

The History of Sociology, Sociological Theory, and the Social Studies of Science: Mutual Uses and Misuses

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The second volume of *Roczniki Historii Socjologii* (*History of Sociology. Annual Review*), apart from a number of interesting research papers, included a scholarly programme announcement publication entitled “Disciplines under Transformation. Benefits of Sociology of Knowledge to the History of Sociology” by Radosław Sojak (2012). In a way, the paper confronts Julita Pieńkosz and Łukasz Dominiak’s article entitled “The History of Sociology in Poland. Assessing the Current State of Affairs,” published in the first volume of the journal. The discussion on the possible benefits of the sociology of scientific knowledge to the history of sociology appears to be especially important regarding the fact that Polish scholars show little (if any) interest in theoretical and methodological problems of the history of sociology. The idea that the history of sociology might utilize some achievements of the sociology of scientific knowledge—its individual statements, observations, theories or research practices—seems to be obvious; however, the means by which Sojak suggests it should happen are not undisputable. Hence, it is worth to return to his paper, also in order to draw attention to the possible benefits of the sociology of scientific knowledge to the student of the history of sociology, which were underestimated in Sojak’s article.

Radosław Sojak provided a number of insights it is difficult to disagree with; however, they do not directly lead to the conclusions he arrived at. In his paper, the author briefly summarized the history of sociology of knowledge until the emergence of social constructivism on the one hand, and the sociology of scientific knowledge on the other. Sojak pinpointed the discipline's ambiguous relation with philosophy—epistemology and philosophy of science (in the latter case, with its students rather than the discipline itself), as well as its troublesome relation with sociology. In the case of philosophy, the problem is that the sociology of knowledge (as a sociologist, I would say: successfully) addressed the core issues of the branch, not so much resolving them, but cutting the Gordian knot of its traditional problems. As the author rightly pointed out, sociologists handled the matter in two ways: either resigning from or pretending to resign from philosophical aspirations. Concerning the discipline's relation with sociology, as the author used a different type of argument. If the sociology of knowledge is broadly defined, then it completely loses any scientific field and becomes indistinguishable from, as Sojak puts it, "the classic core" of sociology. In fact, the author is interested in sociologists' analyses of sciences rather than the sociology of knowledge as such. These studies developed into a (strong) programme of the sociology of scientific knowledge, then underwent institutionalisation, and finally ended up in a state of theoretical disintegration. As a result, they became a part of a new research branch, social studies of science, which evolved towards historiography and social anthropology, distancing themselves from sociology. Sojak assumed ideas of Stephan Fuchs as a starting point for his subsequent considerations. Fuchs concluded that the stability of knowledge generated by a given science branch is correlated with the efficiency of social control in that branch. As the social control in communities of sociologists is inefficient and methodology is hermeneutic, sociologists' work is marked by the obsessive interest in meta-theoretical and methodological problems, as well as by a constant re-interpretation of classic works.

The strongest standpoint of Sojak's paper seems to be his accurate and balanced criticism of the article published by Julita Pieńkosz and Łukasz Dominiak. In their paper, the two authors propounded the view that the study of the history of sociology should be free from presentism, which examines the history of sociology in terms of its utility for contemporary science. Instead, they lean in favour of contextualism, which states that historical ideas can be understood only relative to their historical context. Doing so, they praised Robert Alun Jones's concept of "the new history of sociology" (Jones 1983) published in the 1980s, as well as Robert K. Merton's observation, first formulated in the 1940s, but best known from his paper "On the 'History' and 'Systematics' of Sociological Theory." Merton observed that sociologists lack historical competence, and

consequently, instead of doing history, they merely systematize sociological theory (Pieńkosz, Dominiak 2011: 20–24). The critic made his task easier by waving aside the vast theoretical and methodological literature on the methodology of historiography of sociology, substituting Jones’s “the new history of sociology” with Merton’s “On the ‘History’ and ‘Systematics’ of Sociological Theory” as his main opponent (Merton 1969a). Sojak noted the inconsistency of Merton’s idea of doing the history of sociology, which was inspired by the older sociology of knowledge. He also highlighted some drawbacks of Merton’s proposal, such as doing a truly historical (contextualist) history of sociology and a somewhat utopian idea of restructuring the field by adopting certain theoretical assumptions and/or research programmes. The problem is further exacerbated by the very idea of “the new history of sociology,” a programme by no means a new one, and which cannot be considered a great scientific success. Although the history of sociology has been accepted as a specific research branch by most scholars, there are not many of them who practice it, and their findings are of small interest for mainstream sociologists. They do not need such findings—the centre of their interest is the presentist classical sociology, which bases on the study of sociological theory rather than historiography of their social science branch. This discipline has its own journal (*Journal of Classical Sociology*) and typically constitutes a part of the sociological curriculum. In this form, classical sociology is more an example than a contradiction of Whitehead’s *bon mot* which says that science which cannot forget about its past is lost (more in: Kiliyas 2012: 234–249). Sojak is right concluding that Pieńkosz and Dominiak’s text does not provide any answer to the question of what history of sociology we should do—apart from the suggestion that it should be methodologically correct. It is hard not to agree with his doubt whether formulating certain theoretical principles and/or research programme can lead to a scientific revolution in sociology.

What positive programme, derived from the sociology of knowledge (or: from the contemporary sociology of scientific knowledge), does Sojak suggest? Referring to Stephen Fuchs’s analysis he mentioned earlier, Sojak assumed that it is not theoretical assumptions, but weak social control, typical for the sociological community as a hermeneutic society, that constitutes the problem. Such a community needs classics—and this is why the presentist history of sociology (i.e. classical sociology) dominates the field: any other form would be harmful. For Sojak, the condition for resolving the problems of the history of sociology is the transformation of sociology as a whole, making it an applied science, which was already done by such figures as Kurt Lewin or William Hollingsworth Whyte—the first one as the inventor of support groups, widely applied in psychotherapy, and the second as a student of interactions and a designer of urban space. This concept alludes to Łukasz Afeltowicz and Krzysztof Pietrowicz’s idea presented

in their 2013 work, *Maszyny społeczne (Social Machines)*. In Sojak's view, their idea would make it possible to overcome the opposition between contextualism and presentism. However, the means to achieve that end would not consist in changing the way sociological investigation is done, but in a thorough transformation, which sociology as a whole should be subjected to. The transformed sociology would be an area where there would be place for its new history, and which would take into consideration the ignored history of applied research, done, among others, by Lewin or Whyte.

What are the practical implications of Sojak's proposal for the students of the history of sociology? At first glance, we see that there aren't many, and that the proposal is not addressed to them. However, the paper includes two suggestions. Firstly, it vaguely forecasts that the new applied sociology will enable the sociology historians to overcome the main dilemmas of their field. Secondly, it provides a more specific idea that the history of applied science should also be investigated. The latter is a very good suggestion, and although the researchers of the history of sociology (or more broadly speaking: of social sciences) do not avoid similar topics, sometimes obtaining significant results (Porter 1995, cf. also Thue 2006: 535-553, Nešpor 2011: 56-148-153, Czekaj 2007: 271-306), this issue deserves a special attention. Still, a more detailed analysis reveals that using a rather narrow notion of applied research, Sojak disregarded its significance in actual practising the discipline; I shall return to this issue later on. As for his first argument, his proposal involves a paradox. He starts from a suggestion that sociology is disorganised and consequently has no social authority. However, the author asserts that the reason for this condition is not the lack of a consistent theory or a proper scientific method, but loose social organisation. Yet, he suggests that (another) grand meta-theoretical project would solve the problem! As for the author's solution for the history of sociology, it turns out that it is quite conventional and involves reorganising the canon of classic thinkers. This time, the group would include Whyte and Lewin, who would both serve as a symbol and legitimation of the new direction (Sojak 2012: 31-32; Afeltowicz, Pietrowicz 2013: 368-374, 176-179). Then, what does overcoming the opposition of contextualism and presentism involve?

The history of sociological thought is, to a large degree, the history of millenarism. The branch emerged as a secular project whose aim was to save the society via science-based reforms (or by dispelling the illusion that such a reform is possible). However, in the course of time, as the broader public failed to understand their objectives, sociologists were forced to limit their ambitions and turn their attention to their own discipline, which supposedly was not compatible with scientific standards and/or lacked unity. To remedy this situation, new projects emerged which were expected to obtain these qualities by a theoretical

integration, often under the auspices of the only true theory, or by applying the one, truly scientific method. The utopian nature of these projects (or particular ideas they included) tends to be so well-hidden—or possibly we are so used to them?—that they are considered the quintessence of realism. An exemplary case of such utopianism is Merton's (1982b) idea of “the middle range theory.” Although it is usually considered a suggestion to limit the sociologists' haughty theoretical ambitions, it probably emerged as a means to achieve the synthesis of the grand theory and empiricism, which seemed to be just around the corner then. If we take a look at coursebooks in classical sociology, we will see that similar dreams are still alive (Ritzer 2004: 317-322)! However, it is worth to bear in mind that the millenarian rhetoric is a routine literary tool of the theorists, even if they know well that their work will not spark a scientific revolution. Łukasz Afeltowicz and Krzysztof Pietrowicz's book conveys the impression that they believe in the reality of their project; whether this is the case with Radosław Sojak—it is impossible for me to tell. What is certain is the fact that we can find elements of a “small,” “within-sociological” millenarism in his paper. The author diagnoses that although sociology is socially successful, it does not conform to the scientific standards construed as aprioristic theoretical or methodological criteria. The text also includes a conviction that sociology is fundamentally distinct from natural sciences—a conviction which has been sociologically re-interpreted, but still perceived as a key problem of the discipline, which needs to be solved. It also includes the idea that the supposed success of the natural science branches could be recreated via mimicking them—even if this time it is more about mimicking social practices rather than methods. Then, it is again all about a new project which is supposed to restructure the discipline, and yet another attempt at pouring new wine into old bottles—perhaps not as old as our social science branch, but at least as old as the dream of a new, truly scientific sociology.

Sojak's proposal is typical for a certain, perhaps dominant in Poland, way that people perceive the sociology of knowledge and social studies of science. This way is characterised by the interest in epistemological issues, or, as it is the case of the author of “Benefits of Sociology of Knowledge to the History of Sociology,” in a grand sociological theory, and a relative disregard of the actual achievements of the sociology of knowledge. Although Sojak begins with a pragmatic ANT-based (Actor-Network Theory) notion of arriving at scientific knowledge, his analysis focuses on the theoretical achievements of the sociology of knowledge, and he seems to consider the grand theoretical projects the foundations of sociology. In turn, his final guidelines on how the sociology of knowledge can benefit the history of sociology pertain to theories which could become an inspiration. This role was supposed to be played by Randall Collins's interpretation of the history of philosophy, Fuchs's idea, mentioned in the text,

or Karin Knorr-Cetina's work on epistemic cultures read as a theoretical treatise. Obviously, Sojak highly appreciates *grand theories* based on bold (and at the same time superficial) generalisations, or at best the middle range theories.

Interestingly, a reference to the actual research results of social studies of science may undermine the most important of Sojak's assumptions. Edmund Mokrzycki, as early as in 1980, noticed that positivists propagated the ideal of science which was not founded on any of its branches, but rather on an idealised image of physics. Works of sociologists, anthropologists and historians of science studying the practices of the knowledge production of various science and technology branches revealed fundamental certain similarities between the mechanisms of production, validation and spread of knowledge, which involve similar types of social activities. Their extensive studies dealt with the science branches, which used to be overlooked or ignored by earlier generations of scholars, who were interested mostly in the two supposed opposites—"hard" natural science and humanities. This research, which relates to a whole range of science branches, from geography to medicine, demonstrates that they form a multi-dimensional space, filled with a variety of ways of doing science or technology rather than follow any of those supposed opposites. Therefore, comparative studies of specific disciplines, fields and research methods should constitute the starting point for any analysis of epistemic and social successes and failures of sociology—for instance, comparative research between studies on transformations of political systems, evaluative education studies, geology or neuroscience...

The discovery, that sciences involve a number of inconsistent discourses is one of the most important achievements of the sociology of scientific knowledge (Fleck 1986: 153-160; Mulkey, Gilbert 1984). Therefore, any research on the scientific discourse studies should acknowledge the diversity and incompatibility of meta-theoretical discourses, a variety of types of scholarly writing (such as textbooks, books for the general public, and monographs), everyday discourse of academics, as well as other practical ways of doing scientific research. In this context, talking about sociology in terms of grand projects or a consistent disciplinary discourse seems to be highly problematic. This statement can be reinforced by pointing out that not only social studies of science multiplied and became an autonomous branch of social science. It seems that sociology as a whole proliferated, reaching out to various disciplines to such an extent that the only link between them is the common name "sociology" and the community of institutions of this social science branch. Furthermore, forms of theorising (and ways of referring to theories) have changed too. Nowadays they are often much more down-to-earth, as scholars mostly use theoretical concepts and ideas only as instruments, no longer treating them as building blocks of general sociological theory. This weakens top-down reasoning, similar to the one in Sojak's paper,

which assumes grand theoretical projects as a starting point, and which considers an academic branch to be a whole. It should be noted that these changes also involve a change of the status of classic works, which may no longer be a symbolic keystone of contemporary science. As Peter Baehr and Mike O'Brien (1994: 62) put it in their fundamental work on the status and role of classic thinkers and classical sociology:

Similarly, those of us who value the classical tradition need to consider the possibility that appeal to its uses may become more an elegy to past times and standards than a credible rallying point for disciplinary coherence.

Certainly, these observations also refer to various types of applied research, which is most likely the main source of income for the majority of sociologists. Done on a mass scale, the applied research does not refer to any unified theoretical corpus or a single set of methods, and their outcomes expand the resources of general knowledge only to a limited extent. They do not add prestige to the discipline, but they do exist and it would be wrong to ignore them. Perhaps it would be more appropriate to look more closely at the existing sociology than to create a new applied one?

The way in which I have organized my argument may suggest that there has been some fundamental change in sociology in the last few decades, consisting in the growing number of research fields and theoretical decomposition. Perhaps this is the case, but a historian should be careful with judgments which are not based on a systematic comparison of the present and past phenomena. Since we know it mainly from synthesizing general histories, old-time science often appears more coherent than the present one. But, for example, was American sociology of the interwar period not criticized for the lack of a unifying theory and an excessive diffusion of research effort (Balon 2001: 62-66, Turner, Turner 1993: 59)? It is quite possible that the past sociology was less homogeneous than we currently believe, and the observations that have been presented above apply to it as well.

The theoretical project by Łukasz Afeltowicz and Krzysztof Pietrowicz is emblematic for Toruń's tradition of the sociology of knowledge, initiated by the book by Andrzej Zybertowicz (1995), and pursued by Radosław Sojak (2004) or Krzysztof Abriszewski (2008). This academic enterprise of the Toruń scholars is the only one in Poland which adapts the sociology of scientific knowledge, including the Actor Network Theory and the work of its famous co-founder, Bruno Latour. Furthermore, basing on its theoretical legacy it evolves into an academic project of its own. The ideas propounded by Afeltowicz and Pietrowicz (2013) in their book entitled *Maszyny społeczne (Social Machines)* are original additions to the project, which evoke as much approval as scepticism.

Nonetheless, I do not know how I could possibly use them in my own research practice—be it as a sociology historian or a historical sociologist. The paper by Sojak, which refers to the book, contains an apt criticism of the vagueness of the proposal presented by Dominiak and Pieńkosz, but it cannot be said that it shows how to bring the Afeltowicz and Pietrowicz's ideas into an effective research program for the history of sociology, as it reproduces the pattern that does allow to find out much about the sociology's past. In this respect, the manifesto published in the first issue of *Yearbooks of the History of Sociology* is definitely more useful. The question remains, however, what historians of sociology can expect from the sociology of (scientific) knowledge or social studies of science.

The paper by Pieńkosz and Dominiak will serve me as the starting point for further investigation. I consider it not a specific research or theoretical program, but an appeal to the ideal of studying and disclosing “what it actually was like,” presented by the famous Ranke's Manifesto (1885: VIII). The pursuit of this ideal calls for postulating the emancipation of the history of sociology, which is often treated—as showed by both Merton's old paper and exemplified by Sojak's latest text—as a mere addition to the sociological theory. It is not only about the question of choosing between presentism and contextualism, which refers basically to the study of the history of sociological thought. The question of choice as such does not seem to be particularly interesting. What is more, it seems to me that this question can be settled in one way only. Although we may be presentist trying to find out something new about ourselves, we need to be contextualists if we want to learn about the past.

The theoretical instruments of the social studies of science, such as, for example, ANT and Latour's concept of translation, can help to determine the conditions for a successful liberation of the history of sociology. Examining the conditions for the success or failure of scientific ideas or technological innovations, Bruno Latour (1987: 108-121) emphasised the need of finding social actors who are interested in achieving their own goals using those ideas or innovations. Translation means binding the research goals with the interests of social actors. It varies in forms: from finding allies and cooperating with those, whose problems may be solved by the planned innovation (provided that it can function satisfactorily), just like Rudolf Diesel's idea, at the end of the 19th century, was a promise for MAN to consolidate its market position as a manufacturer of engines for industrial use. The opposite may occur when someone else's actions accidentally enables one to fulfil her or his own agenda. However, the translation frequently becomes a complex process, during which actors are repeatedly forced, in the process of mutual interaction, to change the directions of their actions, and the goals they finally achieve differ from those they originally assumed— as it was in the case of diesel engine, whose inventor had to depart from using the Carnot

cycle. The control over the direction of scientific activity and the authorship of the innovation are the main problems of the actors involved in the process.

Who is, or, how and who could become interested in the process of producing the history of sociology? There is no doubt that this group includes only actors from the Academia. Occasional readers, and often even the very authors of papers (not to mention the reviewers of research projects!) in this area are laymen (of history). Generally, sociologists-theorists tend to be more involved, especially the ones who are looking for distinctive symbols of theoretical identity or symbols legitimizing their own theoretical innovations. They do that as they consider such activity a standard practice of sociology, and historians of sociology can benefit from it trying to multiply the contexts of interpretation of the classics. The history of ideas¹ may become a theoretical framework and, if necessary, offer legitimacy for such works (cf. Nisbet 1973: 3-5). This approach, as exemplified by the practice of editing and publishing readers and selected works of the sociological classics (and writing introductions to such publications, e.g. Szacki 1964), is in fact the opposite of the first type of translation I mentioned earlier (Latour 1984: 108-111). It is not historians who define the research agenda, and the “general sociological partners” are ready to get involved in such projects only when they are able to achieve their own goals—to relate some “classical” ideas to the object of their study. Such a situation makes it difficult for historians to control the directions of research development, and results in a one-sided focus on the (presentist) history of sociological thought. As Merton already pointed out, their position is weak also because theorists are able to create similar works without involving anyone from the outside from their research field.

Some of the changing, fashionable problems and topics of social research may offer certain opportunities to inquiry the history of sociology. Here I do not mean the situation when researchers look for precursors who would legitimise their interest in new research fields, which is typical for classical sociology,² but those popular topics which generate interest in the history of the discipline in some integral way. In the Polish case one such issue was nations and nationalism, which since the time of Bystroń (1916) has been considered especially important for Polish sociology. The interest that the topic has inspired was reflected in a series of studies on the history of sociological thought published by Joanna Kurczewska (1977, 2002) and her students (e.g. Stryjek 2000, Kiliás

¹ Among Polish sociologists, it was associated mainly with Jerzy Szacki, to whom the local history of sociology owes its importance as well as its image.

² For example, Maurice Halbwachs became a symbol of fashionable studies of social memory (Kiliás 2011).

2000).³ Similar results, although not limited to the history of social thought, could be theoretically expected from the growing interest in the field of historical sociology or studies on social memory, so much in vogue nowadays. Scholarly ideas, which evoke public debate, such as Burawoy's concept of public sociology (e.g. Fleck, Hess 2014), can also be a medium that enables smuggling a bit of professional historical research into the sociological mainstream.

Apart from classical sociology, the opportunity for conducting and promoting the results of historical research may be such occasions as anniversaries of events in the history of the discipline or anniversaries of specific academic institutions (e.g. Szacki 1995, Platt 2003, Sułek 2007). They not only enable the researchers to go beyond the history of sociological thought, but they also put emphasis on their specific area of expertise and competence. On the other hand, the majority of such work is created on self-help basis, and works created by invited experts, such as the book about the British Sociological Association, written by Jennifer Platt (2003), are rather rare.

Obviously, the history of sociology is located on the margin of sociology, and the type of historical research in demand is the presentist history of sociological thought. Unfortunately, historians compete in that field with theorists. The latter are as willing and competent to practice the history of sociological thought (in the form of classical sociology) as the former, who play only a secondary role in the field, with no chance to bring out their expertise or push through their own research agenda. Does it not paradoxically suggest the need to move away from the type of research which does not allow the historians to show their own, distinctive type of academic competence? One can see here an analogy to the social studies of science and their way to liberation from the older sociology of knowledge as a positive example of achieving academic independence. However, it is worth remembering that the institutional success of the sub-discipline, mentioned by Radosław Sojak, was rather limited. Moreover, at least in part, it was the result of the applied research conducted within the discipline, applicable to the scientific policy, being a kind of reward for the empirical orientation and independence from disciplinary practices of sociology. The history of sociology cannot count on similar interest outside the Academia, and it has no chance to become an independent institutionalised subdiscipline, not to mention the possibility of its professionalisation. It seems that finding allies outside sociological theory, if not sociology in general, especially among sociologists and historians of science, would facilitate the achievement of these goals. Existing on the margin of sociology is relatively safe, but it entails playing the role of a mere

³ Of course, the first of her books appeared before this problem achieved its current popularity.

supporter, who aspires to the role of a qualified expert, but has no opportunity to demonstrate her or his actual skills. Obviously, the situation of students of the history of sociology in Poland is also difficult because of the disciplinary and institutional divisions and the weakness of potentially related fields.

Ultimately, what can historians of sociology, aspiring to establish their own agenda and research methods, borrow from social studies of science? The answer to this question is difficult because of the diversity of the discipline, which includes various research fields, uses diverse methods and theoretical inspirations, as well as because of the number of ways of doing research on the history of sociology. Although the idea of applying the sociology of knowledge to the study of scientific knowledge lies at the heart of social studies of science, from the beginning, the scholars who practiced it combined theoretical ambitions with empirical research, which most often took the form of a case study. With time, a growing role was played by inspiration and research practices of other social science branches, including ethnography (the name is crucial here, as it evokes more “idiographic” connotations, rather than the alternative term “social anthropology”). In turn, sociology historians have been aspiring to practice sociology in a sociological way for years. Their efforts have produced various outcomes. The results vary, but all too often they take the form of superficial generalizations, such as those offered by students of classical sociology, who present sociology as a supposed self-reflection of the modern society or a science of crisis (e.g. Keller 2004). The success of the social studies of science proves that there is nothing wrong with the idiographic model, typical for historical research, and that sociology could possibly serve as an instrument of analysis, and not a source of a priori conclusions. What seems useful in the heritage of the social studies of science, is not any specific theory or approach, but the tradition of micro-institutional analysis—the search for social conditions of knowledge production, looking for them not in the general culture, intellectual tradition, or in supposed social interests, but in the nature of particular scholarly institutions that produce specific forms of knowledge. In this field, in spite of the existence of rudiments of such research (e.g. Fleck 2011), there is still much to be done.

As we can see, the sociology of scientific knowledge has something to offer to a historian of sociology, and it does not have to be the grand theory. The contribution of the methodology and the theoretical apparatus of the ANT may be interesting and potentially important means of the research on the transmission of scholarly ideas. Until now, they were conducted in the form of an analysis of the hypothetical impact of specific concepts on the work of individual thinkers, or as a study of the reception of scholarly ideas, most often belonging to the great sociological theory. Sociologists of scientific knowledge put a lot of effort into breaking the concept of the spread of ideas whose success was determined by

their compatibility with the natural order. Instruments for such a type of research were developed already in the 1980s by Bruno Latour (1987, 1993), who showed how the success (or failure) of particular ideas was always driven by specific actions of certain actors—and not only the social ones. Although sociologists are less inclined to believe in the inevitable success of “truthful” concepts of social life, they mistakenly considered this phenomenon a peculiarity of the humanities. The instruments developed by the ANT can therefore be useful for the historian of social sciences as well. The scientific revolution that took place in European sociology after the Second World War, which was mainly the result of the inflow and assimilation of new ways of practicing social science, originating in the United States, turned out to be an especially interesting field of research. Recently, many works have been devoted to this issue; their authors emphasize the importance of the national and international, institutional infrastructure of social sciences, including American foundations, which were active in Europe already in the interwar period (Fleck 2011), the International Sociological Association, or the influence of the configuration of local interests, both in the Academia and in the fields related to it (Thue 2006). The course of this revolution in Polish social sciences is still awaiting investigation.⁴

Translated by Marek Placiński.

Kilian Jarosław

The History of Sociology, Sociological Theory, and the Social Studies of Science: Mutual Uses and Misuses

Abstract

The author continues the debate, agreeing with criticism of the opening article by Radosław Sojak. At the same time, he points out that the proposals adduced therein do not concern the history of sociology, but sociological theory in general and do not provide any solution to the methodological dilemmas. The author ends his statements with pessimistic remarks about the identity of the discussed subdiscipline (i.e. history of sociology) as marginalized mainly by inadequate division of research labour.

Keywords : history of sociology, sociological theory, sociology.

⁴ This text was written as part of work on the research project funded by NCN No. 2013/09 / B / HS6 / 00302 Polish Sociology of the 1960s in the Context of International Sociology.