

Trends in Western Civilization Project, No. 4  
THE STATE OF MACRO-HISTORY TODAY

Reflections after interviews with fifteen  
West-European historians

by

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1. What we wanted to know: The state of macro-history today.

The Trends in Western Civilization Project was outlined by Professor Johan Galtung in 1972, and it is financially supported by the Bischof Stiftung (Germany), the Nordic Committee for Research in International Relations\* and the University of Oslo.

Behind this project there was a strong impression that, somehow, the comprehensive study of Western Civilization had fallen into disrepute and disregard among professional historians.

We have seen a lot of traditional narrative histories (histoire evenementielle) of Europe or Western Civilization, there is no lack of grand theory (Marx, Weber, Sorokin, Toynbee...) and, in the last decades, we have got an enormous number of modern 'structural' studies of strategic, but nevertheless highly restricted problems in Western history.

It was this state of research which I, on behalf of our project, set out to investigate during my visits to several West-European universities in July, 1974.

The same questions were put to all of the fifteen historians I met:

- (i) On which problems are you presently working or intending to work?
- (ii) Do you know of any person, project or group doing research on macro-historical problems like Trends in Western Civilization?
- (iii) Can you recommend any book treating European or Western Civilization as a whole?

In addition to these, I asked question of resources and organization and the role of history today.

The following chapters are a series of reflections on the basis of our discussions.

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2. What we all know.

The Trends in Western Civilization Project would not have been undertaken if this kind of enterprise had been a common one among historians. As we all know, it is not. But it might be that somewhere in Denmark, somewhere in England or other places some new initiative had been taken in the direction of Western macro-history. At least this was what Johan Galtung, Tore Heiestad and I assumed.

However, the more I travelled, the clearer one basic conclusion emerged: There are many new and important advances in historical research, but exceedingly few have to do with broad-scale studies and interpretations of the course of Western history. During my journey I heard of one or two historians who were reported to be working on more or less traditional event-histories of the World, but no one seemed to know of any work in progress comparable to, say, the books of the probably leading world-historian today, William McNeill,<sup>1</sup> the editor of The Journal of Modern History, Chicago.

This is not to say that interest in 'the larger questions of history' is minimal among modern historians; in their teaching and discussions certainly most historians touch upon the greater issues of World history or at least Western history. Nor is there any doubt that socially perceptive historians witness with growing uneasiness this gap between teaching and research, and between the historical profession and its public ('society'). But in spite of this uneasiness and such signs of the times as the re-emergence of German Universalgeschichte<sup>2</sup>, the central well-known fact, which too many seem to take for granted, remains: At least in Western Europe there is no such thing as an organized, systematic enterprise to collect and investigate data and theories relevant to several 'main trends' of Western Civilization. This is an historical fact which, in the very civilization that first produced critical and professional historiography, no longer ought to be seen as a normal, necessary, and self-evident state of affairs.

There is a growing need to scrutinize those arguments and those structures which until now have impeded the development of macro-history.

3. Macro-history and the cult of original sources.

'Given the course which historical studies have taken in recent times and which must be continued insofar as history is to produce studies embodying thorough research and precise knowledge, there does exist the danger of losing sight of the universal, of the type of knowledge everyone desires.' (Leopold von Ranke)

Thorough research and precise knowledge connected with first-hand archival study of original sources - those were the marks of academic scholarship by which Ranke and his school established history as a prestigious profession in the last century.

In this way the professionalized Quellenkritik of the new history satisfied at the same time the hegemonic current of positivism and the nationalistic need for a confidence-inspiring genealogy on the basis of the national archives and the prevailing concepts of "thorough", diligent, respectable bourgeois work.

Thus, the whole ethos and respectability of academic historians became directly linked to one particular aspect of the research process: the discovery and critique of original sources. This was clearly perceived and stated in the prospectuses of the leading historical periodicals of the 19th century (Historische Zeitschrift 1859, Revue Historique 1876, The English Historical Review 1886).

It goes without saying that this traditional identification of historical method with first-hand Quellenkritik tends to define projects of universal history outside the centrality and prestige of history proper.

Even today, this limited conception of method still seems to be prevalent among "radical" and "conservative" historians alike. Indeed, it is typical that the application of new insights from sociology and political science and the re-opening of social and local history have in many ways only extended the scene of microscopic, original-sources activity, whereas the problem of transnational universal history - with the exception of economic macro-history - have benefitted to a

much lesser extent from these developments.

Thus, the traditional ethos of historians and the national structure of historical archives still have a dominating effect on historical studies.

Any "way out" of this situation will have to be based on the legitimization of non-original sources as sufficient material for the study of universal history.

Such a legitimization would imply new kinds of scholarship with a heavy emphasis on bibliographical information and methods of data processing on the basis of already published and organized historical material. Of course, a more concentrated work to refine these methods is completely dependent on the basic research done by the original-sources historians. And the errors made by these will inescapably be prolonged and blown up in the works of the comparative and synthesizing historians. This is a dilemma, but the probability of such errors can hardly constitute an absolute argument against comparison and synthesis.

Today, synthesis of world-historical problems are very often by-products of mature original-sources historians' work on more specialized subjects. Again Ranke represents the model historian who in himself combines the roles of a specialist and a generalist. But it seems that Ranke's warning about 'the danger of losing sight of the universal' has not been taken as seriously by his successors as by himself. We have to admit that the "production" of techniques and data directly connected to original sources has increased infinitely more than the supposedly complementary output in macro-history.

This phenomenon is only too evident even in the great works of world history. Usually, they are multi-volume anthologies in which several specialists write chapters about their own speciality. The result of this kind of division of labor is generally a very fragmented view of history.

This lack of any explicit, professional analysis of theories of world history in the great multi-volume works points to the

fact that such insights are regarded as more or less casual by-products of ordinary history or as matters of other subjects: philosophy or religion.

In order to bridge the gap between the narrow and theoretically inexplicit studies of most historians and the unempirical "philosophies of history", it seems necessary to open a new area of specialized studies in history and to constitute theories of world historical problems as a specialty per se. That would mean to extend the professionalization of history to an area which until now has been cultivated by a rich fauna of aging, immensely learned historians, despairing prophets, gifted journalists and subtle theologians.

For smaller countries like the Scandinavian ones, whose world historical role since the time of the Vikings has been extremely limited, it is particularly necessary to leave the traditional ethos of original-sources scholarship as the one prestigious kind of merit.

Otherwise we shall persist in our self-imposed provincialism on the seemingly respectable and pragmatic ground that our national archives simply force us to do so. Such a view ignores the amount of publication of foreign original sources and the vast production of special studies, many of which are more reliable than any singular original source. But above all it ignores the need for a thorough systematization and processing of the flow of data from the specialist studies.

A new ethos of historians would recognize both the necessity of original-sources studies and the need for more refined methods of synthesis. This would at the same time mean to recognize the commitment to both the local and the universal perspective on history.

But it is only a debatable 'impression' that the balance of historical research seems to be seriously destroyed - at the sacrifice of "the universal, of the type of knowledge everyone desires"?

This problem of imbalance was highlighted by the History

Panel of the U.S. Behavioral and Social Sciences Survey<sup>3</sup>:

"Along with this striving for exhaustiveness has gone an inordinate respect for what is called original research - which is not so much research based on original ideas as first-hand research based on personal investigation of the original (that is, primary) sources. Here history seems to be different from other disciplines, whose members are not only pleased to rely on the evidence assembled by others but also base much of their technique of inquiry on the assumption that processed data are available and usable.

The historian who is confronted with official statistics, for example, worries a great deal about how the statistics were collected. The economist (not the economic historian) takes them for what they are worth and tries to manipulate them in such a way as to compensate for error.

The difference in attitudes shows up clearly in the response to some of the new bibliographical services now becoming available. Whereas most social scientists have accepted with alacrity the opportunity to rely on abstracts for information about current publications, historians have remained suspicious. They look upon this kind of second-hand information as potentially misleading and reliance thereon as a kind of abdication of responsibility.

Thus in a report on Bibliography and the Historian, Dagmar Perman cites some of the replies to a questionnaire sent by the Joint Committee on Bibliographical Services to History to a random sample of three hundred members of the American Historical Association (only fifty replies were received).

The responses were as interesting for what was not wanted as for what was: very few requested further annotation of bibliographical entries and almost none wanted evaluations of these entries; on the contrary, many explicitly stated that they did not consider such evaluations as desirable or useful. This was the respondents' attitude toward publications in their own field. Very few had any interest in bibliographical information on other fields, but paradoxically, in so far as they

did want it, they wanted carefully selected, highly annotated and evaluative lists...Nothing comes free, and the insistence on 'original' research is bought at a price. No other discipline builds so slowly, because the members of no other discipline are so reluctant as historians to stand on the shoulders of others.

All historians can recall criticisms of colleagues and students on the ground that their work was too derivative at one point or another, that it relied too heavily on secondary sources. In one such discussion a man's job hung in the balance...until one scholar observed: 'Why not? I always thought I was writing these monographs so that someone else could use them.' "

4. 'A new kind of history': the Annales school.

It has become a commonplace within the historical milieu that, to put it crudely, the model historians of the world are those more or less directly associated with the periodical Annales (Economies, Sociétés, Civilisations) and the Sixième Section of the Ecole pratique des hautes études in Paris. The character of this 'school' and the nature of its world-wide influence have been evaluated by i.a. John Hexter.<sup>4</sup>

It was therefore not surprising to hear that almost every historian I met said that he admired the works of the Annales historians as being the most representative examples of good historical scholarship today. With certain qualifications, this was also the view of Geoffrey Barraclough, whose great world-historiographical survey for UNESCO (to be published) makes him a very reliable guide in these matters.

The essential merit of the Annales 'school' has been to open history to themes and methods which previously belonged to geography, the social sciences, and even the natural sciences. This has been an invaluable contribution; it has given history greater depth and it has sharpened the historians' sense of complex, interdependent structures of society.

But this deepening of history into new transdisciplinary fields of knowledge has happened in a rather one-sided way - in two respects:

(i) In spite of the impressive examples<sup>5</sup> set by Lucien Febvre and in spite of the favourite Annales concept of mentalités, the Annales historians have contributed singularly little to cultural and intellectual history.<sup>6</sup> Unintentionally they may rather have contributed to the apparently prevailing attitude of disdain for Kulturgeschichte among many of the exponents of social and economic history. In any comprehensive study of civilization such an attitude would of course be completely inadequate.

(ii) In spite of the omnivorous, gargantuan spirit of the doyen of Annales, Fernand Braudel, whose large-scale perspective

dominates his major works,<sup>7</sup> the bulk of the Annales research is narrowly specialized, or, in Professor Braudel's own words, the younger historians have become too monogamous; they have become too closely 'married' to a particular, narrowly defined area or method of research. Instead, 'l'historien doit être polygame'.

The lack of polygamy is evident in historians' use of e.g. sociology, from which several micro-sociological methods and techniques have been borrowed, whereas the macro-sociological elements in Spencer, Marx, Durkheim, Weber, Hobhouse have been comparatively neglected.<sup>8</sup>

As Louis Dumont observed, we are approaching a state of specialization in which the specialities have become so specialized that the specialists are on the point of being incapable of mutual understanding.

In the case of many of the Annales historians this problem of communication arising from the rigorous narrowness of subject-matter may have become serious also because of their somewhat esoteric and pretentious language, which my old tutor Richard Cobb has criticized and ridiculed.<sup>9</sup>

The Annales 'school' owes its present hegemonic position to its pure cultivation of Ranke's ideals of factual information and methodological ingenuity. But this maturation of the professionalization of historians has emerged with obvious limitations: macro-history is still largely the business of amateurs, and the process of professionalization has not been extended to the public function of historians, to their capacity to find out and communicate 'the universal, ...the type of knowledge everyone desires' (Ranke).

5. The national limitations of historical research.

To anyone familiar with the historical milieu the general prestige of the Annales 'school' is a trivial matter of fact. Nor is it a surprise to perceive the almost total lack of any regular and concrete cooperation between the Annales historians and their foreign admirers. Top-level historians meet, know each other, exchange papers etc., but they are very seldom engaged in common research programmes crossing national borders.

Seen from the outside the whole Annales culture looks unavoidably very French, and in spite of its influence it has not been able to take a lead in the process of breaking the national limitations of historical scholarship and to inaugurate a new era of transnational, cooperative and comparative research programmes.

This is not to say that such programmes do not exist at all, but that they are of minimal importance compared to the bulk of historical research and, especially, compared to the role such programmes play in the social and natural sciences.

Again and again the national structure of archives is invoked as an argument against transnational research. Surely it is an important impediment, and one of the ways to deal with it would be precisely to reorganize greater parts of historical research along non-national lines. The comparative failure to build up UNESCO<sup>10</sup> as one of the possible centers for such a reorganization may have discouraged people who have seen the need for combined efforts in this field.

There is little doubt, however, that the 19th century advantage for Western historical research - the public function of historians being acknowledged for their contributions to national consciousness and nation building - now has turned into a disadvantage: the national ties of history tend to impede more advanced forms of transnationally organized research programmes, of the kind and extent which today seem to give the social and natural sciences much greater impetus - and

resources - than history, retaining very much of its 19th century insular character.

To overcome this organizational backwardness will probably be one of the great challenges for historians in the coming years.

6. The local organization of historical research.

The relative backwardness at the level of international cooperation (or rather non-cooperation) in history has much to do with the somewhat anarchical structure of research organization at the local, departmental level. Again history generally comes out unfavourably as compared to the social and natural sciences: It is not difficult and not necessary to show that common research programmes and project groups play a much greater role in the organization of these sciences than in history.<sup>11</sup> Some of the reasons for this difference are obvious: Especially the natural sciences have close contacts with the innovation process in industry and have been, to some extent, re-moulded to meet the needs of industrial planning and development, implying greater emphasis on efficient organization. This emphasis has also been conditioned by the organizational demands arising from the collective use of expensive technical equipment within the natural sciences departments themselves.

In the case of the social sciences government policies and grants have often favoured large-scale investigations conducted by project groups. The new data technology, far from having fully come to history, has strengthened the more collective character of social science work. And it has had the advantages of the late comer: It was established and organized at a much more 'modern' stage of societal development than history, which may have become organizationally ossified just because of its century of enormous prestige.

Although the era of historians being national oracles and geniuses has waned long since, the element of individualism seemed to prevail in most historical departments I visited or heard of; the historian's craft is still a basically lonely business. (Cf. p. 7). But there are notable exceptions, such as Peter Laslett's by now well known Cambridge Group for the History of Population and Social Structure or Sven Tägil's group for the study of national borders and border populations in history (University of Lund, Sweden). As a matter of fact,

the smallest historical department I visited, viz. that in Lund, was undoubtedly most advanced with respect to organization. Under the leadership of Professor Jerker Rosén it has made impressively concentrated efforts despite rather adverse conditions as compared to historical departments in e.g. Denmark and Norway.

The increasing trendiness of 'projects' makes it pertinent also to point out some problems arising from this way of doing history. This is particularly important since it will probably soon be clear, even in history, that organizing research in 'projects' is the easiest way of obtaining financial support and attracting students.

But calling an enterprise a 'project' does not automatically make a valuable, common research programme or a sense of research community. The hierarchical university structures still prevail and make crude competition and elitism inescapable.

Thus, as long as the hierarchical structures predominate, 'projects' will only apparently be a more democratic way of doing research. It can easily mean increased efficiency in the form that some people will have more other people working for them.

The concentrated resources invested in projects also make the problem of priority (according to what values?) more fateful than before. As Esben Albrechtsen in Copenhagen observed, the enormous increase in sheer numbers, be it students, staff, or money, which most historical departments have experienced the last ten years, has above all tended to reinforce traditional priorities, whether resources are combined in formal 'projects' or not.

That is why the new project models, until now, primarily have benefitted national or neighbour-countries history and micro-history inspired by micro-sociology. This is conspicuous in two such different places as the Sixième Section itself and the Historical Department in Oslo.

7. The need for theory.

There seems to be an unspoken symbiotic relationship between history and the (other) social sciences: Historians provide data, social scientists provide systematizing and interpretative methods and theory, or, in the less charitable words of Stein Rokkan: 'History is too important to be left to the historians.'

The explosive increase of the amount of new historical data produced by diligent, specialized historians is impressive, but it has created a certain confusion, the confusion arising from unstructured, unrelated knowledge: Histories rather than history. Hence there is a constant need for overview, for simplification, for a framework of common points of reference. Increasingly, this framework is provided by concepts and theories from the social sciences.

A leading exponent for close contacts between history and the social sciences is Hans-Ulrich Wehler,<sup>12</sup> University of Bielefeld. He is now embarking on a long-term study of the evolution of Germany since the 19th century, in the light of i.a. macro-sociological theories of development. The recent revival and recasting of the old macro-sociological traditions seem to a surprising extent to pass unnoticed by most historians although these theories sometimes have a very direct bearing on the interpretation of Western history. This applies particularly to the theories of nation building.<sup>13</sup>

One of the really weak points in quantitative macro-sociology as well as in much Annales research, is the cultural aspect of society.

At the University of Sussex Peter Burke has started a comparative study of European popular culture in the early modern period. He works within an emerging British tradition of cooperation between history and social anthropology, originated around Evans-Pritchard and Keith Thomas<sup>14</sup> at Oxford. Concepts and theories of rationality, magic, popular symbolism applied to the concrete material of European history will

be of great strategic value when we again shall reinterpret that dramatic transformation and purification which was the early modern period.

In Cambridge, Professor J.H. Plumb, as a part of his work has taken up the study of one of those essential processes which normally disappear in the narrative histories of Western Civilization, that is the process of commercialization, in Plumb's study extended to the commercialization of leisure.<sup>15</sup>

This is a point of view which, although limited to the specific case of leisure, generates a common framework for understanding: It points to commercialization as one of those basic, long-term processes which have shaped Western history. In this way a link is established between the epochs of Western history, and the commercialization of leisure becomes an organic part of the broader pattern of social change which Robert S. Lopez has analysed with regard to its medieval break-through.<sup>16</sup> It goes without saying that an explicit theory or hypothesis of such a pattern will stimulate research and make communication and debate between specialists easier.

As J.H. Plumb observed, 'the aridity of old-fashioned specialization' has created a deeply felt uneasiness among many historians, but an even greater uneasiness exists as to the alternatives. How can one reasonably do macro-history? The alternative which Professor Plumb himself seemed to propose, is that of picking up general themes and trends, elaborating them and thereby giving some order to the present anarchic state of monographical event-data.

Having a very perceptive eye to their public, be it their students or the readers of their important review articles, both J.H. Plumb and Geoffrey Barraclough were painfully aware of the gap between teaching and research in history. The topics of teaching are generally very much broader than the objects of research, and more often than not those who lecture in 'European history' or 'World history' have their research background in some narrow problem in the history of their own country. This is a paradoxical situation which means that both teaching and macro-history are comparatively unprofessionalized.

Therefore, one way to reduce the incongruence between teaching and research (and the communication problem between the historians and their public and, not to forget, between the historians themselves) would be to initiate research in theories and concrete empirical problems of macro-historical scope. One useful exercise for such a reorientation could be to take the lecture programme presented to undergraduates and analyze how the problems and theories outlined there could constitute a programme for new, non-microscopic research. A greater research priority of the problems of teaching will necessarily imply a greater emphasis on explicit theories (or rather hypotheses) as clear starting points for macro-historical investigations.

As Geoffrey Best pointed out, the unorthodox 'integrated' curricula of the new polytechnics in Great Britain will ultimately require, and probably stimulate, more transdisciplinary and macro-historical research. The perspectives of teaching may again seem to be ahead of those of research. That is a further reason to bridge the gap between the two.

There is perhaps no history in which one can so easily degenerate into a mere antiquarian as the Chinese one. But precisely in this field Mark Elvin<sup>17</sup> has succeeded in elaborating a theory ('the high level equilibrium trap') which already has proved rewarding to specialists and amateurs as well. It is the kind of book which at once stimulates counter-research and influences new research programmes. Most likely history would be better off if more resources were directed into similar pattern-research. Now Dr. Elvin is engaged in a large-scale computer work on Chinese population history, with special regard to regional differences. Together with American and Norwegian studies of nation building this project marks the beginning of the application of computers to macro-historical work. In this field as in others the coming of the computers will make discussions of methods and theories more urgent than before.

## 8. The Trends in Western Civilization Project

In the background of the preceding seven chapters some words should be said about the nature and aspirations of Professor Johan Galtung's new history project.

Like many other social scientists Johan Galtung has in the past few years become more and more preoccupied with the historical dimension and roots of modern development. The references to the relevant parts of his previous work are contained in the first, introductory paper in this series.<sup>18</sup>

But why take the unit of 'Western Civilization' as a point of departure? First, that this is one way of doing world history, not as an alternative to local and regional histories, but as a necessary supplement, more necessary today than ever before because the structure of economy and politics has become global. A global view on the role of Western Civilization would mean that it is seen both from the inside and, especially, from the outside.

Second, that of all the major cultural configurations in the world Western Civilization is still the most powerful and dominating one. Whether one prefers talking about a 'crisis' of Western Civilization or not, one has to admit the existence of several serious problems created within or by this civilization: problems of ecological imbalance, of uncontrolled industrial development, of diversity without comparable equality, of economic, political, and cultural expansionism. Until recently, the whole concept of 'Western Civilization' has been invoked as an apologia or a smoke screen covering up all these problems.

Today it is necessary, at last, to stop treating Western history as coterminous with world history itself or to see it as the model for modern development.<sup>19</sup> Non-Western history and non-Western societies must be understood on their own terms. But the same applies to Western history as well: the de-universalization of it is an essential precondition to understanding it properly.

In this situation the advantages of the exceedingly broad unit of civilization are that it allows for a truly long-term view of history (roughly, we start with the decline of the West-Roman empire) and at the same time encompasses the full interplay of cultural, economic, and political aspects of history.

This is to say that such crucial phenomena as the Industrialization of the West must be understood in its extreme slowness and in its singularity as compared to developments in other civilizations. But we shall not be over-emphatic as to the importance of industrialization as a Western trademark. Long before the emergence of industry concepts of the character of the West was firmly established in the minds of Westerners and non-Westerners alike, - concepts stressing such factors as e.g. aggressiveness and universalism on the one hand and an extraordinary blend of unity and diversity on the other.<sup>20</sup> In future papers we shall try to analyze in depth this early awareness of particularly Western structures and to trace the apparently remarkable continuity of these structures in Western history, before and after the Industrial Revolution.

We strongly believe that the present problems (or crises) of Western Civilization, which to a large extent have been imposed on the rest of the world, are fundamental also in the sense that they represent historical habits of thought and action. We have to understand how old and how entrenched and how unconscious those habits really are in order to change them, if they ought to be changed.

One of the necessary questions will be: What are the 'holy cows' of Western Civilizations? One of them is undoubtedly the division of labour in the form of narrow role specialization. Since this has brought our civilization to the point where nobody is educated or employed to see the whole (be it ecology or history, not to speak of history of ecology), one obvious remedy will be in history, to trace the tradition of holistic thinking, of broad-scale analysis and interpretation of Western development.

When the United States Western Civilization Project started in 1972-73, Jonas Galting conceived the idea of systematizing, comparing, and synthesizing the methods and theories of Marx, Weber, and other thinkers and perhaps other macro-historians. This work will now be initiated by Professor Galting in order to provide a general framework for more empirical investigations of culture and structures.

The work done by Eirikur on the analysis of long-term trends of population, production, or capital and urbanization will continue, whereas Herit Magrseth will be engaged to finish his research on the role of intellectuals. We shall publish comparative studies by Eirikur on the Islamic and Islamic, and a paper on Christianity and Western development thinking by myself. Forthcoming papers will include a collection of bibliographical essays on macro-history, a revised version of 'Two Ways of World History: Some Similarities between Marxism and Liberalism', by Jonas Galting and a paper on the theories of the evolution of the modern period in Western history.

We should also consider comments on these papers as well as bibliographical references. We should also like to consider for publication a series of interpretative papers on aspects of Western history. In addition, during the next years, to build up an international group of scholars who will be interested in some kind of coordinated macro-historical research.

Any study of Western Civilization will easily get a narrowly West-European bias. There are more or less hidden 'fringes' of this civilization which conventionally have been defined or ignored as semi-primitive, semi-irrational, semi-oriental and certainly not 'typically Western':

1. Women
2. Children
3. The lower classes
4. Non-aristotelian scientists
5. Eastern and Nordic Europe.

To integrate the activities and mentalités of these 'fringes' into a broader concept of 'Western Civilization' would also imply a less dramatic and less pessimistic view of the decline of certain 'Western' positions in the world today. When Rome fell, its poor peasants did not stop.

## 9. References

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2. Ernst Schulir (ed.), Universalgeschichte (Köln 1974). See also Alfred Heuss, Nur Theorie der Weltgeschichte (Berlin 1968); Fritz Wamser, Der Historiker und die Weltgeschichte (Freiburg und München 1965). From a pedagogical point of view: Gert Müller, 'Universalgeschichte in Unterricht und Lehre', in: Neue Sammlung, Jg. 4, (1964), 243-262; Ernst Weymar, Universalgeschichte 1-11 (Review article), in: Geschichte in Wissenschaft und Unterricht, Heft 4-5 (1973).
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4. Especially J.R. Hexter, Bernard Braudel and the 'MONDE BRAUDELLENIEN..', in: The Journal of Modern History, Vol. 44 (1972), 480-539. See also H. Aymard, The Annales and French Historiography (1929-1972), in: The Journal of European Economic History, Vol. 1 (1972), 491-511; the special issue of Daedalus (Spring 1971), 'Historical Studies To-day'; R. Stuart Hughes, The Obstructed Path. French Social Thought in the Years of Desperation 1930-1960 (N.Y., Evanston and London 1966), Chapter 2: 'The Historians and the Social Order'.
5. Lucien Febvre, Un destin: Martin Luther (Paris 1926) and Le problème de l'Incroyance au XVIe siècle (Paris 1942). See also the fine essays edited by Peter Burke, A New Kind of History and Other Essays (London 1973).
6. Of the criticism made by Keith Thomas in The New York Review of Books (December 13, 1973): 'It is no accident that the most enduring monographs produced by the Annales school during the past twenty years have related to material affairs, particularly demography and trade, whereas Febvre's plea for the study of mentalités remains relatively unanswered. There are scores of books which owe their inspiration to The Mediterranean. But where are the worthy successors to Le problème de l'Incroyance au XVIe siècle? This question could also be raised in connection with the recent English editions of articles from Annales: Peter Burke (ed.), Economy and Society in Early Modern Europe (London 1972) and Marc Ferro (ed.), Social Historians in Contemporary France: Essays from Annales (N.Y. 1974).
7. English translations:  
The Mediterranean and the Mediterranean World in the Age of Philip II (London 1972 and 1973), 2 vols.; Capitalism and Material Life 1400-1800 (N.Y. 1973), vol. one. According to Prof. Braudel, the MSS for the two last volumes of this work are just finished.

3. The... exclusion of... But... analysis have... been the case even... Salcott-Parsons,... and... (New York, N.Y. 1966) and... (New York, N.J. 1971).

9. ... (London 1969).

10. ... in... 72-82.

... ... to its readers:

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11. ... ...

12. ... (Frankfurt 1973) is a... ... (Köln 1971); ... (Köln 1973); ... (Köln 1973).

13. See especially Stein Rokkan, Citizens, Elections, Parties (Oslo and N.Y. 1970), Chapter 4: 'Models and Methods in the Comparative Study of Nation-Building'; S.K. Misenstadt and S. Rokkan (eds.), Building States and Nations, Vol. I (London and Beverly Hills 1973), Chapter 2: 'Cities, States and Nations: A Dimensional Model for the Study of Contrasts in Development'; and 'Building States and Nations: A Selective Bibliography of the Research Literature by Theme and Country'; S. Rokkan, 'Dimensions of State Formation and Nation-Building: A Possible Paradigm for Research and Variations within Europe', in Charles Tilly (ed.), The Formation of National States in Western Europe (Princeton 1974).
14. Religion and the Decline of Magic (London 1971) has already become a classic.
15. See Prof. Plumb's contribution to Michael A. Marras (ed.), The Emergence of Leisure (N.Y. 1974).
16. The Commercial Revolution of the Middle Ages, 950-1350 (I.J. 1971)
17. The Pattern of the Chinese East (Stanford 1973).  
See the thorough review article by Ramon H. Myers, 'Transformations and Continuity in Chinese Economic and Social History', in: The Journal of Asian Studies, Vol. XIII, No. 2 (February 1974).
18. Johan Galtung, On Macro-history and Western Civilization, Trends in Western Civilization Project, Paper No.1 (Oslo 1974).
19. See e.g. Johan Galtung, Perspectives on Development: Past, Present, Future. Mimeo. To be published in Essays in Peace Research, Vol. 4 (Copenhagen 1975).
20. The classical theory of Western unity and diversity is discussed in Erik Rudeng, Concepts of Western Pluralism, Trends in Western Civilization Project, No.2 (Oslo 1974).
21. The first tentative result is Some Longitudinal Tendencies, Trends in Western Civilization Project, No.3 (Oslo 1974).

10 Behavioral and Social Sciences: Some U.S.  
Experiences with the "Scientificity of History."

It is difficult to make any generalization about the movement toward the scientific history substantiated by reference to the behavioral and social sciences and behavioral and social sciences in the United States between 1967 and 1980. The National Commission on Science and Public Policy (National Commission on Science and Public Policy, National Research Council of the National Academies, and the National Research Council on Behavioral and Social Sciences Research Council, 1980) was headed by David S. Landes and Charles T. Hays. The report on history which was published in 1980 about the "historians: History and the Behavioral and Social Sciences (1980, 1971).

The report of the commission should be applied as a description of all the work of history in the countries of Western Europe and the United States. The general trends.

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Efforts are being made to do this. By comparison with the other sciences, history is a poor performer. The amount of university funds available for history is small; other things being equal, the amount of money to be allocated in proportion to the amount of research and similar criteria. But we recognize that the amount of research, which are channelled overwhelmingly, into the natural sciences, then to the more "scientific" and equipment-intensive of the behavioral and social sciences.

This is evident in all the statistical data. The annual operating cost per full-time equivalent faculty member of PhD-granting history departments came to \$15,230 in 1967; the next lowest of the social sciences, psychology, spent \$24,020; the mean of six social sciences was \$18,750.

Whereas the average large psychology department was using equipment valued at \$44,100, and the average large department even in less experimental disciplines like economics and

sociology enjoyed a capital stock of \$13,820 and \$31,050 respectively, history could draw on a modest pool of typewriters and ditto machines, plus an occasional adding machine, valued at an average of \$2,350 per PhD-granting department.

The major psychology departments averaged 1,263 square feet of space per faculty member; the mean for the social sciences was 565 square feet; history, as usual at the bottom of the list, enjoyed 227 square feet.

What is more, these departmental figures are only a partial indication of the discrepancy between history and the other disciplines; for in the other social sciences, far more than in history, much of the research is undertaken in institutes and centers independent of university departments. These have their own plants, their own budgets, their own offices: in 1966 they accounted for 35 percent of research expenditures in the social and behavioral sciences (as against 34 percent for departments and 31 percent for professional schools). Most of these institutes, to be sure, are interdisciplinary in character and a number have found places for historians on their staff; but these are a small privileged group and account for a small fraction of these research outlays and facilities.

Spelled out in real terms, this means that the historian - even the elder statesman of established scholarly reputation - rarely receives summer support or has an assistant or secretary of his own; that his department is drastically underequipped with microfilm readers, duplication and photo reproduction equipment, recording equipment, and the other tools of his trade; that he lacks the office space and spatial arrangements most conducive to effective teaching and research; and that he cannot afford the travel and telephonic communications that have made it possible in other disciplines, particularly the natural sciences, to talk of an international community of scholars.

The one really costly installation used and considered indispensable by historians - for the modesty of their circumstances is matched by the modesty of their expectations - is

the Library, and the fact that in Chapter 7, the exponential increase in the volume and cost of new publications has compelled even the profit-making publisher to sacrifice the quality and readability of his books, while the growing dependence on the printed word in all forms of reading matter and a declining interest in the printed book has left historians almost without the tools of their literature and the other kind of literature that is available univ-  
ersally, whether in the form of the printed book or the radio.

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It is not only the individual historians who pursue their research with a minimum of facilities provided in other respects, but also the institutions. A Business Survey Committee's study of the cost of research reveals that during the decade 1930-40 the average scientific committee paying psychology salaries increased their salaries by 5 percent more per dollar of salary than the average of their expenditures for scientific research, while they by 508 percent.'

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It is not only the individual historians of science, but also the institutions that are interested also show the same kind of indifference to the science of science.'

Appendix B: Fifteen historians.

During my visits to various West-European universities, July 1974, I had the pleasure of meeting these historians:

Jerker Rosén Sven Tägil	University of Lund
Esben Albrectsen	University of Copenhagen
Rainer Tamchina	University of Hamburg
Hans-Jürgen Schröder, Claus Schalf	Institut für Europäische Geschichte, Mainz
Hans-Ulrich Wehler, Sidney Pollard (p.t. Bielefeld)	University of Bielefeld
Fernand Braudel, Louis Dumont (comp.sociol.)	Maison des Sciences de l'homme
Geoffrey Best, Peter Burke	University of Sussex
Geoffrey Barraclough	Formerly University of Oxford
J.H. Plumb	University of Cambridge
Mark Elvin	University of Oxford