

Jean-Michel Chapoulie

A Framework for the History of Social and Behavioral Sciences

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A Framework for the History of Social and Behavioral Sciences

by Jean-Michel Chapoulie

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“If you want to understand what a science is, you should look in the first instance not at its theories or its findings, and certainly not at what its apologists say about it; you should look at what the practitioners of it do.”

Clifford Geertz

“The various social sciences are the institutionalized products of social movements.”

Everett C. Hughes

“Scientific culture is made up of all sorts of bits and pieces – material, social, conceptual – that stand in no necessary unitary relation to one another.”

Andrew Pickering

In 1983, while working on a paper about the history of the use of observation in social sciences, I read Jennifer Platt’s paper on the development of participant observation in the United States [Platt 1983]. I was surprised to discover that Jennifer Platt adopted an approach which was, it seemed to me, close to that which I was trying to follow in the paper being written. She left the history of ideas or of “theories” to the side in order to focus her analyses on the study of research approaches; at the same time she adopted a truly historical approach based on accounts and archives and did not content herself with putting forward uncontrollable sources such as “what everybody knows in the environment concerned.”¹

Although trained as a sociologist (and not a historian), I had at that time some experience in the historical approach, gained during research on the development of schooling in France. I had little interest then for what was considered as the history of sociology: it was mainly the study of well-known works, conducted with the resources of the usual techniques used for the exegesis of literary or philosophical texts – having the sole specificity of focusing on texts connected, often, it may be

¹ The elements which made for the originality of the perspective adopted by Jennifer Platt’s paper could already be found in an earlier work by this author presented as being about the sociology of the research process [Platt 1976, 9].

added, a posteriori, to sociology. It was in fact, simply a matter of a history of ideas on society led according to a “presentist” point of view. Moreover, there was no acknowledgement of the limits of this presentism. In some cases there was also an effort to replace the works in their context of production, which generally boiled down to the biography of the author and very general elements of the history of his time. Lewis Coser’s book, *Masters of Sociological Thought* [Coser 1971], was at times quoted as an accomplished example of this conception.² Rapid presentations of research evolutions for a national tradition and era, based on documentation which was generally not specified, could also sometimes be found under the history of sociology label.³ The conceptualizations used in all of these analyses were those offered by a rarely disputed tradition: the classification of works according to the schools of thought postulated (like the Durkheimians, symbolic interactionism, structural functionalism, etc.) and conventional categories and reasoning such as the “influence” of one work on another. The attention given to institutional and financial aspects in the production of research was either inexistent or superficial, as was the attention paid to the publics to whom this research was destined; the examination of argumentation styles was wholly neglected. The ends which the conclusions of these history of sociology analyses revealed referred back to the present of this discipline: celebrating the progress made by successive generations of researchers, the definitive perceptiveness of forerunners or of the great forefathers, putting a forgotten precursor back in his place, unearthing the “true” thinking of an author, denouncing the past errors of those who had not thrown light on the knowledge of present times, etc.

The following year, in 1984, Martin Bulmer’s work, *The Chicago School of Sociology* [Bulmer 1984] was published, which adopted an approach partially similar to that of Jennifer Platt’s paper. To the history of methods Bulmer paid an added attention to the institutional dimension of the development of sociology and to the financing of research and followed a truly historical approach based on a rigorous sorting through of archives.

A little later, Howard Becker, by encouraging me to look at the archives of the University of Chicago sociologist, E.C. Hughes, which had just been deposited at the Joseph Regenstein Library, transformed what up until then had been for me but a research topic of momentary interest, into a long-term investigation topic. At the end of the 1980s, I thus began to do historical research, which was to result notably in

² See for example Szacki [1981]. Moreover, this paper shows that the reserves that I harboured concerning the history of sociology were already commonplace at the time: Szacki deems that this history is “marked with amateurism” and also observes that it is wrongly dissociated from the history of other social science disciplines.

³ See Genov [1989] for typical examples of this type of analysis.

a paper on French sociology of the 1945-1960 period – which adopts the history of methods thread – and in a book on the Chicago tradition in sociology which develops a broader approach.⁴ At the same time I became acquainted with works published on the history of social and behavioral science in the United States, a domain which was to become my main specialized field of research for about ten years. I thus discovered that there were some pieces of research, the oldest of which dated back to the mid 1970s, which departed resolutely, but generally without explicitly indicating it, from what had for a long time been the main subject of history of sociology publications.⁵ Henceforth I will refer to the conception of the history of the disciplines which inspire these works by the expression “conventional conception of the history of social and behavioral science.” We find this type of history for each of the social sciences, written for the most part by researchers who themselves belong institutionally to the discipline which they study. Jennifer Platt was at the centre of the renewal movement for the history of social sciences outwith the conventional history, notably by her work

⁴ See Chapoulie [1991]; Chapoulie [1996]; Chapoulie [1998]; Chapoulie [2001a]. The question of the relation of the sociology of Park and of his successors with American society rapidly became important during the second stage of this research, which was undoubtedly due in part to my exteriority to this sociological tradition and to American society. One of the central issues in American society debates of the first half of the Twentieth century – the “Americanization” of non Anglo-Saxon populations – has indeed no counterpart in French sociology.

⁵ I examined this renewal in a paper [Chapoulie 1995] presented at a reunion of the small group led by Jennifer Platt, the interests of which were focused on the history of social sciences and based on archives and testimonies. Several independent factors contributed to this renewal: the deposition in archives and the subsequent accessibility to documents from educational and research institutions and to researchers’ and administrators’ personal documents; the broadening of research topics among historians with the publication of research on universities, cultural phenomena and social movements; the development of new approaches in the sociological studies of natural sciences towards the end of the 1970s following the works of Kuhn [1962], Bloor [1976], and ethnographical laboratory research (the impact of which seems to me to have been more indirect than direct). Among the first publications (to my knowledge) which bear witness to this renewal of the history of social and behavioral science we find works, written by historians, about the beginnings of American social sciences [Furner 1975; Haskell 1977] and the book of a sociologist [Carey 1975]; some biographies of researchers, such as Matthews [1979]. Some earlier attempts at a renewal which fell within the history of methods: Lazarsfeld [1961]; Obershall [1965]. In 1967, Robert Merton had also proposed a distinction between “systematics” and “history” which, while limiting the domain to the history of sociological ideas, could equally have inspired a renewal of the history of sociology, but which, to my knowledge, did not have this posterity. The renewal started slightly earlier for anthropology than for sociology: see the discussion of the relative advantages of presentism and historicism in Stocking [1965]. I will virtually limit myself to giving references to works in the English language here, but research in the history of social and behavioral science in France has been concerned by a renewal, partially similar in its thematic orientation and documentary sources: see, by way of examples, Mazon [1988], Desrosières [1993], *Genèses* 5 [1981] on the “scholarly representations of society,” and notably the contribution of Christian Topalov, from whom I borrow this expression, as well as the earlier work (influenced by Lazarsfeld) of B.-P. Lécuyer and the work on the Durkheimians by V. Karady. For a rapid history of the main collective attempt to make the domain emerge see Blanckaert [1993]; for a recent account, see Blanckaert *et al.* [1999].

published in 1996, *A History of Sociological Research Methods in America, 1920-1960*, an example of analysis following the history of research methods thread; her activities of research organization in this domain were equally important.⁶

I here propose to present the elements of an approach to the history of social and behavioral science which is different from the conventional history, broader than that of the history of methods, but in which the latter finds its place. This attempt is not motivated by the controversial ambition to contribute to the discredit of the conventional history, which has solid *raison d'être* – a market for its products at the very least.⁷ Incidentally, some of the questions examined by the conventional history of social and behavioral science certainly deserve to hold our attention – such as the question of the relations between the analyses produced by social sciences and the society in which they were produced. It is essentially a matter of proposing a simple formulation of an interrogation framework which is compatible with at least some of the recent works of the non conventional history of social and behavioral science.⁸ In order to do this I will bring to light some of the inadequacies of the conventional history of social and behavioral science, from the point of view of ordinary criteria of social history appreciation, without coming back on some of the well known critiques of it.⁹ The framework proposed is minimalist: it seeks above all to eliminate implicit hypotheses – omnipresent in the conventional history – on the nature of social science disciplines, on the activities therein and on their products. It is simply a matter of proposing an approach to the history of social and behavioral science which is similar, in its intentions and its reasoning, to the approach towards the history of any other sector of activity – a standard history as it were.¹⁰

⁶ Not all of Jennifer Platt's analyses are concerned with the history of methods, for example those which are about sociologist associations or the contacts between research traditions. None, I believe, are conventional history of sociology.

⁷ Some analyses of the uses of disciplinary history are proposed by Szacki [1981], by the introduction (by the last two authors) of the Graham, Lepenies, Weingart compendium [1983], as well as by Kuklick [1999], who deftly describes some of the uses of the history of anthropology and of sociology in teaching and American reviews.

⁸ This essay presents the framework adopted in my work on the Chicago tradition [Chapoulie 2001a] and extends the analyses outlined in Chapoulie [2001b]. It constitutes a sort of abstract conclusion to corresponding pieces of research, but does not correspond to the point of view which I had when I began this research.

⁹ See Collini's succinct recapitulation [Collini 1988, 389]. We can observe that the arguments in favour of the conventional history are rare: see however Boudon 1992, who relies on the history of sciences as practised by a notable number of science philosophers, but who completely ignores the question – which is central for any historical approach – of documentary sources.

¹⁰ I borrow the term "standard history" from a science historian [Roger 1995], to simply designate a conception analogous to that which is tenable, at least publicly, in a specialized field such as the history of education. I'm obviously not unaware that the historian community is no more than another, united around a unique conception of this discipline – even if we limit ourselves to a national tradition.

Social Science Disciplines as Going Concerns¹¹

No matter what the discipline to which one is attached, anybody carrying out research on the history of social and behavioral science in the Twentieth century studies a universe to which he is particularly close.¹² He has at least an approximative practical knowledge of the norms and mores present in the fraction of the universe to which he belongs. He himself occupies a position in this universe which implies a special type of relationship as regards social science works, careers and institutions.¹³ The analysis instruments which he has at his disposal – categories of perception and analysis of intellectual works, producers' policies, documentary approaches, etc. – are either analogous or, on the contrary, alternative to those which are among the objects on which his analyses focus. In addition, before having gained a reflexive knowledge of these criteria, he finds himself confronted with a plurality of value judgements – on the works, their producers, the institutions, the orientation of disciplines, etc. – according to the criteria of the environment which he is studying, but also according to those of the environment to which he himself belongs.

Such a proximity between the universe studied and that of the researcher is not without advantages – learning about the signification of diverse phenomena for people from the environment in which they take place is rendered much easier. But it also constitutes an obstacle which has been for a long time recognized in the objective work necessary for any historical analysis. To some extent it is from this proximity that the presentism of the conventional history of social and behavioral sciences ensues, often oblivious to itself. It is also from this proximity that the weakness in the control of “facts” ensues, since these “facts” are known on the mode of the evidence within each discipline.¹⁴ Finally, it is undoubtedly from this proximity that ensues the excessive (and sometimes exclusive) accent of the conventional history on determi-

¹¹ This expression is borrowed from Hughes.

¹² This assertion is less exact when it concerns periods where such and such a social science did not undergo academic institutionalism – thus generally the Nineteenth century. A large proportion of the work produced up until now has been concerned with the period of academic institutionalisation, but this will certainly change in the years to come. Moreover, the conserved archive documentation is probably richer for the Twentieth century than it is for the Nineteenth.

¹³ On this point see Bourdieu [1997].

¹⁴ For a typical illustration of this assertion, see Martin Albrow's contribution concerning the sociology of Great Britain since 1945, in the Genov [1989] compendium. All of the contributions in this compendium are based on the typical documentation of analyses which I designated by the term “conventional history:” the works published and a diffuse knowledge of the environment. It is obviously not certain that the authors of this type strive towards a truly historical objective: sometimes they simply seek to offer a sort of guide for a rapid visit of a national sociology or a sector of it. However, these guides are generally subsequently used as historical analyses.

nations by factors internal to the disciplines studied – and thus the prolonged negligence of factors such as the sponsors and “publics” of research.

It is never easy to go from the practical knowledge of a universe to reflexive knowledge which presupposes the clarification of its perception and judgement categories. Furthermore, anyone who occupies a place in a neighbouring universe risks projecting appreciation criteria of what is, in his own universe, a “good” analysis, on the object he is studying. He risks applying these criteria to define the list of works and producers which “deserve” to be taken into account, and to decide on the aspects of them and the approaches which should hold his attention.¹⁵ The understanding of social science works or activities demands on the contrary to take into account not only those which have benefited from recognition, but also the others – including those which may have been refused the label “scientific” or those who may not have sought it; as far as works are concerned, those which remain unfinished or unpublished must also be added to the list subject to investigation, as should the authors who have had wholly or somewhat obscure careers.¹⁶

The omnipresence of controversy in the social sciences – on the acceptable approaches and reasoning, on the very ends of analyses – contributes to make more difficult the clarification of analysis categories. The stances which a present day researcher assumes in these controversies almost always have antecedents in the past, so that the adoption of an implicitly normative point of view is all the more probable since there are affinities between the stances established in the domain studied and in the domain to which the researcher belongs.

The accomplishment of a piece of research on the history of social and behavioral science thus demands throughout the research a sort of division between a relativist attitude necessary to grasp the object studied – to define it, to understand the activities and the works – and the intellectually engaged attitude, without which the rigorous analysis which underlies historical analysis activities, is impossible.

The chronically conflictual nature of social science disciplines also risks being misleading in a completely different way, since the conflicts sometimes conceal to an extent the tacit agreement which unites at a given moment those who belong

¹⁵ Here too I draw on analyses which I developed elsewhere concerning the history of French education, which was for a long time a history written by professors and administrators who belonged to the universe they studied.

¹⁶ I'm obviously not suggesting that we infinitely extend the list of works and authors to be taken into account in all research, merely that we recognize the possible benefits of taking certain authors or works, which are too often excluded as being beyond the scope of the discipline studied, into account. For an illustration, see the richness of a simultaneous analysis of the publications of the sociologist Robert Park and the journalist Hutchins Hapgood, in Lindner [1996].

to the same research field.¹⁷ We can notice that there is – or at the very least that there has for a long time been – a broad agreement within certain social science disciplines (notably among the sociologists), especially among those who feel entitled to speak in the name of their discipline, on the principles of distinction between the different social science disciplines which currently have a label. In the case of French or Anglo-Saxon sociology, these notably include the pre-eminent importance hypothesis with regard to the determination of works, of abstract ideas in relation to elements such as documentary methods and concrete research practices or modes of compilation.¹⁸ The institutional and financial conditions of carrying out research are also considered as secondary in comparison to general ideas on the social world since they are left to the side in analyses.¹⁹

The obstacle linked with familiarity does not only concern the sociologists who write the history of sociology, but in a way which is always specific, all those who contribute to the history of other social science disciplines – and it obviously also concerns, although differently, the historians who study disciplines other than their own – or their own. The same phenomenon can also be found, and for the same reasons, for different fields of historical study: for example the history of religions (or rather some of them), a domain which was for a long time reserved to erudites who belonged therein; the history of education, which was for a long time the domain of teachers and administrators; or the history of sciences. In each of these cases, a first type of history was written by the members of the universe concerned, who were often eminent members of it, which always favoured determinations internal to the domain considered. The categories of analysis were not made explicit and the ends of these histories were presentist. Then, in the history of religions and education there appeared another type of history which focused more on understanding than on the celebration of what is or what ought to be and which falls into what I designated as a standard history. Thus there is nothing truly specific in the problems encountered by the history of social and behavioral science.

A comparative approach is the main instrument to break with an excessive familiarity. Several types of comparison have been used in history of social and behavioral science research, either explicitly or not, to guide or renew its examinations. We can carry out comparisons by retaining either the works or the activities as a core element. In both cases the history of natural sciences offers the resources for

¹⁷ On the tacit agreement in disciplinary fields, see Bourdieu [2001].

¹⁸ This pre-eminence attached to theory can also be found in natural sciences – in this case in relation to laboratory experience – according to Hacking [1983, 149-166].

¹⁹ See Platt [1996] for other examples of what the sociologist community of the mid sixties took more or less for granted.

these constituted examinations, even if up until now the emphasis has been placed much more on the works (or the “knowledge”) than on the activities, except in the research of the last twenty years. The comparison with the natural sciences seems indeed to have inspired, often without its being clearly recognized, history of social and behavioral science research. However, this type of comparison finds its limits in the proximity, claimed by the social sciences – and not ascertained – between the two types of discipline: it takes for granted, or that is likely to be, a characteristic claimed by different social science disciplines: to be sciences like the natural sciences.²⁰ The comparison then can only be enlightening as concerns the effects linked to the demand for the science label.

But above all the history of natural sciences itself remains distant from the standard history by its perspective. It was at a late date – and very partially – that it extricated itself from a presentist view and from a privilege attached to factors of internal evolution: a number of science historians take up a steadfast faith in the “progress of sciences.”²¹ The strong implication of philosophers and scientists or ex-scientists from the concerned disciplines in history of science works, is undoubtedly also one of the reasons for the acceptance of normative points of view.²²

A second usable comparison framework is that offered by the historical analyses of different cultural domains (arts, literature, etc.). The history of social and behavioral science can then fall within the domain (recently constituted and at present loosely organized) of cultural history. This path is certainly promising, but the resources as regards comparison are currently too heterogeneous to be easily mobilized.²³ It leads us to mainly take an interest in the “knowledge” constituted by the social sciences, in its diffusion in different sectors of the societies concerned as well as in the consequences of this, and notably to understand the consequences of the status

²⁰ See Turner and Turner [1990] for an interpretation of the failure of sociology to reach this status. An added difficulty lies in the fact that some disciplines, such as anthropology and history, have not, at least in France, been durably marked by the demand for the status of “science.”

²¹ The first rupture with the presentism prevailing earlier in the history of sciences is not so old since it dates back, let us not forget, to the work of Kuhn published in 1962.

²² See Pestre [1998] for an analysis of the differences in points of view between historians of sciences with a background in philosophy and the point of view designated here by the term standard history. We can observe that relying on a comparison with the natural sciences is all the more difficult for the history of social and behavioral science since the studies on these disciplines themselves constitute a field marked by acute controversies.

²³ Analyses from art or literature historians rarely place themselves in a perspective of comparison with other domains and are, on the contrary, often concerned with the research of specificities of these. Bourdieu’s analyses in field terms, inspired notably by Schücking’s work [Schücking 1965], lie on the contrary, within a comparative framework, as do certain works by anthropology historians.

attached to this knowledge.²⁴ This approach can also inspire analyses of the significations of works in the context of their time – a type of analysis widely practiced in connection with religions and for which Lucien Febvre’s work on the problem of religious unbelief in the Sixteenth century provides a non surpassed model [Febvre 1942].

A third comparison framework already mentioned retains production activities rather than the products of social sciences as its starting point. We then compare the groups constituted by producers of social sciences as categories defined by the social division of work, to other categories of the same type: this is what the analyses of the development of social sciences in terms of professionalization (in the Anglo-Saxon sense of the term) did.²⁵ We can also consider social science researchers as groups engaged in complex political transactions with a body of categories which have at their disposal the resources which allow the acquisition of scientific credit – and consider these groups in their relations with other groups engaged in similar transactions, but to other ends.²⁶

All of these types of comparison are likely to provide the instruments to reach an objectivity of the activities which lead to the production of social sciences. The approach which I will develop here is somewhat different. It offers a comparative framework which is broader than that of the history of sciences and takes activities, and not products themselves (social science works), as its starting point. It considers social science disciplines as going concerns among others and starts off from concrete groups which contribute to the production of works likely to be placed under the social science label.

Firstly, let us note that this approach is not new: it was outlined, almost half a century ago, in some of Everett Hughes’s essays on the history of sociology.²⁷ On

²⁴ For an example of the examinations to which this orientation can lead, see the analysis of the Hawthorne survey by Gillespie [1991].

²⁵ It is striking that this model, which sociologists have repeatedly criticised for its analytical inconveniences in the study of work, and which has lost almost all credit in sociology since the 1970s, was borrowed at the same time by the first historians to take an interest in the history of sociology such as Furner and Haskell. A critique of the presentist orientation of this model is developed in Kuklick [1980].

²⁶ This is the model we can find behind certain analyses of the last twenty years of natural sciences: see Latour and Woolgar [1979]. This does not seem to have had much influence on social science historians; one reason, to me seems obvious: Latour’s approach does not go into the analysis of the specifically intellectual dimension of natural science researchers’ activities. Social science historians who have the knowledge necessary to analyse the intellectual activities of disciplines close to their own, are certainly not as inclined to leave this dimension to the side as researchers of a literary training who are unfamiliar with studying scientific disciplines.

²⁷ See in particular three papers – “Going Concerns: The Study of American Institutions” (1962), “The Improper Study of Man” (1956), “The Dual Mandate of Social Science» (1959) – taken up again in Hughes [1971]. The third article quoted, which focuses on the social sciences, adds to this

occasions Hughes completed this approach by another thread of examination borrowed from his sociology of work: after all, remarked Hughes, the work of social science researchers is a craft like another, with its production techniques, its career problems, its collective demands and its professional ideologies.

What are the advantages of an approach to the social sciences as a going concern? It starts off from concrete production activities, from those who participate in them in one way or another and the institutions which they create or invest – all easily perceptible phenomena; it subjects the investigation to the definition of the labels claimed (“sociology,” “social psychology”) and their application to concrete cases, without prejudging the material, intellectual, symbolic or other foundations of these labels. It thus avoids closing the range of mobilizable comparisons. It also avoids postulating before any investigation the existence of properties specific to them, and taking the constancy in time and space of these definitions for granted. This last point is particularly important, for the conventional history of anthropology and of sociology have not questioned themselves on the profound changes which these disciplines have undergone, at least in countries like the United States and France, over the years.²⁸ These changes can surely not be reduced to a continuous progression, or progression in stages, towards a state which is more conform to the (would-be) canons of the scientific method.²⁹ By adopting a definition which is as weak as possible in substance of the domain studied (the social sciences), we avoid prematurely attaching to it the properties which some of the actors of the domain claim. The specific characters of social science activities and products thus find themselves placed at the centre of investigations.³⁰

This approach leads us to study the system of interaction (to speak like Hughes) in which each discipline finds itself implicated.³¹ This notably includes “rival” enter-

point of view suggestions concerning the effects of the development of professions (in the sense that Hughes uses this term). An application of the notion of going concern to another domain (art) can be found in Becker [1982].

²⁸ Abbott [1999] gives a clear presentation of this type of change as regards American sociology.

²⁹ These profound changes, are, at least in certain cases, linked to the social uses of the disciplines, to their sponsors and to their mode of insertion among university disciplines – principles of variations often neglected by the conventional history. For French sociology since 1945, we can distinguish at least three very different periods: the first corresponds to a research enterprise based at the CNRS and lacking a sponsor, centred around G. Friedmann [Chapoulie 1992]; the second to the period where sociology found a place in the university system and, by some pieces of work like *Les Héritiers* by Bourdieu and Passeron, a wider public [Masson 2001]; the third, to the beginning of the 1970s, a period of broadening in the range of sponsors.

³⁰ Analyses on the upholding of limits in natural science disciplines obviously hold arguments in favour of a perspective which does not remove the question of labels from the investigation: see Gieryn [1983], as well as the analyses assembled in Cozzens and Gieryn [1990].

³¹ Here I take up, in adapting it, the inventory proposed by Hughes [1962].

prises – academic disciplines or not – the products of which are potentially substitutable in such and such a context with those of the discipline considered. In the case of the history of American sociology of the pre-1940s, it is clear for example, that we cannot bypass taking into consideration the relation of this discipline with journalism, social work and the progressive reform movement with which sociology shares a part of the same market, not only as concerns publications but also for forms of expertise.³² It is also appropriate to include in the system of interaction of sociology the body of authorities which are recognized as having a legitimacy as concerns the definition of a sector of social life: in the case of modern day France for example, not only the political parties and organizations, but also certain professional groups, unions and different constituted social movements, the spokespersons of important institutions, etc. The system of interaction of anthropology of the Nineteenth and the beginning of the Twentieth century was certainly more limited, but it included (in France and Great Britain) at least the missionaries and colonial administrators.

The financiers and sponsors of research, as well as those who provide political support to each discipline, are another important element, were it by default, of this system of interaction and here too, the constancy in time and space of the existence and form of this support can not be taken for granted.

Other important elements of this system of interaction: the “publics” of social science products – those who are targeted as well as those who are found; the environments from which the social sciences recruit their researchers, their teachers, and their spokespersons – in short, their personnel.

To understand the activities of a discipline’s members, it is necessary to acquire a sort of ethnographical knowledge of the behavior in this environment, of the action and career logics, of the norms in use in work relations in a given period or domain. At least some of these norms vary with the period, the discipline, and the group of researchers concerned, even for aspects as simple as the deference between successive generations. Here the main documentary resources are provided by personal archives and correspondence and, secondarily, by the accounts of people concerned. It also seems to me that the ethnographical knowledge which each one of us can have acquired (at least towards the end of our careers) should discourage us from considering the subjective dimension of the relation with careers and the academic field as being without great consequence on the works themselves.³³

³² Even as regards a domain as studied as the history of research in urban sociology centred around Park in the 1920-1930 period, no analysis, to my knowledge, has given the necessary attention to urban planners and journalists (like Paul H. Douglass).

³³ See also Bannister’s analyses [Bannister 1987] which notably draw on the rich archives left by the sociologist Luther Bernard.

This approach obviously leads us to become interested in institutional mechanisms (university departments, intellectual and professional associations, committees, reviews, etc.), and not only in intellectual works, which were the main elements taken into account by the conventional history. It gives a place to production techniques, documentary sources and the techniques of their processing, that's to say, to the history of research methods.

It also investigates – and does not take as fact – the elements of the discipline's founding ideologies: their intellectual matrix.³⁴ This can include “theories,” “core variables,” research programmes, documentary sources and methods of collecting and processing them, a “craft,” and possibly the “great forefathers” in which some become embodied. The relations between the diverse elements enumerated deserve to be studied, as do the evolutions in academic legitimacies of these disciplines and in the contribution of these different elements to this. As far as sociology is concerned, it seems that in each case the legitimacy obtained has a precarious and revocable nature – a phenomenon which should be more an object of interest for the discipline's historians than a subject of lamentation, and which we can relate to the proximity of sociologists with their “objects of study.”

We can see that the approach which I have just outlined leads us to treat the definition which each discipline proposes for itself as a folk concept which is potentially variable in time and space, and not to accept it as a natural delimitation etched into the nature of things, as the conventional approach does.³⁵ The approach to the social sciences as going concerns thus bypasses the a priori adherence to representations of itself which any discipline claiming a science status seeks to promote and the ends which they claim. It allows us to ask questions on the relations between the different elements which contribute to the production of social sciences, and notably to the relations between analysis schemes, documentary resources and techniques and compilation styles. It also allows us to study the modes of diffusion of these different elements separately.

³⁴ I employ this term in the sense specified here and not in that which Kuhn gives it in his attempt to precise the notion of paradigm.

³⁵ It is undoubtedly sociology which, due to the mitigated recognition it receives, has been most inclined to attribute to itself a definition in strictly intellectual terms (for which Durkheim often provides the standard formulation; see also the more recent attempts to adopt the “Rational Action Theory” as a “founding expression:” see for example Coleman [1990]; Goldthorpe [2000]). In France, history, which is better established, is satisfied – or rather, has for a long time been satisfied – with a looser definition, where the “craft” has more place than an explicit programme.

The Products of the Social Sciences

It is appropriate to pay particular attention as to how the social sciences as going concerns socially justify their existence: some of their products. In the course of their existence, the social sciences have given birth to institutions (associations, university departments, committees, etc.), to teaching and social expertise activities,³⁶ and finally to “analyses” published in the form of works or papers. Among these analyses, we can distinguish more or less clearly between the publications destined to teaching or to a broad public (manuals, encyclopaedias, etc.) and those which are primarily intended for specialists of the domain or neighbouring domains and which devote their contributions to the “advancement” of knowledge in the discipline considered. This type of product constitutes the main objective of research and the discipline’s *raison d’être*. It is thus appropriate to turn our attention to this element.

Among the obstacles which indigenous representations set against the historical analysis, one of the most problematic concerns the nature of these products. The pregnance of the natural science model in its positivist conception (with different variants symbolized by the names Carnap and Popper) was sufficiently strong on a section of the Western social sciences – particularly sociology – for some of its members to endeavour to define the product of research as “laws” or “theories,” or sometimes even “paradigms” or “models.” These representations reflect particular professional ideologies, but do not allow a proper analysis either of researchers’ activities or the diffusion of “knowledge” in the surrounding societies. The starting point should be the analysis of the social properties of these products.

The products of disciplines like sociology, anthropology, or history are above all texts, which conceal, as do all texts, ambiguities: this is clearly shown by their uses and the endless discussions on their interpretation in which their signatories sometimes take part. We must start off from the acknowledgement that these texts do not have a unique signification which could be formulated in a formal language on which at least the community of discipline members would agree.³⁷ They also have

³⁶ This last type of activity has been little studied up until now: see however Gillespie [1991]; Lagemann [1989].

³⁷ We can also uphold that the natural sciences also produce texts which are not lacking in ambiguity (as is suggested by the reports of science historians seeking to reconstitute physics laboratory experiments from the Eighteenth and Nineteenth century). But some of these disciplines also have, especially for teaching purposes, terms in formalized languages devoid, or almost devoid, of ambiguity – which can thus be understood in the same way by researchers from different national traditions. Pestre [1995, 495] challenges the adequacy of the conception as regards natural sciences as explicitly as I challenge it here as regards social sciences. However it is for natural sciences, it is clear that social science analyses necessarily include, in order to be intelligible to a reader, “translations” of the

particular properties which distinguish them from other types of text. Here I will limit myself to the case of sociology, anthropology and history.

Their texts very generally use notions borrowed from the social world studied, as well as notions belonging to the culture of the social environment to which the researchers belong (or in other words: to the culture of higher education graduates). They also generally include more or less elaborated social science notions which exist for certain domains (for example, for demography, for kinship in anthropology) as well as the notions explicitly defined in the particular analysis considered, which do not belong to the discipline's common language. As we have often remarked, notions of social sciences remain indexed by time and culture – these are “semi proper nouns” to take up J.C. Passeron's expression [Passeron 1991, 60-61] – and few of them, if any, are unanimously considered as generic notions.³⁸ Moreover, this is what explains the impossibility of achieving a univocal translation of social science texts in a corpus of definitions and propositions. The repeated attempts of the logical positivist tradition of quantitative research, so present in the two big American sociology reviews, to render social science analyses in this form, clearly illustrate the impossibility of this type of reduction: the empirical signification of supposed abstract terms is only ever very incompletely defined.³⁹ Furthermore, we can observe that those who consider this representation of their activities ends as adequate, generally simultaneously submit their analyses in the form of a text which mixes ordinary language and (half) elaborated social science notions. This particular case thus provides an additional justification to the emphasis placed here on the character of the *text* of the principal products of social sciences.

The texts produced by social science research have another characteristic property: they relate back to a system of validation elements which involves documentary sources and their scholarly “processing.”⁴⁰ These validation elements are associated with the possibility for the reader to control the validation operations on which the conclusions of the research rely. We can remark that judgement criteria on the

universe studied into a language which is accessible to him. Historians' usual ways of writing show that they are generally aware of this difficulty.

³⁸ See for example Blumer's two classic papers, – “What is Wrong with Social Theory ?” (1954); “Sociological Analysis and the ‘Variable’” (1956) – in Blumer [1969].

³⁹ To my knowledge, there is no analysis of the diffusion of this model which, since the beginning of the forties has sought to formulate earlier analyses (for example those of *Suicide*) in the form of a corpus of propositions which could be subjected to empirical verification or refutation. The first example of this type of formulation can be found in a paper by Merton [1945].

⁴⁰ Here we can take up *mutatis mutandis* the characterization which historians broadly accept of their production: “A narration makes itself out to be historical when it displays the intention to submit itself to a control of its adequacy to the past extra-textual reality with which it deals” [Pomian 1999, 34].

validation elements have been extremely variable in time and space, as have the norms which decide on the acceptability of documentary sources and the practises of processing information: it is a field which up until now has been almost wholly neglected, but which is open to historical investigation.⁴¹

We can obviously apply interrogations which have been applied to other types of text, to social science texts, with the appropriate adaptations: the argumentation mode and the rhetoric, the mode of reference to the documentation put forward, but also the possible relations between different texts, constitute as many investigation themes which cannot be neglected. In spite of the attention given – at least in an essay by Geertz [1973] – to the form of social science analyses, this interrogation guide seems to have been little used up until now: it is only quite recently that anthropologists' field notes, this preliminary element of their analyses, have been the subject of examination [Sanjek 1990]. The rare considerations given to the rhetoric used in sociological research reports are still all, or almost all, practitioners' reflections, and do not fall within the scope of an interrogation on the history of the discipline.⁴²

We can see that the approach to the history of disciplines which I have just proposed allows us to considerably extend the list of elements which are likely to contribute to the determining of social science analyses by comparison with the list of determinants taken into account by the conventional history. General systems of ideas on the social world – the main object of the conventional history – only appear in this approach as an element among others: research approaches, the mode of argumentation and the forms of rhetoric in use in a given society are other potentially significant elements. The historical analysis of social science works, leads us thus – just like the analysis of other products of human activity – to take heterogeneous elements into account – the documentary sources, the way of processing documentation, the rhetoric, the categories of analyses and questioning, the social contexts in which the research is carried out including the conditions of financing, the biographical experience of producers, the contexts of publication and reception. The way in which these elements form their effects is one of the objects of the history of social and behavioral sciences.

⁴¹ It is perhaps not pointless to observe that history, with its archive deposits, has up until now opened up more effective possibilities of control than sociology and anthropology, for which the documentation generally belongs to the researcher himself or to private institutions. It is undoubtedly in sociology and anthropology, upon the repetition of partially similar surveys, that the essential of the validation process has been entrusted. The concern of conservation and of subsequent access to sociological survey information has also appeared very recently in France.

⁴² Brown [1997]; Gusfield [1976]; Becker [1986] appear among the first reflections. The relations with studies of science sociology are obvious in the last two cases. As regards political sciences, see also Bazerman's analysis [Bazerman 1988, 277-288].

An Illustration: The Diffusion of Works and the Relations between Generations of Researchers

As a first illustration of the possible extensions of this approach, I propose to give a rapid presentation of the use which I made in my research on the Chicago tradition of certain distinctions to which this analytical framework leads us to pay attention: the question of relations between works and researchers.

Affirming the “influence” of one researcher (or work) on another has been and remains to be a theme which is omnipresent in historical analyses of social sciences. The notion of “influence” however is not the object of any precise elaboration and the term thus simply serves to qualify properties, without this qualification being backed up by the least justification.⁴³ If we consider the different elements previously distinguished which contribute to the production of social science texts, it is however clear that the borrowing of each of these is subject to specific constraints.

The borrowing of a “new” documentary source depends on its admissibility in the environment considered and the technical competences necessary for its use. In the context in which sociology has developed since 1920, it seems clear that admissibility has generally played a more discriminating role than technical mastery of the use of this source: diverse statistical techniques for example, have undergone a sometimes rapid diffusion without being mastered by their users, as was shown by what was to follow.

The type of writing is another element which can easily be borrowed: whether it is a question of the mode of insertion of “data” in reports, of the reference to earlier works, or of style itself, the diffusion here depends upon the significance attached to each of the elements distinguished in the universe of reception. We can add that these elements sometimes play an important role in the identification of texts as belonging to a social science discipline – or as coming under such and such an analysis current within this discipline. In the case of the Chicago tradition in sociology, I showed that writing properties defined the perception of attachment to the tradition much more than the use of analysis notions itself.

The analysis of the diffusion or of the transmission of general analysis schemes from one generation to another is much more delicate. It can not rely on the analysis of the definition of words or expressions, nor on those of references to supposed founding texts: we know that the “career” of certain notions, which were once emblems of a theoretical orientation, has led to their dissolution: this was the case, for ex-

⁴³ The only elaborations of the idea of influence (to my knowledge) can be found among musicologists, like Charles Rosen and art historians, like Baxandall: see in particular Baxandall [1985, 58-62] – a reference which I borrow from a paper by Jennifer Platt.

ample, for totemism, anomie and social disorganization [Lévi-Strauss 1965; Besnard 1987; Chapoulie 2001a]. In this type of case, it is undoubtedly appropriate not to separate the notions of the documentary system by which they are implemented. In the case of social disorganization, a central notion of American sociology between the 1920s and the 1940s, the force of conviction of this notion rested at once on the existence of a statistic system and on a mode of exploitation and interpretation of its products (cartography, etc.), and on its association with another type of document, autobiographies put together on the initiative of researchers. But the link between these two elements of different natures remained weak, and we cannot wonder at the subsequent abandon of the notion of disorganization. This example is enough to suggest that the diffusion of an analysis scheme can not on principle be separated from that of documentary techniques and sources (just like in natural sciences, there is a dynamic specific to experimental mechanisms, as Hacking suggests). The admissibility of a notion or a set of notions in a discipline also depends on its signification in relation to constituted interrogations of the time, and perhaps in relation to the interests peculiar to the discipline considered. In 1920, social disorganization provided a validation tool for a social – and not biological – interpretation of certain behaviors and was in accordance with a justification of the implication of sociologists in this domain in comparison with social workers, psychiatrists, etc.).

Beside the logics of borrowing the elements previously distinguished, it is necessary to give a place to relations between works or researchers which relate back to oppositions, which I designated as the influence by reaction [Chapoulie 2001a]. Indeed, we can note that a not inconsiderable number of social science works are focused on the refutation of an analysis scheme which was in vogue while they were being carried out – or which was previously in vogue. So it goes, for example, for urban sociology monographs of the 1930s which contain repeated challenges as to the existence of biological determinations of behavior. This type of relation is at times somewhat concealed, including for the author himself, and one cannot restrict oneself to markers which are explicit references. Its omnipresence is favoured by the norm of innovation unanimously accepted in social sciences for the appreciation of analyses: challenging a section of predecessors' analyses – notably of researchers from the previous generation – is one of the simplest ways to satisfy this norm.

Conclusion

I have only sought here to propose a formulation – among others which are equally useful – of an analytical framework for the history of social and behavioral

science. Numerous pieces of research from the last twenty years depart from the conventional history framework, but none has developed otherwise than by giving a critique of it. We can observe however that the history of social and behavioral science has begun over the last few years to take a form akin to those of the history of other sectors of social life. Indeed we find herein a mosaic of studies adopting varied cut-outs, as concerns chronology as well as the extension of retained objects: researchers' individual biographies; the history of enterprises of a collective nature (like the Hawthorne surveys); history of institutions (of departments, reviews, committees, foundations); history of relations between national traditions, of the diffusion of analysis schemes or the reactions provoked by it; history of methods or research techniques.⁴⁴ The disciplinary framework is not often retained, and rightly: the contacts, borrowing, etc. between different disciplines (not always the same in all periods or for all domains) have been numerous in all the periods, and sponsors have very rarely been concerned about the specificity of each discipline, in France as well as in the United States. The history of social and behavioral science thus appears to be a framework preferable to narrower frameworks (the history of sociology, etc.) on the condition however that we do not prematurely settle a list of disciplines or concerned university specialities (statistics must sometimes not be excluded, to take but an example). Again one should not lock oneself into this type of framework, forgetting the close links which unite, at least in the case of the United States and Western Europe, the history of social sciences with the history of the State and that of culture and education. The variety of cut-outs and the use of the same sources to different ends by diverse researchers constitute an essential part of the collective control on these analyses, as in other domains like the history of education or the history of work.

I have not gone into the question of the possible uses of this history: the conventional history, an ordinary teaching topic as concerns sociology and anthropology, has constituted an instrument for the upkeep of a common culture between the sub-specialities of these disciplines which are not closely related. It is also a common secondary element of acquisition strategies of a scientific credit by individual researchers or groups of researchers. In the conditions which currently prevail in these disciplines, these uses will not disappear, nor by consequence will this type of history, in spite of its intellectual limits.

The standard history of social and behavioral science certainly can not fulfil these same functions, but it conveys a tendency to extend historical investiga-

⁴⁴ This last domain is that in which it was undoubtedly initially the easiest to become liberated as regards the questioning of the conventional history, and which in consequence has provided some of the first examples of non conventional analysis: see Stocking [1983] on the history of *fieldwork*; Converse [1987] on that of *survey research*; Platt [1996].

tions to an ever widening body of aspects of our societies, which, as such, dispenses with any particular justification, or, to take up one of Stefan Collini's expressions, "part of the historian's function is to help us to escape from, or at least to loosen the hold of those categories of thought we take so much for granted that we become almost unaware of their existence" [Collini 1998, 387]. It is also a potential element to reinforce the social sciences on this form of justification which is the knowledge of the practises by which natural or social phenomena are made into fact.

References

- Abbott, A.
1999 *Department and Discipline*. Chicago: The University of Chicago Press.
- Bannister, R.C.
1987 *Sociology and Scientism. The American Quest for Objectivity (1880-1940)*. Chapel Hill: University of North Carolina Press.
- Baxandall, M.
1985 *Patterns of Intention*. New Haven: Yale University Press.
- Bazerman, C.
1988 *Shaping Written Knowledge. The Genre and Activity of the Experimental Article in Science*. Madison: The University of Wisconsin Press.
- Becker, H.S.
1986 *Writing for Social Scientists*. Chicago: The University of Chicago Press.
1982 *Art Worlds*. Berkeley: University of California Press.
- Besnard, P.
1987 *L'anomie, ses usages et ses fonctions dans la discipline sociologique*. Paris: PUF.
- Blanckaert, C.
1993 "La société française pour l'histoire des sciences de l'homme." *Genèses* 10: 124-135.
- Blanckaert, C., Blondiaux, L., Loty, L., Renéville, M., and Richard, N. (eds.)
1999 *L'histoire des sciences de l'homme. Trajectoires, enjeux et questions vives*. Paris: L'Harmattan.
- Bloor, D.
1976 *Knowledge and Social Imagery*. London: Routledge and Kegan Paul.
- Blumer, H.
1969 *Symbolic Interactionism. Perspective and Method*. Englewood Cliffs: Prentice Hall.
- Boudon, R.
1992 "Comment écrire l'histoire des sciences sociales." *Communications* 54: 299-317.

Bourdieu, P.

1975 "La spécificité du champ scientifique et les conditions sociales du progrès de la raison." *Sociologie et Sociétés* 7: 91-117.

1997 *Méditations pascaliennes*. Paris: Éditions du Seuil.

2001 *Science de la science et réflexivité*. Paris: Raisons d'agir.

Brown, R.H.

1977 *A Poetic for Sociology: Toward a Logic of Discovery for the Human Sciences*. Cambridge: Cambridge University Press.

Bulmer, M.

1984 *The Chicago School of Sociology. Institutionalization, Diversity, and the Rise of Sociological Research*. Chicago: University of Chicago Press.

Carey, J.T.

1975 *Sociology and Public Affairs. The Chicago School*. Beverly Hills: Sage Publications.

Chapoulie, J.-M.

1984 "E.C. Hughes et le développement du travail de terrain en France." *Revue française de sociologie* 25: 582-608; english transl. "Everett C. Hughes and the Development of Fieldwork in Sociology." *Urban Life* 15: 259-298.

1991 "La seconde fondation de la sociologie française, les Etats-Unis et la classe ouvrière." *Revue française de sociologie* 32: 321-364.

1995 "Critical Remarks about Recent Research on the History of the Social Sciences." Paper presented at the Network for the History of Empirical Research in the Social Sciences, Galway, Ireland, May 19-21, 1995.

1996 "E.C. Hughes and the Chicago Tradition." *Sociological Theory* 14: 3-29.

1998 "Seventy Years of Fieldwork in Sociology: From Nels Anderson's *The Hobo* (1993) to Elijah Anderson's *Streetwise* (1991)." Pp. 105-127 in *The Tradition of the Chicago School of Sociology*, edited by L. Tomasi. Aldershot: Ashgate.

2001a *La tradition sociologique de Chicago 1892-1961*. Paris: Éditions du Seuil.

2001b "Comment écrire l'histoire de la sociologie: l'exemple d'un classique ignoré, *Le Paysan Polonais en Europe et en Amérique*, et l'histoire de la sociologie." *Revue d'histoire des sciences humaines* 5: 143-169.

Coleman, J.S.

1990 *Foundations of Social Theory*. Cambridge, MA: Harvard University Press.

Collini, S.

1988 "Reflexions on the historiography of the social sciences in Britain and France." *Revue de Synthèse* 3-4: 387-399.

Converse, J.M.

1987 *Survey Research in the United States. Roots and Emergence 1890-1960*. Berkeley: University of California Press.

Coser, L.

1971 *Masters of Sociological Thought*. New York: Harcourt Brace.

Cozzens, S.E., and Gieryn, T.F. (eds.)

1990 *Theories of Science in Society*. Indianapolis: Indiana University Press.

- Desrosières, A.
1993 *La politique des grands nombres*. Paris: La découverte.
- Febvre, L.
1968 [1942] *Le problème de l'incroyance au 16e siècle. La religion de Rabelais*. Paris: Albin Michel.
- Fisher, D.
1993 *Fundamental Development of the Social Sciences*. Ann Arbor: University of Michigan Press.
- Furner, M.
1975 *Advocacy and Objectivity. A Crisis in the Professionalisation of American Social Science (1865-1905)*. Lexington: University of Kentucky Press.
- Geertz, C.
1973 "Thick Description: Toward an Interpretative Theory of Culture." Pp. 3-30 in *The Interpretation of Cultures*. New York: Basic Books.
- Genov, N. (ed.)
1989 *National Traditions in Sociology*. London: Sage.
- Gieryn, T.F.
1983 "Boundary-Work and the Demarcation of Science from Non-Science: Strains and Interests in Professional Ideologies of Scientists." *American Sociological Review* 48: 781-795.
- Gillespie, R.
1991 *Manufacturing Knowledge. A History of the Hawthorne Experiments*. Cambridge: Cambridge University Press.
- Goldthorpe, J.H.
2000 *On Sociology. Numbers, Narratives, and the Integration of Research and Theory*. Oxford: Oxford University Press.
- Graham, L., Lepenies, W., and Weingart, P. (eds.)
1983 *Functions and Uses of Disciplinary Histories*. Dordrecht: D. Reidel Pub. Comp.
- Gusfield, J.R.
1976 "The Literary Rhetoric of Science: Comedy and Pathos in Drinking Driver Research." *American Sociological Review* 41: 16-34.
- Hacking, I.
1983 *Representing and Intervening*. Cambridge: Cambridge University Press.
- Haskell, T.L.
1977 *The Emergence of Professional Social Science*. Urbana: University of Illinois Press.
- Hughes, E.C.
1959 "The Dual Mandate of Social science: Remarks on the Academic Division of Labor." *Canadian Journal of Economics and Political Science* 25: 401-410.
1962 "Going Concerns: The Study of American Institutions." Paper presented at the Southwestern Sociological Society, now pp. 52-64 in *The Sociological Eye. Selected Papers*, Chicago, Aldine.
1971 *The Sociological Eye. Selected Papers*. Chicago: Aldine.

Jones, R.A.

1983 "The New History of Sociology." *Annual Review of Sociology* 9: 447-469.

Kuklick, H.

1980 "Boundary maintenance in American Sociology: Limitations of Academic 'Professionalization.'" *Journal of the History of the Behavioral Science* 16: 201-219.

1999 "Assessing Research in the History of Sociology and Anthropology." *Journal of the History of the Behavioral Sciences* 53: 227-237.

Kuhn, T.S.

1962 *The Structure of Scientific Revolutions*. Chicago: The University of Chicago Press.

Latour, B., and Woolgar, S.

1979 *Laboratory Life. The Construction of Scientific Facts*. Beverly Hill: Sage.

Lagemann, E.C.

1989 *The Politics of Knowledge. The Carnegie Corporation, Philanthropy, and Public Policy*. Middletown: Wesleyan University Press.

Lazarsfeld, P.F.

1961 "Notes on the History of Quantification in Sociology." *Isis* 52: 277-333.

Lévi-Strauss, C.

1965 *Le totémisme aujourd'hui*. Paris: PUF.

Lindner, R.

1996 *The Reportage of Urban Culture. Robert Park and The Chicago School*. New York: Cambridge University Press.

Matthews, F.H.

1977 *Quest for an American Sociology: Robert E. Park and the Chicago School*. Montréal: McGill-Queen's University Press.

Masson, P.

2001 "La fabrication des Héritiers." *Revue française de sociologie* 42: 477-507.

Mazon, B.

1988 *Aux origines des Hautes Études en Sciences Sociales: le rôle du mécénat américain 1920-1960*. Paris: Cerf.

Merton, R.K.

1945 "Sociological Theory." *American Journal of Sociology* 50: 462-473.

1967 "On the History and Systematics of Sociological Theory». Pp. 1-37 in *On Theoretical Sociology*. New York: Free Press.

Momigliano, A.

1983 *Problèmes d'historiographie ancienne et moderne*. Paris: Gallimard.

Obershall, A.

1965 *Empirical Social Research in Germany, 1848-1914*. Paris: Mouton.

Passeron, J.-C.

1991 *Le raisonnement sociologique*. Paris: Nathan.

Pestre, D.

1995 "Pour une histoire et culturelle des sciences." *Annales* 50: 487-522.

- 1998 "Sciences des philosophes, sciences des historiens." *Le Débat* 102: 99-106.
- Pickering, A.
1992 "From Science as Knowledge to Science as Practice." Pp. 1-26 in *Science as Practice and Culture*, edited by A. Pickering. Chicago: University of Chicago Press.
- Platt, J.
1976 *Realities of Social Research: an Empirical Study of British Sociologists*. London: Chatto and Windus.
1983 "The Development of the 'Participant Observation' Method in Sociology: Origin Myth and History." *Journal of the History of the Behavioral Sciences* 19: 379-383.
1986 "Stouffer and Lazarsfeld: Patterns of Influence." In *Knowledge and Society*, edited by H. Kuklick and E. Long. Vol 6.
1994 "The Chicago School and firsthand data." *History of the Human Sciences* 7: 57-80.
1996 *A History of Sociological Research Methods in America, 1920-1960*. Cambridge: Cambridge University Press.
2001 "The History of the Interview." Pp. 33-54 in *Handbook of Interview Research*, edited by J.-F. Gubrium and J. A. Holstein. Thousand Oaks: Sage.
- Pomian, K.
1999 *Sur l'histoire*. Paris: Gallimard.
- Roger, J.
1995 *Pour une histoire des sciences à part entière*. Paris: Albin Michel.
- Sanjek, R.
1990 *Fieldnotes. The Making of Anthropology*. Ithaca: Cornell University Press.
- Schücking, L.L.
1966 [1931] *The Sociology of Literary Taste*. London: Routledge.
- Stocking, G.W., Jr.
1965 "On the Limits of 'Presentism' and 'Historicism' in the Historiography of the Behavioral Sciences." *Journal of the History of the Behavioral Sciences* 1: 211-218.
1983 "The Ethnographer's Magic. Fieldwork in British Anthropology from Tylor to Malinowski." Pp. 70-121, in *Observers Observed*, edited by G.W. Stocking. Madison: The University of Wisconsin Press.
- Szacki, J.
1981 "Reflexions sur l'histoire de la sociologie." *Revue internationale des sciences sociales* 33: 270-281.
- Turner, S.P., and Turner, J.H.
1990 *The Impossible Science. An Institutional Analysis of American Sociology*. London: Sage.

A Framework for the History of Social and Behavioral Sciences

Abstract: Critical against conventional disciplinary history of social science, this essay presents an analytical framework stemming from historical research of the author on Chicago sociology and french sociology after 1945. The proposed point of departure for research in the history of social sciences is similar to the basic perspective of history (as proposed by Lucien Febvre) or sociology of work in Everett Hughes' style: social sciences are to be considered as social practices whose primary ends are the production of texts, with historical and thus changing properties. Investigations must look at every categories concerned directly or indirectly (or even in abstentia) with the production of social sciences: researcher, concurrent researcher of other specialties or disciplines, scholarly and learned institutions, those who finance research, general audiences, etc.). Heterogeneous elements must be taken into account: the documentary sources, the way of processing documentation, the rhetoric, the categories of analyses and questioning, the social contexts in which the research is carried out, the biographical experience of producers, the contexts of publication and reception. As an illustration of the possible extensions of this approach, a rapid presentation is given of the use made in research on the Chicago tradition of certain distinctions to analyze the diffusion of works and the relations between generations of researchers.

Keywords: history of social sciences, epistemology, methodology, texts of the social sciences.

Jean-Michel Chapoulie, professor emeritus Université de Paris 1 (Panthéon-Sorbonne), has done research on French secondary school teachers (*Les professeurs de l'enseignement secondaire. Un métier de classe moyenne*, Paris, 1987), on American and French sociology (*La tradition sociologique de Chicago, 1892-1961*, Paris, 2001) and on schooling in the Nineteenth and Twentieth century France (forthcoming: *L'école d'Etat conquiert la France*).