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Multiple spectacles: an argument for a reflexive practice of the social sciences

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Between 1941 and 1943 Karl Polanyi, a former journalist and refugee from Austria, was teaching at Bennington College, Vermont and working on a *The Great Transformation,* a book which was to have a major impact both on the development of the European welfare states and on the historical narratives of their emergence. Polanyi's main thesis is well known: it was high time, he argued, to acknowledge the destructive impact on both man and nature of forces that had erupted with the progressive and painful development of free markets since the 18th century. The disentanglement of markets from prior organic social relationships had been especially problematic for the labor force. Polanyi took as a symbolic and actual watershed the English New Poor Law of 1834 which forbade parishes to give relief to able-bodied applicants outside workhouses. With this law, the last tie inherited from the old community maintenance systems was severed. Polanyi argued that Poor Law Reform "abolished the general category of *the poor* [...]. The former poor were now divided into physically helpless paupers whose place was in the workhouse, and independent workers who earned their living by laboring for wages. This created an entirely new category of the poor, the unemployed, who made their appearance on the social scene"[1]. Polyani's statement, and more specifically his use of the term "unemployed" to describe this new category, serves to introduce the main theme of my paper today.

Words and narratives

At first glance, the problem might seem to be merely a question of vocabulary[2]. In the early 19th century, the term "unemployed" merely meant "idle, at leisure". It was almost never used in the sense of "out-of-work worker" in either Britain or the United States. In 1865 Bagehot, an authority on English constitutional law, described the Prince of Wales as "an unemployed youth". Old dictionaries confirm that a new meaning appeared in the 1880s in the form of a collective noun when "the unemployed" began to

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[1] *The Great Transformation : The Political and Economic Origins of Our Times* [1944], Boston : Beacon Press, 1957, p. 224.

[2] I discussed it at length in *Naissance du chômeur, 1880-1910*, Paris : Albin Michel, 1994, ch. 5-7.

make themselves heard, and occasionally rioted. As for the word “unemployment”, it simply did not exist in the English language until it was coined in 1888 by “the inventive Americans” of the Massachusetts Bureau of Labor Statistics, as a London newspaper explained six years later. It took two more decades for “unemployment” to enter into common use, at least in specialized circles. It seems that the terms “unemployed” and “unemployment” only entered the broader public vocabulary in the 1920s or 1930s when unemployment insurance schemes began to operate on a large scale. If these initial observations are granted, then Polanyi’s account involves the description of a period of history with the vocabulary of another, later, period. What difference does it make? I would like to consider this question of words first.

Let’s spend a little more time on the Polanyi case before broadening the issue. Of course, there were plenty of workers with no jobs in England when the 1834 reform was enacted. Nevertheless, contemporaries knew them, not as “the unemployed”, but as “able-bodied paupers”. It can be argued that reformers used a language which was consistent with their laissez-faire doctrine, in other words that their vocabulary was ideological or willfully deceptive. They believed or hoped that the discipline of hunger would drive every potential worker to the labor market. They wanted the laboring population to be forcibly classified into two parts: workers at work and paupers in the Poor House, and they perceived facts accordingly. But were the men to whom their policy denied relief -Polanyi’s unemployed workers- similar to the unemployed of the 1930s and 40s ?

To answer the question, we have to consider the extent to which the description of facts began to change by the end of the century. With the notions of “involuntary unemployment” and “underemployment”, as Beveridge began to put it in the 1900s, the cognitive framework shifted and one crucial point emerged: being out-of-work was not necessarily sinful. This meant that there was a collective phenomenon that could not be explained by individual behavior or by “the character of men” as they used to say then. The labor market was now seen as socially instituted instead of naturally given and, as a consequence, labor markets could be scientifically studied and rationally reformed. Classification of people changed dramatically, and vocabulary with it: the classes which had to be separated in thought and in practice were no longer paupers and workers, but on the one hand those “who will not or can not work steadily or strongly enough to make it possible that they should be employed regularly” as Alfred Marshall used to say[3], that is those who remained at the margin of the labor market, and, on the other hand, those who were actually in it. The bulk of the latter were at work, but some of them could also be out of work sometimes: they were “the genuine unemployed workers”.

With respect to 1834, both knowledge and policy were directed towards a new goal: namely, the transformation of the continuously shifting workers of the first industrial revolution into a stabilized labor force tied to the labor market. Out-of-job workers of the early 19th century could not be seen as “unemployed” because the social conditions for building up the modern wage earner and the modern unemployed worker did not exist and could not even be imagined. Vocabulary changes that took place at the turn of this century opened the way for new institutions, such as unemployment compensation, that created the unemployed workers whom Polanyi knew and imagined as already existing one century before. The rationale for his argument stands, however. Describing the past in anachronistic terms, even if unseen by the writer, allowed him to relate the past to the present in order to make the future he envisioned possible.

The problem of anachronism is familiar to historians. Historical writing cannot but address the past with the questions of the present, and the discipline has long agonized over the implications of historians’ location in time. Should we use old words to tell old stories in their own terms or, on the contrary, our own categories to describe, compare and generalize? Marc Bloch, one of the founders and main exponents of the *Annales* school, raised the issue precisely at the time that Polanyi described out-of-work workers of 1834 as “the unemployed”. Bloch’s

[3] Alfred Marshall to Percy Alden, 28 January 1903, in : *Memorials of Alfred Marshall* [ed. A.C. Pigou], London, 1925, p. 446-447.

response to the problem was cautious and balanced[4]. In substance, he warned historians that anachronism is a deadly error. At the same time, he believed that some well-chosen vocabulary or “nomenclature” should be developed to prevent us from biasing our description of the true reality that lies beyond words, ours as well as those of historical actors. Bloch thus pointed the basic issue of reflexive practice in our disciplines, but remained one step behind making it explicit. His reflections indeed can be used to support two distinct approaches: as a scholar, either one embraces and takes responsibility for the inevitable intrusion of the observer into the process of data production or one works desperately to keep oneself out of the observation situation. If one adopts the second track, the problem is how to render words, tools and methods neutral or how, following the metaphor in my title, to make ones spectacles as transparent as possible. Here historians’ concerns converge with a vast methodological literature in sociology, ethnology, and many other social sciences that have been printed since the 1950s.

There is, in my view, another, more interesting response to the classical problem of objectivity that Bloch raised, an approach which turns an insuperable difficulty into a resource for new discoveries. It rests on what can be described as a reflexive practice of the social sciences. In France, scholars in a number of disciplines are already embracing this alternate approach. From what I have read of some of the other speakers in this seminar, I realize that similar trends are developing in this country. The approach I propose is quite close to what Michael Burawoy describes as “reflexive science” and suggests can coexist with “positive science”[5]. The concept of fluctuating social entities, that Andrew Abbot develops in his article “Transcending General Linear Reality”[6], can be in my view

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[4] See *Apologie pour l’histoire ou le métier d’historien* [written in 1940-1944], Paris : Armand Colin, 1993, pp. 167-178.

[5] Michael Burawoy, “The Extended Case Method”, *Sociological Theory*, vol. 16, n° 1, March 1998, pp. 4-33.

[6] Andrew Abbot, “Transcending General Linear Reality”, *Sociological Theory*, vol. 6, n° 4, Fall 1988, pp. 169-186.

usefully operationalized by the historicization of social description that I propose. But I may be venturing too far and prefer to go on with my much less theorized point of view. What is actually done in empirical work is what our disciplines are about. More than in theory, I am concerned with practice and ways of doing. My paper will therefore focus on existing studies, illustrative of a reflexive social science, eschewing for the time being more general programmatic statements.

Words and figures

I would like to further elaborate the issue of words by telling two stories. Both are taken from recent French research and deal with the use of numbers or statistics to describe the past. Both are then intended for lovers of "hard facts". The first is rather a sad one, the other much less so.

It happens that the French national institute for statistics not only operates as a census bureau, but sometimes ventures into historical research. A few years ago a book was published with the ambition of reconstructing series for the occupied population, worked hours and productivity of labor over two centuries[7]. The authors were smart and highly skilled statisticians who were perfectly aware of the poor quality of 19th-century census data by present standards, owing both to definitional issues and to an inadequate administrative apparatus for data collection. They accordingly used old figures with much caution, arguing that approximation is better than nothing and sufficient for the identification of long-range trends. Interesting new findings resulted from their work[8]. Their study, which twenty years ago would have been unanimously applauded as a breakthrough in economic history, immediately came under the fire of critics. One of them was Alain Desrosières, a statistician

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[7] Olivier Marchand and Claude Thélot, *Deux siècles de travail en France. Population active, structure sociale, durée et productivité du travail*, Paris, INSEE, 1991.

[8] For instance, they found that the peak of agricultural workforce was 1850 (and not later as it was believed so far), and that the female occupation rate peaked just before WWI and only reached that level again recently.

from the same institution who had specialized in the history of statistics[9]. His basic argument was that conventions about what work is had changed too much since 1800 for long-term series to have any relevance. More specifically, the 1896 French census had inaugurated a definition of occupation and the occupied population that precluded any continuity in series. That new definition, which was by that time being adopted by all official statistical bureaus and has not substantially changed since then, defined occupation as gainful work. Older statistics were built on other conventions and probably on a good deal of misunderstanding between statisticians and the people filling in their forms. This was not due to sloppiness or incompetence. Instead, prior the dominance of wage or salaried work, before modern labor contracts or insurance schemes, the notion of “being occupied” or “at work” necessarily remained undefined or rather had too many meanings. It was hard to imagine a notion of occupation that would encompass artizans, farmers’ wives and their children, manufacturing hands and gentlemen living on their income with a light job in national administration. The diversity of forms of employment also explains the failure to formalize the concept of “unemployment. Similar puzzles are being faced today by statisticians and discussed by sociologists and anthropologists in many so-called developing countries. They also were noted in the 1890s by Alfred Marshall on the London docks as well as in Sicilian towns: even if forgotten by standard presentations of his economics, the theoretician of market behavior was perfectly aware that labor markets were not automatically settled by some natural propension to optimize, but had to be instituted in many ways. In spite of this awareness, the authors of the book I am now discussing stuck to their point: even if they granted the relevance of Desrosières’ effort to introduce an element of historical complexity into their series, they did not believe that such subtleties would modify their results in any statistically significant degree. The controversy went on for a while then exhausted itself in a dead-end[10].

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[9] See his *La politique des grands nombres. Histoire de la raison statistique*, Paris : La Découverte, 1993.

[10] See the symposium “Histoire et statistique”, *Genèses*, n° 9, octobre 1992.

A number of lessons can be drawn from this episode. It took more than one century for official statistics to become institutionally strong, essential to policy-making and scientifically unquestionable, at least beyond the limits of professional technicalities. If the black box is now closed, it has not been always the case: in the second half of the 19th century, official statisticians like Farr, Legoyt or Walker were sometimes explicit about the conventional nature of their classifications. Much more so was the generation which invented mathematical statistics in turn-of-the-century Britain and soon entered the census office, Yule, Pearson and particularly Bowley. However, for reasons that are beyond the scope of this paper, at some point in the 20th century, statistical conventions became so strong that they were transformed into facts. Professional statisticians ceased to question the premises underlying their classifications and numbers. This consensus was a necessary condition for the quantitative turn of the social sciences in the 1950s. Both beliefs and tools were available for nurturing the boom of notably quantitative economic and social history. For a long time the only methodological debate that remained open concerned the choice between quantitative and qualitative techniques, and each side pursued its path, undisturbed by the other. In Europe, the worm entered the fruit when in the 1980s some historians of statistics began to move away from a Popperian celebration of progress and to document the controversies, discontinuities, and social and institutional processes that had displaced boundaries between error and truth in matter of figures. Scholars like MacKenzie and Szreter in England, Desrosières and Brian in France, and Daston in the United States re-opened the black boxes of the past. At that point, where I think we still are, the relation between those who make statistics and those who tell their history becomes an issue.

My story of the French occupational series suggests that when professional statisticians look backwards with the same tools and categories they use for shaping and describing the present, a number of considerations, including the contemporary division of labor and bases of scientific authority, prevent them from taking historical objections into account. History of statistics is supposed to be a respectable scholarly specialty that ought not to interfere with making statistics. This is the sad side of the story: reflexive scholars sometimes cannot do more than point out big mistakes, over and over again. They have no hope of preventing them from being committed. It may be much, but not enough for helping new knowledge to emerge.

My second story is less pessimistic, as it suggests how a step forward can be made. We just saw that "work" may be not a simple, universal and stable category. The same can be said of the "city". By the late 18th century, two different ways of defining a city were in use simultaneously[11]. One of them is familiar to us: any continuous settlement having more than 2000 inhabitants was sometimes considered a city. That straightforward and quantitative definition was to become official in France after the Revolution[12] and the 1836 census began classifying settlements according to the size of their population. Since then, statisticians and geographers have argued about the right threshold (2000 or more, or less ?), taking for granted the assumption that the urban character of a settlement should be decided quantitatively, by the number of its population. However, to most people in the 18th century, such an idea was not only new, but strange. Another definition prevailed among administrators, geographers and probably folks of the time. One could read in the French Academy dictionary since 1694 (and still in the last edition of 1936, which raises a certain alarm about the institution): "a city is a collection of houses laid out along streets and closed by wall and ditch"[13]. It meant that a city for most people was a place that had been granted political privileges by the king, notwithstanding its size: many booming commercial or manufacturing towns were not cities, and many quiet and sometimes tiny places had a degree of self-government that made them a city. Around 1750, it happened that the discrepancy between economic dynamics and

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[11] See Bernard Lepetit, *Les villes dans la France moderne (1740-1840)*, Paris : Albin Michel, 1988, ch. 1-2.

[12] In 1808, a local tax was created by national law on goods entering "villes et bourgs de 2000 âmes et plus" [cities and towns with 2000 souls or more].

[13] "un assemblage de plusieurs maisons disposées par rues & fermées d'une clôture commune qui est ordinairement de mur & de fossez".

political structure began to be felt by some observers, who can be identified as reformers in the context of the crisis of the Ancien Régime government. Duality of definitions then is a clue both to a crisis in urban representations and to the impending struggle over classification that was to be settled by the abolition of urban privileges and a new mapping of territorial administration in the 1790s.

Coming back to the present, the contemporary analyst of 18th century French cities, who must select units to enter into her computer faces a choice. She can dwell on some quantitative definition, and this is the most common practice, despite the anachronism that it implies. After all, we know the story that followed, we are right in our definitional conventions and the people of the time were mistaken. But there is an alternative approach, which uses both definitions separately and confronts the findings produced by each approach with one another. Two different maps of urban France then appear and new empirical and interpretative issues are raised that would have otherwise been hidden. One of them deals with the spatial structure of the Old Regime administration and the weight it had on both its reform by revolutionary governments and its long-range economic and demographic dynamics. Crucial links between the political and the economic configurations of urban networks can then be identified: new transportation systems of the 18th century (roads and canals) heavily relied on decisions taken by administrative bodies and state engineers, who were inclined to follow the political mapping of the kingdom. In that way, the old definition of cities had lasting physical consequences which made them felt even when urban privileges had been forgotten by everyone.

The case I have just taken suggests how fruitful it can be to raise doubts about our most self-evident analytical or statistical categories when studying the past. Let me stress the degree of complexity of this apparently simple story. Here we have a discrepancy between our present categories and those which were prevailing in the period we study: we have to explicitly identify the definitional gap in order to use it to describe the past in a more efficient way. But, in that case, we also observe the emergence of our present point of view among people of the past themselves, hence tensions within past representations: we can also play those internal discrepancies in categorization to make sense of what were the stakes of conflicts over definitions and words. Bernard Lepetit, the urban historian from whom I borrow the example, insisted that “no society is entirely blind to itself, nor entirely aware either [...] shifts in vocabulary, contrasts or clashes between distinct taxonomies reveal levels and forms of urbanization as well as statistical indicators do. This is to say that we must look at old images of cities and our own categorizations as equally as we can.”[14] In fewer words: “It is from playing with gaps that we can expect some light”[15]. “Positive” and even quantitative urban history can be methodologically interwoven with a history of cognitive structures of which we are part equally to the people we observe. This is the nice news that the above case carries with it: paying attention to the history of cognitive forms may be useful to write history. History writing indeed is a relational job.

Relational situations

The complexity of the relationship between the observer and what or whom she observes takes on an additional aspect when it comes to inquiring into the present through direct actual interaction. It is the situation that sociologists or anthropologists who work “in the field” know well. Here again, I would like to elaborate from an example. In 1988 the new democratic constitution of Brazil decided that “remains of *quilombos*” had special rights on the land they occupy and work. That legal enactment enlarged to the descendants of African slaves who by thousands had escaped from plantations and settled in the bush in the 17th or 18th centuries the rights that had already beeen granted to “*indígenas*”, or native communities. Such decisions resulted from a complex context which involved the long history of protection of “*indios*” by the Brazilian state, the work of state agencies who, since the 1910s, had been in charge of it and became the cradle of Brazilian anthropology, not to mention the use of Mexican precedents which had found part of their scientific legitimacy in Boas’ work at Columbia University. As you can see, the frame and starting point

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[14] Lepetit, *Les villes...*, p. 22. My translation.

[15] *Ibid.*, p. 21.

of this story already involves the history of both Americas' cultural anthropology. Let us leave this older set of interactions aside and follow what happened. The Brazilian Association of Anthropology was asked by the government to send scholars all around to survey "remains of *quilombos*" and review the claims that had begun to pour into Brasilia offices. One of those scholars, from whom I borrow this story, worked for his doctoral dissertation in a remote rural area in the northeast where a conflict about land had been going on for almost twenty years[16]. When the anthropologist stepped in, the game was almost over: he met with an organized community of people who in a very articulate and definite way identified themselves as descendants of African slaves and who consequently claimed to be entitled to a fairly large piece of property owned by their landlord and employer. An until then unnoticed ethnic community was thus discovered on the Brazilian countryside.

The case only makes obvious the deal on which any observation situation is based: if the scientist acts as a predator who collects "facts" and has authority for transmuting them into certified knowledge, local folks also know of themselves, pursue goals and try to make use of the visitor to fulfil them. What does it imply about the objectivity of social science ? In this case, positive-minded scientists would ask whether the farm workers of Porto da Folha were actually ethnic African Brazilians or not. They would take methodic steps to check facts and uncover true reality that lies underneath the possibly deceptive behavior of the people observed. On the other hand, some radical critical anthropologists would conclude that, since the implication of the observer in the observation situation cannot be eliminated, all claims to objective knowledge are incurably flawed. It is well known how this latter stand has been the subject of extensive controversy, initially in California and subsequently throughout the international field of anthropology, leading in some quarters of the discipline

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[16] See José Maurício A. Arruti, "Subversões classificatorias", paper given at the conference *Ciência, Natureza et Sociedade*, Rio de Janeiro , September 8-10, 1997 (Museu Nacional, Universidade Federal do Rio de Janeiro and Ecole normale supérieure de Paris). Forthcoming in *Genèses*, n° 32, septembre 1998.

to a kind of collective suicide. The field study I am now reporting shows how a reflexive practice of anthropology both provides an alternative to the choice between positivism and relativism, and eventually new findings.

The anthropologist in Porto de Folha did not try to put aside the situation his visit had created, but rather tried to take it as a part of what he had to describe and to make sense of. That led him to work on the related histories of the question he was supposed to answer and of the local community amongst whom he was working. He discovered that standard tools and categories of his own discipline were implied on both sides: at the top, they had supplied the language which shaped the law, and consequently the terms of the question he was commissioned to answer; at the bottom, through the medium of law, church, union, and the black movement, anthropological provided new resources for a group to take shape and define its identity. The inquiry then took a new focus: it dealt with the practical and symbolic interaction between the locals and their larger environment. It appeared that authorities of various kinds –religious, administrative, militant– over a long period of time, had used a large and discontinuous array of classificatory categories to describe, rule or organize the group. Meanwhile the group had not remain passive, but rather had reshaped its identity a number of times according to the situation. They were *trabalhadores rurais* (farm workers) or *camponeses* (peasants) when farm-worker unions flourished in the 1940s and 50s before they were smashed by the local oligarchy. Later, the people or group in this story had gone through two phases since the open conflict with the landlord burst out in 1984. A neighboring village of sharecroppers for the same landlord discovered, with the help of the local Roman Catholic minister and Liberation Theology militants from the state capital, that they were Indians of the Xocó tribe and began to be granted land by courts on that ground. The group we are talking about then revived its thick kinship and ritual ties with their neighbors, started to set up one common organization and was on the verge of merging with them. Circumstances however prevented them to become *indios*, when rumor came from Brasilia that "remains of *quilombos*" had rights. There had been ground for claiming to be Indians, there was ground as well for claiming to be blacks: just another set of features of the community was to be activated. The *quilombo* community of Porto da Folha achieved official recognition last year. Let us hope that now those newborn African Brazilians have won their case against their absentee and exploiting landlord.

I shall use the broader implications of the *quilombo* case as a starting point for suggesting how reflexive practice is a common ground for the social sciences, enlarging the argument in three steps —from disciplines based on field methods, to quantitative surveys, and finally to research based on archives.

Common ground for the social sciences

There is nothing shattering in the story I just told. The situational nature and the plasticity of identities were already shown by sociologists like Howard Becker or Irving Goffman in the 60s and have been the basic assumption of ethnomethodology since then. But the consequences, I think, have recently been pushed much further by anthropologists for reasons quite specific to their discipline, within which an international trend has developed that made our *quilombo* study possible[17]. By now, the most established and self-evident categories like "ethnicity" and "race" or "tribe" and "cast" have fallen apart, or rather changed in meaning: they appear as social and cognitive forms that can be used, along with a series of other resources, by people for shaping their identities in specific contexts. Moreover, processes of this kind are never purely local nor entirely fluid: they involve long periods of time, the society at large, and particularly its institutions. Here anthropologists converge with historians who work on the making of class or nation, for instance. What I would like to underline is not those important discoveries, but the processes which lead to them. It began with questioning the role of the anthropologist in the most symbolic situation of the discipline —fieldwork. Observing observation and taking it as a social relationship has been the starting point of a revolution in anthropological practice, themes and theory.

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[17] For recent theorization, see Jean-Loup Amselle, "L'ethnicité comme volonté et comme représentation : à propos des peuples du Wasolon", *Annales ESC*, vol. 42, n° 2, mars-avril 1987, pp. 465-489, and Benedict Anderson, *Imagined Communities : Reflections on the Origin and Spread of Nationalism*, London : Verso, 1991.

It goes much beyond the necessary criticism of colonial anthropology, much beyond epistemology quietly written at your desk, or the classical book about the book, the anthropologist's meditation following the monograph. Reflexive anthropology actually leads to a distinct monograph. It is not a gesture but changes methods of observation and redefines what is to be observed in the first place. If communities have no fixed boundaries, nor cultures fixed features, standard approaches will no longer do. In other words, questioning the observation situation has been the intellectual pre-requisite and the first practical step for placing larger social relationships at the very heart of traditional fieldwork and local monograph.

My second point takes us away from anthropology as a discipline. What is made apparent by the reflexive work of ethnographers or field sociologists can help dig out buried aspects of other social sciences which are based on quite different methods. Since Paul Lazarsfeld and Columbia sociology formalized quantification in the discipline, the dualism of quantitative and qualitative methods in sociology has largely been taken for granted. This implies forgetting that, at the very start of any figure processing, there are situations of interaction, notably the filling of questionnaires. There would be no sample surveys, opinion pools, or population censuses without someone asking someone something. Such an obvious fact is hidden by the division of labor between those who actually ask questions and those who define them and interpret answers. Rank and file surveyors are people to be briefed and tightly controlled. But it would be worthwhile to listen to them carefully or to observe them in actual working situations. They are perfectly aware that questions do not mean the same to everyone, and responses are given in many other ways than answering them. Scholars who try to take the survey situation into account as a social relationship show what difference reflexive practice of the social sciences can make to methodology.

I have only mentioned so far disciplines which are based on actual interaction of various kinds. They are not so distinct from those which find their data in sleeping archives. Analogies between these types of practice in the social sciences deserve to be explored, and that is my third point. It is intended to show that reflexive practice is a common ground for debate and useful developments in history as well as in anthropology and sociology. This is not wishful thinking as there is evidence of processes endogenous to each discipline with strikingly convergent features. Scholars in all three disciplines have increasingly called into question earlier beliefs concerning the elimination of biases and the very nature of data. Facts are now more clearly seen, not as things waiting to be collected, but as socially constructed through relationships involving three agencies: the scholar, the people she studies and their own social environment. This triad can also be described as including: the observer, the source, and its target. The only difference between studying the present and studying the past lies in the fact that, in the former case, the target and the observer may coincide. But, in other respects, I would argue, the process of data construction is similar.

First, let us discuss the source to target relationship. Scientific history was born at the end of the 19th century with the setting up of an elaborate theory and practice for criticizing sources. One question was basic: what does documentary evidence reveal of the past? Are sources right or wrong, faithful to reality or not, and in what measure? Accordingly, the historian's skill was to eliminate biases from her sources. Reflexive historical scholarship today takes those techniques as its heritage, but looks at sources in a slightly different way that changes everything. To put it in a more abstract form, the relationship between representation and reality is no longer what it used to be. Historical evidence is always representation, whether statistics collected by bureaucracies, images embodied in maps or paintings, or arguments in a court of law: facts and events could not take place nor reach us outside of language. Classical historical critics would carefully inspect the conditions from which those representations resulted and separate out the distortions (by error or intention), dwelling only on those aspects of past reality that had been confirmed as genuine. The reflexive historical critic is more generous and keeps everything. What we now classify as error is always revealing and could have been an active agency in the historical process: representations are real, because they once shaped and today unveil the actual experience of those who made history. Hence scholars now pay closer attention to language, perception schemata, cognitive forms, or "mental tools" as the pioneers of historical anthropology used to put it. Furthermore, what was formerly described as resulting from intention and accordingly discarded as mistaken is now used as evidence of its own. What has become archives used to be much more than that: our documents were part of actual social relations between those who produced them (and later those who did not destroy them) and their contemporaries. Identifying the specific context which gave birth and meaning to our sources, the means and scope of their diffusion and impact, the controversies that surrounded them, the targets they aimed at, are all valuable historical material instead of preliminaries to the inquiry. What happened recently in the northeast of Brazil when the anthropologist visited Porto da Folha, happened everywhere everyday in the past and left behind historian's evidence.

Now, the source to observer relation, or rather, in order to encompass more cases, the observer to source relation. Obviously whatever historians do or say, they change nothing to what happened in the past, whereas social scientists always have some impact on the present state of things: it can be slight, local and fleeting, in relation only with the observation situation, it can be stronger and more enduring through the diffusion of the languages or expertise our disciplines offer to society for making sense of itself, but in any case interference can be presumed and should be analyzed. The difference blurs away though if we consider that there is no past for us, and in that limited sense no past at all until it is made present by those who tell it. What the sociologist does by interfering in the social process, the historian does by merely reading her sources. In both cases there is a bias, and in both cases reflexive practice does not try to put it aside. Instead, it considers biases as resulting from social interaction and their study as a privileged way to knowledge. The difference between students of the past and students of the present gets even thinner if we take into account the social distance between anthropologists or sociologists and the people whom they study. Socially and relationally, they do not have the same position, nor do they have the same reading of the relation. It is the reason why ethnocentrism in its many guises is to anthropology and sociology the exact equivalent of anachronism to history writing. Neither can be suppressed, whatever good intentions or right methodology are mobilized: the painter definitely is in the picture. But both can be made explicit, played with and used as tools: the eye-glasses through which the source people saw or see themselves and their social environment are to be found and used as well as the glasses the scholar was provided by her own time and place. Collecting glasses is not an easy business, but quite a profitable one.

Comparison

That leads us at last to the theme of comparison, which in fact we have stayed with from the beginning. Comparing is usually thought of as considering a number of separate things and talking about their similarities and differences. Paradigms for comparative methods in the social sciences have been defined by linguistics and anthropology, following patterns set by mineralogy cabinets and natural history, and subsequently by ethnography museums. Travelers would methodically collect objects alien to one another and bring them together on the scholars' table. Languages, myths, kinship systems or political institutions in the Trobriand Islands, the Highlands of Burma or the Brazilian jungle of Mato Grosso obviously have no other relation to one another other than their juxtaposition by the same community of Western scholars. Nothing, at least until those societies were transformed by the intrusion of observers and their companion missionaries, military personnel and merchants. That is, for quite short a time. Comparison has also been done by scholars who look over the boarder to other modern nations. They usually compare what they know at home to what can be seen abroad, or possibly to comparable things in two foreign countries. Experience shows that what is often interesting is less the formalized results of comparison, than the peculiar national viewpoint from which it is made. Comparative studies illuminate aspects of what is observed that would never have occurred to the locals, that is to say they teach much about the observer.

Comparison so described falls into the same category as fieldwork or historical writing –it is cultural interaction. It is a matter of spectacles again, and again nothing is lost by taking a relational viewpoint on it. Time is getting short and I cannot elaborate at length. Let me just mention two points.

The first one is so basic an aspect of interaction in comparison that it often remains unnoticed. Comparing implies using words, and words belong to one language which carries with it national idiosyncrasies and scientific traditions. As a consequence, comparative programs generally cut into one society with knives brought from another one. The French are interested in "*intellectuels*" in 19th-century France and Germany, and Germans in "*Bildung*". Americans seek "professionals" in France and the French "*cadres*" in America. There is a good and a bad side to the game. In the worst cases, each side finds what it was prepared to find without noticing that using its own categories has implications on what can be found. More often fortunately, one discovers not only that things are quite different over there, but also that people think differently. The risk of objectifying categories and stereotyping "national" cultures is thus great. Reflexive practice calls on us to shift focus twice: it takes as a part of the comparative inquiry the categories themselves and it historicizes them on both sides. A quick example serves to illustrate the point. That kind of occupation which is now called "professions" in Britain and the United States is similar to what the 19th century French censuses described as "*professions libérales*". For that historical period and that particular group, it is perfectly reasonable to compare census figures across time and space. For later periods however, the same comparison becomes impossible, as the French census takers began to differenciate from "*professions libérales*" those who were employed by government, then those who earned a salary as opposed to those who were self-employed. As a consequence, the French category "*cadre*" has no equivalent in England and the United States, just as there is no French equivalent to the Anglo "profession". French and Anglo categories followed the same path for a period, then diverged. The interesting comparative task then is not to identify some kind of eternal "national difference", but to make sense of the period when things began to diverge from a common starting point. To provide yet another example, one can take the concept of nation. One word in French –"*nationalité*"– can be translated by many different ones in German: *Nation* in the 19th century *Nationalitätenprinzip*, *Staatsangehörigkeit* for the legal meaning, and a series of terms derived from *Volk* when it comes to blood and feelings. This is intriguing indeed and has given birth to many comments on the essence of both cultures and States.

The viewpoint changes when one explores the history of that semantic field in both languages[18]. Two things then appear. First, the stabilization of vocabulary only took place on both sides of the Rhein rather recently, that is in the first decades of the 19th century. Secondly, the diverging solutions resulted from an intense controversy between French and German writers of the time and also, in each country, between those who loved the other one —or, at least the image they had of it— and those who did not. Differences we now take as “national” have not always been so and were constructed in the framework of one common history.

Now, my last point. I think it interesting that some historians who have been discussing comparative methods for years, now recommend shifting from comparison of fixed entities to the study of actual interaction between them[19]. It is a modest and practical proposition, it also is something of a change in paradigm with respect to the linguistic or anthropological tradition. It cannot work in every instance, but the examples I just took are cases in point: French and German elites have always been looking at each other and defining themselves and their own nation’s problems in reference to their neighbors, and those who organized unions for “*cadres*” and theorized that identity in France in the 1920s and 30s constantly referred to the new world power of their century, America. Many familiar topics can be approached in a similar way. In comparative work about, for instance, welfare states, technology, or high culture in various industrialized countries, we can efficiently inquire in the circulation of men, things and models. In a number of cases, the realities we want to compare actually interact,

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[18] See Gérard Noiriel, “Socio-histoire d’un concept : les usages du mot ‘nationalité’ au XIXe siècle”, Genèses, n° 20, septembre 1995. About the similar history of “*Kultur*” and “*civilisation*”, see Norbert Elias, *Über den Prozess der Zivilisation*, vol. 1 [1939], ch. 1.

[19] See for instance Michel Espagne and Michael Werner, “La construction d’une référence allemande en France, 1750-1914. Genèse et histoire culturelle”, *Annales ESC*, vol. 42, n° 4, juillet-août 1987, pp. 969-992, and Michel Espagne, “Sur les limites du comparatisme en histoire culturelle”, *Genèses*, n° 17, septembre 1994.

interacted, or have been through interaction with some common third element. I will stop here, getting back to the domain of classical anthropology, supposedly the less amenable to comparative work interested in interaction. In Eric Wolf's wonderful book *Europe and the People Without History*[20], each of the case studies takes as its starting point the direct or indirect contact of an isolated society with expanding Europe. The ways in which ancient societies reacted to the contact and tried to manage it are indeed powerfully telling about what they were and provide an efficient standpoint for comparing them. The ways in which the intruders observed new worlds also tells much of themselves and, furthermore, helps to understand the status of our archives. That work is a full-scale demonstration of what putting interaction and the observation situation at the center means and gives.

[20] Eric R. Wolf, *Europe and the People Without History*, Berkeley, Ca, University of California Press, 1982.