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Looking Back at *Social Knowledge in the Making*

Charles Camic, Neil Gross and Michele Lamont

We are grateful to Matteo Bortolini for initiating a symposium around *Social Knowledge in the Making* (SKM). As a collective project, this book was with us for several years and was a welcomed opportunity for stimulating dialogue between the three co-editors. It is with pleasure that we now respond to Matteo's invitation to reflect on the fate of the adventure two years after the book's publication. We address how it has been received, whether the reception has met our expectations, and respond to the specific reactions of Kelly Moore, Johannes Angermuller, and Kristoffer Kropp published in this symposium. We appreciate that these talented sociologists of the social sciences and the humanities took on the challenge of engaging our work.

What set the three editors off on a common adventure that resulted in the publication of SKM was a shared feeling of frustration concerning the field of the Sociology of the Social Sciences and the Humanities (SSH, for short). Knowledge in this fledgling field has typically been produced by three intellectual communities that largely operate in isolation: sociologists of knowledge, intellectual historians, and scholars in "science and technology studies" – especially, in the latter case, scholars concerned with economic knowledge. One typically observes a low level of cumulation of knowledge within and across those fields. It was our desire to go beyond the production of juxtaposed case studies, along with our shared belief in the importance of promoting theoretical development, which led us to bring together historians and sociologists interested in SSH. We were also frustrated that while the field of STS has been growing by leaps and bounds over the last decades, an analogous synthetic effort was still lacking for SSH, which we believed slowed down the development of the field. Thus we set ourselves on the path of

creating a conversation between a broad and diverse interdisciplinary group of authors who, while largely aware of their respective research, were not generally in conversation with one another. For this purpose we organized two conferences (one at the Radcliffe Institute for Advanced Studies and one at the Russell Sage Foundation) which led to the book under discussion.

One of the challenges that comes with the production of interdisciplinary books is that they typically do not have a readymade audience if their topic is not already fully constituted as an object of knowledge. Such interdisciplinary books often face the challenge of having to create an audience, instead of simply following a path of diffusion that is already well-defined by previous books on a similar topic. To some extent, this was the fate met by our book SKM. Of course, we were not working *ex nihilo.* Several recent and important books had been produced on SSH. But typically they were not engaged in the type of dialogue we were hoping for. Thus, we took the gamble of prying a diverse group of sociologists and historians to think more systematically about practices in the realm of knowledge production, diffusion, and application. While we were satisfied with the outcome, we also felt that the book did not receive the resounding echo we were hoping for. Of course, sessions around the book were organized at various professional meetings, and many colleagues discussed the book with us. But nevertheless we felt that perhaps some did not see the necessity of our proposed agenda. Was this because SSH scholars are content with thinking of knowledge production as a historically contingent processes unworthy of broader theorization? Or that the field of STS is one in which cumulation counts for little? Or is it that sociology is largely marginal to the STS effort, with the result that few of our STS colleagues would be taken by our proposal? Or that some STS folks engage with high status theories (with Deleuze, Latour, Haraway and others rotating as flavor of the month)

more as status signals than with an eye for thinking empirically about how the world works? Of course, we do not have the evidence needed to answer these questions. But we are left wondering about the trajectory of the book, less than two yeas after it hit the bookstores (or Amazon's website).

There is no denying that the field of the sociology of knowledge has experienced considerable growth over recent years, especially in France and in Europe more broadly (largely thanks to the influence of a Bourdieusian-inflected sociology of symbolic fields). The amount of scholarship coming out of Paris on intellectual movements, schools and politics, is impressive. And it is often produced by particularly strong group of social scientists who are as theoretically sophisticated as they are skilled at empirical research. Despite these strengths, considered globally, the sociology of knowledge remains a small and somewhat marginal field in the broader sociological landscape. The size of this field is, at present, a real limitation to knowledge diffusion and to our ability to shape in a significant way a sociology of the social sciences and the humanities. But this situation need not remain as it is: A stronger case needs to be made for a general engagement of sociologists with the conditions of knowledge production. This should be a basic component of the training of graduate students, particularly in the context of methods courses where students are encouraged to reflect on what Bourdieu called "construction d'object" (or theorization).

These reflections relate to the reviews that Matteo invited for the symposium. Turning to these directly, we would begin by saying that we greatly appreciate the three review essays; the reviewers' thoughtful and largely favorable response to our project is heartening as we seek to catalyze research of the making – and the evaluation and diffusion – of social knowledge. We find their more critical comments valuable as well – even as we can't help noticing that the

critiques pull in contrary directions, as we might expect from scholars so different from one another: Kelly Moore, one of the major proponents of a macro-level political sociology of science; Johannes Angermuller, a leading discourse analyst; and Kristoffer Kropp, a sociologist of knowledge in the tradition of Pierre Bourdieu. The differences among these three scholars afford an enlightening variety of perspectives on SKM, but they also result in critiques that want different things. Moore wishes that our volume gave more attention to the role played in social knowledge by the "larger-scale dynamics" of social and political institutions; Angermuller faults us for focusing *too much* on "societal processes and dynamics [that have] a wider political relevance," rather than offering a fine-grained discourse analysis of knowledge production; while Kropp finds us *insufficiently relevant*, especially according to a Bourdieusian standard of reflexivity.

In light of our opening remarks, we are gratified that all three reviewers concur with us that social knowledge warrants much greater attention from scholars that it has thus far received; rather than dismissing our project as a dead end, the reviewers strongly endorse it – to the point that each of them would like to see the project encompass additional topics within its purview. In this regard, our only quarrel is that it's hard to imagine that *any single volume* could address all of these topics, particularly when a line of research is barely out of the starting gate. We intended SKM to be an opening salvo, not a culmination, and we would be disappointed if it failed to whet the reviewers' appetite for more. That said, we are pleased that SKM actually contains one or more chapters directly related to each of the subjects the reviewers identify as neglected – that the volume offers a beginning, though hardly the last word, on all of them. (This is one of the advantages of having a three-person team at the editorial helm.)

Accordingly, we hope that readers of SKM will understand that our wish to cast the net widely at this stage – and to enlist into the dialogue as many of the existing strains of work on social knowledge-making as we could squeeze into one volume – necessarily prevented us from concentrating on any one of these lines of work in the detail that scholars specializing in them might want. In adopting this plan, we were certainly aware that we were setting ourselves up for the charge that SKM is too "heterogeneous"; and, indeed, this criticism is the one point of agreement across the three reviewers, even as each has in mind a very different solution to the problem. Regardless, we ourselves do not see this "heterogeneity" as a drawback, given that we were aiming to construct a Big Tent. Had we (prematurely) narrowed our focus, we might have avoided the charge of heterogeneity, but greater exclusivity would have compromised our goal (while opening us up all more to the charge that we were neglecting certain topics). For the truth is that the very genre of an edited volume, considered as a knowledge-making practice, is unavoidably either too inclusive ("heterogeneous") or too exclusive ("narrow"): each objective necessarily comes at the price of the other – as we actually see in three essays in the symposium itself. Like us, Matteo, in his role as an editor of *Sociologica,* decided to be inclusive of different voices, only to leave us with reviews that, when taken together, lack the unity that the critics find missing in our volume. Yet, had he gone the opposite way, the symposium would offer a narrower, less stimulating, range of critiques. We like the choice the editor made.

These are global responses to the Moore, Angermuller, and Kropp essays. Some more specific responses are also in order. Let us begin with a comment on the Moore piece. Although it is true, as we noted above, that Moore is widely known for her effort, with Scott Frickel, to push the sociology of science back in an institutional and political economic direction in the years following the rise of the sociology of scientific knowledge—which forefronted the micro

and eventually bogged down in epistemological imponderables—her work has always highlighted resistance alongside broader social forces. Her book *Disrupting Science* took up the macro historical conditions that underlay the growth of the physical sciences and engineering in the U.S. in the post-World War II period, but no less the organized opposition to certain uses of science and technology mounted by a minority of scientists—for example, scientists turned peace activists who were inspired by Quaker ideals. Given this orientation, it is no surprise that she takes special interest in the chapter in our volume by Igo, which speaks not to political activism among scientists, but to that spontaneous resistance to governmentality that may be exhibited by humans who come under particular forms of social scientific study, and to the efforts that must be made by social knowledge workers (in this case, those in public opinion polling firms) to overcome it. For a similar reason, Moore appreciates the chapters by Lemov and Knorr Cetina, where the resistance displayed comes not from people who object to being put under a microscope, but from the complexity and often ineffable nature of social scientific data, which can prove far less tractable than makes or users of social knowledge may hope. Moore wishes that more of our chapters had highlighted the resistance theme, and that we had made more of it in our introduction. Indeed, she feels that such a move would have helped us better specify what is "social" about social knowledge.

We appreciate the point—and note that it was our interest in the challenges faced by social knowledge workers in virtue of the sometimes recalcitrant nature of their objects of investigation that led us to solicit the very chapters she flags. Yet in social knowledge making object resistance is not constant. Often, unruly subjects can be made at least temporarily ruly; hopelessly complex financial flows can be mapped; files of incredibly complexity can be assembled, even if they do not ultimately yield the results desired for them—just as in the

physical and biological sciences what Joan Fujimura calls "standardized packages" for doing research can, with effort, be assembled from the messiness of organic materiality. Igo, for instance, tells the story of pollsters who had to overcome resistance by building trust with the public, and by convincing skeptical Americans that taking part in opinion polls was a contribution to the democratic project. But the point of her analysis, in the chapter for our volume and in her book *The Averaged American*, is that eventually resistance *was* overcome. Polling became a well-established institution and social technology—and transformed citizens and American political life. We need studies of social knowledge making and use in the later phases of object stabilization, where knowledge practices are institutionalized, as well as in the early phases, and we saw (and continue to see) no reason (other than political priors) to privilege one over the other in the articulation of a broad program for research.

Angermuller, for his part, also wishes that our volume had more to say about what he studies: discourse. There can be no doubt that language and meaning are essential to social knowledge making, as they are to every form of social activity, and we applaud efforts at social analysis that attempt to bring this out. But we would strongly resist any suggestion that the study of social knowledge be *reduced* to the study of discourse and discursive practice. Makers and users of social knowledge position themselves discursively, to the extent they do, as part of the process of positioning themselves socially and attempting to achieve interactional as well as broader social ends. To focus only on the former is to ignore the multitude of social and institutional dynamics in which social knowledge makers are ensnared, as well as the material effects of their activity. In fact, a solely discursive approach would seem to verge on the idealism that the sociology of knowledge, in several of its classical forms, was invented to counter.

Finally, Krupp questions the intellectual coherence of the research program we propose, expresses some skepticism about its explanatory utility, and wonders how, concretely, studies of social knowledge making might be made to contribute to social scientific reflexivity. On the first two points, we cannot help but feeling misread. Krupp reads us saying that our project is new in that it focuses on social knowledge—a category that he finds unjustifiably broad. As to utility, Krupp heaps attention on the ten claims we made about social knowledge practices at the end of our introduction, and asserts that they are only significant to the extent they can be transformed into research hypotheses. But the point of our volume was *not* to define social knowledge as a new object of investigation. As we make clear in the introduction, and as we have already said here, a fair amount of research about social knowledge has already been carried out, by sociologists of knowledge, intellectual historians, and others. Our intervention was not primarily to call for more such work (though we would like to see it), but rather to push this work toward a focus on the *practices* through which social knowledge is made and utilized. And while here, as in any other area of social scientific investigation, clear definitions are important, it was exactly our focus on practice that kept us from offering a definition of social knowledge that would once and for all rule certain kinds of epistemic claims as belonging to the category and others as not belonging. For the boundaries separating "social" from other forms of knowledge are not stable historically. They are made and remade in different ways in different contexts through practices of boundary work that are among the activities to which we want to call attention, in much the same way that—as STS scholars have shown us—the boundaries between science and non-science are fluid and often at issue in scientific disputes. It is the strong practice focus that, we believe, gives coherence to the chapter volumes; while the ten summary points about the

practices sketched by chapter authors were intended to do no more than flag some thematic overlaps.

As to reflexivity, we agree with Krupp that it is interesting to ask "how the knowledge gained from studies of social knowledge making should and could be transmitted to other social knowledge makers?" Yet we believe the best way to answer this question is through empirical research. Rather than assuming that knowledge of the process of knowledge making will lead automatically to heightened reflexivity, or outlining theoretically why and how it should in principle, the next step for meaningful work on reflexivity is to consider it as a practice: to study the actually existing historical, institutional, and intellectual conditions under which social scientists, broadly construed, engage in practices that have a significant reflexive component; the varying forms of reflexivity exhibited in various times and places; the consequences (if any) for the content of knowledge claims; and the interactional, political, professional, and even psychological interests these practices serve. The chapters in our volume by Heilbron and Strathern begin to explore these questions but much work remains to be done—as is true with respect to social knowledge making practices more generally.