

Disciplines under Transformation. Benefits of Sociology of Knowledge to the History of Sociology*

RADOSŁAW SOJAK

Nicolaus Copernicus University in Toruń

At first glance, the question about the potential benefits of the sociology of knowledge to the history of sociology seems to be clear and inherently practical. However, even a basic familiarity with the sociology of knowledge, or sociology in general, makes us see that it is a nebulous issue whose path does not directly lead to practical recommendations. In order to address this issue, we should first answer more complex (or perhaps more fundamental) questions: What type of history of sociology and what type of sociology of knowledge do we have on our minds when we ask about the benefits of the latter to the former?

If we can say that sociology is *de facto* a history of subsequent sociological projects, then we can state the same about the sociology of knowledge. The matter is further complicated because sociology, despite shifts in theoretical paradigms, can be said to have institutional continuity. On the other hand, in the

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case of the sociology of knowledge, we are dealing with two distinct intellectual traditions which have different cultural and institutional origins. This distinction made “the sociology of knowledge” achieve certain independence in the last decades.

In spite of these circumstances, the question of which history of sociology may benefit from sociology of knowledge remains a complex one. Let’s assume—not to get involved in redundant discussions—that sociology still “breathes” with two lungs: a humanistic one and a social one.¹ In this way, it inherits, at least partially, a problem which is typical for philosophy. A problem which entails a difficulty in differentiating doing sociology from doing the history of sociology.

I will discuss the issues I have presented separately in two fragments of the text. Next, I will present conclusions which I drew from these considerations and answer the question: towards which sociology does the history of sociology, actively supported by the sociology of knowledge, lead us? Finally, I will demonstrate—against the theoretical character of the majority of this work—some possible practical recommendations for the use of the sociology of knowledge by the history of sociology.

Too near—too far: the irremovable indeterminacy of the sociology of knowledge

The development of the sociology of knowledge can be examined in categories of two principal disputes. The first, a dispute which led to the foundation of the sociology of knowledge, was about epistemological issues. Sociology, which was emerging at the beginning of the 20th century, constituted a kind of intellectual claims on philosophy. At least this is the way philosophers perceived the issue. They saw sociology as another variant of relativism, which got involved into a self-referential paradox, or defined it as another radical embodiment of historicism, connecting it with the Marxist tradition.² Young *Wissenssoziologie* attempted at tackling these accusation in several ways.

¹ Let’s assume that textual analyses, re-interpretative games, and exegesis of ideas are characteristic of humanities; that humanities gravitate towards qualitative theory and methodology; that books are their main product and a means of preserving knowledge. In the case of social sciences, however, empirical analyses are more natural with their abstract modelling and attempts at quantification; natural methodology is rather quantitative, and a swift exchange of ideas is based on papers as a basic means of communication in science.

² The sociology of knowledge—with certain nuances—was perceived in such a way by both representatives of the Frankfurt School (cf. Czerniak 1990), as well as Karl Raimund Popper (1993), who came from a completely different tradition.

The first method was attempting at defining categories of knowledge in a radically non-epistemological way: acknowledging that truthfulness or falsehood assigned to knowledge is in fact neutral, and has no influence on speculations within *Wissenssoziologie*. Mannheim's (1992) concept of relationism is the earliest program of this type. From the perspective of the future development of the sociology of knowledge, this path should be considered the most prolific—the Strong Programme in the Sociology of Knowledge followed the same path, “revitalising” the discipline half a century after Mannheim (cf. Bloor 1991; Barner, Bloor 1996). The problem is that it was not applied consistently at the beginning of the 20th century, and it was neutralised by philosophical “dodges” of the sociology of knowledge.

Attempts at radical and consistent formulation of hypotheses on social determinants of knowledge were a second dodge of this type. Again, the best example comes from Mannheim's thought and an attempt to differentiate the sociology of knowledge from Marxist tradition by using the total conception of ideology. However, this path turned out not to be prolific. Most of all, it did not resolve the self-referential paradox, but rather shifted it to another level of analysis.

In this situation, Mannheim—although similar threads can be found in the works of other early-20th century sociologists of knowledge—tried to reanimate the Marxist theory of an “epistemologically privileged subject,” thus laying foundations for unattached social intelligence. In Durkheim's theoretical system (1983, 1990; cf. Szacki 1964), which was almost completely bereft of any Marxist inspiration, a similar function was performed by the belief that new forms of social order gradually limit the influence of social factors on arriving at knowledge, at least within some selected institutions and systems.

Now, it is but a step to the fourth means of rescuing the sociology of knowledge from philosophical critique, namely conscious self-limitation. This strategy has the greatest influence on Mannheim's thought. As a result, the sociology of knowledge obtains the right to address socially conditioned knowledge, which in practice was limited to these disciplines of knowledge which the anti-positivist term *der Geisteswissenschaften* embraced. Durkheim—despite all the differences—actually applies the very same strategy when he directs his attention towards primitive classification systems.³

³ It should be noted, however, that such moves can be interpreted as rational strategies. The beginning of the 20th century was marked by a surge in philosophical interest in science, development of neo-positivist conceptions and high hopes that the process of doing science could be formalised and captured by algorithms. In such a situation, the sociology of knowledge tried to evade attacks by giving philosophy a subject which it was most interested in. It has to be pointed out that this is probably why *Wissenssoziologie* was unable to incorporate Ludwik Fleck's (1986) concept.

All of these strategies have serious consequences both for locating the sociology of knowledge within a particular discipline and for the methodology which dominates within its framework. Concerning the latter, it is hard to dismiss the feeling that it actually constitutes a mixture of armchair anthropology and historiography of ideas. However, the former consequence is far more important.

Involving the project of the sociology of knowledge in a conflict with philosophy, addressing issues related to knowledge at a general epistemological level and limiting its scope to broadly construed humanities resulted in the fact that the sociology of knowledge could not be told from sociology. Marek Ziółkowski's (1989: 15) rhetorical question summarises this mechanism best: "there is a threat that the sociology of knowledge, in relation to sociology, will become a science about the tail of a snake (Does a snake have a tail? Why, a snake is only a tail!)." After so many years, the comment still holds true. It is difficult to find a more accurate diagnosis of a majority of disciplinary, theoretical, and identity problems of the sociology of knowledge. On the one hand, "knowledge" is such a common phenomenon, both natural and fundamental, that it is virtually impossible for any discipline concerned with the human world to study it without at least implicitly making assumptions about the nature of knowledge. On the other hand, a proper operationalisation of such a common phenomenon in terms of category and scope of research without violating solidified and silent methods of capturing knowledge in different fields of research seems impossible.

The consequences of such construed character of the sociology of knowledge are most evidently manifested within the framework of its phenomenological programme. A close reading of *The Social Construction of Reality* (Berger, Luckmann 1983), or, in my opinion, of the underestimated *Invitation to Sociology* (Berger 1988), soon comes to the conclusion that extending the definition of the sociology of knowledge leads to a complete blurring of borders between *Wissenssoziologie* and sociology. However, it should be remembered that Berger and Luckmann's programme constitutes a summary, a crowning of a certain stage of development, not an idiosyncratic research programme. Interweaving sociology and the sociology of knowledge in such a way is characteristic of the whole tradition of classic European sociology. Max Scheller and Karl Mannheim, together with Emil Durkheim and Florian Znaniecki (despite his critical attitude towards the discipline) are considered to be representatives of the sociology of knowledge. However, Karl Marx, Max Weber, Georg Simmel or George Herbert Mead should also be included in the list. William Thomas's theorem of the definition of the situation constitutes one of the key concepts in the sociology of knowledge; whereas without Marcel Mauss's, and later on Claude Levi-Strauss's, investigations into the original forms of classification, it would be difficult to understand the relations

between the sociology of knowledge and cultural anthropology. A discipline constructed in this way did not have any chances to emerge as an independent research field—it was too close to sociology’s classic core to become a separate field of applied knowledge, with clearly defined subject and methods of application.⁴ Hence, there is nothing strange in the fact that the sociology of knowledge, even despite Robert K. Merton’s attempts to adapt it in America, was gradually becoming marginalised along the criticism of classic theories and classic methods of doing sociology. In effect, if it was not for the phenomenological project, the sociology of knowledge could be said to have disappeared in the 1960s as a research field aspiring to become independent.

A renaissance took place quite unexpectedly in the second half of the 1970s. It was connected with the emergence of a number of research programmes which identified themselves, to a degree, with the strong sociology of the Edinburgh school of thought. Although a new sociology of scientific knowledge (SSK) underwent a far-reaching theoretical disintegration as early as in the second half of the 1980s, it managed to institutionalise itself, which made it possible for the discipline to function steadily despite internal arguments.

SSK differed from the original version of *Wissenssoziologie* mainly because of its practical nature and a peculiar “scientific style.” Most of all, the new scientific field was empirically oriented and did not limit itself to working with texts. Old methodology of the historiography of ideas was nearly completely ousted by methods from field anthropology and history. At the same time, knowledge produced by exact sciences was at the centre of interest for SSK. It opened the second serious conflict in the history of the sociology of knowledge, which sometimes is called *Science Wars*.

The conflict is assumed to have started with Alan Sokal’s hoax (cf. Sokal, Brickmont 1998; see Sojak 2000); however, a lively debate between representatives of SSK and scientists was held as early as in the second half of the 1980s. This time, the main subject of the debate was the status of scientific knowledge. Representatives of both sides seemingly debated about issues similar to those addressed at the beginning of the 20th century—however, this time “practising” scientists were more active than philosophers, and the focus shifted from epistemic issues to the unskilful use of scientific terminology by social scientists. Yet, as the debate progressed, scientists, who often did not agree with philosophical implications, gradually acknowledged that the reconstructions of the scientific research process made within SSK were accurate.⁵

⁴ I tried to spell out this thought in a different paper (Sojak 1996).

⁵ This fact should be underscored, especially in the context of the reservation scientists held against the neo-positivistic programme of logical reconstruction of the research process (cf. Mokrzycki 1980).

Science Wars, similarly to the founding debate with philosophers, concluded with shifting the subject of SSK. Despite having been primarily concerned with physics, which was a model representative of exact sciences, the rapidly developing field of research drifted away towards analyses of medicine, technical activity, legal applications of scientific knowledge, as well as research practices in social sciences. In effect, despite being theoretically varied and addressing a number of different subjects, the new sociology of knowledge underwent a process of profound institutionalisation and produced a number of detailed reconstructions of the research process in exact and natural sciences, which were historical, ethnomethodological and conflicting in nature. However, SSK was moving increasingly far away from the mainstream of sociological investigations—numerous empirical studies, referencing anthropology rather than sociology, an explicit criticism of the traditional sociological terminology, an ability to more efficiently obtain funds for research in cooperation with scientists, engineers and architects virtually rendered SSK a separate scientific discipline. Hence, although *Wissenssoziologie* used to be too close to the core of sociology to be able to influence the discipline it emerged from, SSK is too far from sociology to exert any direct influence on it.

The history of the development of the sociology of knowledge and its consequences influence the way the findings made within the sociology of knowledge can be used by the history of sociology. However, these consequences are not positive. Although *Wissenssoziologie* of the early 20th century investigated the scientific output of humanities, and made it only along another purpose: constructing a theory of sociology, at such a general level, which made its application difficult.⁶ However, two issues are noteworthy in this context. First of all, Mannheim's notion of explaining the content of knowledge by referring to the social foundations of ideas. Secondly, Durkheim's concept of analysing cognitive content and categories in the context of morphological features of groups which produce knowledge. However, both of them are involved in serious problems.

In the case of Mannheim, we should remember about the notion of socially unattached intelligence—after all, sociologists belong to the group and, by this virtue, sociological knowledge should be considered as a manifestation of historical synthesis rather than an ideology or utopia. Even if we omit this reservation, there is another, even more complex, problem. Mannheim's sociology of knowledge is based on the methodology of imputed interest which dangerously leans towards circular reasoning: what people believe in and know is explained by interests, but interests are reconstructed on the basis of what people believe in

⁶ Which resulted in raising postulates—which differed in details—about making the sociology of knowledge empirical (see Merton 1982, Znaniecki 1984, Ziłkowski 1985, Majewski 1993).

and know. Such an approach was later on fundamentally criticised within SSK, but it is true that this problem was not satisfactorily resolved by the sociology of knowledge (see Woolgar 1981).

With Durkheim's programme, instead of theoretical problems, we face applicative ones. The idea of explaining what people believe in and what they know, by referring to morphological features of social groups, has been incorporated—an issue I am going to discuss in a short while—as one of the essential programs of SSK. However, in its classic form, the programme lacks operationalisation and is constrained to common-sensical observations which state some undefined congruence between classification systems and methods of organising the social world. Importantly, despite what the author of *The Elementary Forms of the Religious Life* suggests, namely that the explanation for the mechanism of congruence is hidden in the way the system of social control functions, he does not elaborate on this issue.

Durkheim's programme has been systematically developed in Stephen Fuchs's works (1991, 1992, 1993). Fuchs formulated a detailed theory of relations between the way scientific life is organised and the content of knowledge, together with the dominating forms of scientific practices. This notion has been reconstructed in detail in Polish literature on the topic; however, from our perspective, what is important encompasses only the general assumptions and the theoretical model of scientific organisation which dominates in humanities and is present in social sciences.

Fuchs's theory is based on the assumption that precision, stability, and "certainty" of scientific knowledge depends on the strength and aptitude of mechanisms of social control which bring the scientific community together. If social control is stronger, sciences produce results which are more objective and closer to the truth, as construed by classical philosophy of science. As one might guess, if social control is weaker, the knowledge which is produced in sciences is closer to *doxa*: opinions and beliefs. Furthermore, Fuchs assumes that the strength of control mechanisms depends on two factors. The first one is the degree to which scientists have to rely on each other's work. The second involves the precision and measurability of tasks which they undertake. The control is stronger when scientists rely on each other more and if learning goals of scientific activity are more precise. Importantly, Fuchs's conception avoids ontologising the differences registered between individual scientific fields, since the specificity of the subject of research is not an element of the explanatory model.

Fuchs distinguished three dominating types of organisation of scientific life: positivist, hermeneutic, and pragmatic communities. Hermeneutic communities are characteristic for humanities, which is an effect of a low threshold for entering this science, weak interdependencies between scientists and low precision

of formulating scientific problems. As a result, scientific fields which emerge from this state of affairs are dominated by textual analyses which are unrelated to one another, their primary tool is re-interpretation, and which are organised, at best, in schools of thought. Such fields are also highly “self-conscious,” which is manifested in putting most scientific method into methodological deliberations and meta-considerations.

It is precisely this conception which allows us to pinpoint the first potential benefits of the sociology of knowledge to the history of sociology. However, they will appear only in the conclusions of the theme which we have to address now, and which is connected with the question: “Which history of sociology?”

History of sociology by design

The situation in which history of sociology has found itself can be explained well by a circumstance which Edmund Mokrzycki (1980: 6) identified when he said that “meta-sociological arguments are also sociological arguments and arguably constitute the most important theme in the history of sociological thought.”

As long as re-interpretations of sociological categories are considered to be one of the methods of investigating the social world, the strategy of doing the history of sociology as sociology as such will be up-to-date. This “commotion” affects even the conceptions and programs which attempt to make history of sociology a more autonomous discipline. A good example illustrating this phenomenon is a text by Julita Pieńkosz and Łukasz Dominiak (2011); however, let’s now focus on a proposal which is more widely recognised in sociology, which was formulated by Robert K. Merton. For Merton, the starting point was a criticism of the fact that theoretical and historical thoughts are mixed in sociology—the very situation which was diagnosed by Mokrzycki. The problem is that the American sociologist offers a solution which in fact solidifies the state that he is attempting to dismantle. Below are the points of the plan and the “truly historical” history of sociology:

- The complex origins of different types of sociological ideas and their development;
- The relations between a given theory and its changing sociological sources;
- The social rank of the supporters of particular theories;
- The relations between a given theory and changing forms of organisation of science;

- Proliferation and modification of the centres of the sociological thought;
- The means by which the changing social structure and culture influenced a theory (cf. Merton 1976: 2).

Let us leave aside the harsh remark that the list above is redundant in many aspects; rather, let us point out in the first place that in no way can the list be considered a project of a purely historical character. We are dealing with a sketchy program of the “sociology of sociological knowledge” in the style of the late *Wissenssoziologie*. Obviously, one can defend this project, pointing out that the analysis of the institutional and cultural foundations of knowledge must include such tedious historical analyses as the examination of personal and institutional documents, bibliographic analyses, reconstructions of correspondence and diffusion of theories and ideas, etc. However, they are not the gist of the Mertonian proposal, but rather, a shamefully hidden “technical” piece of work, which is to become a fertiliser for a much more satisfying and rewarding, theoretical work. It is worth noting that referring the content of knowledge to the social context in a situation when the history of theory and theory are consciously unified, must lead to criticism, and thus to the re-introduction of historical reflection to the core of the theoretical project of sociology.

It does not change the fact that proposals which more consistently aim at breaking with the tradition of mixing sociology and history of sociology also face problems. One of them—though perhaps not the most important—is the asociological perception of history. Pieńkosz and Dominiak (2011: 10) write about the history of sociology *sensu stricto*, without feeling obliged to explain what they mean by the term. The authors quote John Peel (1971, as in: Pieńkosz, Dominiak 2011: 22), who naively writes: “One must therefore write about the history that really took place, not about the Whig myths.” What, however, is in fact this history? Pieńkosz and Dominak, analysing the extant Polish literature in the field of the history of sociology, write, among others:

In turn, the [classics of sociology] wrote about such international classics as Max Weber and Emil Durkheim. These works on the history of sociology are focused on sociological authorities, whose timelessness has to be emphasised time and time again. . . . A major part of the articles mentioned . . . approaches the classics’ works without taking into consideration the historical, biographical or social context in which these works were created, at the same time expecting that they will solve contemporary problems. (Pieńkosz, Dominiak 2011: 11)

Further discussion assures that the authors turn this fact into a slight complaint against contemporary researchers. However, such a vision of the history of sociology will certainly make us confused. If history is to be separated from theory, as an autonomous research field, then it should deal exclusively with

the analysis of the context of creating sociological knowledge, but to do so, it must abandon the educational claims of marking the hierarchy of validity. Do physicists need to be reminded of Isaac Newton's or Albert Einstein's "relatively timeless value"? Do professional historians of physics have to do this? On the other hand, when physicists use their findings, are they required to take into account the biographical and historical context of the discovery? Do they have to repeatedly remind everyone about Newton's alchemical practices and Einstein's temperament? One can, of course, use the arguments of anti-positivists and point out the essential differences between social existence and inanimate matter, but if we want to take the history of science seriously, then psychology, sociobiology or even evolutionary psychology have already made their way towards such a model. I suggest that subsequent reformers of the history of sociology are trying to find a solution to a fascinating problem: "how to eat your cake and have it, too," or how to construct an autonomous history of sociology, which will not be done only on the occasion of theoretical investigations, and how to keep its influence on sociology itself.

The opposition of presentism and contextualism in the approach to the history of sociology captures the entire debate well. The problem is—to refer to Mannheim's metaphor—that the former of these approaches is an ideology of the *status quo*, and a dream of returning to the golden age of sociology. The latter, in turn, is a utopia, understood in both Mannheim's sense, as a revolutionary project of changes, and in the colloquial sense, as a wishful program which ignores a simple fact that is frequently repeated in sociology: history is always a history of someone and, what's most important, it is always told for a reason. In turn, from the perspective of the sociology of scientific knowledge, the aspirations pursued by contextualists appear to be described improperly. The sociology of scientific knowledge would suggest that practising "real history" is, in fact, a cover for returning to a mundane, disciplinary, institutional and autonomous history of sociology. We can agree with such reformulated postulate of contextualists, and a further analysis will be conducted on the basis of this assumption.

It is exactly here that we may indicate the benefits that the SSK has for the history of sociology. The sociology of knowledge makes it possible for us to observe the development of the history of sociology from the perspective that eludes the opposition of ideological presentism and utopian contextualism. In the light of Fuchs's concept, the way of studying the history of a given discipline is not the result of any intra-disciplinary plan, but a function of the basic ways of organizing the research effort. As long as sociology is a hermeneutic research field with low interdependence of researchers, low entry costs, and dispersed mechanisms of intra-disciplinary control, the history of sociology will be a method of doing

sociology in general and will have no chance for a substantive and institutional autonomy. The mechanism of this dependence is simple—to develop, hermeneutic fields need strong internal differential impulses, as well as legitimacy and equality for meta-level investigations. Practising the history of your own discipline (especially from the presentist perspective) naturally accomplishes these functions. On the one hand, by reproducing the history of earlier categorical differences, the history of sociology provides a countless number of potential research problems from the history of ideas, sociology and the sociology of knowledge. On the other hand, by meticulously analysing the social and institutional context of creating sociological knowledge, the history of sociology constitutes a plane of meta-level analyses, which facilitates further categorical revolutions and research on the heritage of the discipline. In other words, in the present state, sociology needs a history (presentist, Whig, etc.) in a state that it currently has.

One can even state that any other history of sociology would be harmful to it in its present shape. One of the most important empirical findings of the sociology of scientific knowledge is that naturalisation (ontologisation, granting the status of a true knowledge) is a function of a specific process of forgetting the social, confrontational and arbitrary circumstances of the emergence of knowledge. History or historically-oriented sociology of knowledge are disciplines whose program aims to reconstruct these circumstances, and at the same time undermine the claims of science for objectivity and certainty. It is no coincidence that Thomas Kuhn, for whom analyses and historical premises were the starting point, struck a blow for the public status of science in the 20th century, in a way far more pronounced than Paul Feyerabend with his purely philosophical approach. A meticulous historical analysis led Michel Foucault (1987) to question the self-awareness of modern psychiatry; Simon Schaffer and Steven Shapin (1985) proved that experimental procedures are arbitrary; Donald MacKenzie's (1981) historical research unveiled the forgotten controversy of statistical methods which are used even nowadays. Physics and other experimental sciences, as well as psychiatry or statistics, which are institutionalised in so many non-academic contexts and are embedded in various social practices, are able to function alongside and despite these historical exposures. Could sociology do it, too...?

Let's leave this somewhat pessimistic digression and summarise the main topic of the analysis. The sociology of scientific knowledge allows us to look at the problem of the history of sociology from a broader perspective and reformulate it as a problem of sociology, rather than subsequent projects of the history of sociology. The history of sociology will not be able to establish itself as an autonomous research field, which reconstructs social, institutional and cultural contexts of producing knowledge independently from sociology unless sociology

itself becomes a social discipline (similar to psychology), and not a derivative of humanities (similar to philosophy).

There are two general conclusions of the discussion. First of all, the state of the history of sociology is a function from the entire discipline, and the autonomous history of sociology cannot be established without moving sociology towards a model of social sciences determined by psychology. Secondly, if this condition is met, the history of sociology will need to move from facts to processual syntheses. Here, it is worth to return to the initial question: Can the sociology of (scientific) knowledge aid the history of sociology in realising these tasks?

Let's start with the first, more fundamental matter. The sociology of scientific knowledge, in many detailed studies, both historical and ethnographic, argued that the basic factor which determines that fields of science become more similar to the general model of exact sciences is because they make practical attempts to intervene in the world outside the lab.⁷ It is crucial here to capture the aspect of intervention—in the context of sociology, it is not only to make researchers leave their offices, but, above all, to make them take responsibility for designing future states of the social world. In this context, even the most meticulous ethnographic description of any piece of the social system is barely an introduction to the proper work. Importantly, this is not about great ideological projects or macro-social interventions, but about attempts to redesign the social world on a micro scale, about practical solutions to everyday problems related to lifestyles, organization of interaction in physical space, social trust, etc. It boils down to implementing in social sciences *understanding by doing*, a principle known from engineering. One can hypothesise that researchers confronted with such an idea for sociology, besides the doubts of methodological and moral nature, will also point out the lack of tradition of such a way of practising sociology. Here, the research field for the history of sociology lies open—this kind of practical sociology has existed and has also been successful. However, it was either not written down at all, or—in the view of the lack of interest on the part of sociologists—it was incorporated into the traditions of other disciplines (management studies, psychology, and even architecture). Let us point out three areas from practical sociology which may be of interest for the history of sociology.⁸

Most of all, the whole American practical sociology, with its prominent representatives, albeit not recognised in Europe, such as Kurt

⁷ Let us add that this is not an innovative result of the sociology of knowledge—Robert K. Merton proposed a similar one already in the work *Science, Technology and Society in Seventeenth Century England* (1938). Its value lies in a detailed analysis of the mechanism of practical intervention in the reality outside the lab.

⁸ Here, I partially reconstruct the reasoning from Łukasz Afeltowicz and Krzysztof Pietrowicz's, *Maszyny społeczne. Wszystko ujdzie o ile działa* (in press).

Lewin (1943) and William Hollingsworth “Holly” Whyte (1968, 1980) awaits synthesising its history. The former, known primarily for the unsuccessful field theory, has a considerable number of practical achievements, the effects of which were, among others, the introduction of the extensively used concept of *gatekeepers* and, perhaps more importantly, the preliminary development of the methodology for support groups—a technique commonly, but not exclusively, used in today’s psychotherapy. “Holly” Whyte, in addition to organisational analyses, became famous for detailed analyses of everyday interactions in the urban space, and for projects of the organisation of such an urban space that allowed for a more efficient management of the functioning of large social collections in one physical space.

Lewin and Whyte are presented here symbolically. Between the wartime research of the first scholar, and the urban studies in the 1960s of the second, there lays a whole tradition of practical American sociology related to marketing research, industry and labour organization or political influence. Unfortunately, this tradition culminated with the famous program entitled *Camelot*, which was an excuse to formulate a radical sociology program and to depart from fulfilling practical tasks for the sake of confrontationally oriented ethnography of local communities, and, most often, for the sake of abstract analyses of the class system. From this perspective, the fact that radical sociologists wrote a specific mythology (since it is difficult to call their reconstructions “history”) of the achievements of the American practical sociology should be regarded as extremely harmful to the development of sociology. In this way, a large part of the praxeological tradition, instead of being incorporated into sociology, has been pushed out of it and has become an element of other disciplines in the social sciences.

The second subject of the history of sociology could be the Scandinavian traditions of sociological interventions, which are related mainly to the sociology of industry and the sea (see, for example, Emery, Thorsrud 1976, Johansen 1987). The Norwegians lent a lot of prestige to developing a method of negotiations in planning and implementing social changes and technological innovations in industrial processes. Part of this tradition was later incorporated into the concept of sociological intervention by Alain Tourain, which, among others, was “tested” in Poland. This tradition—especially the Scandinavian experiments—remains on the fringe of the discipline, exerting influence on the self-awareness of sociology only to a small degree.

Finally, let us analyse the cooperation of sociologists and anthropologists with the sector of modern technologies and IT. It is worth noting that although sociologists vastly described the impact of the IT revolution on the society, they paid little attention to their own (and other social researchers’) role in this process. Importantly, the problem does not concern the “ideological”

support of these changes, but the real participation in the design of engineering transformations of the social fabric. In the 1970s PARC (originally Xerox PARC), a research and development centre founded by the Xerox corporation, was established in California. Its aim was to aid IT with interdisciplinary planning and the design of new technological solutions. Such researchers as Lucy Suchman (1987), Eleanor Wynn and Julian E. Orr (1996) cooperated with the centre at various stages of its development. Nowadays, we indirectly owe them such objects of our everyday life as the “big green button” in most photocopiers, or Google’s minimalist design. What is more important, the access to these inventions was not the result of an ingenious idea, but the result of laborious, methodologically advanced research of ethnographic and ethnomethodological character.

To summarise this topic, it is worth reconstructing the steps of our reasoning.

The path to the institutional and research autonomy of the history of sociology leads through the transformation of the whole discipline, and not through the theoretical reformulation of the principles of the history of sociology itself.

The transformation of sociology would have to be based on a shift towards the paradigm of applied social sciences, the main objective of which would be designing and conducting the process of change in the social reality.

Such a change would be enhanced by the development of research in the history of practical sociology, the practical (!) aim of which would be a re-incorporation of schools, trends and praxeological theories from other disciplines back to sociology.

In other words, it would be a work similar to that of Wolf Lepenies in the book *Between Literature and Science: The Rise of Sociology* ([1988]1997); however, we should not stop at reconstructing a specific forgotten path of the development of sociology, but strive for its revitalization.

What’s next?

The discussion may leave the reader with the conviction that the transformations I presented are only projects. Numerous premises show that moving sociology towards more practical social sciences (such as psychology or economics) and simultaneously distancing it from the ideal of classically understood humanities has already begun in some geographical areas.⁹ Thus, the history of

⁹ This process is well illustrated by Gabriel’s Abend (2006) comparative study of American and Mexican sociology.

sociology does not have to design a great change of the whole discipline—is should only take part in it.

However, if this diagnosis is accurate and if the previously mentioned findings of the sociology of scientific knowledge are correct, the process of autonomising the history of sociology must have begun. If so, can the sociology of knowledge be useful for the autonomous history of sociology? I doubt that such usefulness could be revealed in the first decades of the history of sociology as an autonomous field of research. Let us assume, to simplify the matter, that we can speak of two basic models of practising history: factual and processual. The symbol of the latter are undoubtedly the 19th-century German philosophers of history as well as the Braudel's *Annales* school from France. But how much time had to pass and how many analyses had to be conducted to move from history, understood as a local and tedious reconstruction of individual facts, to a processual approach. Today, professional history seems to be based on a balance between these two trends, and their intertwining seems to prejudge the entire discipline.

In this context, it is worth noting that historians of sociology, discouraged by subsequent attempts to move away from sociological theory or sociology in general, see the chance for the autonomy of their research field in the form of a meticulously factual program. This should probably be considered a longing that is natural, and functional from the perspective of the development of the discipline. At this stage, however, it will be difficult for the history of sociology to draw from the tradition of the sociology of scientific knowledge. There are many indications that basic techniques and research tools taken from the work of historians remain a common methodological resource for both areas. At the same time, the sociology of scientific knowledge has little interest in social sciences as a subject of research.

However, when the stage of initial institutional consolidation of the history of sociology is finished, the sociology of scientific knowledge will become a source of theoretical models verified in the analyses of other disciplines, thus opening the history of sociology to comparative research integrating its findings in the sociologically oriented history of science. There are three basic theoretical models that can usefully structure the work of historians of sociology. I have already mentioned the first one: Stephan Fuchs's concept of the morphological analysis of research communities. It offers a relatively simple and elegant model explaining the organizational and epistemological diversity of science and scientific knowledge. Additionally, its basic categories are relatively easy to operationalise and measure. The strength of this concept also lies in the fact that it allows explaining the diversification of scientific disciplines, as well as historical and geographical differences within one discipline.

The second theoretical model that could be included in the analysis of the history of sociology was provided by Karin Knorr-Cetina (1999) in the concept of epistemic cultures. The model bears a reference to the concept of research styles that allow for microanalysis of differences in functioning between individual research centres or theoretical schools. The level of theoretical conceptualization of this idea is slightly lower than in the case of Fuchs's project, and its application to sociology would require an initial re-interpretation.

The third theoretical model comes from the well-known work of Randall Collins *The Sociology of Philosophies: A Global Theory of Intellectual Change* (1998). This multifactorial analysis, which is focused on the network model and generation as the basic analytical category and takes into account institutional elements and political processes, allows a broad synthesis of the development of any scientific discipline.

Finally, it is worth mentioning one important problem concerning the application of the theoretical models under discussion. They all concern the already-institutionalised sciences. The history of sociology is relatively short, and its significant part (especially in such regions as Central Europe) includes a non-institutionalised phase, a phase in which idiosyncratic factors associated with historical cases and coincidences, researcher's personalities and their lives, exert an immeasurable influence on the development of the discipline. It is also a stage of development in which it is often difficult to talk about a stable community of researchers, a fact which hinders all analyses based on tools adapted to the study of a community. Anyway, if we carefully consider SSK's investigations into the early developmental stages of science—such as the studies of Shapin and Scheffer or Bazerman (1988)—it will turn out that their methodology is definitely closer to that of history than sociology. Perhaps the initial question should at least partly be reversed? Maybe an autonomous history of sociology—if it deals with the analysis of the origins of the discipline, and presents the results not only in an idiographic way—will be a source of benefits for the sociology of scientific knowledge?

Translated by Marek Placiński.

Radosław Sojak

How can the History of Sociology Benefit from the Sociology of Knowledge

Abstract

In the first part of the article, the author considers theoretical possibilities of reconciliation between epistemological perspectives in sociology of knowledge and history of sociology, as the space for meta-level reflection. An outline of the development of these disciplines in the 20th century follows and their relations shown with sociology and philosophy. The last part is devoted to the three cases of neglected history of applied sociology of social sciences.

Keywords: sociology of knowledge, history of sociology, applied sociology.

