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Regarding Sociological Reflectivity

Dr.ECCLESTON

Assistant Professor, Department of sociology, Gondwana University, Gadchiroli Magarashtra

Abstract -

The self-observation of the social sciences as it is applied in the philosophy of the social sciences is criticized in this article, along with many epistemological stances. The intriguing construction of wholes under labels that arises from a process of "distillation" or "abstraction" of a "position" that is somewhat divorced from actual research practices and from the tangible claims and findings that researchers generate, disseminate, and debate is what this type of reflection entails. In light of this, I advocate for more sociological reflexivity that is based on sociometrical methods used in science and technology studies, cultural sociology, and empirical study on practices in the natural sciences. I provide two instances to show how sociological self-observation can be used to enhance sociological research: I talk about patterns in the application of comparisons in respect to possible applications of comparisons, and I talk about the process of selecting examples in regard to potential methods of selection.

Keywords: case selection, comparison, reflexivity, participant objectification, sociology of the social sciences, and philosophy of the social sciences.

Introduction

Practicing social scientists frequently make reference to theoretical and epistemological groups when talking about divisions within their own field and options available to them. The speaker's stance determines the labels assigned to these groupings. Social scientists may draw a distinction between "political" or "applied" work and "scientific" or "analytical" study; this distinction typically aims to discount the latter and elevate the former. As an alternative, academics should value the critical work and devalue the positivist or "mainstream" scholarship differently, rewarding "critical" or "interesting" research.

These designations have a lengthy history, and how symbolic differences are made is heavily influenced by concepts about "science." The opponents of "dominant epistemologies" or "Western science," as well as proponents of conventional wisdom, have rekindled the oppositions conveyed by these terms in recent years.

Here, I treat the inferred sociological claims made by the application of these labels seriously. I present a critical analysis of the social science self-observation that forms the basis of contrasting viewpoints in these discussions. As a result of a process of "distillation" or "abstraction" of a "position," which is somewhat removed from actual research practices and from the tangible claims and findings that researchers produce, share, and debate, I note that this self-observation involves the curious construction of wholes under labels. In light of this, I advocate for more sociological reflexivity that is based on sociometrical techniques found in science and technology studies, cultural sociology, and empirical study on practices in the natural sciences.

I offer two instances to show how the kind of reflexivity I am promoting could enhance social science research. I start out by going over how comparisons are utilized and debated in the

domains of sociology and related ones. By putting aside, the dichotomy between "traditional" and "radical" comparisons, we may examine the useful histories of particular practices and the various ways in which we can employ comparisons to responsibly achieve a variety of objectives. In my second illustration, I go over the selection process used in the social sciences, emphasizing elements that support certain cases more than others in ways unrelated to the strategic goals of particular research projects. In my argument, research methodologies that either better capitalize on the focus on specific cases or counteract its drawbacks can be informed by insights of collective empirical trends in case selection.

Reflection as Self-Awareness

Inquiring about reflexivity as self-observation or self-description, I draw on the work of German sociologist Andre Kieserling (2000, 2004). It is possible to analyse any field of practice by looking at the shapes and contents that it provides for its self-observation in these terms. So, we can inquire, how do the social sciences, or sociology, see and define themselves? In what ways do they observe themselves, in what ways are they distributed, and how does this distribution relate to the space of possible self-observations?

More precisely, I hazard a guess that there is an imbalance in the representation of two types of self-observations. On the one hand, we have methodological reflection at the level of specific research projects. Here, we can discover conversations regarding issues like case selection, variation control and limitation, source interpretation, and handling missing data. This also encompasses the more focused idea of reflexivity in qualitative research, which is defined as the investigation of the feelings, intentions, and perspectives of researchers.

Conversely, philosophical methods have historically had a significant influence on the social sciences' self-observation. The epistemological stance of various research types is a topic of reflection that is frequently addressed. For example, academics have argued about the benefits and drawbacks of "interpretivism" and "realism." These terms are applied (e.g., "positivism") and employed in self-identification gestures (e.g., "empirical-analytical sociology").

This contemplation can be found in academic writings as well as in speech acts that are more commonplace and entail the creation and invocation of opposing camps. These designations are always the outcome of a process of "abstraction" or "distillation" of a "position," which is done at a distance from the actual methods of research as well as the tangible conclusions and assertions that scientists generate, disseminate, and discuss.

In this type of thinking, wholes are curiously constructed under labels—labels that typically contain normative connotations—and can be either positive or negative. Abstracted contemplation on "the scientific method" has been employed by a variety of writers and projects to discredit a variety of alternative traditions. However, it should be noted that in this process of distillation, those who are critical of "the scientific method," "objectivity," or "Western science" frequently align with those who are its most ardent supporters.

The discussion of epistemological orientations would be based on a particular understanding of culture if we take the claims made about sociology in the building 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. It would be a cultural theory that places a strong emphasis on commitments and substance, or what were formerly known as "values," whether

they are stated or attributed, and connects them to either a higher good (science! reason!) or a higher ill (oppression! capitalism!).

This macro-cultural perspective of culture forms the basis for critical concepts of discourse and ideology. Many colleagues in the sociology of culture are actively working against this understanding of culture, which has long been contested by microsociologists (Dominguez Rubio 2014; Jerolmack and Khan 2014; Lizardo et al. 2016; Mangione and McDonnell 2013). At the very least, it needs to be understood in the context of other factors that influence knowledge.

I, along with others, see an opportunity to reexamine the social sciences based on sociological observations of research in various disciplines, observations that we evaluate according to the same criteria that we would use to evaluate studies of the art world, humanitarianism, or religion. This breaks from the circular opposition between ideology and critique of ideology. In these domains and the sociology of culture at large, sociologists have mainly shifted away from the investigation of actual activities and institutions as a substitute for the remote diagnosis of ideological content.

There are some clear benefits to working closely with the social studies of the natural sciences if reflexivity—a self-observation among practicing sociologists that can guide the advancement of the social sciences—is a concern. It is significant in the context of my thesis because writers in this tradition are well-suited to discuss and avoid the role that notions about "science" play in the building of camps on both sides. Additionally, it is particularly helpful because of the bridging that this tradition accomplishes between the actual research methods and the particular knowledge claims, as well as between the context of knowledge production and its content.

Recognizing Patterns in Empirical Sociological Studies of Natural Science

We should keep in mind that our colleagues in the sociology science and the social studies of science more generally have fought quite successfully to replace 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. This is important to keep in mind when we seek to complement the philosophy of the social sciences with a more sociologically informed reflection. I want to highlight here a key finding from empirical studies in the natural sciences: the understanding of science's disunity (Galison and Stump 1996; Knorr Cetina 1999).

Until recently, the question of whether or not the social sciences are similar to "the natural sciences" dominated discussions of self-reflection in the philosophy of the social sciences and in more general discourse referred to as theory. The nineteenth-century Metrocentric drew heavily from the concept of the natural sciences in the singular, albeit via the lens of beliefs about science held by economists. This concept affected Weber and Durkheim as they developed their methodological frameworks. It was continued in positivist discussions during the 1960s and 1970s, and terms like "scientific," "empirical," "positivist," "quantitative," "interpretivist," "qualitative," and "critical" still refer to it. These discussions are not limited to the past; ideas about "scientific" research also influenced the founding declarations of an organization established in 2017 under the moniker "empirical-analytical sociology" as a rival to the German Society for Sociology (Esser 2018; Hirschauer 2018).

Studies that focus on the practice and material culture of science, such as ethnographies and historical accounts, have demonstrated how science is produced in specific places using specific methods and equipment, and how this influences the outcomes and content of scientific research (Star 1989). There is ongoing debate on the exact legacy of these findings. These accounts were never "relativist" in the sense that some opponents have understood it, despite being "constructivist" in a wide sense. This legacy of research continuously demonstrates the amount of methodical labour, expertise, effort, and contemplation required to produce conclusions that are considered valid in science. Though discoveries are occasionally contingent, contextual, historical, and transient, they are never purely random (Biagioli 1996). These findings challenge not so much the sciences as they do the philosophy of science, and more specifically, a particular philosophy of science that reduces all scientific activity to an idealized representation of theoretical physics. Some scholars have chosen to be provoked by these studies in the name of "science," which is why the "science wars" have arisen.

These works do demonstrate; however, how various logics and intricate, multifaceted setups are employed in natural scientific research. Scientists work in laboratories that are extremely different in and of themselves, collect data in the field, research historical processes, and manipulate abstracted data on computers.

I would argue that there are some similarities between populism and Turner's (1989) gesture, given the studies on the diversity of scientific practices. When social scientists make gestures toward "the people" or "the oppressed" in a way that suggests a single referent and validates the speaker, we do detect a form of populism in their self-representation. However, the benefits of the speaker's utterance to those invoked are left vague and unanalysed. There is a populism of "science" in addition to this populist of "the people." This is evident when the ideal of science—which is associated with reason and advancement—becomes embroiled in a flag-waving exercise where there is only a tenuous connection between the flag and the variety of initiatives it supports and legitimizes. Even though some of these projects, like those grouped together under the heading "empirical-analytical" sociology, are undoubtedly good on their own, their quality is diverse and cannot be simply labelled in a way that excludes others who depend on research procedures that are accountable in their own unique ways.
As has been noted, for instance, in a recent analysis of the implications of the rise of big data for social research, quantitative research in the social sciences is not in any simple way like scientific work, nor are different forms of quantitative research like each other (Savage and Burrows 2017). Methods linked to scientific assertions are not "positively" similar. However, it is important to note that neither of these is identical in a negative sense, especially in situations when broad criticisms of "Western science" or "dominant epistemologies" paint a uniform picture of "mainstream" sociology. I would argue that calling these conflicts "epistemological conflicts" would be misleading due to the discrepancy between labels and research methodologies. Instead, they are epistemic zed conflicts, which means that other factors are at play and that the labels' perceived coherence is insufficient to explain them.

Which Participant Objectification Type Is It?

In a lecture at the British Royal Anthropological Institute in 2000, Pierre Bourdieu (2003) 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 ecological reflexivity of phenomenology in that it endeavours to increase scientific 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," defined as the objectivation of the subject and operations of objectivation, as well as the latter's conditions of possibility, has real cognitive effects because it allows the social analyst to grasp and master the pre-reflexive social and academic experiences of the social world that he unconsciously projects onto ordinary social agents.

In this line, Bourdieu draws a distinction between individual researchers' reflexivity and sociologically informed reflection, which I basically agree with. In my opinion, however, his usage of the phrases "narcissistic" and "scientific" is misleading. I agree that it is vital to distinguish between sociological and individual researchers' reflexivity, but I do not believe the latter should be ignored. Reflexivity about individuals' backgrounds, individual study initiatives, and specific research techniques will remain relevant. If we accept that the fetishization of individual reflexivity, which was common at the time of Bourdieu's work, has problematic characteristics, it should not be rejected in the name of "science" for the reasons stated above.

Bourdieu and others who used his notions have made significant contributions to sociological research in sociology and allied sciences. Attention to specific dynamics of fields helps prevent 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 heavily inspired by discourse-theoretical approaches.

George Steinmetz has also conducted research on sociology's imperialist past. A Bourdieusian approach can also help us explain the epistemic zed conflict discussed above by analysing field dynamics (Calhoun et al. 2010; Schmitz et al. 2020) and what Bourdieu (1990) refers to as "the scholastic point of view."

However, participant objectification is not confined to Bourdieusian approaches. To foster reflexivity that can inform research processes, we can aim to get closer to actual content, specific practices, and what individuals are saying in specific papers. I've been influenced by ethnographic, situation-oriented research, which has helped to bridge the gap between content and externalist approaches to the sciences and social sciences. Science and technology studies (STS) inspires me with its emphasis on knowledge sites, objects, and activities. To be clear, I am not advocating for STS; rather, I am arguing that, while remaining attentive to content and particular knowledge claims, we can also raise questions about the overall output, following the tradition of sociologists of science. Taking up Robert Merton's (1987) approach, we might understand his sociology of science as an investigation of the "cognitive and social patterns in the practice of science."

Though I would argue we are quite accustomed to wearing different hats at different times, 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 out (see Franzen et al. 2019).

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How Do Social Scientists Practice Comparison?

For example, we can inquire about the methods used by social science researchers when conducting comparisons (Deville et al. 2016; Krause 2016). There is some evidence in this regard for polarization in the meta-conversation, where the concept of "science" as a single entity has been central to the debate. In the sake of "science" (and, we might add, "progress" and "reason"), some academics have policed an extremely restricted concept of comparison. Written contributions are subject to this policing, but informal discussions and peer review may be much more so. It is evident in the acquiescent citation of the purported impossibility of comparing apples and oranges.

Re-examining similarities has been the recent focus of academics from "unorthodox" or "anti-orthodox" backgrounds across various disciplines (Jensen 2011; McFarlane and Robinson 2012; Scheffer and Niewöhner 2010). Thoughtful consideration and excellent research are presented in this book, but occasionally writers in these discussions place undue pressure on one another's studies in an attempt to meet unrealistic or counterproductive standards of transgression, resistance, or innovation.

However, a closer examination of the procedures connected to conventional comparison models in the social sciences reveals that their motivations are not an impersonal dedication to the "scientific method" or to the ideology of "positivism." Instead, specific research issues in certain domains of the life sciences function as a template for specific quantitative methods, which in turn Mold perceptions of research in general (Krause 2016).

The clinical trial is one of these specific procedures used in certain areas of the life sciences that I would like to draw attention to. Social scientists have imported some presumptions into their simulation of the clinical trial, such as the notion that the primary objective of comparative research is explanation. Furthermore, it is frequently inferred that we ought to look for a specific form of causal explanation, one that is connected to general linear reality, as defined by Andrew Abbott (2005). This type of explanation suggests that cause and effect are clearly distinct from one another. It perceives causative elements competing with one another, much to a horse race; hence, it is typically seen significant to determine the relative importance of variables. Although statistical comparisons and small-n comparisons in American sociology differ greatly (Mahoney and Goertz 2006), there are some shared assumptions regarding the significance of explanation. These shared assumptions have greatly influenced the conceptualization of concepts such as "culture" and "the state."

In actuality, comparing methods in the social and scientific sciences have always been even more varied. For instance, in the COVID-19 pandemic scenario, clinical trials are of great importance to all of us in the hunt for vaccinations and treatments. However, even in this specialized field of study, clinical trials make up a very small portion of research and comparative practice. In terms of purpose and methodology, the comparisons made between scenarios and assumptions by epidemiologists differ greatly from those made in clinical trials. The comparisons that virologists make in order to comprehend the clinical impacts of SARS 2 in relation to SARS 1, MERS, influenza, HIV, dengue, and the mosquito-borne virus chikungunya are also different. For instance, a study of SARS 2 and chikungunya offers theories of how to account for reports of COVID19's long-term effects in certain individuals.3.

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I have suggested that we can uncover forms of comparison that are valuable but relatively underutilized if we expand the range of purposes for which they are useful, such as the complete range of kinds of explanation (Abbott 2016; Abend 2020), but also description and critique. Here, I want to draw attention to an underappreciated comparison type: the asymmetric comparison (Krause 2016).

The idea that each comparable example requires the same level of attention is one that appears to constrain the social sciences' existing comparison methods. Explicitly comparable research designs require careful consideration of which cases to include and which not to include when they are written and evaluated. However, the majority of qualitative designs still only take into account one case, and studies that are explicitly comparative or single-case focus too little on other pertinent cases and literatures (or the greater diversity of the world), which could be taken into account by using less in-depth research or information that is more widely available.

Asymmetric comparisons have some justifications for skepticism, but we should carefully consider whether and how these justifications apply to any particular research study. First, it does appear necessary to offer regulated and equal conditions for each participant if the comparison is to serve as a staging ground for a just competition between causes. That being said, this is not applicable to all explanatory tasks, nor is it appropriate for conceptualization-focused comparisons (Krause 2016).

Secondly, the inclination towards symmetrical comparison, meaning that all cases receive the same level of attention, appears to be associated with measurement or description issues. Since researchers are working with pre-existing data sets, symmetry of attention can appear as the default when measurement is unproblematically believed to be true or false, or when measurement has no costs. There is no reason to take into account information of a lesser calibre if measurements or descriptions are accurate or not.

However, the majority of social science research faces more challenging trade-offs when it comes to measurement-related issues. There are trade-offs between the many goals of research programs; other strategic goals are matched with information quality. Many types of comparisons are made during conception and description, even though we don't always make them obvious. These stages also serve as the basis for explanatory efforts. The majority of these comparisons lack balance. Naturally, we would anticipate that empirical research would add fresh, in-depth, high-quality knowledge about certain circumstances; yet, such study can also benefit from knowledge about other examples obtained from secondary sources (and even from exercises in pure thought).

How Are Cases Selected by Social Scientists?

I have argued that the social sciences' self-observation of comparative research has concentrated on epistemological orientation, which has been extracted from and dissociated from actual research methods, and that it could be beneficial to examine comparison procedures in greater detail. Strong (auto-)ethnographies of comparison practices in the social sciences are starting to emerge (Akrich and Rabeharisoa 2016; Deville et al. 2016), it can also be helpful to examine the space of possibilities for comparison practices across studies.

I'd want to provide an additional illustration of the kinds of discussions that could arise from the kind of self-observation I'm suggesting: The way biologists use model systems, such as the fruit fly or the mouse, has always piqued my curiosity. I made this comparison between social

scientists and biologists in a paper with Michael Guggenheim (Guggenheim and Krause 2012; Krause 2021).

Selecting specific organisms to study allows biologists to address issues related to sickness, development, and life and death. Certain organisms have been utilized more than others and have developed into "model organisms," drawing disproportionately large amounts of funding and attention and influencing entire scientific disciplines. Perhaps the most well-known of these are the fruit fly and the mouse, but other examples include rats, dogs, and frogs, as well as certain types of viruses, bacteria, or entire ecosystems, most of which are found on islands (Kueffer, and Richardson 2013).

Drawing from the perspectives of science philosophers and interdisciplinarity (Padberg 2014), I would differentiate between material and epistemic research objects. The individual object that may be accessed through unique traces made by unique tools and devices is known as the material object. It is distinguished from an epistemic research object—whatever it is that researchers are attempting to understand—by its function as a tool for understanding something else. An epistemic research object is a conceptual entity that depends on particular intellectual and disciplinary traditions. Next, I inquire about the selection process for material study objects (Krause 2021). This is not the same as asking how they ought to be selected, which has been the focus of a robust theoretical and methodological literature (Gerring and Cojocaru 2016; Passeron and Revel 2005; Tavory and Timmermans 2014; Chen 2015; Ermakoff 2014). By this, I'm suggesting that we stray from the usual questions we pose to our students and professionals in other domains, and instead ask them about actual trends among practitioners, even as we are driven to address normative and practical issues.

As a factor, convenience is both unimportant and incredibly intricate. Convenience can be used to explain everything from the most intrinsic ("the easiest way to study important phenomenon x") to the most extrinsic ("funding was provided by the military"). Here, I want to use the phrase to talk about the basic, non-strategic, practical aspects of convenience. Because they are located in an area where universities and researchers are concentrated, research locations and items in a variety of substantive fields are picked. According to Mario Luis Small, whose analysis of Chicago's role in urban sociology has been extremely valuable to me (Small 2014), PhD students who do not engage in what is commonly referred to as "research abroad" or "international fieldwork" typically conduct their qualitative studies close to their university, with the exception of those who have partners or family living abroad.

Another important reason why some cases are given more weight than others is historicism. Political science and sociology in particular appear to have a considerable bias in favor of research materials that may be presented as "the most advanced" in their respective fields. One particularly significant variation of this, which I would refer to as macro-historicism, has been recognized by postcolonial thinkers as the favoring of some nations as the most developed nations (Chakrabarty 2000). Although they coexist in the present, the concepts of "modernity" and "development" have assumed a trend in history and placed examples inside that history. These theories have been used to support emphasizing particular nations above others and to suggest that, even if it could take some time, the lessons learned in these nations can be applied to other situations. Due to this, the Global South and the variety of concerns that cases from the Global South have been compelled to address have received far less attention.

Another way to focus on "advanced cases" is to focus on the "most advanced" cases within a subject, a concept I refer to as micro-historicism. Urban sociologists concentrate on developing cities because they believe that studying these areas is akin to studying the future and what other cities may become into. Analysing labour also frequently entails researching the most sophisticated types of work. Because the developmentalist viewpoint is frequently normatively charged, it can have a dystopian or utopian bent, almost entirely depending on the personalities of the authors involved.

It has been demonstrated by cognitive scientists (Lakoff 1987), and cognitive sociologists (Brekhus 2015) that professionals and scientists alike understand categories schematically (Griffiths and Stotz 2008). Scholars choose cases based on popular schemas to the extent that social scientific categories are also popular categories. For instance, international management research significantly overrepresents multinational corporations with headquarters in the United States and well-known brands (Collinson and Rugman 2010). This research study bias may align with the general public's cognitive prejudice toward the term "corporation."

Scholars have largely focused on nongovernmental organizations (NGOs) rather than states in affected regions or even the United Nations in one of my own research areas, the study of humanitarian relief, and among NGOs, they have particularly focused on one NGO, Doctors without Borders (Fassin 2010; Redfield 2013). This is due to a variety of factors, some of which are well-founded academically in each particular research; nonetheless, overall, the causes are rather disproportionate, and they align with the widespread recognition of that organization among global charities.

Another group of theories focuses on the impact of the schemas that researchers have due to their occupation in a certain social setting or status rather than their membership in the general population. This might be viewed as a profession-specific disposition or as a position that overlaps with racialized, gendered, and class hierarchies. However, I also want to draw attention to the less interesting, subcultural sides of this: Studies of social movements by academics have mostly focused on movements that they found appealing until recently; the far Right and racial mobilizations have received less attention. Interest in managerialism has been sparked by academics' experiences in their own jobs, and the institution is frequently brought up in conversations about audit culture, rankings, and neoliberalism (Espeland and Sauder 2016; Strathern 2000). We can investigate differences within and between scholarly communities in various fields, socioeconomic groups, and geographical locations if we investigate theories regarding the impacts of academic subcultures.

Our knowledge is shaped by the factors that support various research objects in a way that is occasionally reflected within subfields but infrequently across them. Certain fields see the development of conventions surrounding privileged material research objects, leading to the development of norms that dictate the study of overstudied situations. In these situations, the central cases are regarded as the foundation for general understanding and theory, even in the face of study on additional cases. It is specifically suggested for biologists and other life scientists to concentrate their efforts in this manner. They consider this openly as well. I would contend that if we also thought more openly about circumstances that were privileged collectively, we could better take advantage of attentional concentration and lessen negative effects.

Conclusion

I've noticed that the social sciences' self-observation frequently concentrates on the epistemological trajectories of various research methodologies, addressed somewhat apart from real research procedures, instruments, or real research questions and claims we might be making. Traditional philosophy of science places a strong emphasis on epistemological groupings. In recent years, this focus has been strengthened by both critics of "dominant epistemology" or "Western science" who have been influenced more by philosophical and macro-cultural approaches than by the full spectrum of sociological inquiry, as well as by some "orthodox" attempts to distinguish their work from that which authors deem "too political." Concepts related to "science" are still crucial for both projecting and criticizing broad ideologies. In doing so, sociologists frequently overlook research from the social studies of science, which demonstrates that the idea of science as a unique entity lacks either positive or negative referents. Plato's conception of science does not exist in the sky or among the physicists, biologists, or virologists. As some critics have suggested, there isn't a single hegemonic apparatus either. Within the broad community of social scientists, there exist various systematic research approaches that offer valuable insights and should be thoroughly considered.

I've talked about a few of the insights we may make about sociology, and to a lesser extent, the social sciences more widely, when we examine them around these oppositions, rather than via these epistemic zealous ones. In my opinion, normative discussions within the field regarding how to enhance sociological research can be addressed through the application of sociological self-observation. Questions of both collective and individual technique are raised, for instance, by my findings regarding the process of selecting instances and how they could be selected.

In addition to individual methodology, we should talk more about the various facets of collective methodology. That is, we should talk about individual decisions in light of observations regarding trends in the decisions made by others, with an eye toward the overall effects of various initiatives. When I refer to collective approach, I do not mean individual or educational efforts to standardize our language. I am also not considering suggestions from individuals or educational institutions for improved "ontologies" or "epistemologies."

I'm considering making more haphazard attempts. We ought to work on strengthening the connections between the results of various investigations conducted using various techniques and methodologies. In an effort to reduce duplication in our output as a group and address geographical, case-based, and educational provincialism, we can also attempt to employ an examination of patterns in knowledge production.

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