

Monika KRAUSE, *Model Cases: On Canonical Research Objects and Sites*
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Trying to write the history of a discipline is undoubtedly an ambitious undertaking, not least because the beginnings of a discipline and its differentiation are often difficult to discern or because—especially in the social sciences and humanities—there are very different traditions of research and thought, and cross-national communication about them has often proved problematic. These problems are probably the background for the claim made a couple of years ago by Austrian sociologist Christian Fleck¹ that the history of the historiography of sociology to date can hardly be described as a success story, regardless of whether we are speaking of daring attempts to tell the history of the discipline as a whole, more modest efforts to trace individual national traditions, or a historical approach exclusively focused on certain classical figures, such as the so-called founding fathers. There have been some very good attempts to write the history of (international) sociology and sociologists, to be sure. And yet: Fleck cannot help but present an enormously long list of deficits, which quite generally characterizes the deplorable state of the historiography of sociology. As he points out, (a) most of the biographies written by sociologists on the classics are written in a highly conventional way—at any rate in a style that is often far from the methodological standards of discussion expected, for instance, in the field of “intellectual history”;² (b) there are hardly any studies on the work and functioning of large and influential sociology departments and hardly any collective biographies of research groups;³ (c) there is almost no historical semantics of sociological concepts;⁴ (d) realistic assessments of the viability and fruitfulness of a theoretical research program (here in the sense of Imre Lakatos’s use) are hardly ever attempted;⁵ (e) there was and is not much interest in the question of how sociological concepts—once they have emigrated to other disciplines—unfold their effects there and have an impact on sociology via a kind of reverse migration process back to the

¹ Christian FLECK, 2015. *Skizze einer Methodologie der Geschichte der Soziologie*, in C. Dayé and St. Moebius, eds, *Soziologiegeschichte. Wege und Ziele* (Berlin, Suhrkamp: 34–111).

² *Ibid.*: 47.

³ *Ibid.*: 51.

⁴ *Ibid.*: 67.

⁵ *Ibid.*: 68.

original discipline;⁶ (f) to date, very few analyses of private funding agencies that support sociological research have been carried out;⁷ and (g) for authors in the history of sociology to take a close look at research institutions is comparatively rare; in fact, this perspective is more likely to be adopted in the history of science, i.e. by historians.⁸ In view of this diagnosis—and Fleck's characterization seems plausible and still valid today—any attempt by sociologists to scrutinize their own discipline in terms of its historical development will certainly be somewhat risky, since it will have to come to terms with criticisms such as those outlined by Fleck.

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With her new and—to anticipate the quintessence of this review—brilliant book, *Model Cases: On Canonical Research Objects and Sites*, Monika Krause, professor in sociology at the London School of Economics, does indeed venture into the shallows of sociological historiography, although it is striking that she undermines the categories mentioned by Christian Fleck from the outset. She neither attempts biographical sketches of classics, nor does she want to describe national traditions of sociological thought or take a closer look at individual departments and their influence or impact. What she presents in this comparatively short book (slightly over 125 pages of text) is, instead, a highly original argument which is based on the idea that, within the history of the discipline, individual branches of research have emerged and developed precisely *because*, at the beginning of the investigations, there was specific material and/or there were specific epistemic objects of research. Her convincing hypothesis is that these objects highlighted the importance of the respective research questions to all those who were only vaguely interested in the topic and thus then guided and shaped further research in the future.

[...] I offer a set of distinctions that can be used to examine patterns in the production of academic knowledge, starting from the distinction between material research objects on the one hand and epistemic research objects on the other hand. The material object is the specific object accessed through particular traces, produced by specific tools and instruments. It is defined by its role as a tool toward understanding something else, and it is distinguished from an epistemic research object, whatever it is that researchers are trying to understand—their target of inquiry, which is a conceptual entity and depends on specific intellectual and disciplinary traditions. [2]

⁶ *Ibid.*: 71.

⁷ *Ibid.*: 83.

⁸ *Ibid.*: 85.

More influenced by the history of science (including the history of the natural sciences) than by science and technology studies (STS), Krause is fascinated by the fact that in biology, for example, specific (and only specific) material objects are analyzed over and over again and that these repeated investigations of privileged objects (model cases) over a long period generate knowledge in a certain field of research. Just as, in the history of biology, certain creatures such as the fruit fly served—and still serve for various reasons—as privileged objects of investigation for the analysis of, for example, genetic mutations, so for the development of urban sociology only certain cities (Berlin, Chicago, or Mumbai) have been decisive, while other cities (for whatever reasons) have tended to be ignored [2]. Krause immediately makes it clear that the use of certain model cases and the consequent disregard of other cases cannot be characterized as either good or bad from the outset; the choice of objects—as with every choice—is always associated with advantages and disadvantages at the same time. However, if one wants to learn something about the history of the discipline, one should adequately appreciate the importance of the choice of these research objects, because only then will it become clear why the discipline has developed in a certain direction. “The fact that social scientists use model cases without reflecting on their use has mostly disadvantages. I will suggest that we can better exploit the advantages and limit the disadvantages of privileged material research objects by reflecting on the role they play.” [3] With this thrust, Krause indicates at the very beginning of her book that she is concerned with more than just a history of the discipline. As becomes clear in the course of the investigation, Krause immediately draws conclusions from her (historical) analyses and also provides (to my knowledge, this has rarely, if ever, been done in this way in a study of the history of sociology) advice for the course of future research; indeed, very concrete advice, about how good research should proceed and which traps are to be avoided. In this respect, Krause’s book is much more than a history of the discipline of sociology. Yes, the book deals with the history of the discipline, but it does so with the systematic intention of guiding future research; yes, it is a book about epistemology, but not one that uses abstract arguments within this field; instead it discusses epistemological problems on the basis of concrete research questions and thus nudges researchers in more fruitful directions. And no, it is not a book on sociological theory, but nevertheless it has enormous theoretical implications, because it shows how closely the starting point of research is related to the framing of theoretical problems.

This ambitious project is realized by Krause in six chapters. In the first chapter (“Material Research Objects and Privileged Material Research Objects”), her understanding of model cases—already mentioned in the introduction—is explicated once again, but now in a more elaborated form. As she explains with reference to the history of urban sociology, a privileged research object stands for something: “it is a stand-in for something else” [14]. New York’s East Harlem or Chicago’s Southside, from the perspective of researchers at the beginning of the 20th century, stood for certain forms and problems of urban structures; they stood for certain ways of life that were distinctly different from others, such as rural ones; these urban areas then served as model cases for further investigation and theorizing, so that epistemic targets then emerged that were considered quasi-self-evident in a certain scholarly and subdisciplinary tradition. Harlem and Chicago’s Southside only became canonical objects of research through the activities of the researchers studying them; studies of these urban quarters shaped the theoretical questions that would guide urban sociology’s further development. Krause even makes the plausible claim “that all research is using stand-ins in some way or another to explore the role that implicit or explicit conventions concerning material research objects and privileged material research objects play in some fields of research” [15]. Something very similar can also be shown with regard to the sociology of work and industrial sociology: Alain Touraine’s research on Renault, begun in the late 1940s, focused for various reasons on a very specific factory in Boulogne-Billancourt near Paris, and it was precisely this setting that again shaped further research, insofar as it was here that specific and at that time highly influential research questions would be raised [22] that would have a decisive impact—not least on the development of postwar sociology in Western Europe, since it was often *industrial* sociology which was at the center of sociology’s rise after 1945. Touraine and his colleagues’ choice of this factory was, not least, made for reasons of contingency; as already mentioned, it was close to Paris and the location was therefore easy for researchers to access. But no matter how problematic one may consider contingencies like this, it is also clear that with the establishment of such model cases, the debate within the subdisciplines of sociology of work/industrial sociology acquired a common focus, where knowledge could be accumulated. A privileged material object provided support and orientation for further research. The disadvantages of such privileging are, of course, also immediately obvious, the first being the resulting lack of variance in the cases, quite apart from the fact that it remains somewhat unclear how and whether the conclusions

drawn from the study of the privileged case can then be applied to other cases [32]. Krause's conclusion is a simple and, at the same time, a highly important one: Since model cases are almost constitutive for the emergence of subdisciplines within sociology, that has to be taken into serious consideration. What needs to be done in terms of better research practice today is clear: Instead of continuing to hand down research practice guided by (original) model cases in an unreflective manner and then simply transferring the conclusions drawn from them to other cases, sociology, according to Krause, must contextualize and historicize the original choice of model cases and then also focus on cases that have been neglected so far. To use my own words: Instead of always looking only at "Babylon" Berlin, it would be quite helpful if sociologists would do more research on Hamburg or Munich, or even do something which only German ethnologists seem to do these days; namely, study so-called medium-sized cities, in which forms of urbanity can be found that differ considerably from those in Berlin, but which are likely to be no less important for the overall development of (German) society.

In the second chapter ("How Material Research Objects are selected"), Krause explores the question of the reasons why certain research objects are selected and then become privileged, both in the past and present. Here, too, she is helped by analyses of the kind made for the history of biology, insofar as the selection criteria there are not so different from those in the social sciences. Such questions have, of course, been asked before (and Krause explicitly mentions Robert K. Merton here). But at the same time, Krause emphasizes that the rationalistic argumentation found in Merton's and others' work probably does not carry too far, because the contingency of object selection is, in fact, all too obvious. In this context, she rightly points out that not least funding agencies exert considerable influence on the choice of object or pre-structure a corresponding choice, which is of central importance, as entire research traditions are derived from it. Indeed, it sometimes happens that funding agencies control the categorical interpretation of the facts and thus have a concrete influence on the research results. This is a serious problem. However, perhaps even more interesting is the fact that the research process is set on a certain track from the very beginning by the choice of research objects. In this context, two additional points are made by Krause: First, the problem is not only that funding agencies, research conventions, subcultural prejudices, and stereotypes play a role; no less problematic in this context are teleological assumptions about the further course of history, insofar as researchers tend to concentrate on the supposedly "most advanced cases". Second, a research environment

shaped by activists, who, as it were, dictate the questions the discipline has to raise [45-50], is always something which should be carefully looked at in order to avoid one-sided conclusions being drawn. Krause—again at the end of the chapter—draws the following lesson from this: What sociology needs are systematic reflections on the status of those supposedly “most advanced cases,” studies that run counter to subcultural biases in the process of case selection or that focus precisely on those cases that are supposedly uninteresting or even taboo. To illustrate all this with an example from the field of economic sociology chosen by this reviewer: It is, after all, fair to ask whether the United States or China are the best cases by means of which to study modern capitalism, or whether studies of small countries like Switzerland or Singapore might not also provide considerable and maybe better insights, perhaps even about the future of capitalism.

The third chapter (“Model Cases and the Dream of Collective Methods”) brings to our attention certain peculiarities of model cases in the social sciences, and also points out that the analogies to biology, as drawn by Krause herself, only hold up to a certain point. Precisely because in the social sciences the number of possible material cases is rather small, and precisely because the variance of the cases is often also somewhat limited, it makes all the more sense to use one methodological tool much more frequently and systematically than in the past—*restudies* of the privileged material object! Krause’s advice—again at the end of the chapter—is to go into the specifics of the previously chosen object much more systematically than before and to try to analyze that object using new questions and categories. If this is not done properly, sociological research runs the risk of always asking the same questions, in a highly stereotypical way.

The fourth and fifth chapters (“How Subfield Categories Shape Knowledge” and “The Schemas of Social Theory”) essentially discuss two topics. On the one hand, Krause analyzes how certain periodizations and regional (ethnocentric) perspectives shape the choice of research objects. Such categories lead into the (well-known) problem that only Western phenomena and those associated with so-called modernity are judged worthy of investigation. On the other hand—and this seems a less obvious point—Krause calls for more reflection on the canon in sociology, since it can hardly be overlooked that the so-called classics and founding fathers of the discipline have shaped sociology to a considerable extent with their peculiar material objects of investigation and the theoretical questions resulting from these. Those who refer to Weber, Durkheim, or Marx in their research should make it explicit exactly why these authors’ works are treated as classics (and not simply as those

of “normal” colleagues who died a long time ago) and address the question of whether it would not perhaps also make sense to treat the authors of the classics “only” as colleagues for once, taking into account all their weaknesses, deficits, blind spots and idiosyncrasies. For, as Krause convincingly points out, the “canonization” or “consecration” of the work of certain authors as classics also has considerable side effects, which are anything but positive; this is because obvious research questions, which may have been asked in other cases, are simply overlooked [90]. Krause therefore makes the following suggestion:

I would highlight again the distinction between “application” and “comparison” in work that builds on previous work cast as theory. Application transfers insights and concepts derived from paradigmatic examples to other objects. “Comparison” compares the paradigmatic example as a material research object with other material research objects and uses this to consider and develop the insights and concepts of the original contribution further. We need more work that takes the stand-ins of approaches as empirical objects and compares them to other empirical objects. We can also look at paradigmatic examples of one approach with the tools of another approach. [98]

In the last chapter (“The Model Cases of Global Knowledge”) Krause then critically asks to what extent her discussion of model cases could be related to the debate on global knowledge, whereby she on the one hand welcomes the opening up of sociology toward postcolonial theory, but at the same time also warns against certain one-sided tendencies here. She critically points out that postcolonial theorists often aim at “application” rather than “comparison”, and thus run into the same problems as those of conventional sociology when they take the examples of the global south for granted. But she also warns against a sociology that moves too far into the waters of discourse analysis, as has occurred with at least some postcolonial theorists. In this respect, she advises sociologists to take a closer look at neglected cases of modernity than they have done so far, focusing above all on non-Western, non-postcolonial contexts, and to try out hitherto somewhat neglected combinations of regional and disciplinary approaches, precisely because—as this reviewer would put it—it is not self-evident that sociology is or should be primarily concerned with the “most advanced” countries, while ethnologists should somehow be responsible for the “rest”. In short, Krause pleads for a sociological imagination that transcends disciplines, regions, and epochs, a plea that is absolutely compelling from her analysis of model cases and their consequences.

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It should be clear by now that Monika Krause has presented us a book that is extraordinarily original and elegantly written and that one would

not have expected—as it is based on an enormous amount of reading—from a comparatively young author. In the historical analysis of model cases, Krause demonstrates her unsurpassable command of the sociological literature. But as has already been mentioned, Krause's book is much more than a foray into the history of the discipline. It is a theoretical book as well, since—with great self-confidence, and rightly so—she also draws theory-relevant conclusions from all her considerations. This supreme knowledge reminds this reviewer of Howard S. Becker's *Tricks of the Trade: How to Think about your Research While You're Doing It* (1998)—and by mentioning this name I want to make clear at the same time the kind of attention that this new book by Monika Krause deserves.

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