

# On Sociological Reflexivity

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## Abstract

This article offers a critique of the self-observation of the social sciences practiced in the philosophy of the social sciences and the critique of epistemological orientations. This kind of reflection involves the curious construction of wholes under labels, which are the result of a process of “distillation” or “abstraction” of a “position” somewhat removed from actual research practices and from the concrete claims and findings that researchers produce, share, and debate. In this context, I call for more sociological forms of reflexivity, informed by empirical research on practices in the natural sciences and by sociomaterial approaches in science and technology studies and cultural sociology. I illustrate the use of sociological self-observation for improving sociological research with two examples: I discuss patterns in how comparisons are used in relation to how comparisons could be used, and I discuss how cases are selected in relation to how they could be selected.

## Keywords

reflexivity, sociology of the social sciences, philosophy of the social sciences, participant objectification, comparison, case selection

When practicing social scientists discuss divisions among themselves, and choices open to them, they routinely reference theoretical and epistemological groupings. The way these groupings are labeled depends on the speaker’s position. Social scientists might contrast “scientific” or “analytical” research with “political” or “applied” work—a contrast that usually tries to elevate what is subsumed under “scientific” and devalue what is labeled “political.” Alternatively, scholars might distinguish “critical” or “interesting” research from “positivist” or “mainstream” scholarship, valuing the critical and devaluing work labeled as positivist.

These labels have a long history;<sup>1</sup> ideas about “science” play an important role in how symbolic distinctions are drawn. The oppositions expressed in such labels have been reinvigorated in recent years by defenders of orthodox positions and by those who critique “dominant epistemologies” or “Western science.”

Here, I take the claims implied in the use of these labels seriously as sociological claims. I offer a critique of the self-observation in the social sciences that underlies opposing positions in these conversations. I note that this self-observation involves the curious construction of

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wholes under labels, which are the result of a process of “distillation” or “abstraction” of a “position” somewhat removed from actual research practices and from the concrete claims and findings that researchers produce, share, and debate. In this context, I call for more sociological forms of reflexivity, informed by empirical research on practices in the natural sciences and by sociomaterial approaches in science and technology studies and cultural sociology.

I use two examples to illustrate how the kind of reflexivity I am advocating might help improve research in the social sciences. I first discuss how comparisons are used and discussed in sociology and adjacent fields. Moving beyond an opposition between “traditional” and “radical” comparisons allows us to consider the practical origins of specific conventions and the different ways in which we can use comparisons to pursue a range of goals in an accountable manner. In my second example, I discuss how cases are selected in the social sciences, highlighting factors that sponsor some cases over others in ways that are not related to the strategic aims of individual studies. I will argue that observations concerning collective empirical patterns in case selection can inform research strategies that either better exploit the concentration of attention on certain cases or mitigate against its disadvantages.

## REFLEXIVITY AS SELF-OBSERVATION

I draw on the German sociologist Andre Kieserling (2000, 2004) to ask about reflexivity as self-observation or self-description. Every field of practice can be examined with a view to what forms and what content it gives to its self-observation in these terms. We can thus ask, How does sociology, or how do the social sciences, observe and describe themselves? What are the different ways they do so, how are they distributed, and how does this distribution compare to the space of possible ways of observing themselves?

We can put these questions into dialogue with research on textbooks (Lynch and Bogen 1997), methodology textbooks, research articles (Abend, Petre, and Sauder 2013), the views of department heads (Akresh 2017), the construction of internal divisions (Calhoun, Duster, and VanAntwerpen 2010), peer review (Hirschauer 2010; Lamont 2009), hiring and promotion committees (Hamann 2019), and everyday conversation and gossip. A full review of the evidence being beyond the scope of this article, I would like to submit that sociology observes itself in a range of ways, which fill in the space of possibilities in an uneven manner, with some duplication and some options underused and underexploited.

I venture, more specifically, that two forms of self-observation are disproportionately represented. On the one hand, we have methodological reflection on the level of individual research projects: Here we find, for example, discussions about how to select a case, how to limit and control variation, how to interpret sources, and how to deal with missing data. This also includes the more specific notion of reflexivity in qualitative research understood as researchers’ examination of their feelings, motives, and position.<sup>2</sup>

On the other hand, self-observation of the social sciences has traditionally been heavily influenced by philosophical approaches. Reflection often focuses on the epistemological orientation of different kinds of research. Scholars have debated the virtues or faults of “realism” or “interpretivism,” for example. These labels are sometimes used in gestures of self-identification (e.g., “empirical-analytical sociology”) and sometimes ascribed (e.g., “positivism”).

We find this reflection in scholarly contributions but also in more everyday speech acts that involve the construction and evocation of opposing camps. In all cases, these labels are the result of a process of “distillation” or “abstraction” of a “position” somewhat removed from actual research practices and from the concrete claims and findings that researchers produce, share, and debate.

This kind of reflection involves the curious construction of wholes under labels, which usually carry normative baggage, with the whole's being either negative or positive. A range of authors and projects have used abstracted reflection on "the scientific method" to dismiss a range of other traditions. I would like to point out, however, that in this practice of distillation, critics of "the scientific method," "objectivity," or "Western science" often converge with its staunchest defenders.

If we take the claims made about sociology in the construction of these schools and camps seriously as *sociological* claims, as part of a sociology of the social sciences that would have to be part of a sociology of culture, the discussion of epistemological orientations would build on a certain understanding of culture. It would be an understanding of culture that focuses heavily on content and commitments—what used to be called "values"—explicit or ascribed, and relates it to either some higher good (science! reason!) or some higher bad (oppression! capitalism!).

This understanding of culture is macro-cultural, and it underlies critical notions of ideology and discourse. It is an understanding of culture that has long been challenged by micro-sociologists and against which I see many colleagues in the sociology of culture actively working (Becker 1984; Benzecry and Krause 2010; Calhoun and Sennett 2007; Dominguez Rubio 2014; Jerolmack and Khan 2014; Lizardo et al. 2016; Mangione and McDonnell 2013; Vaisey 2009; Zerubavel 1993). It should at least be contextualized within accounts of other factors shaping knowledge.

In a departure from this circular opposition between ideology and critique of ideology, I, along with others, see an opportunity to renew reflection on the social sciences based on sociological observations of research in different disciplines, observations that we judge by similar standards, which we would use to judge studies of the art world, of humanitarianism, or of religion. In these fields, as well as in the sociology of culture more broadly, sociologists largely have moved away from the remote diagnosis of ideological content as a substitute for analysis of actual practices and institutions.

There have long been calls for a serious sociology of sociology and a serious history of sociology (Calhoun 2007; Fleck 1999; Daye and Moebius 2015; Turner and Turner 1990), and the sociology of sociology is now a strong field of its own. This conversation also has borrowed from scholars engaged in social studies of the natural sciences (Knorr Cetina 1981b; Lynch and Bogen 1997; Woolgar 1988), resulting in a range of social studies of the social sciences (e.g., Camic, Gross, and Lamont 2011; Deville, Guggenheim, and Hrdličková 2016; Gieryn 2006; Guggenheim 2015; Kohler 2019; Lezaun 2007; Lezaun and Calvillo 2013; Michael 2004).

If we are concerned with reflexivity as a self-observation among practicing sociologists that can inform the improvement of the social sciences, the work in close dialogue with the social studies of the natural sciences has some distinct advantages. It is important in the context of my argument because of the role of ideas about "science" in camp-construction on both sides, which authors in this tradition are well equipped to address and sidestep. It is also especially useful because of the bridging done in this tradition between the context of knowledge production and its content, between concrete research practices and specific knowledge claims.

## LEARNING FROM EMPIRICAL SOCIOLOGICAL RESEARCH ON THE NATURAL SCIENCES

When we seek to complement the philosophy of the social sciences with a more sociologically informed reflection, we can remember that our own colleagues in the sociology of

science, and the social studies of science more broadly, have fought quite successfully to displace philosophy as a desk-based reconstruction of what good knowledge looks like in the abstract with a sociological orientation toward what research actually looks like. Here, I want to emphasize one important insight from empirical research on the natural sciences: the insight into the *disunity* of science (Galison and Stump 1996; Knorr Cetina 1991, 1999).

Until recently, self-reflection in the philosophy of the social sciences, as well as in broader conversations labeled as theory, has been dominated by a debate about whether the social sciences are or are not like “the natural sciences.” The idea of the natural sciences in the singular—albeit mediated by ideas about science among economists—was an important reference for the *Methodenstreit* in the nineteenth century, and it influenced both Weber and Durkheim when they outlined their methodological programs. It was carried forward in debates about positivism in the 1960s and 1970s and is still seen in labels like “scientific,” “empirical,” “positivist,” “quantitative,” “interpretivist,” “qualitative,” and “critical.” These are not only old debates; notions of “scientific” research informed, for example, the foundational statements of an association founded in the name of “empirical-analytical sociology” in 2017 as an alternative to the German Society for Sociology (Esser 2018; Hirschauer 2018).

This idea of “the natural sciences” that circulates in sociology has not been informed by empirical sociological research on the natural sciences, as Karin Knorr Cetina (1981b) noted already in 1981 with direct reference not only to her own pathbreaking research but also to the self-reflection of the social sciences.

Ethnographies and historical studies paying attention to the practice and material culture of science have shown how science is produced in particular locations, with particular practices and tools, and how this shapes the content and results of scientific work (Knorr-Cetina 1981a; Latour and Woolgar 1979; Lynch 1982; Star 1989). The precise legacy of these studies is and remains contested. Although “constructivist” in a broad sense, these accounts were never “relativist” in the way construed by some opponents. Research in this tradition consistently shows how much systematic work, effort, skill, and reflection goes into producing what counts as scientific results. We sometimes see how findings are contingent, contextual, historical, and temporary, but they are never simply arbitrary (Biagioli 1996).

Some scholars have chosen to be provoked by these studies in the name of “science”—hence the “science wars”—yet these findings challenge not so much the sciences but rather the philosophy of science and more particularly a specific philosophy of science that reduces all scientific activity to an idealized representation of theoretical physics.

What these works do show is that research within the natural sciences operates according to different logics, with diverse setups that are elaborate and complex. Scientists collect data in the field, study historical processes, work with abstracted data on computers, and are employed in laboratories that are themselves highly diverse.

If we take these findings seriously, and they are empirical findings, what are we to make of calls for sociological unity in the name of science? Jonathan Turner (1989:421, 432), for example, called in his Presidential Address to the Pacific Sociological Association in 1988 to “commit to science rather than ideology, smugness, and tolerance of any thought” and advocated for “a mature and well-integrated science of society” on these terms.

In light of the research on the diversity of scientific practices, I would suggest Turner’s (1989) gesture bears some similarity to populism. We do find a kind of populism in the self-representation of some social scientists, when they gesture toward “the people” or “the oppressed” in a way that suggests a unified referent and legitimizes the speaker, while leaving the actual benefits of the speaker’s utterance to those invoked unclear and unanalyzed. In addition to this populism of “the people,” there is also a populism of “science,” which we

see when the ideal of science, with its connotation of reason and progress, is entangled in an exercise of flag waving with a very loose link between that flag and the range of projects it legitimizes and celebrates. These projects may all be individually excellent, such as in the projects brought together under “empirical-analytical” sociology, but their excellence is diverse and cannot be easily labeled in a way to exclude others who rely on research practices that are accountable in their own ways.

Quantitative research in the social sciences, for example, is not in any simple way like scientific work, and different forms of quantitative research are not like each other, as has been highlighted, for example, in a recent analysis of the implications of the rise of big data for social research (Savage and Burrows 2017). Practices associated with claims to scientificity are not alike “in a good way.” But neither are they alike in a bad way, which is worth emphasizing in contexts where general critiques of “Western science” or “dominant epistemologies” present a homogenized picture of “mainstream” sociology. Because of the disconnect between labels and research practices, I would suggest it would be misleading to call these conflicts “epistemological conflicts.” Rather, these are epistemicized conflicts, meaning they involve other stakes and cannot be explained by the construed coherence of the labels involved.

## WHAT KIND OF PARTICIPANT OBJECTIFICATION?

In a lecture at the British Royal Anthropological Institute in 2000, Pierre Bourdieu (2003:281) proposed the term “participant objectification” for his vision of reflexivity for sociologists:

Scientific reflexivity stands opposed to the narcissistic reflexivity of postmodern anthropology as well as to the egological reflexivity of phenomenology in that it endeavours to increase scientificity by turning the most objectivist tools of social science not only onto the private person of the enquirer but also, and more decisively, onto the anthropological field itself and onto the scholastic dispositions and biases it fosters and rewards in its members. “Participant objectivation”, as the objectivation of the subject and operations of objectivation, and of the latter’s conditions of possibility, produces real cognitive effects as it enables the social analyst to grasp and master the pre-reflexive social and academic experiences of the social world that he tends to project unconsciously onto ordinary social agents.

In this passage, Bourdieu distinguishes between the reflexivity of individual researchers and sociologically informed reflection in a way I broadly follow. In my view, however, his use of the terms “narcissistic” and “scientific” is misleading. I agree it is important to distinguish sociological from individual researchers’ reflexivity, but I would not argue the latter should be rejected. Reflexivity concerning individuals’ background, individual research projects, and specific research practices will continue to be important. If we grant that the fetishization of individual reflexivity, widespread at the time of Bourdieu’s writing, has problematic aspects, it should not, for reasons discussed above, be rejected in the name of “scientificity.”

Bourdieu and others using his concepts have made important contributions with sociological research on sociology and related fields. Attention to particular dynamics of fields helps avoid two tendencies: “an internal reading of the text which consists in considering the text in itself and for itself” and “an external reading which crudely relates the text to society in general” (Bourdieu 2005:32–33). Both tendencies are present in critiques of the social sciences that are influenced primarily by discourse-theoretical approaches.

I would highlight research on internationalization and its unequal effects by Marion Fourcade (2006), Fernanda Beigel (2014a, 2014b), and Johan Heilbron (Heilbron 2014; Heilbron, Guilhot, and Jeanpierre 2008) as well as research on sociology's imperialist past by George Steinmetz (2007b; 2013; 2016). A Bourdieusian approach also can help us explain the epistemized conflict discussed above through the analysis of field dynamics (Calhoun and VanAntwerpen 2007; Calhoun et al. 2010; Schmitz et al. 2020; Steinmetz 2007a; see also Abbott 2001) and as a result of what Bourdieu (1990) calls "the scholastic point of view."

Yet participant objectification need not be limited to Bourdieusian methods specifically. For a reflexivity that can inform research practices, we can try to get closer to actual content, to specific practices, and to what people are saying in specific papers. I have been inspired by the ethnographic, situation-oriented research that has done much to bridge the divide between content versus externalist approaches to the sciences and social sciences. I am inspired by the attention to sites, objects, and practices of knowledge in science and technology studies (STS). But in defending my own past papers, I would argue that we also can ask questions about the aggregate of output, in the tradition of sociologists of science rather than STS, while staying attuned to content and specific knowledge claims. Here, we can follow Robert Merton (1987:2), who framed his work in the sociology of science as an inquiry into the "cognitive and social patterns in the practice of science."

I have had discussions with a very thoughtful historian of science who questions whether it is possible to examine sociology as a form of knowledge and its specificities as a sociologist, from the inside (see Franzen et al. 2019), but I would argue we are quite used to wearing different hats at different times. We can, based on observed patterns, compare assumptions and reality, compare reality to the space of possibilities, and discuss some paths less traveled that might be useful for research.

## HOW DO SOCIAL SCIENTISTS PRACTICE COMPARISON?

We can ask, for example, how scholars in the social sciences practice comparison (Deville et al. 2016; Krause 2016). In this area, we can find some evidence for a polarization in the metaconversation, in which the idea of "science" as a singular entity has played an important role. Some scholars have policed a very narrow definition of comparison in the name of "science" (and we might add "progress" and "reason"). This policing is found in written contributions but perhaps even more so in peer review and informal conversations. It is present in the unquestioning reference to the supposed fact that it is impossible to compare apples and oranges. Other scholars have rejected comparisons as a research strategy entirely, in the name of the unique or in the name of resistance against dominant epistemologies or against power relationships more generally. Steinmetz (2004) has analyzed the long history of this latter tradition in an important article.

Recently, scholars in "unorthodox" or "anti-orthodox" traditions in different disciplines have begun revisiting comparisons (Fox and Gingrich 2002; Jensen 2011; McFarlane and Robinson 2012; Robinson 2004; Scheffer and Niewöhner 2010; Sorensen 2008). This work provides thoughtful reflection and exemplary pieces of research, yet authors in these conversations sometimes burden each other's research with the weight of expectations of transgression, resistance, or innovation in a way that may not be realistic or helpful.

Yet if we take a closer look at the practices associated with traditional forms of comparison in the social sciences, we see it is not an abstract commitment to the "scientific method" or to "positivism" as an ideology that is driving them. Rather, particular research problems in particular corners of the life sciences serve as a model for particular quantitative techniques, which then shape understandings of research more broadly (Krause 2016; Stengers 2001).

Among these particular practices in particular corners of the life sciences, I would highlight the clinical trial. In emulating the clinical trial, social scientists have imported certain assumptions, such as the assumption that explanation is the main goal of comparative research. Moreover, it is often implied that we should seek a particular kind of causal explanation, one associated with what Andrew Abbott (1988, 2005) calls general linear reality. This kind of explanation posits a clear separation between cause and outcome. It sees causal factors in competition with each other, like in a horse race; it is then usually thought to be important to establish how important variables are in relation to other variables. While there are important differences between comparisons using statistical techniques and small-*n* comparisons in American sociology (Liebersohn 1991; Mahoney and Goertz 2006), some assumptions about the importance of explanation are shared, which has had a significant impact on how “culture” and “the state” have been conceptualized, for example.

Some practitioners of these particular forms of research actively associate these practices of explanation with abstract talk of the “scientific method.” Critical accounts at times participate in this abstraction. By construing “orthodox comparison” as an opponent, they have tended to overstate the homogeneity of traditional practices of comparison. When they call this opponent “Western science” or “colonial anthropology,” they fold this homogenized construction into even larger systems of domination.

In reality, practices of comparison have always been even more diverse in the natural sciences and in the social sciences. In the context of the COVID-19 pandemic, for example, we are all very interested in clinical trials in the search for vaccines and treatments, but clinical trials are only a very small part of research and comparative practice even in this specialized area. The comparisons between assumptions and scenarios used by epidemiologists are very different from the comparisons used in the clinical trial in aim and in procedure. Different again are the comparisons virologists use when trying to understand the clinical effects of SARS 2 in the context of SARS 1, MERS, influenza, HIV, dengue, and the mosquito-borne virus chikungunya. The comparison between chikungunya and SARS 2, for example, provides hypothesis for how we can explain observations about the long-term effects of COVID-19 in some patients.<sup>3</sup>

I have argued that if we broaden the range of aims for which comparisons are useful, such as the full range of kinds of explanation (Abbott 2016; Abend 2020; Pickvance 2001), but also description and critique, we can identify forms of comparison that are useful but relatively underused. Here I would like to highlight one type of underused comparison: the asymmetric comparison (Krause 2016).

One assumption that seems to limit current practices of comparison in the social sciences is that all compared cases need to be given equal amounts of attention. In writing and assessing explicitly comparative research designs, we spend a lot of time thinking about which cases should or should not be included. Meanwhile, most qualitative designs still consider only one case, and both single-case and explicitly comparative studies spend too little time thinking about other relevant cases and literatures (or about the larger diversity in the world), which could be considered by drawing from information that is widely available or drawing on less in-depth research.

There are reasons to be skeptical of asymmetric comparisons, but we should examine carefully how and whether these reasons apply for any given research project. First, if the concern is for the comparison to be a staging ground for a fair competition between causes, then it does seem important to provide controlled and equal conditions for all competitors. However, this is not relevant for all kinds of explanatory projects, nor is it relevant for comparisons that aim at conceptualization (Krause 2016; see Mahoney 2007; Tilly 1984).

Second, the preference for symmetrical comparison, that is, comparison where the same attention is devoted to all cases, seems to be related to concerns about measurement or

description. Symmetry of attention appears as the default if measurement has no costs because researchers are working with existing data sets; it also can appear as a default if measurement is unproblematically assumed to be either true or false. If measurements or descriptions are either correct or incorrect, there is no justification to consider information that is of lower quality.

Yet most social science research encounters more complicated trade-offs with regard to questions of measurement. We face trade-offs between different aims of research projects; quality of information is balanced with other strategic aims. Although we tend to not make them explicit, many kinds of comparisons go into conceptualization and description, including in the conceptualization and description that are the foundation also of explanatory projects. Most of these comparisons are asymmetrical. Of course, we would expect empirical research to contribute new in-depth, high-quality knowledge on some cases, but such research can be enriched with information about other cases from secondary sources (and even by pure thinking exercises).

## HOW DO SOCIAL SCIENTISTS SELECT CASES?

I have suggested that self-observation of the social sciences concerning comparative research has been focused on epistemological orientation, which has been distilled out of and away from concrete research practices, and that it might be useful to look more closely at how comparisons are practiced. We are beginning to see strong (auto-)ethnographies of how comparison is practiced in the social sciences (Akrich and Rabeharisoa 2016; Deville et al. 2016; Stöckelová 2016); it can also be useful to look at how comparison is practiced across studies in terms of its space of possibilities.

I want to offer another example of the kind of conversations we might have based on the kind of self-observation I am proposing: I have been interested in the way biologists use model systems, like the fruit fly or the mouse. In an article with Michael Guggenheim (Guggenheim and Krause 2012), I compared what social scientists do to what biologists do in this regard (see also Creager, Lunbeck, and Wise 2007; Howlett and Morgan 2010; Krause 2021).

Biologists select particular organisms for study, which they use to answer questions about life and death, development, and disease. Some organisms are more frequently used than others and have become “model organisms,” attracting disproportionate amounts of attention and resources and shaping whole fields of research. The fruit fly and the mouse are perhaps the most famous of these, but we also can think of rats, dogs, and frogs as well as particular kinds of viruses, bacteria, or whole ecosystems, mostly islands (Kueffer, Pyšek, and Richardson 2013).

Based on this and on work by philosophers of science on interdisciplinarity (Löffler 2010; Padberg 2014), I would distinguish between material research objects and epistemic research objects. 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 for 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. I then ask how material research objects are chosen (Krause 2021:44–53). This is different from asking how they should be chosen, which is the subject of a strong methodological and theoretical literature (Burawoy 1998; Chen 2015; Ermakoff 2014; Flyvbjerg 2006; Gerring and Cojocar 2016; Merton 1987; Passeron and Revel 2005; Ragin and Becker 1992; Tavory and Timmermans 2014). With this, I am proposing to take a detour that we often ask of our students and of professionals in other



fields: to ask also about empirical patterns among practitioners even as we are motivated to respond to normative and practical concerns.

Sociologists do not empirically choose cases entirely to maximize epistemic gain in accordance with whatever methodological principles they espouse, which has long been shown to be true for biologists (Clarke and Fujimura 1992). A range of factors lead to the choice of some material research objects over others; these include relatively mundane vectors of influence and are not reducible to those identified by overarching critiques of ideology.

*Convenience* as a factor is both banal and almost infinitely complex. The most intrinsic (“the easiest way to study important phenomenon x”) to the most extrinsic (“funding was provided by the military”) can be discussed under the label of convenience. I want to use the term here to discuss nonstrategic, mundane, logistical aspects of convenience. Research sites and objects in a range of substantive areas are chosen because they are situated in an area where universities and researchers concentrate. As Mario Luis Small—whose discussion of the role of Chicago in urban sociology has been very important for me (Small 2007, 2014)—observed, PhD students who do not do what, relative to the location of their university, is called “research abroad” or “international fieldwork” very often conduct their qualitative studies near their university, with the common exception being students who have partners or family elsewhere.

Historicism is also a major factor in privileging some cases; there seems to be a strong bias, certainly in sociology and political science, in favor of material research objects that can be framed as “the most advanced” within their category. Thinkers associated with post-colonial thought have identified one very consequential version of this, which I would call *macro-historicism*: the privileging of some countries as the most advanced countries (Chakrabarty 2000). The notions of “modernity” and “development” have posited a movement in history and situated cases within that history even though they coexist in the presence. These ideas have been used to justify a focus on some countries rather than others and to imply that lessons from these countries can be transferred to other cases, even if it may take some time. This has severely limited attention to the Global South and the range of issues that cases from the Global South have been made to speak to.

There is another version of the focus on “advanced cases,” which I call *micro-historicism*, that is, a focus on the “most advanced” cases within a subfield. Urban sociologists focus on growing cities on the assumption that to study growing cities is to study the future, it is to study what other cities will become. Similarly, to study work often means studying the most advanced forms of work. The developmentalist orientation is often normatively charged; as such, depending almost on the temperament of the authors involved, it can take on a utopian or a dystopian bent. The most advanced forms of work might be studied to examine the forms of contradiction or resistance they might generate. In a different version of critical work, the most advanced forms of governmentality might be studied to understand the future of a society of control.

Cognitive scientists (Lakoff 1987; Rosch 1973) and cognitive sociologists (Brekhus 2015; DiMaggio 1997; Zerubavel 1993) have shown that categories are understood in schematic ways in the general population, and experts and scientists are no exception to this (Griffiths and Stotz 2008). To the extent that social scientific categories are also popular categories, such popular *schemas* influence case selection among academics. For example, U.S.-based multinational firms that own strong brands are heavily overrepresented in international management research (Collinson and Rugman 2010). This bias in research studies might coincide with the cognitive bias in the general population vis-à-vis the category “corporation.”

In one of my own fields of research, the study of humanitarian relief, scholars have focused heavily on nongovernmental organizations (NGOs) as opposed to states in affected regions, say, or even the United Nations, and among NGOs, they have in particular focused on one NGO, Doctors without Borders (Fassin 2010; Fox 1995; Redfield 2013). There are a number of reasons for this, including some good scholarly reasons in each individual study, but it is overall somewhat disproportionate, and it coincides with the general public's high level of recognition of that organization among international charities.

Another set of hypotheses concerns the influence of schemas that researchers carry not by virtue of being members of the general population but by virtue of inhabiting a particular social world or position. This can be conceptualized as a position that intersects with class and racialized and gendered hierarchies or as a profession-specific disposition. But I wish to also highlight the more mundane, subcultural aspects of this: Until recently, scholars of social movements tended to study movements they liked; there have been fewer studies of the extreme Right and of racist mobilizations. Academics' experiences inside their own workplaces has spurred interest in managerialism, and the university features prominently in discussions of audit culture, rankings, and neoliberalism (Espeland and Sauder 2016; Strathern 2000). If we explore hypotheses about the effects of scholarly subcultures, we also can explore variation within and across scholarly communities in different disciplines, different social groups, and different parts of the world.

The factors that promote different research objects shape our knowledge in a way that is sometimes reflected within subfields but rarely across them. In some fields, conventions develop around privileged material research objects, whereby norms develop that say we *should* be studying overstudied cases. In such instances, despite research on other cases, the central cases are seen as the basis of general insight and theory. Researchers in biology and related life sciences are explicitly encouraged to focus their attention in this way. They also reflect explicitly on this. I would argue that we could exploit concentration of attention better and mitigate disadvantages better if we also reflected more explicitly on collectively privileged cases.

## CONCLUSION

I have noted that the self-observation of the social sciences often focuses on epistemological orientations of different kinds of research, discussed at some remove from actual research practices, tools, or any actual research questions we might be asking and any claims we might be making. The focus on epistemological groupings is a feature of the traditional philosophy of science; it has been reinforced in recent years by some "orthodox" attempts to draw a distinction from work that authors consider "too political" but also by critics of "dominant epistemology" or "Western science" who have been influenced by philosophical and macro-cultural approaches rather than by the full range of tools of sociological inquiry.

Ideas about "science" continue to play an important role in both the projection and the critique of overarching ideologies. In this, sociologists often do not take into account findings from the social studies of science, which show that the notion of science as a singular entity has no referent, in either a positive or a negative sense. There is no platonic idea of science up in the sky, or over there with the physicists, biologists, or virologists. Nor is there a unified hegemonic apparatus as some critics imply. There are different kinds of systematic research practices, which we could learn from and which we could take seriously also within the community of social scientists, which is diverse along multiple axes.

I have discussed a few of the observations we can make when we look at sociology, and to some extent the social sciences more broadly, not through these epistemicized oppositions but around them. I would suggest that sociological self-observation can be used to speak to normative debates in the discipline concerning how we can improve sociological research. My observations about how cases are selected and how they could be selected, for example, raise questions of collective methodology as well as individual methodology.

We could spend more time discussing different aspects of collective methodology in addition to individual methodology; that is, we can discuss choices for individuals in light of observations about patterns in everyone else's choices, with a view to what different projects add up to. When I speak of collective methodology, I am not thinking of individual or school-based attempts to standardize the terms we use. Nor am I thinking of individual or school-based proposals for better "epistemologies" or better "ontologies."

I am thinking of more piecemeal efforts. We should try to improve how we link the findings of different studies, produced with different methods and approaches. We also can try to use an analysis of patterns in knowledge production to avoid duplication in our collective output and to remedy geographic, case-based, and school-based provincialism.

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## NOTES

1. See, for example, Calhoun and VanAntwerpen (2007), Dayé (2016), Steinmetz (2014), and Turner (1994). See Krause (2021:69–71).
2. See, for example, Pillow (2003) and contributions to two special issues of *Forum Qualitative Sozialforschung*, edited by Katja Mruck, Wolff-Michael Roth, and Franz Breuer (2002, 2003).
3. Immunologist Danny Altmann, speaking on the *Guardian's* science weekly podcast on July 7, 2020 (<https://www.theguardian.com/science/audio/2020/jul/07/covid-19-why-are-people-suffering-long-term-symptoms-podcast>).

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