrschrift fuer Sozial u. Wirtschaftsgeschichte,* the *Revue d'Histoire E. & S.,* or the *Annales d'Histoire E. & S.* It must be admitted that the economic half of this combination was overwhelmingly preponderant. There were hardly any social histories of equivalent caliber to set beside the numerous volumes devoted to the economic history of various countries, periods, and subjects. There were in fact not very many economic and social histories. Before 1939 one can think of only a few such works, admittedly sometimes by impressive authors (Pirenne, Mikhail Rostovtzeff, J. W. Thompson, perhaps Dopsch), and the mono-

graphic or periodical literature was even sparser. Nevertheless, the habitual bracketing of economic and social, whether in the definitions of the general field of historical specialization or under the more specialized banner of economic history, is significant.

It revealed the desire for an approach to history systematically different from the classical Rankean one. What interested historians of this kind was the evolution of the economy, and this in turn interested them because of the light it threw on the structure and changes in society, and more especially on the relationship between classes and social groups, as George Unwin admitted.[2] This social dimension is evident even in the work of the most narrowly or cautiously economic historians so long as they claimed to be historians. Even J. H. Clapham argued that economic history was of all varieties of history the most fundamental because it was the foundation of society. The predominance of the economic over the social in this combination had, we may suggest, two reasons. It was partly owing to a view of economic theory which refused to isolate the economic from social, institutional, and other elements, as with the Marxists and the German historical school, and partly to the sheer headstart of economics over the other social sciences. If history had to be integrated into the social sciences, economics was the one it had primarily to come to terms with. One might go further and argue (with Marx) that, whatever the essential inseparability of the economic and the social in human society, the analytical base of any historical inquiry into the evolution of human societies must be the process of social production.

None of the three versions of social history produced a specialized academic field of social history until the 1950's, though at one time the famous *Annales* of Lucien Febvre and Marc Bloch dropped the economic half of its subtitle and proclaimed itself purely social. However, this was a temporary diversion of the war years, and the title by which this great journal has now been known for a quarter of a century—*Annales: économies, sociétés, civilisations*—as well as the nature of its contents, reflect the original and essentially global and comprehensive aims of its founders. Neither the subject itself, nor the discussion of its problems, developed seriously before 1950. The journals specializing in it, still few in number, were not founded until the end of the 1950's: we may perhaps regard the *Comparative Studies in Society and History* (1958) as the first. As an academic specialization, social history is therefore quite new.

What explains the rapid development and growing emancipation of social history in the past twenty years? The question could be answered in terms of technical and institutional changes within the academic disciplines of social science: the deliberate specialization of economic history to fit in with the requirements of the rapidly developing economic theory and analysis, of which the "new economic history" is an example; the remarkable and worldwide growth of sociology as an academic subject and fashion, which in turn called for subsidiary historical service-branches analogous to those required by economics departments. We cannot neglect such factors. Many historians (such as the Marxists) who had previously labeled themselves economic because the problems they were interested in were plainly not encouraged or even considered by orthodox general history, found themselves extruded from a rapidly narrowing economic history and accepted or welcomed the title of "social historians," especially if their mathematics were poor. It is improbable whether in the atmosphere of the 1950's and early 1960's someone like R. H. Tawney would have been welcomed among the economic historians had he been a young researcher and not president of the Economic History Society. However, such academic redefinitions and professional shifts hardly explain much, though they cannot be overlooked.

Far more significant was the general historization of the social sciences which took place during this period, and may retrospectively appear to have been the most important development within them at this time. For my present purpose it is not necessary to explain this change, but it is impossible to avoid drawing attention to the immense significance of the revolutions and struggles for political and economic emancipation of colonial and semicolonial countries, which drew the attention of governments, international and research organizations, and consequently also of social scientists, to what are essentially problems of historic transformations. These were subjects which had hitherto been outside, or at best on the margins of, academic orthodoxy in the social sciences, and had increasingly been neglected by historians.

At all events essentially historical questions and concepts (sometimes, as in the case of "modernization" or "economic growth," excessively crude concepts) have captured even the discipline hitherto most immune to history, when not actually, like Radcliffe-Brown's social anthropology, actively hostile to it. This progressive infiltration of history is perhaps most evident in economics,

where an initial field of growth economics, whose assumptions, though much more sophisticated, were those of the cookery book ("Take the following quantities of ingredients *a* through *n*, mix and cook, and the result will be the take-off into self-sustained growth"), has been succeeded by the growing realization that factors outside economics also determine economic development. In brief, it is now impossible to pursue many activities of the social scientist in any but a trivial manner without coming to terms with social structure and its transformations: without the history of societies. It is a curious paradox that the economists were beginning to grope for some understanding of social (or at any rate not strictly economic) factors at the very moment when the economic historians, absorbing the economists' models of fifteen years earlier, were trying to make themselves look hard rather than soft by forgetting about everything except equations and statistics.

What can we conclude from this brief glance at the historical development of social history? It can hardly be an adequate guide to the nature and tasks of the subject under consideration, though it can explain why certain more or less heterogeneous subjects of research came to be loosely grouped under this general title, and how developments in other social sciences prepared the ground for the establishment of an academic theory specially demarcated as such. At most it can provide us with some hints, at least one of which is worth mentioning immediately.

A survey of social history in the past seems to show that its best practitioners have always felt uncomfortable with the term itself. They have either, like the great Frenchmen to whom we owe so much, preferred to describe themselves simply as historians and their aim as "total" or "global" history, or as men who sought to integrate the contributions of all relevant social sciences in history, rather than to exemplify any one of them. Marc Bloch, Fernand Braudel, Georges Lefebvre are not names which can be pigeonholed as social historians except insofar as they accepted Fustel de Coulanges' statement that "History is not the accumulation of events of all kinds which occurred in the past. It is the science of human societies."

Social history can never be another specialization like economic or other hyphenated histories because its subject matter cannot be isolated. We can define certain human activities as economic, at least for analytical purposes, and then study them historically. Though this may be (except for certain definable purposes) artifi-

cial or unrealistic, it is not impracticable. In much the same way, though at a lower level of theory, the old kind of intellectual history which isolated written ideas from their human context and traced their filiation from one writer to another is possible, if one wants to do that sort of thing. But the social or societal aspects of man's being cannot be separated from the other aspects of his being, except at the cost of tautology or extreme trivialization. They cannot, for more than a moment, be separated from the ways in which men get their living and their material environment. They cannot, even for a moment, be separated from their ideas, since their relations with one another are expressed and formulated in language which implies concepts as soon as they open their mouths. And so on. The intellectual historian may (at his risk) pay no attention to economics, the economic historian to Shakespeare, but the social historian who neglects either will not get far. Conversely, while it is extremely improbable that a monograph on provençal poetry will be economic history or one on inflation in the sixteenth century intellectual history, both could be treated in a way to make them social history.

II

Let us turn from the past to the present and consider the problems of writing the history of society. The first question concerns how much societal historians can get from other social sciences, or indeed how far their subject is or ought to be merely the science of society insofar as it deals with the past. This question is natural, though the experience of the past two decades suggests two different answers to it. It is clear that social history has since 1950 been powerfully shaped and stimulated, not only by the professional structure of other social sciences (for example, their specific course requirements for university students), and by their methods and techniques, but also by their questions. It is hardly too much to say that the recent efflorescence of studies in the British industrial revolution, a subject once grossly neglected by its own experts because they doubted the validity of the concept of industrial revolution, is due primarily to the urge of economists (doubtless in turn echoing that of governments and planners) to discover how industrial revolutions happen, what makes them happen, and what sociopolitical consequences they have. With certain notable exceptions, the flow of stimulation in the past twenty years has been one

way. On the other hand, if we look at recent developments in another way, we shall be struck by the obvious convergence of workers from different disciplines toward sociohistorical problems. The study of millennial phenomena is a case in point, since among writers on these subjects we find men coming from anthropology, sociology, political science, history, not to mention students of literature and religions—though not, so far as I am aware, economists. We also note the transfer of men with other professional formations, at least temporarily, to work which historians would consider historical—as with Charles Tilly and Neil Smelser from sociology, Eric Wolf from anthropology, Everett Hagen and Sir John Hicks from economics.

Yet the second tendency is perhaps best regarded not as convergence but as conversion. For it must never be forgotten that if nonhistorical social scientists have begun to ask properly historical questions and to ask historians for answers, it is because they themselves have none. And if they have sometimes turned themselves into historians, it is because the practicing members of our discipline, with the notable exception of the Marxists and others—not necessarily *Marxisants*—who accept a similar problematic, have not provided the answers.[5] Moreover, though there are now a few social scientists from other disciplines who have made themselves sufficiently expert in our field to command respect, there are more who have merely applied a few crude mechanical concepts and models. For every *Vendée* by a Tilly, there are, alas, several dozen equivalents of Rostow's *Stages*. I leave aside the numerous others who have ventured into the difficult territory of historical source material without an adequate knowledge of the hazards they are likely to encounter there, or of the means of avoiding and overcoming them. In brief, the situation at present is one in which historians, with all their willingness to learn from other disciplines, are required to teach rather than to learn. The history of society cannot be written by applying the meager available models from other sciences; it requires the construction of adequate new ones—or, at least (Marxists would argue), the development of existing sketches into models.

This is not, of course, true of techniques and methods, where the historians are already net debtors to a substantial extent, and will, or at least ought, to go even more heavily and systematically into debt. I do not wish to discuss this aspect of the problem of the history of society, but a point or two can be made in passing. Given

the nature of our sources, we can hardly advance much beyond a combination of the suggestive hypothesis and the apt anecdotal illustration without the techniques for the discovery, the statistical grouping, and handling of large quantities of data, where necessary with the aid of division of research labor and technological devices, which other social sciences have long developed. At the opposite extreme, we stand in equal need of the techniques for the observation and analysis in depth of specific individuals, small groups, and situations, which have also been pioneered outside history, and which may be adaptable to our purposes—for example, the participant observation of the social anthropologists, the interview-in-depth, perhaps even psychoanalytical methods. At the very least these various techniques can stimulate the search for adaptations and equivalents in our field, which may help to answer otherwise impenetrable questions.[6]

I am much more doubtful about the prospect of turning social history into a backward projection of sociology, as of turning economic history into retrospective economic theory, because these disciplines do not at present provide us with useful models or analytical frameworks for the study of long-run *historical* socio-economic transformations. Indeed the bulk of their thinking has not been concerned with, or even interested in, such changes, if we except such trends as Marxism. Moreover, it may be argued that in important respects their analytical models have been developed systematically, and most profitably, by abstracting from historical change. This is notably true, I would suggest, of sociology and social anthropology.

The founding fathers of sociology have indeed been more historically minded than the main school of neoclassic economics (though not necessarily than the original school of classical political economists), but theirs is an altogether less developed science. Stanley Hoffmann has rightly pointed to the difference between the "models" of the economists and the "checklists" of the sociologists and anthropologists.[7] Perhaps they are more than mere checklists. These sciences have also provided us with certain visions, patterns of possible structures composed of elements which can be permuted and combined in various ways, vague analogues to Kekulé's ring glimpsed at the top of the bus, but with the drawback of unverifiability. At their best such structural-functional patterns may be both elegant and heuristically useful, at least for some. At a more modest

level, they may provide us with useful metaphors, concepts, or terms (such as "role"), or convenient aids in ordering our material.

Moreover, quite apart from their deficiency as models, it may be argued that the theoretical constructions of sociology (or social anthropology) have been most successful by excluding history, that is, directional or oriented change.[8] Broadly speaking, the structural-functional patterns illuminate what societies have in common in spite of their differences, whereas our problem is with what they have not. It is not what light Lévi-Strauss's Amazonian tribes can throw on modern (indeed on any) society, but on how humanity got from the cavemen to modern industrialism or postindustrialism, and what changes in society were associated with this progress, or necessary for it to take place, or consequential upon it. Or, to use another illustration, it is not to observe the permanent necessity of all human societies to supply themselves with food by growing or otherwise acquiring it, but what happens when this function, having been overwhelmingly fulfilled (since the neolithic revolution) by classes of peasants forming the majority of their societies, comes to be fulfilled by small groups of other kinds of agricultural producers and may come to be fulfilled in nonagricultural ways. How does this happen and why? I do not believe that sociology and social anthropology, however helpful they are incidentally, at present provide us with much guidance.

On the other hand, while I remain skeptical of most current economic theory as a framework of the historical analysis of societies (and therefore of the claims of the new economic history), I am inclined to think that the possible value of economics for the historian of society is great. It cannot but deal with what is an essentially dynamic element in history, namely the process—and, speaking globally and on a long time-scale, progress—of social production. Insofar as it does this it has, as Marx saw, historical development built into it. To take a simple illustration: the concept of the "economic surplus," which the late Paul Baran revived and utilized to such good effect,[9] is patently fundamental to any historian of the development of societies, and strikes me as not only more objective and quantifiable, but also more primary, speaking in terms of analysis, than, say, the dichotomy *Gemeinschaft-Gesellschaft*. Of course Marx knew that economic models, if they are to be valuable for historical analysis, cannot be divorced from social and institutional realities, which include certain basic types of human communal or kinship organization, not to mention the structures and

assumptions specific to particular socioeconomic formations or cultures. And yet, though Marx is not for nothing regarded as one of the major founding fathers of modern sociological thought (directly and through his followers and critics), the fact remains that his major intellectual project *Das Kapital* took the form of a work of economic analysis. We are required neither to agree with his conclusions nor his methodology. But we would be unwise to neglect the practice of the thinker who, more than any other, has defined or suggested the set of historical questions to which social scientists find themselves drawn today.

III

How are we to write the history of society? It is not possible for me to produce a definition or model of what we mean by society here, or even a checklist of what we want to know about its history. Even if I could, I do not know how profitable this would be. However, it may be useful to put up a small and miscellaneous assortment of signposts to direct or warn off future traffic.

(1) The history of society is *history;* that is to say it has real chronological time as one of its dimensions. We are concerned not only with structures and their mechanisms of persistence and change, and with the general possibilities and patterns of their transformations, but also with what actually happened. If we are not, then (as Fernand Braudel has reminded us in his article on "Histoire et Longue Durée"[10]) we are not historians. *Conjectural* history has a place in our discipline, even though its chief value is to help us assess the possibilities of present and future, rather than past, where its place is taken by *comparative* history; but actual history is what we must explain. The possible development or nondevelopment of capitalism in imperial China is relevant to us only insofar as it helps to explain the actual fact that this type of economy developed fully, at least to begin with, in one and only one region of the world. This in turn may be usefully contrasted (again in the light of general models) with the tendency for other systems of social relations—for example, the broadly feudal—to develop much more frequently and in a greater number of areas. The history of society is thus a collaboration between general models of social structure and change and the specific set of phenomena which actually occurred. This is true whatever the geographical or chronological scale of our inquiries.

(2) The history of society is, among other things, that of specific units of people living together and definable in sociological terms. It is the history of societies as well as of human society (as distinct from, say, that of apes and ants), or of certain types of society and their possible relationships (as in such terms as "bourgeois" or "pastoral" society), or of the general development of humanity considered as a whole. The definition of a society in this sense raises difficult questions, even if we assume that we are defining an objective reality, as seems likely, unless we reject as illegitimate such statements as "Japanese society in 1930 differed from English society." For even if we eliminate the confusions between different uses of the word "society," we face problems (a) because the size, complexity, and scope of these units varies, for example, at different historical periods or stages of development; and (b) because what we call society is merely one set of human interrelations among several of varying scale and comprehensiveness into which people are classifiable or classify themselves, often simultaneously and with overlaps. In extreme cases such as New Guinea or Amazon tribes, these various sets may define the same group of people, though this is in fact rather improbable. But normally this group is congruent neither with such relevant sociological units as the community, nor with certain wider systems of relationship of which the society forms a part, and which may be functionally essential to it (like the set of economic relations) or nonessential (like those of culture).

Christendom or Islam exist and are recognized as self-classifications, but though they may define a *class* of societies sharing certain common characteristics, they are not societies in the sense in which we use the word when talking about the Greeks or modern Sweden. On the other hand, while in many ways Detroit and Cuzco are today part of a single system of functional interrelationships (for example, part of one economic system), few would regard them as part of the same society, sociologically speaking. Neither would we regard as one the societies of the Romans or the Han and those of the barbarians who formed, quite evidently, part of a wider system of interrelationships with them. How do we define these units? It is far from easy to say, though most of us solve—or evade—the problem by choosing some outside criterion: territorial, ethnic, political, or the like. But this is not always satisfactory. The problem is more than methodological. One of the major themes of the history of modern societies is the increase in their scale, internal homo-

geneity, or at least in the centralization and directness of social relationships, the change from an essentially pluralist to an essentially unitary structure. In tracing this, problems of definition become very troublesome, as every student of the development of national societies or at least of nationalisms knows.

(3) The history of societies requires us to apply, if not a formalized and elaborate model of such structures, then at least an approximate order of research priorities and a working assumption about what constitutes the central nexus or complex of connections of our subject, though of course these things imply a model. Every social historian does in fact make such assumptions and hold such priorities. Thus I doubt whether any historian of eighteenth-century Brazil would give the Catholicism of that society analytical priority over its slavery, or any historian of nineteenth-century Britain would regard kinship as central a social nexus as he would in Anglo-Saxon England.

A tacit consensus among historians seems to have established a fairly common working model of this kind, with variants. One starts with the material and historical environment, goes on to the forces and techniques of production (demography coming somewhere in between), the structure of the consequent economy—divisions of labor, exchange, accumulation, distribution of the surplus, and so forth—and the social relations arising from these. These might be followed by the institutions and the image of society and its functioning which underlie them. The shape of the social structure is thus established, the specific characteristics and details of which, insofar as they derive from other sources, can then be determined, most likely by comparative study. The practice is thus to work outwards and upwards from the process of social production in its specific setting. Historians will be tempted—in my view rightly—to pick on one particular relation or relational complex as central and specific to the society (or type of society) in question, and to group the rest of the treatment around it—for example, Bloch's "relations of interdependence" in his *Feudal Society,* or those arising out of industrial production, possibly in industrial society, certainly in its capitalist form. Once the structure has been established, it must be seen in its historical movement. In the French phrase "structure" must be seen in "conjuncture," though this term must not be taken to exclude other, and possibly more relevant, forms and patterns of historical change. Once again the tendency is to treat economic movements (in the broadest

sense) as the backbone of such an analysis. The tensions to which the society is exposed in the process of historic change and transformation then allow the historian to expose (1) the general mechanism by which the structures of society simultaneously tend to lose and reestablish their equilibria, and (2) the phenomena which are traditionally the subject of interest to the social historians, for example, collective consciousness, social movements, the social dimension of intellectual and cultural changes, and so on.

My object in summarizing what I believe—perhaps wrongly—to be a widely accepted working plan of social historians is not to recommend it, even though I am personally in its favor. It is rather the opposite: to suggest that we try and make the implicit assumptions on which we work explicit and to ask ourselves whether this plan is in fact the best for the formulation of the nature and structure of societies and the mechanisms of their historic transformations (or stabilizations), whether other plans of work based on other questions can be made compatible with it, or are to be preferred to it, or can simply be superimposed to produce the historical equivalent of those Picasso portraits which are simultaneously displayed full-face and in profile.

In brief, if as historians of society we are to help in producing—for the benefit of all the social sciences—valid models of sociohistoric dynamics, we shall have to establish a greater unity of our practice and our theory, which at the present stage of the game probably means in the first instance to watch what we are doing, to generalize it, and to correct it in the light of the problems arising out of further practice.

IV

Consequently, I should like to conclude by surveying the actual practice of social history in the past decade or two, in order to see what future approaches and problems it suggests. This procedure has the advantage that it fits in both with the professional inclinations of a historian and with what little we know about the actual progress of sciences. What topics and problems have attracted most attention in recent years? What are the growing-points? What are the interesting people doing? The answers to such questions do not exhaust analysis, but without them we cannot get very far. The consensus of workers may be mistaken, or distorted by fashion or—as is obviously the case in such a field as the study of public disorder

—by the impact of politics and administrative requirements, but we neglect it at our peril. The progress of science has derived less from the attempt to define perspectives and programs a priori—if it did we should now be curing cancer—than from an obscure and often simultaneous convergence upon the questions worth asking and, above all, those ripe for an answer. Let us see what has been happening, at least insofar as it is reflected in the impressionistic view of one observer.

Let me suggest that the bulk of interesting work in social history in the past ten or fifteen years has clustered around the following topics or complexes of questions:

(1) Demography and kinship
(2) Urban studies insofar as these fall within our field
(3) Classes and social groups
(4) The history of "mentalities" or collective consciousness or of "culture" in the anthropologists' sense
(5) The transformation of societies (for example, modernization or industrialization)
(6) Social movements and phenomena of social protest.

The first two groups can be singled out because they have already institutionalized themselves as fields, regardless of the importance of their subject matter, and now possess their own organization, methodology, and system of publications. Historical demography is a rapidly growing and fruitful field, which rests not so much on a set of problems as on a technical innovation in research (family reconstitution) that makes it possible to derive interesting results from material hitherto regarded as recalcitrant or exhausted (parish registers). It has thus opened a new range of sources, whose characteristics in turn have led to the formulation of questions. The major interest for social historians of historical demography lies in the light it sheds on certain aspects of family structure and behavior, on the life-curves of people at different periods, and on intergenerational changes. These are important though limited by the nature of the sources—more limited than the most enthusiastic champions of the subject allow, and certainly by themselves insufficient to provide the framework of analysis of "The World We Have Lost." Nevertheless, the fundamental importance of this field is not in question, and it has served to encourage the use of strict quantitative techniques. One welcome effect—or side effect—has been to arouse a greater interest in historical problems of

kinship structure than social historians might have shown without this stimulus, though a modest demonstration effect from social anthropology ought not to be neglected. The nature and prospects of this field have been sufficiently debated to make further discussion unnecessary here.

Urban history also possesses a certain technologically determined unity. The individual city is normally a geographically limited and coherent unit, often with its specific documentation and even more often of a size which lends itself to research on the Ph.D. scale. It also reflects the urgency of urban problems which have increasingly become the major, or at least the most dramatic, problems of social planning and management in modern industrial societies. Both these influences tend to make urban history a large container with ill-defined, heterogeneous, and sometimes indiscriminate contents. It includes anything about cities. But it is clear that it raises problems peculiarly germane to social history, at least in the sense that the city can never be an analytical framework for economic macrohistory (because economically it must be part of a larger system), and politically it is only rarely found as a self-contained city state. It is essentially a body of human beings living together in a particular way, and the characteristic process of urbanization in modern societies makes it, at least up to the present, the form in which most of them live together. The technical, social, and political problems of the city arise essentially out of the interactions of masses of human beings living in close proximity to one another; and even the ideas about the city (insofar as it is not a mere stage-set for the display of some ruler's power and glory) are those in which men—from the Book of Revelation on—have tried to express their aspirations about human communities. Moreover, in recent centuries it has raised and dramatized the problems of rapid social change more than any other institution. That the social historians who have flocked into urban studies are aware of this need hardly be said.[11] One may say that they have been groping toward a view of urban history as a paradigm of social change. I doubt whether it can be this, at least for the period up to the present. I also doubt whether many really impressive global studies of the larger cities of the industrial era have so far been produced, considering the vast quantity of work in this field. However, urban history must remain a central concern of historians of society, if only because it brings out—or can bring out—those specific aspects

of societal change and structure with which sociologists and social psychologists are peculiarly concerned.

The other clusters of concentration have not so far been institutionalized, though one or two may be approaching this stage of development. The history of classes and social groups has plainly developed out of the common assumption that no understanding of society is possible without an understanding of the major components of all societies no longer based primarily on kinship. In no field has the advance been more dramatic and—given the neglect of historians in the past—more necessary. The briefest list of the most significant works in social history must include Lawrence Stone on the Elizabethan aristocracy, E. Le Roy Ladurie on the Languedoc peasants, Edward Thompson on the making of the English working class, Adeline Daumard on the Parisian bourgeoisie; but these are merely peaks in what is already a sizeable mountain range. Compared to these the study of more restricted social groups—professions, for instance—has been less significant.

The novelty of the enterprise has been its ambition. Classes, or specific relations of production such as slavery, are today being systematically considered on the scale of a society, or in intersocietal comparison, or as general types of social relationship. They are also now considered in depth, that is, in all aspects of their social existence, relations, and behavior. This is new, and the achievements are already striking, though the work has barely begun—if we except fields of specially intense activity, such as the comparative study of slavery. Nevertheless, a number of difficulties can be discerned, and a few words about them may not be out of place.

(1) The mass and variety of material for these studies is such that the preindustrial artisan technique of older historians is plainly inadequate. They require cooperative teamwork and the utilization of modern technical equipment. I would guess that the massive works of individual scholarship will mark the early phases of this kind of research, but will give way on the one hand to systematic cooperative projects (such as the projected study of the Stockholm working class in the nineteenth century)[12] and on the other hand to periodic (and probably still single-handed) attempts at synthesis. This is evident in the field of work with which I am most familiar, the history of the working class. Even the most ambitious single work—E. P. Thompson's—is no more than a great torso, though it deals with a rather short period. (Jürgen Kuczynski's

titanic *Geschichte der Lage der Arbeiter unter dem Kapitalismus,* as its title implies, concentrates only on certain aspects of the working class.)

(2) The field raises daunting technical difficulties, even where conceptual clarity exists, especially as regards the measurement of change over time—for example, the flow into and out of any specified social group, or the changes in peasant landholdings. We may be lucky enough to have sources from which such changes can be derived (for example, the recorded genealogies of the aristocracy and gentry as a group) or from which the material for our analysis may be constructed (for example, by the methods of historical demography, or the data on which the valuable studies of the Chinese bureaucracy have been based). But what are we to do, say, about Indian castes, which we also know to have contained such movements, presumably intergenerational, but about which it is so far impossible to make even rough quantitative statements?

(3) More serious are the conceptual problems, which have not always been clearly confronted by historians—a fact which does not preclude good work (horses can be recognized and ridden by those who can't define them), but which suggests that we have been slow to face the more general problems of social structure and relations and their transformations. These in turn raise technical problems, such as those of the possibly changing specification of the membership of a class over time, which complicates quantitative study. It also raises the more general problem of the multidimensionality of social groups. To take a few examples, there is the well-known Marxian duality of the term "class." In one sense it is a general phenomenon of all post-tribal history, in another a product of modern bourgeois society; in one sense almost an analytical construct to make sense of otherwise inexplicable phenomena, in another a group of people actually seen as belonging together in their own or some other group's consciousness, or both. These problems of consciousness in turn raise the question of the language of class—the changing, often overlapping, and sometimes unrealistic terminologies of such contemporary classification[13] about which we know as yet very little in quantitative terms. (Here historians might look carefully at the methods and preoccupations of social anthropologists, while pursuing—as L. Girard and a Sorbonne team are doing—the systematic quantitative study of sociopolitical vocabulary.[14])

Again, there are degrees of class. To use Theodore Shanin's

phrase,[15] the peasantry of Marx's 18th Brumaire is a "class of low classness," whereas Marx's proletariat is a class of very high, perhaps of maximal "classness." There are the problems of the homogeneity or heterogeneity of classes; or what may be much the same, of their definition in relation to other groups and their internal divisions and stratifications. In the most general sense, there is the problem of the relation between classifications, necessarily static at any given time, and the multiple and changing reality behind them.

(4) The most serious difficulty may well be the one which leads us directly toward the history of society as a whole. It arises from the fact that class defines not a group of people in isolation, but a system of relationships, both vertical and horizontal. Thus it is a relationship of difference (or similarity) and of distance, but also a qualitatively different relationship of social function, of exploitation, of dominance/subjection. Research on class must therefore involve the rest of society of which it is a part. Slaveowners cannot be understood without slaves, and without the nonslave sectors of society. It might be argued that for the self-definition of the nineteenth-century European middle classes the capacity to exercise power over people (whether through property, keeping servants, or even—via the patriarchal family structure—wives and children), and of not having direct power exercised over themselves, was essential. Class studies are therefore, unless confined to a deliberately restricted and partial aspect, analyses of society. The most impressive—like Le Roy Ladurie's—therefore go far beyond the limits of their title.

It may thus be suggested that in recent years the most direct approach to the history of society has come through the study of class in this wider sense.[16] Whether we believe that this reflects a correct perception of the nature of post-tribal societies, or whether we merely put it down to the current influence of *Marxisant* history, the future prospects of this type of research appear bright.

In many ways the recent interest in the history of "mentalities" marks an even more direct approach to central methodological problems of social history. It has been largely stimulated by the traditional interest in "the common people" of many who are drawn to social history. It has dealt largely with the individually inarticulate, undocumented, and obscure, and is often indistinct from an interest in their social movements or in more general phenomena of social behavior, which today, fortunately, also includes an interest in

those who fail to take part in such movements—for example, in the conservative as well as in the militant or passively socialist worker.

This very fact has encouraged a specifically dynamic treatment of culture by historians, superior to such studies as those of the "culture of poverty" by anthropologists, though not uninfluenced by their methods and pioneering experience. They have been not so much studies of an aggregate of beliefs and ideas, persistent or not —though there has been much valuable thought about these matters, for example, by Alphonse Dupront[17]—as of ideas in action and, more specifically, in situations of social tensions and crisis, as in Georges Lefebvre's *Grande Peur,* which has inspired so much subsequent work. The nature of sources for such study has rarely allowed the historian to confine himself to simple factual study and exposition. He has been obliged from the outset to construct models, that is, to fit his partial and scattered data into coherent systems, without which they would be little more than anecdotal. The criterion of such models is or ought to be that its components should fit together and provide a guide to both the nature of collective action in specifiable social situations and to its limits.[18] Edward Thompson's concept of the "moral economy" of preindustrial England may be one such; my own analysis of social banditry has tried to base itself on another.

Insofar as these systems of belief and action are, or imply, images of society as a whole (which may be, as occasion arises, images either seeking its permanence or its transformation), and insofar as these correspond to certain aspects of its actual reality, they bring us closer to the core of our task. Insofar as the most successful such analyses have dealt with traditional or customary societies, even though sometimes with such societies under the impact of social transformation, their scope has been more limited. For a period characterized by constant, rapid, and fundamental change, and by a complexity which puts society far beyond the individual's experience or even conceptual grasp, the models derivable from the history of culture have probably a diminishing contact with the social realities. They may not even any longer be very useful in constructing the pattern of aspiration of modern society ("what society ought to be like"). For the basic change brought about by the Industrial Revolution in the field of social thought has been to substitute a system of beliefs resting on unceasing *progress* toward aims which can be specified only as a *process,* for one resting on the assumption of permanent order, which can be described

or illustrated in terms of some concrete social model, normally drawn from the past, real or imaginary. The cultures of the past measured their own society against such specific models; the cultures of the present can measure them only against possibilities. Still, the history of "mentalities" has been useful in introducing something analogous to the discipline of the social anthropologists into history, and its usefulness is very far from exhausted.

I think the profitability of the numerous studies of social conflict, ranging from riots to revolutions, requires more careful assessment. Why they should attract research today is obvious. That they always dramatize crucial aspects of social structure because they are here strained to the breaking point is not in doubt. Moreover, certain important problems cannot be studied at all except in and through such moments of eruption, which do not merely bring into the open so much that is normally latent, but also concentrate and magnify phenomena for the benefit of the student, while—not the least of their advantages—normally multiplying our documentation about them. To take a simple example: How much less would we know about the ideas of those who normally do not express themselves commonly or at all in writing but for the extraordinary explosion of articulateness which is so characteristic of revolutionary periods, and to which the mountains of pamphlets, letters, articles, and speeches, not to mention the mass of police reports, court depositions, and general inquiries bear witness? How fruitful the study of the great, and above all the well-documented, revolutions can be is shown by the historiography of the French Revolution, which has been studied longer and more intensively perhaps than any period of equal brevity, without visibly diminishing returns. It has been, and still remains, an almost perfect laboratory for the historian.[19]

The danger of this type of study lies in the temptation to isolate the phenomenon of overt crisis from the wider context of a society undergoing transformation. This danger may be particularly great when we launch into comparative studies, especially when moved by the desire to solve problems (such as how to make or stop revolutions), which is not a very fruitful approach in sociology or social history. What, say, riots have in common with one another (for example, "violence") may be trivial. It may even be illusory, insofar as we may be imposing an anachronistic criterion, legal, political, or otherwise, on the phenomena—something which historical students of criminality are learning to avoid. The same may or may not be true of revolutions. I am the last person to wish to discourage

an interest in such matters, since I have spent a good deal of professional time on them. However, in studying them we ought to define the precise purpose of our interest clearly. If it lies in the major transformations of society, we may find, paradoxically, that the value of our study of the revolution itself is in inverse proportion to our concentration on the brief moment of conflict. There are things about the Russian Revolution, or about human history, which can only be discovered by concentrating on the period from March to November 1917 or the subsequent Civil War; but there are other matters which cannot emerge from such a concentrated study of brief periods of crisis, however dramatic and significant.

On the other hand, revolutions and similar subjects of study (including social movements) can normally be integrated into a wider field which does not merely lend itself to, but requires, a comprehensive grasp of social structure and dynamics: the short-term social transformations experienced and labeled as such, which stretch over a period of a few decades or generations. We are dealing not simply with chronological chunks carved out of a continuum of growth or development, but with relatively brief historic periods during which society is reoriented and transformed, as the very phrase "industrial revolution" implies. (Such periods may of course include great political revolutions, but cannot be chronologically delimited by them.) The popularity of such historically crude terms as "modernization" or "industrialization" indicates a certain apprehension of such phenomena.

The difficulties of such an enterprise are enormous, which is perhaps why there are as yet no adequate studies of the eighteenth-nineteenth century industrial revolutions as social processes for any country, though one or two excellent regional and local works are now available, such as Rudolf Braun on the Zurich countryside and John Foster on early nineteenth-century Oldham.[20] It may be that a practicable approach to such phenomena can be at present derived not only from economic history (which has inspired studies of industrial revolution), but from political science. Workers in the field of the prehistory and history of colonial liberation have naturally been forced to confront such problems, though perhaps in an excessively political perspective, and African studies have proved particularly fruitful, though recent attempts to extend this approach to India may be noted.[21] In consequence the political science and political sociology dealing with the modernization of colonial societies can furnish us with some useful help.

The analytical advantage of the colonial situation (by which I mean that of *formal* colonies acquired by conquest and directly administered) is that here an entire society or group of societies is sharply defined by contrast with an outside force, and its various internal shifts and changes, as well as its reactions to the uncontrollable and rapid impact of this force, can be observed and analyzed as a whole. Certain forces which in other societies are internal, or operate in a gradual and complex interaction with internal elements of that society, can here be considered for practical purposes and in the short run as entirely external, which is analytically very helpful. (We shall not of course overlook the distortions of the colonial societies—for example, by the truncation of their economy and social hierarchy—which also result from colonization, but the interest of the colonial situation does not depend on the assumption that colonial society is a replica of noncolonial.)

There is perhaps a more specific advantage. A central preoccupation of workers in this field has been nationalism and nation-building, and here the colonial situation can provide a much closer approximation to the general model. Though historians have hardly yet come to grips with it, the complex of phenomena which can be called national(ist) is clearly crucial to the understanding of social structure and dynamics in the industrial era, and some of the more interesting work in political sociology has come to recognize it. The project conducted by Stein Rokkan, Eric Allardt, and others on "Centre Formation, Nation-Building and Cultural Diversity" provides some very interesting approaches.[22]

The "nation," a historical invention of the past two hundred years, whose immense practical significance today hardly needs discussion, raises several crucial questions of the history of society, for example, the change in the scale of societies, the transformation of pluralist, indirectly linked social systems into unitary ones with direct linkages (or the fusion of several preexisting smaller societies into a larger social system), the factors determining the boundaries of a social system (such as territorial-political), and others of equal significance. To what extent are these boundaries objectively imposed by the requirements of economic development, which necessitate as the locus of, for example, the nineteenth-century type industrial economy a territorial state of minimum or maximum size in given circumstances?[23] To what extent do these requirements automatically imply not only the weakening and destruction of earlier social structures, but also particular degrees of simplification,

standardization, and centralization—that is, direct and increasingly exclusive links between "center" and "periphery" (or rather "top" and "bottom")? To what extent is the "nation" an attempt to fill the void left by the dismantling of earlier community and social structures by inventing something which could function as, or produce symbolic substitutes for, the functioning of a consciously apprehended community or society? (The concept of the "nation-state" might then combine these objective and subjective developments.)

The colonial and ex-colonial situations are not necessarily more suitable bases for investigating this complex of questions than is European history, but in the absence of serious work about it by the historians of nineteenth- and twentieth-century Europe, who have been hitherto—including the Marxists—rather baffled by it, it seems likely that recent Afro-Asian history may form the most convenient starting-point.

V

How far has the research of recent years advanced us toward a history of society? Let me put my cards on the table. I cannot point to any single work which exemplifies the history of society to which we ought, I believe, to aspire. Marc Bloch has given us in *La société féodale,* a masterly, indeed an exemplary, work on the nature of social structure, including both the consideration of a certain type of society and of its actual and possible variants, illuminated by the comparative method, into the dangers and the much greater rewards of which I do not propose to enter here. Marx has sketched out for us, or allows us to sketch for ourselves, a model of the typology and the long-term historical transformation and evolution of societies which remains immensely powerful and almost as far ahead of its time as were the Prolegomena of Ibn Khaldun, whose own model, based on the interaction of different types of societies, has of course also been fruitful, especially in pre-history, ancient, and oriental history. (I am thinking of the late Gordon Childe and Owen Lattimore.) Recently there have been important advances toward the study of certain types of society—notably those based on slavery in the Americas (the slave-societies of antiquity appear to be in recession) and those based on a large body of peasant cultivators. On the other hand the attempts to translate a comprehensive social history into popular synthesis strike me so far as

either relatively unsuccessful or, with all their great merits, not the least of which is stimulation, as schematic and tentative. The history of society is still being constructed. I have in this essay tried to suggest some of its problems, to assess some of its practice, and incidentally to hint at certain problems which might benefit from more concentrated exploration. But it would be wrong to conclude without noting, and welcoming, the remarkably flourishing state of the field. It is a good moment to be a social historian. Even those of us who never set out to call ourselves by this name will not want to disclaim it today.

REFERENCES

1. See the remarks of A. J. C. Rueter in *IX congrès international des sciences historiques* (Paris, 1950), I, 298.
2. R. H. Tawney, *Studies in Economic History* (London, 1927), pp. xxiii, 33-34, 39.
3. J. H. Clapham, *A Concise Economic History of Britain* (Cambridge, Eng.: University Press, 1949), introduction.
4. Two quotations from the same document (Economic and Social Studies Conference Board, *Social Aspects of Economic Development,* Istanbul, 1964) may illustrate the divergent motivations behind this new preoccupation. By the Turkish president of the board: "Economic development or growth in the economically retarded areas is one of the most important questions which confronts the world today . . . Poor countries have made of this issue of development a high ideal. Economic development is to them associated with political independence and a sense of sovereignty." By Daniel Lerner: "A decade of global experience with social change and economic development lies behind us. The decade has been fraught with efforts, in every part of the world, to induce economic development without producing cultural chaos, to accelerate economic growth without disrupting societal equilibrium; to promote economic mobility without subverting political stability" (xxiii, 1).
5. Sir John Hicks's complaint is characteristic: "My 'theory of history' . . . will be a good deal nearer to the kind of thing that was attempted by Marx . . . Most of [those who believe ideas can be used by historians to order their material, so that the general course of history can be fitted into place] . . . would use the Marxian categories, or some modified version of them; since there is so little in the way of an alternative version that is available, it is not surprising that they should. It does, nevertheless, remain extraordinary that one hundred years after *Das Kapital,* after a century during which there have been enormous developments in social science, so little else should have emerged." *A Theory of Economic History* (Oxford: Clarendon Press, 1969), pp. 2-3.

6. Thus Marc Ferro's sampling of the telegrams and resolutions sent to Petrograd in the first weeks of the February revolution of 1917 is plainly the equivalent of a retrospective public opinion survey. One may doubt whether it would have been thought of without the earlier development of opinion research for nonhistorical purposes. M. Ferro, *La Révolution de 1917* (Paris: Aubier, 1967).

7. At the conference on New Trends in History, Princeton, N. J., May 1968.

8. I do not regard such devices for inserting direction into societies as "increasing complexity" as historical. They may, of course, be true.

9. P. Baran, *The Political Economy of Growth* (New York: Monthly Review Press, 1957), chap. 2.

10. For an English version of this important article, see *Social Science Information,* 9 (February 1970), 145-174.

11. Cf. "At stake in a broader view of urban history is the possibility of making the societal process of urbanization central to the study of social change. Efforts should be made to conceptualize urbanization in ways that actually represent social change." Eric Lampard in Oscar Handlin and John Burchard, *The Historians and the City* (Cambridge, Mass.: M.I.T. Press, 1963), p. 233.

12. This work is in progress under the direction of Professor Sven-Ulric Palme at the University of Stockholm.

13. For the possible divergences between reality and classification, see the discussions about the complex socioracial hierarchies of colonial Latin America. Magnus Mörner, "The History of Race Relations in Latin America," in L. Foner and E. D. Genovese, *Slavery in the New World* (Englewood Cliffs, N. J.: Prentice-Hall, 1969), p. 221.

14. See A. Prost, "Vocabulaire et typologie des familles politiques," *Cahiers de lexicologie,* XIV (1969).

15. T. Shanin, "The Peasantry as a Political Factor," *Sociological Review,* 14 (1966), 17.

16. Class has long been the central preoccupation of social historians. See, for example, A. J. C. Rueter in *IX congrès international des sciences historiques,* I, 298-299.

17. A. Dupront, "Problèmes et méthodes d'une histoire de la psychologie collective," *Annales: économies, sociétés, civilisations,* 16 (January–February 1961), 3-11.

18. By "fitting together" I mean establishing a systematic connection between different, and sometimes apparently unconnected, parts of the same syndrome—for example, the beliefs of the classic nineteenth-century liberal bourgeoisie in both individual liberty and a patriarchal family structure.

19. We look forward to the time when the Russian Revolution will provide historians with comparable opportunities for the twentieth century.

20. R. Braun, *Industrialisierung und Volksleben* (Erlenbach-Zurich: Rentsch, 1960); *Sozialer und kultureller Wandel in einem ländlichen Industriegebiet . . . im 19. und 20. Jahrhundert* (Erlenbach-Zurich: Rentsch, 1965). J. O. Foster's thesis is being prepared for publication.

21. Eric Stokes, who is doing this, is conscious of applying the results of work in African history. E. Stokes, *Traditional Resistance Movements and Afro-Asian Nationalism: The Context of the 1857 Mutiny-Rebellion in India* (forthcoming).

22. *Centre Formation, Nation-Building and Cultural Diversity: Report on a Symposium Organized by UNESCO* (duplicated draft, n.d.). The symposium was held August 28–September 1, 1968.

23. Though capitalism has developed as a global system of economic interactions, in fact the real units of its development have been certain territorial-political units— British, French, German, U. S. economies—which may be due to historic accident but also (the question remains open) to the necessary role of the state in economic development, even in the era of the purest economic liberalism.