Draft (September 23, 2006)

University of Michigan
“ Practicing Pierre Bourdieu ” Conference
September 28-30, 2006

“ Science of Science ” and “ Sociology of Scientific Knowledge ” : Making Sense of a Missed Encounter

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Bourdieu and the social studies of science : a missed encounter. The title of this presentation implies two things at least. First, some regret of mine that the encounter did not take place, second the belief that it could have taken place. The reason for this is : in my own work, I find inspiration on both sides and and tools in both boxes.

Writing the history of our disciplines, though, is not quite compatible with regrets and beliefs. My aim in this paper cannot be anything else than trying to make sense–historically and sociologically–of what has happened in that quarter of the social sciences.

I. Bourdieu's model of the city of knowledge

Most of the authors I focus on gathered around a label they used as a flag : " sociology of scientific knowledge ". However, some independant sociologists and many historians came to play a prominent role in the endeavor without enrolling into any " school ". Hence the common use of less denominational terms as " social studies of science " or " science studies " for spotting this new strand of research out of the well-established and broader field of the " history of science " or the now contested " sociology of knowledge ".

Bourdieu's rejection of what he called " the new sociology of science[1] " (2001 : 22) was quite strong. It took two successive forms. First, silence–a strategy for fighting competitors which is pretty common among French sociologists and was consistently used also by Bourdieu. Attention and criticism came much later, with the lectures Bourdieu gave at the Collège de France in 2001 : " Science de la science et reflexivité ". Silence indeed lasted quite a while, if one remember that the Edinburgh group (Barnes, Bloor, MacKenzie, Shapin) multiplied publications along the 1970s and had become quite visible by 1980. They happened to be imported in France by Callon and Latour (of whom this move was the only capital then) in 1982-84. One decade later, some major works (pecularly Shapin & Schaffer 1985, translated in 1993) would be highly praised by French historians of repute, like Chartier who was then an intellectual ally to Bourdieu with an independant mind. However, as it happens often with cultural imports, a local meaning was from the start imposed onto the social studies of science by the importers–two bold outsiders who were determined to bring about some kind of upheaval of the French tradition in the history of science. In 2001, Bourdieu suggested his own reading of the British initiative : at the very same time as himself published his path-breaking article on the " scientific field " (1975, 1976), Bloor, Barnes and Collins would have independently co-initiated the European resistance to Merton's structural-functionalist paradigm in the history of science (2001 : 41).

What were Bourdieu's arguments against the new science studies ? First this " sub-field " shows many features that have not much to do with a well-regulated scientific activity : philosophical debates are paramount (realism vs idealism, dogmatism vs scepticism) and empirical results scarce, emphatic statements on intellectual rupture (new vs old) multiply and very little follows, rivarly and controversy prevail (2001 : 21-24).

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[1] " la nouvelle sociologie de la science " – all translations of Bourdieu's words are mine.

Substantially, now, the new sociology of science has often taken the laboratory as its scale and unit of observation (2001 : 47-66). This led to interesting results, like sheding light on the process by which the confusion of experimentation vanishes under the veil of a public scientific language, thanks to which some measure of agreement is eventually reached and a long series of attempts transformed into a certified experimental fact. This, in a way–Bourdieu comments–is a mere confirmation of Bachelard's view according to which facts are social constructs. But most of laboratory studies show serious shortcomings. They develop too cynical a view on scientific life, as they make believe that scientists constantly develop conscious strategies and stratagems for self-promotion. More deeply, laboratory studies are limited by an interactionist view that prevents them to see both structures (like the unequal distribution of credibility between laboratories) and personal dispositions which make someone a scientist and allow her/him to enter the field. In Bourdieu's opinion, the structures of the field and related habitus regulate the collective production of creed and this makes irrelevant any interpretation in terms of individual or collective cynicism.

Bourdieu's most serious criticism is that a good number of new sociologists of science share or leave the door open to a radically relativist view of science. Bloor's principle of symmetry (1976) is considered as quite ambiguous in that respect, Barnes holds that science is less determined by “ the nature of things ” than by social interests (1974), Collins and Pinch insist on the non-rational methods that are used to bring scientific controversies to a close (Collins 1985). Eventually, Latour and Woolgar–whose book (1979) “ give an enlarged image of all the shortcomings of the new sociology of science[2] ” (2001 : 55)–suggest that facts being artefacts, they are fiction. This “ semiological vision of the world ” turns reality into a text, which leads to the same dead-end the proponents of the linguistic turn are stuck in (2001 : 59).

According to Bourdieu, relativism is an unacceptable answer to an unescapable question, an old and basic puzzle that he claimed he had solved decisively. His writings on science suggest he was haunted by this major problem : once logical norms have been considered as radicaly historical, is it still possible “to save reason” (2001 : 160) ? Since the illusion that nature speaks is dissipated and it is realized that historical conditions determine scientific statements on nature, how to escape from “relativism” ? With his usual desarming modesty or fake shyness (2001 : 108), Bourdieu stated that he had solved this problem that had been embarassing philosophers since Kant's a priori conditions of knowledge began to be historicized. And the answer is : “ Objectivity is the product of intersubjectivity in the scientific field ” (2001 : 163). The author rather provocatively notes that this is rather close to what Popper wrote on the same issue in 1945. In the very peculiar social world scientists built, social constraints took shape historically that force actors into abiding by mechanisms of logical control and universalization. These specific rules of scientific fields give truth a strength that is rooted in nothing else than the mere logic of competition for pre-eminence in science.

The basic historical condition for scientific fields to emerge is independence with respect to worldly powers in society, and the shaping of specific procedures regulating competition

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[2] “ donne une image grossie de tous les travers de la nouvelle sociologie de la science ”

and the decision about what is true or not. Then, at the very heart of Bourdieu's vision of science there is what he calls "autonomy of scientists". There is no science without a set of institutions where selected people (competence is the price to be paid for entry) basically interested in knowledge (and sufficiently disinterested in any other interest) compete for recognition and precedence amongst peers without interference from any secular power (religion, politics, economy, the media, etc.). This historical construct is the necessary and sufficient sociological base for scientific truth to take shape.

Autonomy of scientists, then, is both a descriptive and normative criterium for analyzing and evaluating historical situations, scientists and their output. Let's call this model " the city of knowledge ".

My purpose here is to discuss this vision of science as far as the social sciences are concerned. I would like to confront it to historical situations I selected in order to show how decisive interaction between scientists and the secular world was for some major developments in the 20th c. social sciences. Before I come to the point, I still wish to make two observations on method.

First, I do not discuss theoretical statements in a direct or abstract way. I think with cases instead. I visit historical situations which do not seem to fit in Bourdieu's vision, and I try to explore what interpretative tools appear to be useful for making sense of them.

Second, I do not want to state any opinion of mine on the situations I describe or their consequences : were the changes I observe for the better of the worse, good or bad for science or society, this is a matter of opinion and should be left outside of the picture. The only point I argue here is that there is some measure of consent about these episodes being of some importance in the historical development of the disciplines involved.

2. Testing the city-of-knowledge model in the social sciences

Autonomy of science, in Bourdieu's view, does not mean isolation of science from society. It only implies that scientists usually define what Kuhn calls their puzzles and the ways for solving them in their own terms, and not according to what secular powers wish those terms to be. However, I wish to argue that, in the social sciences most of the times–ever since they have been institutionalized in universities and/or special research institutes–it can be observed that scientists are engaged in two simultaneous conversations. On the one hand, they talk with peers : from this viewpoint, research choices can be described as positions taken with respect to other scientists. Modalities are extremely varied, but, in the main, this aspect of action can be analyzed as the result of situations and strategies scientists develop for power (sometimes mere recognition) through knowledge (and some other resources) among competitor scientists. On the other hand, social scientists usually talk also with society at large, that is with and in specific realms of action, groups of people, kinds of institutions in society. On this, I wish to call attention now.

Let me underscore in the first place that the weight given by any individual or group of scientists in any specific situation to recognition by peers, and to recognition by the secular world respectively cannot be deducted from a model, but only infered from observation. Bourdieu's model which strongly contrasts scholastic and secular rewards in my view implies strong moral and misleading overtones. It cannot be for instance presumed that everyone's dream in French acedemia is or should be to become a professor at the College de France–" a place were heretics are consecrated, away from any mundane power on academic institutions[3] " (2004 : 107). It cannot be presumed either that winning this supposedly supreme recognition precludes anyone from also winning more secular status rewards. Ironically or sadly enough, Bourdieu's successor at the College happens to precisely be of the elite-cherished and media-boosted kind of intellectual he despised most–not to mention the successor not being a former student of the Ecole normale supérieure but of a business school. Let us leave the debate on hierarchies of value aside.

I chose the cases I wish to reflect upon now among situations where the scientists involved were, or are now considered as major characters in the history of their disciplines. The common feature of these cases is that the scientists sought and found support in the world of action and, in close relation with this move, redefined paradigms in their discipline.

When modern social sciences began to be institutionalized in universities at the end of the 19th c., those who referred to those new disciplines found themselves in a position of claiming to be scientists in the same measure and manner than natural scientists. New historians, psychologists, sociologists, etc. worked hard at defining scientific theories and methods, curriculae and degrees that protected their disciplines from the amateurs who until then overwhelmed learned societies. They would struggle indeed for the same measure of legitimacy and autonomy their fellow natural scientists enjoyed. By the same token, social scientists shared in the common conviction of scientists at the time that science was naturally useful. Durkheim, who was a professor of educational science[4] at the Sorbonne, used to say that " We think our research would not be worth consuming one single hour work if it were only for the sake of speculation.[5] " The " purest scholars "[6] were eager to be useful, and professed that science per se was good for society.

It should be stressed that categories like " basic " and " applied " research were not in use and would have had no meaning at the turn of the 20th c. Once academic freedom was (more or less) granted through some form of (usually limited) university self-government, tensions between autonomy and heteronomy of the social sciences disappeared or became invisible. A large array of forms of interaction between scientists and society took shape and eventually results of the interaction were embodied in science.

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[3] " un lieu de consécration des hérétiques, à l'écart de tous les pouvoirs temporels sur l'institution académique "

[4] At the Sorbonne, he was first substitute of Ferdinand Buisson in the chair of " pédagogie " (1902), then " chargé de cours " in " science de l'éducation " (1906).

[5] " Nous estimons que nos recherches ne mériteraient pas une heure de peine si elles ne devaient avoir qu'un intérêt spéculatif. "

[6] Bourdieu speaks of " les chercheurs les plus purs " (2001 : 7), i.e. the less tarnished by worldly interests.

Let us take the French Third Republic–often called by historians " Teachers' Republic "–as a case in point. The social sciences were " quite fashionable[7] " then (Hauser 1903 : 17). A good number of geographers enrolled under the banner of the " civilizing mission " of the French empire, and human geography was partly born from those new uses (see e.g. Rabinow 1989). In the 1900s and 10s, if " region " was a basic concept for the French school of human geography headed by Paul Vidal de la Blache, this had much to do with the interaction between university geographers and local interests. Segments of provincial notability wanted either to rehabilitate pre-revolutionary provinces culturally, or organize economic local interests for boosting trade and getting public works from the central state, or both. When WWI came, geographers discussed with the administration a plan for dividing France in administrative regions (Chartier 1980, Robic 2000). Sociologists of the Durkheim school being mostly philosophers, they were the only sociologists in a position to fight for being admitted in universities. They were tighly related with the education of teachers and given chairs where they were supposed to teach pedagogy, ethics, or social economy : this relationship to the teaching profession and the Republican regime had some weight on Durkheim's theoretical concerns about institutions that were supposed to achieve or not an anomic or integrated form of society (see e.g. Lacroix 1976, 1981). Even though they disappeared from the standard history of the Durkheim school, it would be appropriate to also mention a number of his followers who had even more practical concerns, like the lawyer Duguit (Didry 1990) or the unemployment reformer Lazard (Topalov 1994), or to consider more carefully the fact that the economist-sociologist Simiand worked for years at the Ministry of Commerce.

Let us summarize this first point. In turn-of-the-20th-century France, part of the social scientists–those who already were in academia or succeeded in entering it–were well engaged in the process of bulding up a " city of knowledge ". The way their story is usually told strongly insist upon this increasing isolation in the ivory tower that French university was supposed to be then. But this official disciplinary history is increasingly challenged by findings that show how much university professors were directly or indirectly conversing with secular powers. From them social scientists borrowed many of the problems, objects, topics, scales of description, etc. they were interested in. Then, thanks to the miracle that happens daily in the city of knowledge, all of this impure items were transmuted into pieces of science–or, as Barry Barnes puts it, social interests were translated in scientific interests.

Let us now move to another kind of historical configuration. We have just considered scholars who enjoyed independance in their chairs and power over their students and junior aides, freely chose their topics and selected whoever peer in the world they wanted to talk with. Let us now turn to another kind of scientists : they work in university departments on collective projects included in larger programs funded by non-academic institutions, and have to constantly negociate the terms of their research and justify results. Neither ideal-type is real. We noted earlier that the independant professor of yesterday did not quite exist. What about the new scientist ?

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[7] Hauser, an economic historian, wrote : " terriblement à la mode ", playing with the amibuity attached to the French word " terrible ", which means both " terrific " and " terrible ".

We now consider organizational systems by which developments in science seem to be directly determined by what is useful to worldly powers. The main points I wish to argue about them are that 1/ some (or many) social scientists took an active part in setting up those systems, 2/ such changes had powerful effects on the contents of science itself, but 3/ heteronomy of scientists sometimes was much less clearcut that one might think. The city-of-knowledge (or autonomy-of-science) model is unable to make sense of such situations–unless entire parts of the history of our disciplines are dropped down.

In the two cases I am going to visit in some detail now, scientists either take the initiative of a new relationship between science and practice or, at least, work out this new relationship in co-operation with external powers. Who did make the first move is unclear, and not quite important anyway. What matters is that a paradigmatic revolution in a discipline may have resulted of the interaction.

In January 1929 Bronislaw Malinowski (1880-1942) published an article entitled “Practical Anthropology” in *Africa*, the journal of the International Institute of African Languages and Cultures. The Institute had been created three years earlier as an institutionalized forum where scientists and “practical men” could talk of colonial reform and devise common plans for promoting knowledge and betterment of the black continent. Malinowski was involved in the institution. In the chair of anthropology of the London School of Economics and Political Science, he was not anymore the middle-aged aristocratic Polish immigrant who had lost his estate, but the well known anthropologist of the Trobriand Islands, though rather marginal a character in British anthropology. His 1929 article was supported by the leadership of the Institute, but triggered some controversy. Malinowski answered in 1930 with another article : “The Rationalization of Anthropology and Administration”.

As de L’Estoile–who recently unburied and aptly interpreted the episode (1994)–notes, this series of Malinowski’s pieces are not part of the consecrated opus of the great anthropologist–the observer of *kula*, the theoricist of the relationship between witchcraft and technology, the first practicioner of fieldwork. But, for the sociology of science, there is not such thing as minor writings or unsignificant actions.

Indeed, Malinowski did not only offered to colonial administrators the support of anthropologists, he did not do less than redefining both the object and the method of anthropology. He declared that anthropology should not any longer be the realm of antiquarians who collect skulls, handicrafts and rituals before they vanished. Anthropology should take as its main topics cultural contact, social change, and especially the transformation of native political systems. In order to be able to fulfill this new mission, anthropologists should not shut themselves in their laboratories or museums : they must go out to the bush–with the support from local colonial administrators–and directly observe what was changing in lives and societies of the natives.

It has been argued that Malinowski’s people and other field anthropologists were poorly considered and treated by British administrators in the African colonies. It seems that actual practices were contrasted and, on the whole, some degree of co-operation prevailed. But the main point for our present discussion is elsewhere : if Malinowski’s offer could not prevent the eventual demise of the British empire in Africa, it deeply transformed British anthropology. Physical anthropology and the study of exotic customs declined into sub-fields of the discipline and lost intellectual relevance, whereas from the 1940s on political and social anthropology were considered as major contributions of the new British school. Fieldwork became the basic requirement of the discipline. By the same token, long established scientific hierarchies crumbled down and new ones appeared . The LSE–where Malinowski would teach–became the focal point in research and indeed helped train "practical anthropologists" who deeply renewed the discipline. Oxbrige, on the other hand, declined as long as a new generation did not came and replace professors of the Meyer Fortes kind. Between 1930 and 1950, what can be called a kunhian revolution in anthropology had occured.

Malinowski and his followers had been able to grasp that there was a segment of the colonial elite–a quite well defined and tighly networked group–which intensely discussed what they privately described as a crisis of British colonial administration. They were seeking a response in a fuller implementation of indirect rule. There were fights about this strategy among colonial administrators–civilian and military–and even more so between colonial reformers and colonists. Nevertheless, de L'Estoile (2004) found a number of examples of administrators in Africa who, as early as the 1920s, tried to promote new personnel and procedures in order to get a better understanding of local tensions about, for instance property rights or the selection of local chiefs by British authorities. Young educated men and women eager to serve in new ways were available to rally around a program such as Malinowski's, who could use his then marginal LSE chair of anthropology to train them. He also invested reform institutions as the IIALC, which were filled with missionaries, military and civil servants, and deserted by certified academic anthropologists: Malinowski succesfully used them as a launching base.

One aspect of Malinowski's argument deserves to be quoted at length : "One of the refuges from this mechanical prison of culture is the study of primitive forms of human life as they still exist in remote parts of our globe. Anthropology, to me at least, was a romantic escape from our overstandardized culture. [...] And now, after twenty years of anthropological work, I find myself, to my disgust, attempting to make the science of man into as bad and deshumanizing an agency to man as physics, chemistry, and biology have been for the last century or so denaturalizing to nature. In short, I am attempting to make anthropology into a real science [...]" (1930 : 406). "[...] romance is fleeing anthropology as it has fled many human concerns. We functional anthropologists have to rely upon the other attraction which science presents, the feeling of power given by the sense of control of human reality through the establishment of general laws. Science is thus the most practical form of activity [...]" (*ibid.* : 408).

This can be seen as mere rhetoric. However, there is no reason to suspect that Malinowski did not believed in the usefulness of the new anthropology he was promoting. In any case such a rhetoric was quite common at the time in the social sciences.

Let's observe another significative case that took place in and around the University of Chicago in the 1920s. Big changes were taking place in the definition and organization of the social sciences. One could read for instance in the "Green Bible", the textbook published en 1921 by Robert E. Park (1864-1944) and Ernest W. Burgess (1886-1966):

"Sociology seems now [...] in a way to become, in some fashion or other, an experimental science. It will become so as soon as it can state existing problems in such a way that the results in one case will demonstrate what can and should be done in another" (Park & Burgess 1921 : 45).

A similar vision was being systematized at the same time in one of the foundations of the Rockefeller empire : the Laura Spelman Rockefeller Memorial Fund was then redirecting its action toward reforming the social sciences in American universities. The promotor of this new policy was Beardsley Ruml (1894-1960) who, being 26 and a Ph.D in psychology from Chicago, was appointed director of the Memorial in 1921. Ruml stressed the necessity of research to make social work more efficient:

“The great practical need for greater knowledge concerning the forces that affect the behavior of individuals and societies is definitly realised by the ablest leaders of social welfare organisations, and in many cases research departments have been established or proposed in an attempt to get light on some of the more pressing problems.” (Ruml 1930)

Problems were many and serious: “the problems of child life, of leisure time and recreation, vocational problems, problems affecting the immigrant, the aged and the poor and problems of neighborhood relationships” (quoted by Bulmer 1980 : 72). But universities were unable to answer appropriately:

“[...] the requirements of classroom instruction limit markedly the possibilities of contact with social phenomena. As a result, production from the universities is largely deductive and speculative, on the basis of second-hand observations, documentary evidence and anecdotal material. It is small wonder that the social engineer finds this social science abstract and remote, of little help to him in the solution of his problem.” (*ibid.*)

Ruml then argued for a program which would prompt universities into studying the social and economic life of their communities, in close relationship with “the agencies and organisations which are engaged in practical work, social welfare organisations, industries, city and state gouvernment, etc.” (*ibid.* : 73).

This policy led to setting up the Local Community Research Committee (1924-29), which was replaced (1930) by the Social Science Research Committee. In both institutions, Rockefeller people and Chicago academics closely co-operated. Their common argument was not that the university should become more sensitive to community needs–as defined by local reformers: indeed, this was already the case since the very foundation of the department of sociology, and sometimes in mistaken ways according to Park who used to mock uninformed good-doers. The new doctrine was that science should become less bookish and more empirical, and for that very reason, more scientific. Insulation of disciplines should end, and programs be devised around issues.

University leaders and Rockefeller men on the one hand and, on the other one, professors and/or heads of department–like Albion Small, Park and Burgess for sociology, Leon Marshall for economics, Charles Merriam for political science– shared or proclaimed to share such ideas. They were not in same positions nor had same priorities, but they belonged to the same social world. Prominent professors were distinguished members of the local elite, many were active in reform circles and so connected to the self-proclaimed progressive commercial interests of the big city. Some academics, like Merriam, even had engaged in active politics and only failure pushed them back to the campus. As for leaders of foundations, they had been university trained and often, at some point in their careers, went back to academia as professors and heads of academic institutions. If both categories of actors shared a common language, though, sometimes tensions developed between them. Academics would try to retain control over the distribution of funds, and scatter them among individual or rather limited projects. Foundation personnel and their allies among professors would promote broader programs and tighter control of spending and results. Transition from LCRC to SSRC–as well as the demise of Burgess program on local communities–resulted from these tensions.

What is important for us here is that innovation in research in the social sciences at Chicago in the 1920s and 1930s was related to a well defined interaction between foundations and departments. Studies of local communities, urban institutions, crime and delinquency, race prejudice, family disturbances, voting behavior–some of the new fields of empirical research being named here with the vocabulary of the time–all emerged as relevant topics. Human ecology, social interaction and a number of lesser theoretical constructions took shape. A limited degree of fieldwork was experimented along with new statistical techniques. Evaluation of research was made by committees that were distinct from the ordinary academic institutions of judgment by peers. American sociology entered a new age at the very time sociologists started to negociate their ways and means with secular authorities.

It can be objected to the choice of my stories that in both of them, the Rockefeller foundation was involved. Many other situations could have been chosen, however, that would have also shown that, in the interwar period, some social scientists deliberatly chose to take wordly powers as allies supporting them in the scientific competition and this brang about significative changes in science. With such a move, scientists indeed lost some measure of autonomy and threw science onto the stage of a broader history – that can be also called politics.

3. Making sense of the city-of-knowledge model in Bourdieu's work

One of the main methodological breakthroughs the sociology of scientific knowledge has provided was to bring scientists back into the history of their times. Scientists are people who do many sorts of things, among which science. They produce bits of knowledge, no doubt, and sociology must try to make sense of that science : right or wrong, contents matters. Scientists are also social beings who engage in multiple spheres of relationships and practices, and this is interesting also, notably for understanding their scientific activity itself. Such a program ignores the fossilized opposition between external and internal analysis of science. Among other tasks, it implies to explore the varied links the world of science and scientific practices can have with many other worlds and practices in society. Such links are bi-directional : society acts on science, scientists act on society. Thanks to this point of view, history of science is embedded again in history–not only in the history of thought. This gave birth to scores of monographs, from which new views both on science and society emerged.

Bourdieu, indeed, had quite a similar program–he usually called it “ social history of the scocial sciences[8] ”. He talked of sociology (and related disciplines) as a field many times, but not in any systematic way. An early essay co-authored by Passeron (1967) on French sociology and philosophy was openly more in the tone of controversy than objectivation. Later on, *Homo Academicus* (1984), by defining the position of the discipline in the academic field as a whole, came close to an empirical study of how French sociology worked on the eve of 1968. At the end, the last Bourdieu's work, *Esquisse pour une auto-analyse* (posthumous publication written in 2001) gives many clues on the French social scientific field as he saw it as a young student in philosophy and all along his career. But we do not find in Bourdieu's work any consistent empirical inquiry in the history of the social sciences.

I would argue that Bourdieu's relationship to theory and his theoretical framework itself prevented him to implement any consistent program on the topic. Two reasons for this.

The first reason is that it was pointless for theory. Bourdieu's students and close followers have much and usefully worked at developing a “ social history of the social sciences ”. Other scholars around the world freely used Bourdieu's inspiration together with other ones with the same general intent. But their findings had almost no impact on the development of Bourdieu's theory. They were seen as mere illustrations to it, or classroom examples. They were never used by Bourdieu for modifying, improving, correcting or changing the model in any measure : “ A world apart ”, the central chapter of *La Science de la science* is nothing but a further refinement of the model of the scientific field Bourdieu had defined in 1975-76. A quarter of a century of empirical work did not change one comma to it.

The second reason why Bourdieu did not implement his empirical program on the sociology of science is that it cannot be implemented. One can speak here of a sociology written in the conditional tense : “ If I had time, I would (or I could, or I should) show that... ” . Bourdieu constantly used such formulae when he needed to jump over gaps in empirical results. Of course, no one can know everything. But the theory of fields implies to make believe the contrary : one cannot know a part if one do not know the whole. The theory calls for detailed investigation in every relevant aspect of a number of articulated fields, but it is rather obvious that building such a cathedral is an unending task. One of the most brilliant and inspiring books in the sociology of scientific (in this case : philosophical) knowledge is Bourdieu's *Ontologie politique de Martin Heidegger* (1988). When it came to analyzing the philosophical field in the Weimar Republic, Bourdieu confessed : “ The task being so big, one cannot prevent oneself from thinking that the method is worth more than any peculiar use that can be made of it, because one cannot have control of all what would be necessary to know (in philosophy, history, politics, etc.) to be as rigourous as one should.[9] ” I suggest this translation : “ I cannot prove what I say, but believe me. This

[8] “ histoire sociale des sciences sociales ”

[9] “ Devant l'ampleur de la tâche [...], on ne peut pas ne pas penser que la méthode vaut mieux que l'application que l'on peut en faire, faute de pouvoir maîtriser l'ensemble des

story is not that important anyway, it is my method which counts. ” In *Homo Academicus* a program was already sketched for studying sociology sociologically : “ In order to effectively move from the sociology of the field as a space of positions, to the sociology of cultural productions I am sketching here, it should be necessary to relate the trajectories that correspond to the main positions to the evolution of corresponding productions [...].[10] ” (1984 : 149) One life later, the job is still to be done.

Even though we find no full empirical analysis of the sociological field in Bourdieu’s writings, there is a good number of scattered but repetitive and consistent comments about how he saw it. What picture do they draw ?

Descriptions of the past and present of sociology by sociologists are a wonderful material for approaching the sociology of the discipline at any given time. But doing this work about Bourdieu alone would be of limited interest, as his descriptions can only be understood in relation with his competitors’ own descriptions. It would be a huge work, and may be scholars of my generation are still too much involved–though in an indirect manner–to do it properly. This is to confess I have not done any solid inquiry, and can only present a few tentative observations on Bourdieu describing the (French) sociological field (here I am caught doing sociology in the conditional tense).

First, Bourdieu consistently claimed that sociology being a science, it should organize and work as a scientific field. In *Homo Academicus*, a young and enterprising sociologist showed that the structural position of the discipline in the French university system provided it with the a privilege of being the most disinterested–hence the most scientific–discipline in the whole system. In that book, empirical results on University of Paris professors are interpreted through an ideal-typical model with two opposite poles : the secular one and the scientific. At the first pole of the academic field, legitimation and rewards come mainly from wordly powers (notability status, political support, business) and/or academic power (bureaucratic honors and influence), at the other pole they come from scientific authority. At one pole, one find disciplines which serve order, at the other one disciplines which serve thought. The same model is used for opposing schools (law and medicine vs science and humanities), or specialties within schools : medicine vs biology, philosophy and literature vs the social sciences, etc. In this system of structural positions, sociology finds itself alone at the genuine scientific pole in the Facultés des lettres et sciences humaines (1984 : ch. 2). However, sociology itself can be structurally analyzed according to the same polarization. Troubles begin here.

In the 1970s, Bourdieu’s vision of an autonomous scientific field could be comforted by a paradoxical ally. Robert K. Merton’s theory of “ scientific communities ” was, in a way, the doctrine against which Bourdieu started to think of science as a field where powers-that-

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savoirs (philosophiques, historiques, politiques, etc.) qui seraient indispensables pour lui donner toute la rigueur nécessaire. ”

[10] “ Pour opérer réellement le passage de la sociologie du champ comme espace de positions à la sociologie des productions culturelles qui est ici esquissée, il faudrait mettre en relation les trajectoires correspondant aux principales positions avec l’évolution des productions correspondantes [...]. ”

be and outsiders competed : the Frenchman wanted to tear down the enchanted description of the internal organization of science as a world of peaceful co-operation. Even so, Merton–as well as Lazarsfeld and many other American functionalist sociologists–provided Bourdieu with an analytical scheme that confirmed his vision of the autonomy of science : science takes shape through a process of institutionalization. Disciplines build up boundaries, a cluster of concepts and theories, common practices and institutions, procedures for competition, debate and decision about truth. In spite of Bourdieu's fight against the reconstruction of the history of sociology by the Columbia multinational task force (Pollak 1979), this aspect of the mertonian view was broadly accepted. Some degree of agreement on this occurred in French sociology, and " institutionalization of sociology " became a common topic for scholars who worked with Bourdieu (e.g. Karady 1971, 1974, 1976, 1979) as well as with Lazarsfeld himself (Lécuyer & Oberschall 1968, Lécuyer & Karady 1973, Lécuyer 1978) or his French Trojan horse Raymond Boudon (e.g. Besnard 1983).

In spite of that temporary shared vocabulary the march forward of sociology as a consolidated social science did not take place. Bourdieu early pointed out what he considered as elements of internal weakness that prevented sociology from fully being an autonomous scientific field. With the post-1968 fast growth of university faculty, many newly hired professors of sociology were also, he said, general writers or journalists[11] (1984 : 99 n. 1) and this allowed them to use in the field means of legitimation originating out of it (ibid. : 103 n. 1, 157). With the growth of government-funded reasearch programs, ignorant technocrats could hire low-grade and dependant scholars whose tasteless work broke with the very principles of academic autonomy (ibid. : 77 n. 22, 162). On top of this the development of big administrative units of data production multiplied powerless " wage-workers of research[12] " (ibid. : 163-164). Indeed, young Bourdieu's world of academic excellence chatting around the courtyard of the Ecole normale supérieure was being overwhelmed by newcomers.

Apparently, Bourdieu's vision of science and scientists in general became even more pessimistic at the end of his life : " I think the world of science is threatened today to undergo a frightening step backward. The autonomy science had gradually conquered against religious, political, even economic powers [...] is extremely weakened [...][13] "

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[11] " un grand nombre d'écrivains, d'écrivains-journalistes et de journalistes-écrivains " (99 n. 1)

[12] " salariés de la recherche ". My bold translation is not literaly correct : people working for public statistical institutes were salaried personnel indeed. The use of " wage-workers " is chosen here because Bourdieu's term seems to me loaded with a good measure of " aristocratic " (or, more simply, academic) contempt for the greyish mass of dependant research workers who worked there.

[13] " Je crois en effet que l'univers de la science est menacé aujourd'hui d'une redoutable régression. L'autonomie que la science avait conquise peu à peu contre les pouvoirs religieux, politiques ou même économiques [...] est très affaiblie. / [...]. Les savants désintéressés [...] risquent d'être peu à peu marginalisés [...] au profit de vastes équipes

(2001 : 5-6). “ Disinterested scientists ” were being replaced by “ large teams of researchers organized in a quasi-industrial way ” (ibid. : 6). Once again, the problem is loss of autonomy, that is the prime condition for a scientific field to exist. It is serious a situation : “ Science being endangered, it becomes dangerous.[14] ” (ibid. : 6)

In the specific field of the social sciences–which interested Bourdieu most–the picture was not brighter. On sociology itself, Bourdieu had quite strong a “ practical sense ”, that he usually expressed by quick, allusive and biting notes, and sometimes under the thick guises of the theory of fields. However, though Bourdieu constantly pleaded for instituting socioloy as a genuine scientific field, he never described actual French sociology as one.

Actually, Bourdieu more often talked of sociology as a victim of a curse : anyone can say something about what sociology is the science of, society. It seems that there is no “ entrance cost ” into the discipline, whoever has ideas on social things can publicize them with as much authority as a scholar who had inquired into the topic for a long time following exacting and exhausting methods. Sociology is threatened from outside : by journalists in the first place, and also by politicians, bureaucrats, social thinkers of any description. The condition of sociology has dramatically changed since Bourdieu was a young philosopher who chose to become a sociologist in the late 1950s and early 1960s. The matter with sociology at that time was the contempt in which intellectuals–that is philosophers–held the mundane and rude things that sociologists painfully studied (1997 : 44-53, 2004 : 15-34). Today, much admired Sartre or lesser philosophers are not the foe any more. They have been sadly replaced by TV speakers and media-darling self-proclaimed intellectuals.

As for the threats from inside, it seems that the worm has awfully grown in the fruit. Now, external pressures do decide what or whom is good or bad in science. Some or many sociologists–no name is given in print by Bourdieu–get substantial rewards by accepting to speak like wordly powers want them to do. Other ones–here Bourdieu only mention himself–are persecuted for not giving up : “ [...] social scientists, pecularly sociologists, are under close scrutiny. Either in a positive way which brings them much material and symbolic rewards, when they have chosen to serve the dominant view on social world–this can be done by omission, and in this case poor scientific ability is enough to get the result. Or attention can be given in a negative, malicious and sometimes destructive way, to those who simply by doing their job, try to unveil some truth about the social world.[15] ” (2001 : 7) Much is said here in a few sentences. I do not want to comment on Bourdieu’s

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quasi-industrielles, travaillant à satisfaire des demandes subordonnées aux impératifs du profit. ” (2001 : 5-6)

[14] “ [...] la science est en danger et, de ce fait, elle devient dangereuse ”

[15] “ [...] les spécialistes de ces sciences [sociales], et en particulier les sociologues, sont l’objet d’une très grande sollicitude, soit positive, et souvent très payante, matériellement et symboliquement, pour ceux qui prennent le parti de servir la vision dominante, ne fût-ce que par omission (et, en ce cas, l’insuffisance scientifique y suffit), soit négative, et malveillante, parfois destructrice, pour ceux qui, en faisant tout simplement leur métier, contribuent à dévoiler un peu de la vérité du monde social. ” (2001 : 7)

bitterness and his sense of being the last genuine sociologist left in this world. What is of interest for my present purpose is to observe how he denied French sociology any feature that could make it look like a scientific field. Secular powers (the media, bureaucrats, etc.) now rule the discipline because (some, most of, all of ?) colleagues have betrayed. Homo academicus has lost the battle.

Bourdieu also revealed his view on the field when he acted in it. Mention by a scientist of other scientists in publications are quite good an index to the actual structure of the field. Such implicit descriptions must be serialized for being meaningful. When footnotes and refences are studied systematically in a rationally chosen set of works published by several competitors, much can be learnt from comparing their networks of references. Again, I limit myself today to very narrow a source, which does not deal with sociology in general but with sciences studies only : how Bourdieu constructed that field of in his 2001 College de France lectures ?

In this source, Bourdieu's quotation strategy can be described by rather consistent features[16].

1. Great thinkers of the past–mostly philosophers reflecting on science–are quoted and discussion engaged with them. Bourdieu indeed aimed at solving the big issues they raised, which are always with us : Kant, Leibnitz, Pascal, Durkheim, Wittgenstein, Poincaré, Carnap.

2. More recent authors or Bourdieu's contemporaries are described as his masters or inspirers. They were the exponents of the classical French tradition of philosophy and history of science : Bachelard, Koyré, Canguilhem, Vuillemin (a special tribute being given to him). One mention of Merleau-Ponty can be classified in this category.

3. Theory holds that any revolution in science implies coming to terms with and integrating tradition (2001 : 37-39). Bourdieu accordingly goes into a classical operation of " predecessor's selection " (Kucklick 1980) and reconstructs a past for sociology of science. First there is Robert K. Merton and a few American allies (Ben-David, Hagstrom) who theorized the autonomy of scientific communities (ironically Popper is also mobilized here). Then comes Thomas Kuhn who introduced the idea of the discontinuous progress of science. Last, we find the first and main proponents of the " new sociology of science ". Their ideas are more lengthy and critically presented than Merton's or Kuhn's, but nevertheless integrated in a shared history. After all, European resistance to Mertonian functionalist visions came simultaneously from Paris (Bourdieu), Edinburgh (Bloor and Barnes) and Bath (Collins).

4. A number of authors are quoted because they provide interesting facts which are used as illustrations of Bourdieu's theory. They are all foreigners, and very few of them are

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[16] A quantitative and more complete presentation of this description will be given in a further version of this paper. I left aside the section entitled " Esquisse pour une auto-analyse " (2001 : 184-220), because there exist an enlarged separate publication of it (2004)

related to the sociology of scientific knowledge school : M. Polanyi 1951, S. Cole & J. Cole 1967, N.C. Mullins 1972, M.D. Grmek 1973, F.L. Homes 1974, G.N. Gilbert & M. Mulkay 1984, J. Tompkins 1988, M.J. Nye 1993, G.L. Geison 1995, M. Biagioli 1998, P. Lazlo 2000. One French author survives, who is a specialized philosopher of physics : M. Bitbol 1996.

5. A very limited number of presently active authors are appreciated not only for the data they provide, but also for the concepts or bits of theory they suggest. Once again all of them are from abroad : P.B. Medawar 1964, E. Garfield 1975, T. Shinn 1988, J. Heilbron & R.W. Seidel 1989, Y. Gingras 1991-2002, I. Hacking 1992, M. Friedman 1996. Three of them have been published by Bourdieu in *Actes de la recherche*. In the same category, two French authors are mentioned for their theoretical import : Bouveresse and Passeron– both are close to Bourdieu in complex ways

6. Eventually, one foe is chosen in the present French landscape, and taken as the villain : Latour plays the role, together with his co-authors Callon and Woolgar. Some of the English-speaking old (L. Hargens 1978) or new sociologists of science (B. Barnes 1974, K. Knorr-Cetina & M. Mulkay 1983, A. Pickering 1992, M. Lynch 1993) are criticized too, but in a softer and never scornful manner.

If one tries two draw a map of the present field of science studies as Bourdieu saw it, in the first place we find two positions from which resistance to the functionalist paradigm sprang in the mid-1970s : the Edinburgh and Bath scholars in Britain, and Bourdieu himself in France. The first group, because of its theoretical shortcomings, has been unable to develop a coherent set of works, and “ the new sociology of science ” eventually developed in a nebula of irresponsible authors, many of whom sank in relativism and cynicism. The worse of all has two charcateristics: being French, he is close, having theoretical ambitions, he stands as a competitor[17]. Eventually, the field includes a number of good scholars able to contribute to theorize some aspect of the field : almost all of them belong to the English-speaking world, they are not quite young and few of them have been influenced by the new sociology of science. There is also a good number of specialized scholars who are wise enough not to theorize too much, but are good at providing useful examples. In France, the present field of sociology of science is a desert, if one excepts Bourdieu himself and his noisy incompetent challenger.

Such a description of the scientific debate in sociology of science leads to the unescapable conclusion that there is nothing like a “ scientific field ” involved in this specialty. No note is taken by Bourdieu of the rather solid institutionalization of sciences studies in the English-speaking world since the 1980s with a number of publications in old and established journals (Annals of Science, History of Science, Osiris), the creation of a new one (Sciences studies, then Social Studies of Science), of an international forum with an annual publication (Sociology of the Sciences Yearbook), not to mention countless debates, conferences and symposia. One can think that Bourdieu was nostalgic of the time

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[17] Once again, I am not taking Latour’s (or anyone’s) side in this dispute, I am only trying to describe Bourdieu’s action. I might add that I agree with every single piece of criticism Bourdieu makes against Latour in this book.

when, from the 1950s to the 1970s, a handful of a towering individual authorities controlled the French field of philosophy and history of science, opened it to selected students and would have closed it to phoney outsiders who by no means existed : I take Bourdieu's emotional memories of his lengthy face-to-face discussions with Canguilhem (2004 : 40-45) as allusions to this former and missed state of the field. Bourdieu never mentioned that a " Société française pour l'histoire des sciences de l'homme " was created in 1986 (" Une histoire... " 1988). There converged ordinary presentists from various disciplines, critical sociologists and historians who had read Foucault, Bourdieu and Shapin, old-style scholars in the history of ideas, remnants of Lazarsfeld's French troops, and more. Since then, this learned society has worked a lot and kept alive a low-key continuous debate between various intellectual strands interested in the history of science : is not this a field-constructing institution ?

My concluding remark will be an hypothesis about Bourdieu's blindness to the growing activity in the history of the social sciences in France. Paradoxically, his students and followers have played an important role in this process, sometimes in isolation, sometimes in relation with people using other strands of inspiration. Bourdieu's surprisingly limited investment in a field of empirical enquiry he constantly stressed the importance of might mean that his concern and conversation were basically philosophical. They were about the nature of truth. On this, talking with the great dead was more crucial than with the living.

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