

Does Sociology Need Open Science?*

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Abstract - English

Reliability, transparency and ethical crises pushed psychology to reorganize as a discipline over the last decade. Political science also shows signs of reworking itself in response to these crises. Sociology sits on the sidelines. There have not been the same reliability or ethical scandals, at least not in the limelight, nor has there been strong disciplinary moves toward open science. This paper therefore investigates sociology as a discipline looking at current practices, definitions of sociology, positions of sociological associations and a brief consideration of the arguments of three highly influential sociologists: Weber, Merton and Habermas. Based on this disciplinary review, I suggest that sociology is no different from its neighboring disciplines in terms of reliability or ethical dilemmas. Therefore, sociology should adopt open science practices immediately. Weber, Merton and Habermas – three very different social thinkers epistemologically – offer strong arguments that favor what we know as “open science” today. Open science promotes ethics and reliability, reduces fraud and ultimately increases the value of sociology for policymakers and the public. The paper concludes with some basic steps individual researchers can take to move sociology toward open science.

Abstract - German

Zuverlässigkeit, Transparenz und ethische Krisen haben die Psychologie im letzten Jahrzehnt dazu gebracht, sich als Disziplin neu zu organisieren. Die Politikwissenschaft zeigt auch Anzeichen einer Überarbeitung als Reaktion auf diese Krisen. Die Soziologie steht am Rande. Es gab nicht die gleiche Zuverlässigkeit oder ethische Skandale, zumindest nicht im Rampenlicht, und es gab auch keine starken disziplinarischen Schritte in Richtung ‚Open Science‘. Dieses Paper untersucht daher die Soziologie als eine Disziplin, die aktuelle Praktiken, Definitionen der Soziologie, Positionen soziologischer Assoziationen und eine kurze Betrachtung der Argumente von drei einflussreichen Soziologen betrachtet: Weber, Merton und Habermas. Basierend auf dieser disziplinarischen Überprüfung schlage ich vor, dass sich die Soziologie in Bezug auf Zuverlässigkeit oder ethische Dilemmata nicht von den benachbarten Disziplinen unterscheidet. Daher sollte die Soziologie sofort Open Science Praktiken anwenden. Weber, Merton und Habermas - drei erkenntnistheoretisch sehr unterschiedliche Sozialdenker - liefern starke Argumente, die das befürworten, was wir heute als „Open Science“ kennen. Open Science fördert Ethik und Zuverlässigkeit, reduziert Betrug und erhöht letztendlich den Wert der Soziologie für politische Entscheidungsträger und die Öffentlichkeit. Das Papier schließt mit einigen grundlegenden Schritten, die einzelne Forscher unternehmen können, um die Soziologie in Richtung Open Science zu bewegen.

Keywords: Open Science, Transparency, Reproducibility Crisis, Weber, Merton, Habermas, Science Community

The Crisis in Science. The Crisis in Sociology?

Science is in crisis. It appears less reliable, reproducible and ethical than policymakers, the public and other scientists expect. Widespread failures to replicate previous research or even simply obtain others' replication materials, a publishing industry often at odds with the goals of scientific research, lack of awareness of the biases in standard research practices and individual researchers who become ethically corrupted in seeking recognition and funding are a few of the reasons. In this paper I argue that sociology is no exception to these common science problems. If this is true, sociology in practice is quite far from sociology by definition. This is a problem I believe needs to be addressed immediately.

Scandals that brought social science into the limelight mostly occurred in disciplines other than sociology. Some important events: in *psychology* – Diederik Stapel spent a career faking data and results that were published in at least 54 articles that led to millions of Euro in funding until several whistleblowers outed him (more details available in the 2012 final report of the joint Levelt, Noort and Drenth committees); in *political science* – LaCour and Green published a study in *Science* that attitudes toward gay marriage could be changed if heterosexual people listened to a homosexual person's story, but it turns out LaCour fabricated results of a follow up survey that never took place as uncovered by Broockman (Broockman, Stanford, and Aronow 2015) leading to its retraction; and in *economics* – Reinhart and Rogoff published numerous studies identifying a negative impact of high debt rates on national economic growth, when in fact several points in their dataset had conspicuously missing values. When these values were added there was no longer support for their claim as identified by Herndon, Ash and Pollin (2013).

That sociologists are more scientific and ethical in their research behaviors than members of neighboring disciplines is a theoretically possible explanation, but presumptuous given they face similar lack of incentives and therefore interest in replicating or checking each other's work (Gove and Zelditch 1979; Freese 2007). Sociology journals, for example, do not retract articles despite evidence of serious methodological mistakes¹. Also, journals rarely publish replications meaning

¹ A search of the *Retraction Watch* database (www.retractiondatabase.org) reveals no retractions from the very top sociology journals and only two among the well-known, one in *Sociology* and another in *Social Indicators Research* (as of Sept. 7th, 2019). Whereas psychology has dozens, not just those from Stapel. Evidence of severe errors in analysis and interpretation in some cases (Brezna 2015; Cohen 2016; Macdonald 2011) suggests that editors of the best sociology journals are thus far unwilling to retract problematic articles. Also, Carina Mood accurately pointed out mistakes in the interpretation of odds-ratios in some *ASR* articles, but the editors refused to publish her findings, much less consider retractions. She shared her exchange with *ASR* in an email to me and discusses some of it in a working paper (Mood 2017).

there is little incentive for methodological critique. Given that sociology is similar to all science in the necessity to publish or perish, I assume questionable if not occasionally fraudulent research practices take place but are not identified². Sociologists most often work with survey data, interviews and participant observation. The opportunities to alter or even fake data are there, just like experimental research. The researcher is the vessel that turns observation into evidence regardless of the origin of that evidence. I assume intentional fraud is exceptional. Questionable research practices done without researchers' own knowledge of the consequences is more pervasive. For example, selective reporting of cases, poor research notes or the use of leading statements or actions that are not reported in the methods.

Although sociology produced many public goods throughout history (Thibodeaux 2016), the institution of publish-or-perish leads to reiteration more than knowledge development (Sørensen 1996; Warren 2019). Sociologists, like other scientists, are doing sociology as an end rather than a means and therefore policymaker distrust of sociology may be justified³ (Carrier 2017). Therefore, in the next two sub-sections, I review the discipline of sociology and search for evidence that it suffers from similar problems as other disciplines.

When Evidence is Not Evident

Take myself as an example. I was taught early on that publications are 'the currency of our trade'. In the mid-20th Century a 'regression revolution' took place, after which sociological research using quantitative data was judged almost entirely by whether it arrived at statistically significant (partial) correlation coefficients. P-value cutoffs became synonymous with 'important findings'. I learned to tinker with the data until I found a significant effect, and then re-write my research design to predict this effect in the first place – a formula for success. I honestly thought this was normal back then. This kind of behavior on the surface is indistinguishable from scientific research in the sense that looking at results, refining theory and then re-running analyses is a productive interplay of induction and deduction (Burawoy 1998). Below the surface there is a

² An exceptional recent event was the retraction of one of Legewie's sociological studies (Science 2019)

³ For example, a 2018 study of Germans found that only 26% thought that scientists *did not* adjust their results to match their expectations (Wissenschaftsbarometer 2018); a senior congressman in the US referred to the social sciences as the "sociological gobbledygook" (Arriagada 2019); the Wall Street Journal used "fake news comes to academia" as a headline and there were several opinion-editorials questioning the credibility of sociology and the social sciences (Gutting 2012; Meyer 2018); and, in a US survey, only 20% of Americans believed that scientists always acted in the public interest (Funk et al. 2019).

major difference. If researchers consider hundreds or thousands of ways of analyzing or interpreting their data, they are essentially testing ‘every’ possible theory. When they chose only one version to report from the thousands, but pretend they had a theory predicting this unique result all along, it appears as though they found evidence in favor of that theory. The reality is that they have instead either selected a theory that fit certain findings or selected certain findings to fit a theory. It does not matter if the research is qualitative or quantitative. Researchers could easily only report those parts of texts, interviews and observations that support the theory they want or the interpretations they prefer. If we observe researchers behaving in this manner, it means their reported findings are a highly selective set of rare associations among data that correspond little, if at all, to our social worlds. Their real findings – the thousands of other versions of the data, analysis or interpretation are thrown away. Imagine thousands of sociologists repeating a similar process, the bias in findings and theory that would result is terrifying, especially when used to make ‘rational’ decisions or design effective policies.

Although my own participation in sociology provides anecdotal evidence, a more convincing case of ethnomethodological research targeting certain segments of social science shed further light on the problem. Three researchers with backgrounds outside of sociology, wrote a series of 20 papers that presented fake results and unethical conclusions. They wrote the papers to mimic the style of articles published in journals well-known for sociological research on topics of identity, hegemony and marginalization (Lindsay, Boghossian, and Pluckrose 2018). By trying to publish in these areas they were studying what they perceived to be “grievance studies” rather than ‘scientific’ research. Seven of their papers were published or had revise and resubmit recommendations before their true intentions became publicly known, at which point they had to cancel the project. The details are harsh, for example one paper contained sections copied from Hitler’s *Mein Kampf*. Another suggested men should be trained as canines to prevent rape, and a third that white men should be forced to sit in chains on the floors of university classrooms, instead of normal desks. What I find most shocking is that *they all contained faked data, non-existent methods or conclusions not supported by the data*. That these studies easily flew under the radar of a number of high impact journals raises skepticism about the reliability of sociology, if not social sciences in general.

An anonymous reviewer of this manuscript suggested that the journals targeted by Lindsay et al, are not “sociology” journals, thus implying that their research is not “sociology”. This line of argumentation is problematic and if followed would represent a definition of sociology closed to

topical diversity, only ‘allowed’ to be that which exists in general interest or mainstream journals. The authors publishing in several of the journals that were targeted, such as *Sexuality and Culture*; *Fat Studies: An Interdisciplinary Journal of Body Weight and Society*; *Sex Roles*; and *Gender, Place and Culture* are sociologists, or working in interdisciplinary departments that include sociology. The journals themselves list social, behavioral and geographic research in their descriptions. To delineate sociology as only that which is published in mainstream journals, like with “sociology” in the title, is not open science. It would construct a disciplinary wall excluding a huge population of sociologists working on studying “collective behavior” and “social problems” related to race, class, gender, marginalization, discrimination and identity (quotation marks added to reference the definition of sociology presented in Box 2).

The Lindsay, Boghossian, and Pluckrose research is a reminder that peer reviewers and journal editors are part of the causes of the reliability problem. Not necessarily because they do not do a good job, but because it is far too great a burden to hold three or so, somewhat randomly available, reviewers responsible for *ensuring* that a paper is reliable. With low reliability, the chances for sociology to address global problems are slim. Regardless of whether sociologists challenge white-heterosexual-male power structures, like the aforementioned “grievance studies” goals, or seek to identify the causes of rising income inequality for example, the problem remains: *if studies are not based on real data or cannot provide reliable evidence of their claims, there is no reason to trust their conclusions or recommendations*. Even in highly interpretivist undertakings, the logic should correspond to the evidence. The reviewers found the logic compelling in these fake studies, but few questioned the data and methods. Without both, the reliability problem persists.

Misinformation, Bias and Hacking

Like all social science disciplines, sociology contains a large amount of quantitative research. The p-value is ubiquitous in this research but many sociologists are misinformed about its meaning. In a randomized experiment, researchers try to claim counterfactually, what would have happened if a treatment was not administered to a group of people. The p-value provides information that supports or opposes these claims. Sociologists rarely conduct experiments, but during the ‘regression revolution’ they began to apply p-value logic to their non-experimental research. The p-value became the gold standard, and the results of studies are now often judged

entirely on whether they have a p-value below a certain threshold or not⁴. It is problematic to judge studies in this way, because the p-value is only useful if the data-generating model is correct. In experiments, random assignment of participants makes the data-generating model correct in theory, and the p-value then implies how likely the observed difference between treatment and control group would be if the treatment had no effect⁵. In non-experimental research there is no treatment, thus the researcher must select a set of independent variables from non-experimental data that represent an unbiased data-generating model of the real world (Pearl 2010). Then the p-value of a regression coefficient can be interpreted similarly to that of an experimental treatment.

The p-value only evaluates the association of data and statistical model. Again, if there is anything misspecified about the model like confounding, suppression, or contamination, then the p-value is uninformative (Wasserstein and Lazar 2016). Moreover, if the data sample is not a simple random sample, then the reliability of the p-value decreases if not disappears. Most sociologists struggle to understand this. In a survey of communications researchers, 91% of students and 89% of postdocs and professors got most or all of the definition of a p-value wrong (Rinke and Schneider 2018). I am aware of no such survey in sociology but it is reasonable to assume sociology is similar because communications research is highly sociological, often performed by those employed as sociologists. If you are not convinced see how many of the Rinke and Schneider test questions you would have gotten correct, I myself would have done poorly prior to investigating this problem as part of my interest in open science. This is not meant to implicate myself or anyone; this is a serious problem in sociology that requires correction at the level of methods teaching. It also requires transparency so we can check each other's usages of the p-value to allow constructive criticism and increased awareness of its implications.

As a result of misinformation about p-values, researchers now regularly use the term “significance” and over time, a “significant coefficient” became synonymous with *proof* of a theory or *significant knowledge* about the social world. Again, a p-value indicates the likelihood that researchers would observe the same data if they repeated the same experiment several times, if the null hypothesis of no effect or no group difference was true. The p-value does not say anything about the effect size or meaningfulness nor anything about the theory, it *only* says something about the data collected; and *only if* the data-generating model is accurately represented by the statistical model. When p is less than 0.05 it means that there is a less than 5% likelihood of randomly

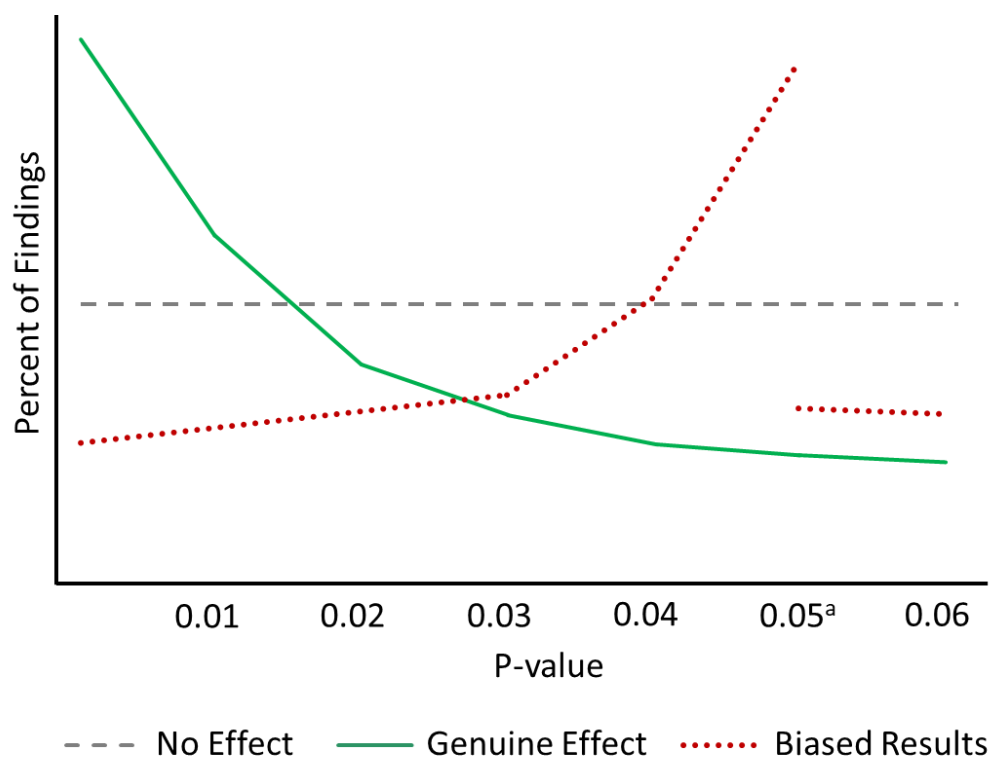
⁴ P-value here is synonymous with a t-test and its equivalents, e.g., a t-value of 1.96 equals a p-value of 0.05.

⁵ In reality, the most carefully designed experiments often have things happen that confound treatment effects.

observing the patterns found in the data, and therefore is evidence against the null hypothesis. Instead, sociologists routinely equate it with the true population parameter, and therefore assume that a p of 0.05 indicates a 95% likelihood of truth in their findings. A completely false conclusion.

By comparing p-values across sociological research we will discover the depth of this problem. This is an undertaking that has shed light on highly biased practices in many other disciplines.

Figure 1. How to Visualize Bias in p-Values



^aThis could be any pre-determined threshold such as $p < 0.01$ and 0.001 and result in the same expected distributions in all three cases.

Figure 1 demonstrates how p-values across many repeated studies should look under three different realities. The first reality would be no effect of a given treatment or independent variable. If true, the distribution of p-values across all studies together should look something like the grey dotted line and be uniformly distributed across all possible p-value levels. The second is a true effect. In such cases, there should be more and more findings with lower and lower p-values as shown by the skewed green line. If scholars report *all* of their studies on a particular topic in journals, or at least journals publish a random sample of these studies, then the distribution should

follow one of the first two lines. If, instead, all published studies look like the red dotted line, then there is some kind of human-induced bias across findings. When studies' p-values hover just below the threshold, here shown as the density between 0.045 and 0.050, this is a likely indication of **p-hacking** – *the intentional efforts of scholars to submit only results with a p-value below a given threshold and to discard the rest*. Whereas a sudden drop off of values on the non-significant side of the threshold as shown with the large break in the red dotted line at 0.05 indicates **publication bias** – *the refusal of journal editors and peer reviewers to publish studies with non-significant results*. Publication bias only refers to the break in the red dotted line, not the curved distribution below the cutoff.

Recent efforts at studying this phenomenon offers striking evidence of bias, where p-values across topics follow the red dotted line. For example, Head et al. (2015) text mined p-values out of all open access papers in the *PubMed* database. As social sciences and especially sociology rely mostly on pay walled publication systems for their top journals, these results are an irregular sample of all possible studies. None the less, Head and colleagues find evidence of bias in the p-values. This p-hacking is present in several disciplines and highest in the cognitive sciences. There is no clear evidence of this in studies of “human society” but there were less than 200 cases leaving concerns about inference due to a small-N. Similar results supporting the biased red dotted line in Figure 1 were found in studies by Simonsohn, Nelson and Simmons in psychology (2011; 2014) and Brodeur, Cook and Heyes in economics (2018).

In sociology, an earlier study by Wilson, Smoke and Martin (1973) found that 80% of studies published in the top three sociology journals of that time rejected the null hypothesis, in other words have p-values below a threshold. This suggests publication bias, if not p-hacking. Sahner (1982, Table 5) analyzed all article submissions to the *Zeitschrift für Soziologie*, 1972-1980. Of those that contained significance tests, 70% were significant at $p < 0.05$ suggesting that authors prefer to submit significant results. More recently, Gerber and Malhotra (2008) reviewed articles published in *American Journal of Sociology* (AJS) and *American Sociological Review* (ASR) and *The Sociological Quarterly*, and specifically looked at the boundary of $t = 1.96$ (i.e., $p < 0.05$) to find that as many as 4-out-of-5 studies were ‘significant’. This suggests publication bias as well. Sociology has yet to have a systematic review of p-hacking by comparing p-values within ‘significant’ results. However, there is good evidence that the perverse incentives to publish-or-perish among scholars and the perverse incentives to provide ‘significant’ findings among journal editors leads to bias toward results that appear attractive. The practice of journal editors and

reviewers to only publish studies with significant p-values is perhaps the greatest single problem in the credibility of modern quantitative science.

If playing with data to produced researcher's preferred results to meet institutionalized incentive criteria was not bad enough, researchers who might only pursue a hypothesis test in a dataset *if* they observe some 'desirable' pattern in that data first. In doing this they have invalidated the p-test in the first place because they have conditioned their test on prior conditions that do not represent the distribution of potential outcomes, i.e., they inflated the Type I error by some (most likely unknown) amount as discussed in Devezer et al (2020).

Sociology Needs Open Science, Just like the Other Disciplines

The evidence presented in the previous section suggests that sociology likely suffers from very similar problems as other disciplines. Therefore, I now ask in the following two sub-sections if there is something unique about sociology that it should not adopt open science practices; either by disciplinary definition or based on the arguments of three influential sociological scholars.

Sociology is Not Unique

The state of the art in sociology does not generally follow the ethical codes of the historically largest sociological associations of the last century in Germany, the United States and Japan, shown in Box 1. Codes with similar statements are universal in social science disciplines.

Box 1. Transparency in Sociological Association Ethics Codes
<i>American Sociological Association</i> 'Code of Ethics' 2019, 12.4.d. – Consistent with the spirit of full disclosure of methods and analyses, once findings are publicly disseminated, sociologists permit their open assessment and verification by other responsible researchers, with appropriate safeguards to protect the confidentiality of research participants.
<i>German Sociological Society</i> 'Ethik-Kodex' 2019 I.1.2 – [When presenting or publishing sociological findings, they have to be described without omitting important results; i.e., that would falsify the findings. Details of the theories, methods and research designs that are important for the assessment of the research results and the limits of their validity are given to the best of one's knowledge.]
<i>Japanese Sociological Society</i> , 'Code of Ethics' 2019, Article 9 – [Members must maintain open attitudes and behaviors to ensure a place for mutual criticism and verification.]
[author's translation; for original language see Appendix A]

Journals and the publication process are key catalysts in spreading problems in science. Sociology journals require little if anything from authors in terms of data sharing and reproducibility. Following the ethical codes found in sociology associations, 78% of the major sociology journals have long-standing transparency policies (Zenk-Möltgen et al. 2018). Unfortunately, these policies are mostly artifacts on paper without any enforcement. Without enforcement, authors cannot be relied upon to practice these standards on their own. For example, only 37% of sociology articles published in the mainstream journals between 2012-2014 include shared data and/or materials (*ibid.*). Sociologists are free to hide the data and materials that led to their findings without recourse. In 2015, a small group of sociologists tried to obtain materials from the authors of 53 prominent sociological studies. They obtained these from just 19%, and only 20% of the authors they contacted bothered to respond despite several requests (Young 2015).

Other disciplines addressed this problem through the development and adoption of the *Transparency and Openness Promotion Guidelines* (TOP) (Nosek et al. 2015). These guidelines along with the institutional support of the Center for Open Science provide guidance for journals to improve science. Journals can become signatories of TOP, and in doing so they either adopt and enforce new transparency guidelines, or certify that they already meet certain transparency standards. Most of the top psychology journals and several political science journals signed on. Other major journals such as the *Journal of Applied Econometrics* and later the *American Economic Review* adopted their own enforced transparency guidelines.

Until 2017, the only higher ranking sociology journals that signed TOP were *Sociological Methods and Research* and *American Journal of Cultural Sociology*⁶. At the time of writing this, the flagship journals *AJS* and *ASR* neither signed TOP nor enforce their own guidelines. Of top German sociology journals, the *Kölner Zeitschrift für Soziologie und Sozialpsychologie* is the only signatory and no Japanese sociology journals signed. Given that intransparency is pervasive in sociology, most research cannot be (a) checked for errors, (b) reproduced or (c) simply critiqued. Even when exact reproducibility is not the goal, as often is the case with context-specific interpretive research, most research methods remain shrouded in mystery. This requires readers to take a giant leap to trust what others report. Part of the problem is that sociologists express little interest in reproduction or checking others' works. There are few replications in the history of

⁶ In 2017, Elsevier dictated that its journals adopt guidelines and this added *Social Science Research* to the list.

sociology, and if anything, they decreased over time until recently (Freese and Peterson 2017). For example, searching the articles in AJS and ASR reveals 22 with a replication study from 1950-1980 and only 8 from 1981-2010.

A counter argument to open science in sociology might be that sociology is special. That it is not a science by definition and therefore does not need to follow scientific methods and ethics. Simply looking at the popular usage of the terms *sociology* and *science* from a linguistic perspective suggests this argument is false. It is the task of philosophers and methodologists to argue at length about things like, ‘what is science’; but dictionaries and the usage of language on a day-to-day basis offer an insight into sociology’s public or more general ontologies. Looking at major dictionaries’ definitions in English, German, and Japanese in Box 2 and 3⁷ suggests sociology is a science in popular denotation.

Box 2. Sociology, a science by definition

English – The science of society, social institutions, and social relationships; specifically: the systematic study of the development, structure, interaction, and collective behavior of organized groups of human beings.

German - [Science; study of the coexistence of humans in a community or society, of the manifestations, developments and regularities of social life.]

Japanese – [A science that seeks to clarify the mechanisms of social life, social organization, social problems, etc., in relation to human social behavior.]

[author’s translation; for original language see Appendix B]

The “-ology” in the word sociology derives from Latin (“logos”) to denote *the study of* something. Sociology is the study of the social, or *the science of the social* consistent with all three definitions in Box 2⁸.

⁷ Online dictionaries Merriam-Webster and Oxford (English), Duden (German) and Weblio (Japanese) accessed August 30th, 2019. Author’s own translations. See Appendix for the original language in Boxes 2 and 3.

⁸ Sociology is a science just like political science, psychology, social psychology and economics. In particular, economics is not more scientific than sociology, just because they tend to express their theories in the form of utility maximizing functions does not change the fact that they use socio-economic data to develop and test theories about the world just like all the others. Their over-reliance on math may even detract from the soundness of scientific practices in economics (Levinovitz 2019).

As sociology is a science by definition, the next question is whether it is a science in practice. Looking at popular definitions in Box 3 suggests *science* is systematic, practical, logical and reliable studying of things.

Box 3. Science, practical and reliable by definition

English - the intellectual and practical activity encompassing the systematic study of the structure and behavior of the physical and natural world through observation and experiment.

German – [Reasoned/logical, orderly, reliable knowledge-producing research activity in a particular area.]

Japanese – [A system or exploration of rational knowledge aiming at formally understanding areas of the world such as nature and society. It features facts based on experiments and observations, and systematic consistency based on logical reasoning. It is classified into natural science, social science, humanities, etc. according to the difference in research object and method. In the narrow sense, it refers to natural science.]

[author's translation; for original language see Appendix C]

I have no doubt that sociologists aspire to be systematic, logical and thorough in their research. However, simply having these intentions does not guarantee they are practiced, a problem I discuss in the following three sub-sections.

Stereotypes from outside sociology have a tradition of suggesting it is un-scientific, for example because sociologists employ constructivist scientific methodologies or statistical models without formal equations/theorems. In a 1983 speech, George J. Stigler explained why disciplines such as sociology do not have a Nobel Prize. His reasoning was, “that they already had a Nobel Prize in literature” (Kuttner 1987:25). Such stereotypes reflect deep misunderstandings of what sociology and science are (see Boxes 1 and 2), but at the same time indicate a poor reputation of sociology. The problems are not entirely from outside sociology. Internally, some sociologists look down on others, and attempt to exclude them from sociology or sociological funding; e.g., naturalism versus interpretivism, or a larger share of funding for quantitative research. Any internal conflicts surrounding these inequities or divisions can only cause further damage the perception of sociology among other disciplines and the public.

Open Science is Sociology

A movement is underway across science to correct the problems identified thus far. Various academics, associations and funding agencies are now pushing to “open” science. To make it more transparent, reproducible, accessible and less biased. The various efforts fueling this process can collectively be labeled the Open Science Movement (OS-Movement) and suggests individual and institutionally sanctioned practices of sharing of all research materials, preregistering research plans, open access and data, eliminating publication bias and results ‘hacking’, and a renewed interest in replication. This movement arrived in the social sciences only in the last decade or so. In a short period the OS-Movement led to rapid changes in psychology, and initiated incremental changes in other areas such as international relations, economics, communications, political science and social psychology. As I will discuss herein, sociology changed very little thus far.

The basic ideas of the OS-Movement are consistent with the ideals of three of sociology’s key historical contributors. Robert K. Merton is an obvious choice because he repeatedly argued for openness in science, including communal research for the public good. I also discuss Max Weber and Jürgen Habermas, whose linkages to open science ideals are less obvious on the surface and whose different takes on social science between a more positivist and more critical and interpretivist are striking. Thus, finding something on which they would agree in terms of open and ethical science, should help unite sociology under a common goal of opening the discipline.

Merton proposed norms for sociology that are similar to those in the Open Science Movement. According to his norm of *organized skepticism* sociologists must consistently certify the knowledge they produce. According to *communalism* they must do this in a community of sociologists⁹. These norms demand that all scientists have access to the same knowledge or materials of knowledge construction, and have the opportunity to participate in scientific exchanges without exchanges taking place in secret (following his norm of *universalism*). All together, these norms call for open access, open data and transparency (communalism and universalism), and reproducibility (communalism and organized skepticism) identical to the goals of the OS Movement. Merton proposed these norms as a paradigm shift during his time, and the OS Movement proposes similar norms today calling for a similar paradigm shift (not only in sociology but across all science, see for example Chubin 1985). Nearly a century has passed and sociology is still nothing like Merton’s ideal definition.

⁹ Merton used “communism” in the original text, but this word has a different connotation today that may cause confusion.

Some decades before Merton, Weber stated that the task of a sociologist is to provide facts while engaging in critical self-clarification (Weber 1919:505). He was careful to define “facts” as both objective and subjective. Weber argued that the effective sociological teacher avoids imposing subjective ideology (e.g., political or cultural preferences) when presenting facts to students or colleagues (*Werturteilsfreiheit*: ‘value-free’/‘judgement-free’). Each sociologist needs to have in mind their own subjective interests and goals and how these relate to the facts they present in order to do this. Weber proposed therefore that sociologists cannot separate the object of inquiry from the method of inquiry, namely the sociologists themselves cannot be entirely exogenous to the social world they observe (*verstehendes Erklären*; something like ‘self-awareness’ and ‘clarification’ in the research process; Weber [1922:495]). To maintain this self-awareness as a researcher in the process of research, a community of researchers is necessary to provide insights that lead to self-awareness through feedback. The impact of a community is limited to transparent and continual critique (as Merton also argued). For example, the hoax papers by Lindsay, Boghossian, and Pluckrose were so attractive subjectively, that editors and reviewers rarely bothered to question their methods or data. When papers are not scrutinized then they are not reliable. They do not consist of findings but illusion and possibly sophistry. This is not a useful knowledge but a bunch of academics playing a game to win notoriety and citations, or push particular private agendas.

The prestige one can gain through academic work is theoretically unlimited. This potential for prestige accumulation produces “ego-maniacs” (Sørensen 1996:1358). Bruno Frey calls the academic institution a system of “Publishing as Prostitution”, because we face a monumental tradeoff between pursuing our “Own Ideas and Academic Success” (Frey 2003)¹⁰. If we as sociologists cannot provide useful knowledge (‘ideas’) to the social world, then we as the OS-Movement should call on public and private funding agencies to discontinue supporting us sociological researchers because this would be evidence that our knowledge is not useful (‘success’ oriented only). This is not a radical or self-defeating position, unless our only reason for practicing sociology is to have a comfortable job with research funding. By supporting this position, just like being transparent before and after we conduct our research, we ensure our goals are ethical and target useful information as opposed to private status accumulation, in line with the sociology Weber and Merton had hoped for.

¹⁰ Perhaps in an obfuscated act of irony that motivates open science, Frey actually engaged in publishing-as-prostitution-style misconduct after he published a similar paper in four different journals (Autor and Frey 2011).

This is also in line with Habermas' insistence on a normative change at the level of communication. He advocated for a rationalization of communication that would open up all communication for consumption. This would reign in individuals' intentions to perform communication to serve their own interests and promote instead a goal of understanding between individuals (Habermas 1984:94-101). When the public sphere is commodified and controlled by private interests, there is asymmetric production of communication (those who control the communication channels control the content) and asymmetric consumption (higher quality communications become more expensive) (Habermas 1989). This distorts communicative action. If we apply his social norms to sociology, then open and transparent communication will reshape science in the same ideal way that Habermas imagined for society. In fact, in the same ideal way that the OS-Movement imagines for society. When the common values of science shift toward more openness it can solve the 'pathologies' of science, namely profiting from closed knowledge communications and the rigid norm of publish-or-perish.

These three sociologists are monumentally influential, but worlds apart in their philosophies, arguments and research; Habermas in particular across disciplines, not only sociology. Weber was mostly a positivist, describing the world as he encountered it; whereas Habermas was largely an interpretivist and described a world with hidden meaning and power structures. However, they similarly expressed concern about scientific misconduct and the influence of private interests in science; as a result, they both promoted socially responsible science. Weber, like Merton, asserted that science as a vocation can be problematic. Individuals may place vocational goals ahead of scientific goals, and this can impact the reliability of their results. Habermas, like Merton, argued that morality and social utility are necessary scientific research norms. All three theorists set forth ideal sociological practices that are still unrealized by the discipline as a whole today. Each in their own way argues that without meeting these ideals, sociological knowledge is not useful. In my view, production of useful knowledge is a necessary condition for practicing sociology and consuming public funds in this process.

The OS-Movement embodies the ideals of all three of these theorists in different ways. It follows Merton and Habermas' calls for open and communal discourse. The OS- Movement is for all scholars. It does not exclude anyone. It calls them in. It asks scientists to make themselves and their discipline reliable and trustworthy. It has no claims to philosophy of knowledge. It is not a positivist movement. It is by definition *open*. The goals are transparency, access and reproducibility. This is not a movement to dictate what scientists investigate nor the tools they

should use. It is a movement to open the black box surrounding what they do, whatever it may be, to create a community of quality control and dialog. It is also a movement to smash the paywalls blocking others from accessing findings. It is a movement to increase ethical practices. If effective, its outcomes will extend beyond reliability and access. The results of a recent survey suggest that open science practices contributed to an increase in public trust in science the United States (Schwarber 2019). Open science will increase trust in sociology not only among the public, but also policymakers, other disciplines and sociologists themselves.

Many sociologists are unaware of the OS-Movement, or they believe it is not relevant for them. Some think that because sociologists rarely conduct experimental research or often use qualitative and interpretivist methodologies, transparency and reproducibility do not apply to their research. Some think the OS-Movement is a cult, a band of thugs trying to gain notoriety by disproving or shaming others (Janz and Freese, in-press). Others might argue that open science is not important for sociologists because they do not cure diseases or plan space travel, for example. I believe these assumptions are based on misconceptions of the ethical and practical goals of the OS-Movement if not an underestimation of the importance of sociology. In my view, open science is a paradigm shift that sociology must follow, or accept its own failure as a scientific discipline. Sociology can effectively study and solve social problems, the kind that are far more insidious than the spread of a disease or development of a technology, like group conflict (i.e., war or racism) or social inequality (i.e., injustice or close-mindedness). But it is hindered by norms of closed science. Thus, it is time for sociology to finally actualize, and claim its place as a reliable science for solving social problems.

Weber called on scientists and teachers to expose inconvenient facts without promoting one or another position. In this essay, I have the subjective position that sociology must adopt open science. None the less, I also present what I believe to be an objectively inconvenient fact in sociology: Our rich universe of theory and critique is unsubstantiated, because we do not provide the materials for nor engage in this substantiation process. Sociology is not special in this regard; however, other neighbor disciplines have taken the lead in reforms. This risks putting sociology at a (further) disadvantage in terms of public trust, funding and contributions to science as a whole.

Resistance to Open Science

Achieving open science ideals of the OS-Movement and laid out by Weber, Merton and Habermas will not be easy in sociology. Researchers using qualitative methods are more often skeptical of open science. They often feel it will disadvantage them vis-à-vis researchers using quantitative methods. The resistance of qualitative researchers to adopt transparency and reproducibility standards was one issue among many in a recent fissure in the Germany Sociological Association (DGS [*Deutsche Gesellschaft für Soziologie*]), that, like sociology itself, unfortunately split along ideological, and somewhat methodological, lines. A group of researchers using mostly quantitative methods left the DGS in protest and founded the Academy for Sociology (AS [*Akademie fuer Soziologie*]) in 2017. Key clauses in the AS charter and goals are to promote “clear methodological standards”¹¹ and “reliable research”¹². The lack of transparency among its researchers and in its guidelines, was a primary motivation for the founders of the AS to part ways with the DGS. These founders of the AS claim that sociology must be analytical, empirical and transparent; in other words, they demand open science. They are opposed to researchers that put forth creative analyses with no empirical basis or empirical analyses that are so opaque that other sociologists cannot understand how or why their conclusions are meaningful. The members of the AS are mostly working with quantitative methods, and their complaints against the DGS are against a subset of members working mostly with qualitative methods, especially interpretivist methods because these are methods ‘without’ evidence¹³. But this is a simplification, the AS is a movement of empiricism and open science that does not exclude interpretivist research methods.

Regardless of differences in methodologies, the DGS positions itself in opposition to open science because it does not support institutionalization of open science. Their position is that while there are benefits to having qualitative data available for future secondary analysis and it is good to share data when possible, the decision should be in the hands of the researchers themselves without penalty or recourse (DGS 2019). Without favoring institutional changes, the DGS favors

¹¹ <https://akademie-soziologie.de/akademie/satzung/>; Paragraph 1 under “2. Purpose of the Organization”.

¹² <https://akademie-soziologie.de/akademie/gruendungsaufuf/>; Paragraph 1 under “Goal of the Academy”.

¹³ There are few interpretivists in the AS and this ostensibly suggests that it is a ‘positivist’ organization, or that the split was one of qualitative versus quantitative; however, neither reflects the position of the AS. There are also many sociologists doing analytical and empirical work remaining in the DGS, some who participate in both organizations. According to the AS membership requirements, anyone can join regardless of “scientific orientation, methods or research topics” and the purpose is to forward “open science” goals (authors own communication with Josef Brüderl, 25th of September 25th through October 3rd, 2019) and <https://akademie-soziologie.de>, last accessed October 4th, 2019, author’s translation and interpretation of “Die analytisch-empirische Vorgehensweise ist für alle sozialwissenschaftlichen Ausrichtungen, Arbeitsweisen und Schwerpunktsetzungen offen”.

the status quo of closed research practices. The American Sociological Association (ASA) has *de facto* the same position, because it enforces nothing and lets researchers decide without penalty whether they share none, some or all of their materials when publishing in ASA journals.

It should be understood by everyone involved that this is not something that should label the AS as the ‘good guys’ and the DGS ‘the bad guys’. The position of the DGS and ASA reflects the preferences of many sociologists, especially those using qualitative methods. Given the sensitive and contractual nature of data collection in many qualitative studies, it is understandable that researchers working qualitatively are reluctant about open data. Two studies, one in Germany in 2004 (Mauer, Medjedovic, Opitz, Stiefel and Witzel) and another in Switzerland in 2012 (Krügel and Ferrez) offer insights into a common reluctance to share research materials (as reported in Herb 2015). In both countries, around half of the respondents (60 and 47% respectively) were willing to share data, but only 15% would do so publicly and the remainder only with conditions or restrictions placed on the data. However, respondents tended to believe that the written or verbal consent attained from their participants legally prevented them from sharing the data.

These surveys also revealed that current practices in qualitative research do not include sharing data as part of designing studies. Although about half of the surveyed researchers were willing to share data, less than 20% ever asked for their participants’ consent to do so. When researchers do not plan to share data by default, the problem is institutional and procedural rather than simply a reflection of individual preferences. The issue is also legal and ethical. Sharing information that would jeopardize the well-being of participants is wrong, thus not a part of the OS-Movement’s goals.

Researchers often question why data sharing is so important. The answer relates to bias and hacking. Researchers using qualitative methods cannot simply p-hack, but they can take phrases and words out of context and construct or interpret things to their liking. A study by Riemann (2003) published in *Forum: Qualitative Social Research* (“Qualitative Sozialforschung”) asked several researchers to qualitatively analyze the same narrative autobiography data. The results were striking because they went in different directions with little in common¹⁴; interestingly, this crowdsourced qualitative research project is still on going as readers are encouraged to add their own analyses of the original transcript in both German and as an English translation). This is

¹⁴ <http://www.qualitative-research.net/index.php/fqs/issue/view/17>. Interestingly, this crowdsourced qualitative research project is still on going as readers are encouraged to add their own analyses of the original transcript (in both German and as an English translation).

precisely where researcher variability is both a strength and weakness of sociology, and science in general. Different insights regarding the same social worlds are precisely what moves science forward. New ways of seeing, knowing, framing and constructing are the backbone of the historical progression of sociological knowledge. At the same time, when several researchers look at the same data and come to very different conclusions, the knowledge gained is more about the researchers than the subject.

Without a plan and a theoretical perspective, results of qualitative research may be highly selective, just like researchers using quantitative data and choosing to report one model out of thousands. As in, the results may not represent anything generalizable to human societies, behaviors or cognitive processes. Without transparency it is impossible to know if researchers had a plan, and what they expected to find, and therefore it is also impossible to know the true meaning of their results. The first two recommendations in designing a qualitative study recommended from participants in the *Workshop on Interdisciplinary Standards for Systematic Qualitative Research* organized by Lamont and White (2005:4) are:

- “Situate the research in appropriate literature; that is, the study should build upon existing knowledge”, and
- “Clearly articulate the connection between theory and data”

Here the transparency of research design and contextualization of the research question within previous theory and findings is perhaps even more important than sharing data, because many qualitative studies simply cannot make their data public even with redaction due to unforeseen exposure of their subjects. That the methods and goals of researchers are necessary features for understanding and trusting the relevance and meaning of their findings is nothing from the OS Movement. This is simply following the scientific method and the methods of sociology.

Transparency of the methods and goals of researchers is necessary for understanding and trusting the relevance and meaning of their findings. In addition, it makes further scientific discourse possible. The necessity of this becomes obvious when trying to address an outside the researcher’s specialized area. For example, Cramer (2015) studies public opinion using qualitative methods and in her studies she is ethically and legally unable to share her particular participants’ data. However, her work is a model of how to do open science with qualitative research. Moreover,

because public opinion scholars predominantly use quantitative methods, she has to be extremely transparent and carefully describe her methods to in order for them to understand and see that her findings are useful in their own work. Researchers using qualitative and quantitative methods alike cannot possibly share their entire workflow and fit within the word limits of journal articles. This means online sharing is necessary; although this is not a norm, by now the resources for this are easy to find and nearly unlimited.

A taskforce of political scientists investigated whether researchers using qualitative methods should support transparency guidelines (Luke, Vázquez-Arroyo, and Hawkesworth 2019:20–21). They argue that transparency policies would impose a positivist understanding of science on researchers, and narrow the realm of what constitutes “research”. This is not the overall position I am familiar with in the OS-Movement, but it demonstrates that so far the OS-Movement failed to offer clear ideas about transparency for those working in qualitative research. A huge red flag in the idea of transparency guidelines is offering any particular group of researchers the opportunity to diminish the value or reputation of other groups as opposed to giving them an opportunity to stand on equal empirical and logical footing. If this is the goal of any group claiming they are part of the OS-Movement, then these groups need the ethical tenets of open science just as much as those hiding their data and methods. Transparency in open science is a way for *all* researchers to reveal the process, ideas and materials on which an argument or theory is founded, and to support a more ethical scientific community.

Actions to Take Now

Experiences from other disciplines suggest that open science practices are *worth it*. They are increasingly rewarded in academia, with growing funding opportunities, greater exposure of results leading to career advancement, greater exposure of research and data leading to improved future science and helping other scientists, being a part of a movement to improve science has intrinsic/ethical benefits, and doing science in a more open and communal fashion is can be enjoyable (Brenzau, Rinke, and Wuttke 2019; McKiernan et al. 2016; Nosek, Spies, and Motyl 2012; Tennant 2020).

I now speak directly to the sociologist (or any social science) researcher who is hopefully convinced, about what *you* can do now to improve both your own work and sociology as a whole.

Transparency

Make all the materials associated with a research paper or book available online. This means data¹⁵, methodological steps, analyses and software code. The practical reason is that others can follow your work and expand it in the future. Yes, this means that others may replicate or critique your work, but from a career perspective, you want others interested in your work regardless of whether they want to criticize or applaud it. In this process you should support principles of constructive criticism, and whether the criticism you receive is useful to you depends on how well you react to it; for example, I replicated a study (Brezna 2015) calling its results into question, then Weakliem (2016) replicated my study and called my results into question¹⁶. Thanks to an innovative open comment platform at *Sociological Science*, we were able to engage in constructive discussion thereafter. Constructive exchange can lead to collaboration with critics to generate better future research as collaborative rather than conflictive. Being transparent forces you to be extra careful. Knowing everything will be public information raises your attention to detail. Thus, not sharing your workflow publicly can indirectly foster lower quality standards, in addition to creating possibilities for misconduct. When research is transparent, scholars no longer need to spend months or years trying to reproduce other's work. They need not 'reinvent the wheel' each time they want to study something. All this enables rather than hinders knowledge, and increases inter-researcher trust.

Transparency is not that much extra work. During the research process you should take high quality notes for yourself. You will often return to your data and research in the future and thus need those notes. This is a best practice with or without sharing your work. When you engage in this best practice, you have a deep familiarity with your data and can draw meaningful conclusions and easily redact identifying characteristics in your data. In case you cannot share data, you can still reveal everything else about the research process. Alternatively, you might reconsider whether you really need to anonymize. When you ask participants for informed consent, in some cases you can ask permission to share details about their lives publicly and avoid the problems of anonymization (Jerolmack and Murphy 2019).

The 'transparency work' of the qualitative research process can be reduced by software platforms that provide semi-automated annotation and coding (Kapiszewski and Karcher 2019).

¹⁵ Data should be shared only when legally possible and when it does not harm participants.

¹⁶ <https://www.sociologicalscience.com/articles-v3-6-109/>

Even if you do not share data, you can build an open workflow from the beginning that allows others to understand every step of the data generating process (Steinhardt 2018).

If you are using quantitative methods, immediately stop hiding your work. If you ran 100 models and 99 did not support your hypothesis, then this is your finding. If a journal does not want to publish this, point the editors and reviewers to the importance of null results and the problems of publication bias. If they still refuse, consider boycotting this journal and sharing your negative experience with peers.

Preregistration

Preregistration can drastically reduce bias and hacking prior to collecting data. When you clearly outline your plans including how you will analyze the data, before conducting the research, there is little room for hacking so long as you stick to the plan. Moreover, preregistration can be done directly with a journal. This means you write the article prior to data collection and then simply add the results later – a process known as *registered reports*. The journal has agreed to publish the article regardless of the results and therefore the incentive to hack is dramatically reduced. This agreement comes from a peer review process without results. This means peer reviewers cannot reject results that they do not like or that do not support their research. In a preregistration you must think much harder about factors such as meaning, causality, inter-subjectivity and ‘how the world probably works’. You cannot hide behind results in this process and therefore you must anticipate counterarguments and explore counterfactual logic. This improves the clarity of theory and research, creating an immense gain in efficiency and effectiveness.

Regardless of the methods you use there are many opportunities to take advantage of preregistration. Some forms of qualitative research, for example those involving grounded theory and interpretivist methods, require decisions during the research process that cannot be foreseen. This uncertainty can be outlined in a preregistration stating explicitly when flexibility is and is not admissible (Haven and Grootel 2019). Some scholars using quantitative methods might assume that preregistration is not possible because they work with secondary survey data. But the regularity and release of these survey data are known in advance, and these scholars can preregister their studies before the data are collected with the knowledge of which questions and countries will be available.

Decommodify science

The central functions of the scientific publishing industry are printing and disseminating knowledge, which historically solved a problem of how to share knowledge across universities and countries. The business functions of publishing, however, come with harmful byproducts. Publishing firms extract profits from scientists twice. First, scientists provide free labor in the form of editing and peer reviewing, in addition to producing the results for the articles to be printed. Next, researchers, or their employers, must purchase the product of their own labor; labor not paid for by the publishers. The journal article as a product comes at a high cost, and often only in packages of journals meaning that universities have to pay for extra material their scholars do not use.

Sometimes publishing houses neglect science in favor of profits, but Elsevier has been particularly problematic. They sponsored arms fairs, created and sold ‘fake’ journals to pharmaceutical companies to publish ‘results’ supporting their drugs, purchased the *Social Science Research Network* and created paywalls or removed legally shared working versions of articles, charge fees for open access articles, and actively lobby against open access legislation¹⁷. This brought massive protests against Elsevier¹⁸. You can take action and refuse to review for or publish with unethical publishers if you feel it is justified. Thus, you should inform yourself about the publishers. Your libraries are a source of information, because they deal with the business side of publishers.

If you are in Europe, check if your institution is a signatory of ProjektDEAL. A consortium of universities are collectively bargaining with publishers via ProjektDEAL demanding that publishers reduce fees and eliminate the double paying of universities. The primary objective is that publishers sign country-wide subscription agreements that enable access for all universities at once. Wiley agreed to such a model and this marks a paradigm change. It indicates how the publishing industry looks in the future, so long as the OS Movement proceeds. If you are not in Europe, consider starting a similar initiative, for example the entire University of California system¹⁹ recently followed ProjektDEAL and boycotted Elsevier (Kell 2019).

¹⁷ For a concise summary with links see Tal Yarkoni’s blog entry <https://www.talyarkoni.org/blog/2016/12/12/why-i-still-wont-review-for-or-publish-with-elsevier-and-think-you-shouldnt-either/>

¹⁸ For example, The Cost of Knowledge and ProjektDEAL.

¹⁹ This includes 10 universities, 5 medical centers and several research institutions that collectively produce roughly 10% of the world’s academic publications.

You can also work around the publishing business. Prior to submitting an article or after it is published, you have the right to share a *preprint* – a draft of the paper you share publicly so long as it is not published elsewhere or sold for profit²⁰. Posting preprints reduces the power that publishing firms have over science, in addition to giving others immediate access to your work. But simply posting preprints on your academic website is not open enough. Use a preprint service, for example through the *Open Science Framework*, to ensure that your preprints appear in search engines such as *Google Scholar*. This enables scholars to find and directly access research results based on the words they contain, uninhibited by paywalls. Preprint services are free and open access.

Finally, there is widespread support for open access. Practicing sociologists are not a major deterrent to this. At least not those who responded to the Wiley author survey, where 67% of respondents would like to see their academic societies provide more open access publications (Roscoe 2020). In 2018 open access was the sixth most important thing an academic society should be doing, and in 2019 it was number one. This is a monumental shift in just one year. Changing to open access would be financially harmful to publishing houses and reduce the revenues that academic societies draw from the publication process – a key reason the ASA does not support open access requirements attached to grant funding (Cohen 2019). This means we are operating in an academic setting where having hoarding money in academic associations is more important than removing barriers to global access to science.

Conclusion: The Theory in Open Science

The difference between a theory and an assumption is reliable research. Given current sociological practices we cannot be sure the knowledge we produce is reliable, for example when the research process and resulting data are hidden, then any mistakes or scientific misconduct are unknowable. Theories derived from unreliable results are unreliable. An ultimate goal of sociology is the production and refinement of theory to predict, explain, demystify, or (re-)interpret features of the social world. Theories are the building blocks of social change. If we do not have useful theories of poverty, discrimination, group identity or climate change for instance, then we cannot tackle related social problems. Practicing transparency allows you to produce reliable theory and, when we all do it, sociology is reliable.

²⁰ You should investigate the exact details of this as they vary by publisher and journal.

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Appendix A – Box 1 Original Language

Deutsche Gesellschaft für Soziologie ‚Ethik-Kodex‘ 2019 I.1.2²¹ – Bei der Präsentation oder Publikation soziologischer Erkenntnisse werden die Resultate ohne verfälschende Auslassung von wichtigen Ergebnissen dargestellt. Einzelheiten der Theorien, Methoden und Forschungsdesigns, die für die Einschätzung der Forschungsergebnisse und der Grenzen ihrer Gültigkeit wichtig sind, werden nach bestem Wissen mitgeteilt.

[When presenting or publishing sociological findings, the results are presented without falsifying omissions of important results. Details of the theories, methods and research designs that are important for the assessment of the research results and the limits of their validity are given to the best of our knowledge.]

American Sociological Association ‚Code of Ethics‘ 2019, 12.4.d.²² – Consistent with the spirit of full disclosure of methods and analyses, once findings are publicly disseminated, sociologists permit their open assessment and verification by other responsible researchers, with appropriate safeguards to protect the confidentiality of research participants.

[Entsprechend dem Geist der vollständigen Offenlegung von Methoden und Analysen gestatten Soziologen nach der Veröffentlichung der Ergebnisse ihre offene Bewertung und Überprüfung durch andere verantwortliche Forscher, wobei angemessene Schutzmaßnahmen zum Schutz der Vertraulichkeit der Forschungsteilnehmer getroffen werden.]

Japanese Sociological Society ‚Code of Ethics‘ 2019, Article 9²³ - <相互批判・相互検証の場の確保> 会員は、開かれた態度を保持し、相互批判・相互検証の場の確保に努めなければならない。

[Platz für gegenseitige Kritik und Überprüfung sichern] Die Mitglieder müssen eine offene Haltung bewahren und sich bemühen, einen Platz für gegenseitige Kritik und Überprüfung zu gewährleisten (die Verifizierung).

[(Securing a place for mutual criticism and verification) Members must maintain an open attitude and behavior to ensure a place for mutual criticism and verification.]

²¹ https://bds-soz.de/BDS/fachgruppen/ethik/Ethik-Kodex_Satzung_141003.pdf, accessed Sept. 13th, 2019.

²² <https://www.asanet.org/code-ethics>, accessed Sept. 13th, 2019.

²³ <https://jss-sociology.org/about/ethicalcodes/>, accessed Sept. 13th, 2019.

Appendix B – Box 2 Original Language

German - Wissenschaft, Lehre vom Zusammenleben der Menschen in einer Gemeinschaft oder Gesellschaft, von den Erscheinungsformen, Entwicklungen und Gesetzmäßigkeiten gesellschaftlichen Lebens.

[Science, teaching of the coexistence of people in a community or society, of the manifestations [institutions?], developments and laws of social life.]

English - die Wissenschaft der Gesellschaft, der sozialen Institutionen und der sozialen Beziehungen; konkret: die systematische Untersuchung der Entwicklung, Struktur, Interaktion und des kollektiven Verhaltens organisierter Gruppen von Menschen.

[the science of society, social institutions, and social relationships; specifically: the systematic study of the development, structure, interaction, and collective behavior of organized groups of human beings.]

Japanese –人間の社会的行為と関連づけながら、社会生活・社会組織・社会問題などのしくみを明らかにしようとする学問。

[Eine Wissenschaft²⁴, die versucht, die Mechanismen des sozialen Lebens, der sozialen Organisation, der sozialen Probleme usw. in Bezug auf das soziale Verhalten des Menschen zu klären]

[A science that seeks to clarify the mechanisms of social life, social organization, social problems, etc., in relation to human social behavior.]

[author's own translations]

²⁴ 学問 can also be translated as 'area of study'.

Appendix C – Box 3 Original Language

German - (ein begründetes, geordnetes, für gesichert erachtetes) Wissen hervorbringende forschende Tätigkeit in einem bestimmten Bereich.

[Logical, orderly, reliable knowledge-producing research activity in a particular area.]

English - the intellectual and practical activity encompassing the systematic study of the structure and behavior of the physical and natural world through observation and experiment²⁵.

[Die intellektuelle und praktische Tätigkeit umfasst das systematische Studium der Struktur und des Verhaltens der physischen und natürlichen Welt durch Beobachtung und Experiment.]

Japanese – 自然や社会など世界の特定領域に関する法則的認識を目指す合理的知識の体系または探究の営み。実験や観察に基づく経験的実証性と論理的推論に基づく体系的整合性をその特徴とする。研究の対象と方法の違いに応じて自然科学・社会科学・人文科学などに分類される。狭義には自然科学を指す。

[Ein System oder eine Erforschung rationalen Wissens, das darauf abzielt, Bereiche der Welt wie Natur und Gesellschaft formal zu verstehen. Es enthält Fakten, die auf Experimenten und Beobachtungen beruhen, und systematische Konsistenz, die auf logischen Überlegungen beruht. Es wird in Naturwissenschaften, Sozialwissenschaften, Geisteswissenschaften usw. nach dem Unterschied in Forschungsgegenstand und Methode. Im engeren Sinne bezieht es sich auf die Naturwissenschaft.]

[A system or exploration of rational knowledge aiming at formally understanding areas of the world such as nature and society. It features facts based on experiments and observations and systematic consistency based on logical reasoning. It is classified into natural science, social science, humanities, etc. according to the difference in research object and method. In the narrow sense, it refers to natural science.]

²⁵ This was taken from Oxford dictionary because Merriam-Webster offers a first definition of *science* as, “the state of knowing: knowledge as distinguished from ignorance or misunderstanding” and only in the third definition does it address the practice of science, as knowledge, “...especially as obtained and tested through scientific method”. But to get the ‘science-in-practice’ definition one also needs to look up “systematic” and “scientific method”, whereas Oxford’s definition has these concepts included without further need to look up words.