Visions of a Field: Recent Developments in Studies of Social Science and Humanities
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What is This?
Visions of a Field: 
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Christian Dayé¹

Abstract
This field review discusses several recently published books that are concerned with historical, cultural, philosophical, or sociological aspects of the social sciences and humanities (SSH), past and present. It investigates similarities and differences between the various perspectives and approaches, and analyzes how these are informed by different visions of the field of SSH studies. In concluding, the review discusses three recurrent themes that will presumably move in the focus of debate in the near future: the debate on positivism in SSH and its “epistemological others;” the impact of the Cold War on the gestalt of the SSH; and, finally, the adequacy of science, technology, and society approaches to describe techniques and practices in the SSH.

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Field Review

Reviewed Books


Visions of the Field

This field review discusses several recent publications that are concerned with historical, cultural, philosophical, or sociological aspects of the social sciences and humanities (SSH), past and present. It finds itself in the strange, but interesting situation that a recurrent lament among those interested in exploring the SSH is that no such field exists. Science and Technology Studies (STS), it is said, is overly concerned with the natural sciences, and studies on the SSH often lag the theoretical and conceptual sophistication achieved within STS. This view fueled recent proposals for a “new sociology of ideas” (Camic and Gross 2001), and it motivated the editors of one of the books reviewed, *Social Knowledge in the Making*, to express the hope that their book would “open up an agenda of new questions” and thereby “encourage more research in this vein—research that not only would eventually correct some of the existing imbalance between the study of natural knowledge and the study of social knowledge but (more importantly) would raise the general level of understanding of the processes by which different forms of social knowledge are produced, evaluated, and put to use” (Camic, Gross, and Lamont 2011, x). These authors envision the
field of SSH studies as not yet extant; moreover, the development of such a field should be proactively promoted.

Apparently, here the guiding vision of a field is as an institutionalized discourse on predefined issues, as an academic tribe dwelling within the boundaries of its established territory (Becher and Trowler 2001). In the case at hand, however, such definition of a field leads the reviewer into a dilemma—if the lament cited above is true, there is not much to review. Accepting that SSH studies are scattered over a broad variety of publication outlets and disciplines, the definition that guides this review is broader. It assumes that scholars, although belonging to different academic tribes, nonetheless take notice of what members of other tribes say on that topic. A historian of economics working on research instruments may not publish in STS journals, but may nonetheless be well informed about STS work on instruments, and even use that work. This broader but also more futile vision of a field offers as a way out of the apparent dilemma of reviewing a field that, in the eyes of many, does not exist.

What Did/Do Scientists Do?

The first book discussed here, *Social Knowledge in the Making*, is a collection of essays that emerged out of a series of two seminars held in 2005 and 2007. As mentioned previously, the editors frame their project by arguing that the nonexistence of a field (in the first sense described previously) for social studies of SSH is a problem, but a problem that can be solved. The authors suggest that a sustained focus on practice could function as a means to create contiguity and could weave together the various threads of scattered research projects into a complex but interrelated academic field. Practices are defined by Camic, Gross, and Lamont as “the ensembles of patterned activities [...] by which human beings confront and structure the situated tasks with which they are engaged” (Camic, Gross, and Lamont 2011, 7; emphasis in original). This definition includes “both the ‘taken-for-granted routines’ to which Swidler calls attention and to which Schatzki refers as ‘open sets of nonregularized actions’” (Camic, Gross, and Lamont 2011, 7; emphasis in original). Most of the chapters in *Social Knowledge in the Making* indeed focus on practices of knowledge production, evaluation, and dissemination, with the restriction that some authors do not fully follow the above-mentioned definition, so that the focus on practice sometimes comes to mean a focus on what persons actually do or did (in contrast or in addition to what they thought), leaving aside the search for patterned activities.
The book is structured in three parts. Part I is concerned with "Knowledge Production in the Disciplines" (part I) and comprises texts by Andrew Abbott, Anthony T. Grafton, Rebecca Lemov, Neil Gross and Crystal Fleming, and Johan Heilbron. Part II, "Knowledge Evaluation Sites" brings together chapters by Michèle Lamont and Katri Huutoniemi, Laura Stark, and Marilyn Strathern that ask: how are project proposals evaluated in the SSH? And how, in turn, do scientists design their proposals in anticipation of the evaluative deliberations? Finally, part III, "Social Knowledge beyond the Academy," contains chapters by Sarah E. Igo, Sheila Jasanoff, Grégoire Mallard and Andrew Lakoff, Daniel Breslau, and Karin Knorr Cetina. Their texts explore how social knowledge is used outside the sciences.

One striking feature of Social Knowledge in the Making is that it brings together essays by widely renowned ivy league professors—Andrew Abbott, Anthony Grafton, Sheila Jasanoff, or Karin Knorr Cetina, to name a few. This is apparently a consequence of the implied vision of the field. The higher the reputation of the contributors, the editors might have thought, the likelier the volume will become a milestone in the field’s establishment.

Abbott’s text focuses on those branches of SSH that conduct library research, which means that their data are materials that are recorded and deposited in libraries, archives, and comparable places. Abbott argues that three major ruptures have marked library research in the twentieth century—the World Wars I and II and the academic market crash of the 1970s—and shows that these ruptures have left their marks on how librarians and researchers conceived of the correct practices of library research. These conceptions diverged and caused and continue to cause major tensions. The preferred practice of library researchers is to go into the library and browse the shelves, hoping to find serendipitously items they did not search for. Librarians, on the other hand, conceive of library research as starting with databases, thereby producing a list of items to be inspected which can then be gathered by the librarian or by interlibrary loans. Defining students as another important client group, university librarians increasingly attempted to restrict researchers’ access to the shelves.

Grafton’s chapter on the history of professional history in America argues that this branch of academia today wrestles with dilemmas that can be explained by changes “in the late nineteenth century, when [...] an artistic form of writing once cultivated by and for members of a social and political elite was reconfigured by the anxious, underconfident hand of upwardly mobile professional scholars” (Grafton, in Camic, Gross, and Lamont 2011, 90). Jasanoff’s chapter discusses objectivity in regulatory science, using
empirical studies of contemporary regulation processes in the United States, the United Kingdom, and Germany to show that each of these political cultures achieves objectivity by different procedures and devices. Whereas numerical data and formal models play important roles in US policy processes, in the United Kingdom personal characteristics of the acting persons count decisively in comparable processes: the presiding individuals achieve “epistemic authority” by their ability to communicate knowledge in a way that is accessible to everyone. German parliamentary inquiry commissions, in contrast, establish objectivity by being staffed by politicians and experts according to the distribution of seats in the parliament. The chapter by Knorr Cetina deals with financial analysis, arguing this field went through a process of “scientification” in the 1950s and exploring how contemporary analysts experience and express the tensions involved in their trade between cool logical thinking and highly emotive involvedness with market processes.

Sarah E. Igo contributed an interesting chapter that is suggestive for how STS perspectives can inform sociological and historical studies of the SSH. Although her material comes from her book on The Averaged American (Igo 2007), she approaches it with a new question that relates in a very suggestive way to an established line of thinking within STS. Igo refers to the complex ways “nature” is made researchable in the natural sciences. Model organisms, genetically standardized specimens (mice, Drosophila flies), and even quarks are natural phenomena that undergo transformation to become accessible to research, and STS literature has explored many of them in sometimes intricate detail. No comparable attention, she argues, has been devoted to the ways in which practices of social research attempt to align their objects. She argues that “persuasion is one crucial, if nearly always unspoken or invisible, practice that social investigators rely on in gaining access to relevant research materials, making theirs a solicitous science” (Igo, in Camic, Gross, and Lamont 2011, 286). She then investigates some practices applied by public opinion pollsters in the 1930s and 1940s and describes how American citizens reacted to the results of such polls. She concludes that despite the failure of pollsters like George Gallup and Elmo Roper to convince the public of the merits of statistical sampling and the reliability of their results, they nonetheless successfully established the opinion survey as a crucial technology in American politics.

The Practice of Scientific Philosophy

In line with the main claim of Social Knowledge in the Making, Joel Isaac’s Working Knowledge: Making the Human Sciences from Parsons
to Kuhn is also characterized by a strong and sustained focus on scholarly practices. This book sets out to describe the scenery in which Thomas Kuhn began to form the ideas that finally merged into one of the classics in twentieth-century historiography and philosophy of science, *The Structure of Scientific Revolutions* (Kuhn 1970). Usually, Kuhn’s work is treated as a kind of watershed, breaking with a restricted cognitive understanding of what science is and thus inaugurating the era of “post-positivism.” Isaac (2012, 4), however, argues that “Interpreting Kuhn’s breakthrough as an episode in the overcoming of ‘positivism’ or ‘traditional epistemology’ in the human sciences conflates the reception of Kuhn’s book with the historical context of its composition.” In fact, at the time when Kuhn shaped his argumentation, Harvard was home to many scholars from a variety of disciplines who, although nowadays often decried as positivists, shared a practice-oriented vision of how knowledge is created in the human sciences. Looking for traces of Kuhn’s thought, Isaac finds, for instance, physiologist L. J. Henderson, who promoted the case method as a mode of both instruction and research. He finds Percy W. Bridgman and S. S. Stevens developing operationism within psychology, a perspective that sets a concept synonymous with a corresponding set of operations, thereby attempting to diminish the discrepancy between scientific discourse and practice. He describes philosopher W. V. O. Quine claiming that even analytic propositions, and not only synthetic ones, are capable of revision in light of empirical experience. He also finds sociologist Talcott Parsons and his project of a general social theory, which emerged out of a perceived need to order and systematize the increasing mass of (war-related) research in the social and behavioral sciences since the 1940s.

Regardless the diversity of their projects, Isaac (2012, 5) argues that “These figures came to identify the creation of knowledge not with the abstract cogitation of ‘pure reason’ or with the iron laws of induction, but with the working knowledge and craft-like skill that typified the education and practical investigations of professional scientists.” They shared a “scientific philosophy” that emerged out of a reflection of scientific practice. Also, they agreed that university courses should not confront students of the sciences with abstract and prescriptive versions of epistemology, but rather introduce them to the practice of scientific research by reflecting on cases from the history of science. The specificity of Kuhn’s approach must be evaluated against this background. When one does so, however, much of the positivism versus post-positivism debate appears to be an invention *post festum* of science historiography.
The scholars who shared this practice-oriented “scientific philosophy” organized themselves in formal or informal organizations and groups that developed in parallel and across the traditional departmental structure of Harvard University. One is the Pareto Circle, a loose group of scientists and scholars who, under the leadership of L. J. Henderson, discussed the ideas of Italian sociologist Vilfredo Pareto, especially his notion of a social system, a notion that crucially informed many of the later social science projects of Harvard (especially Talcott Parsons’s). Another such group was the Science of Science Discussion Group, a platform for debates on philosophical issues raised by the logical empiricists of the Vienna Circle. Isaac introduces the notion of “interstitial academy” to describe the network of formal and informal organizations that mostly were not part of the Harvard’s official organization chart, but that proved vital for the establishment and development of certain directions of research and of philosophical reflection on science, and converged to create a practice-oriented scientific philosophy. Some of these became formally part of the university, either by readjusting the orientation of existing departments, as was the case with the increasing dominance of an operationist epistemology in psychological research, or by introducing new ones, such as the Department of Social Relations.

Members of the interstitial academy emphasized the importance of seeing science as a practice, both in pedagogic and in epistemological perspective. This is where Isaac’s analysis leads again to Thomas Kuhn. The analysis is comprehensive, both with regard to continuities and innovations to be found in Structure. From the brief overview provided here, two aspects can be meaningfully singled out: (1) the importance of scientific training in the fortification of paradigms: the scientific philosophy of Harvard’s interstitial academy emerged out of attempts to provide students with an understanding of what it meant to do science; and (2) the insight that there need not be a common set of rules to maintain the stability of a professional group, but that it suffices to have a set of recognized achievements which could be used as models for further research. Kuhn himself predominantly attributed this latter insight to his reading of Wittgenstein, but, as Isaac’s analysis suggests, it was also operational at the cross-disciplinary research endeavors that characterized Harvard’s interstitial academy.1

Cold War Social Science

The remaining two books, Shaky Foundations: The Politics–Patronage–Social Science Nexus in Cold War America, written by Mark Solovey, and a volume Solovey coedited with Hamilton Cravens, Cold War Social
Science: Knowledge Production, Liberal Democracy, and Human Nature, are representative of a series of recent publications and conferences on the history of SSH during the Cold War. In Shaky Foundations, Solovey explores the history of three major science funding sources for the social sciences in the United States during the Cold War, namely the National Science Foundation (chapters 1 and 4), the military (chapter 2), and the Ford Foundation (especially its Behavioral Science Program, chapter 3). The rise of these patrons offered SSH in North America the opportunity to explore their usefulness and applicability to problems of the nuclear age, thereby fueling the process of professionalization of these sciences and promoting the separation of the social and behavioral sciences from the humanities.

Through the historical materials and documents of these patrons, Solovey traces the crucial debates about whether the social sciences met “scientific standards.” These standards, however, were defined by the patrons’ boards, which overwhelmingly consisted of natural scientists who imposed their understanding of terms such as objectivity, neutrality, and clarity. The ensuing debates on the character of the social sciences have been, as Solovey analyzes with reference to Gieryn (1999), marked by attempts of demarcation; they consisted of boundary work, and social scientists applied several strategies to present their work as “scientific,” which meant eligible for funding. Solovey’s (2013, 189) most consequential argument is that “Those debates along with the development of specific funding policies and programs became critical in establishing the widespread acceptance of those scientistic and social engineering commitments in the new patronage system, which, in turn, encouraged their prominence throughout the social and psychological sciences during the first two Cold War decades.”

Undoubtedly, the idea to follow the debates on scientism and social engineering in the historical documents of some of the major patrons of science in the Cold War era is cogent, and Solovey realizes it within a broad study design that opened up a vast amount of historical material. The book is important because many issues in the history of the social sciences in twentieth-century America (and beyond) can only be fully understood with reference to patronage relations. Solovey’s analysis is convincing, and while he refrains from inflationary use of the notion of boundary work, he also does not offer much conceptual development of the term. His detailed descriptions of the conflicts surrounding the scientistic and social engineering perspectives informing the funding policies, and the heated debates about the nature and role of social science into which these conflicts led, tremendously increase the value of the book, especially given the presence of a certain regression narrative, to which I will return subsequently.
However, the second part of his argument—namely, that the high currency of scientistic and social engineering arguments among the patrons fostered their prominence within the social sciences—is not elaborated further. I think that the crucial question arising out of Solovey’s treatment of the historical material is whether and how this rhetoric affected the actual practices of social scientists. Two ways that Solovey’s study could have addressed this question immediately come to mind. One would have been to seek more explicitly the relations of his material to ongoing historiographic debates within the disciplines under scrutiny. In the historiography of American sociology, for instance, there is an ongoing debate on “mainstream”—what was mainstream sociology, if it ever existed, and what role did the funding organizations play in establishing and consolidating it (see, e.g., Turner and Turner 1990; Turner 1994, 1998; Calhoun and VanAntwerpen 2007). There are several points of connection in this literature that would help elaborating on the impact of funding policies on the development of the social sciences. A second way would have been to look more closely, if the sources allowed for such research, at how officials decided on which proposals to fund—and, more importantly perhaps, which not. Such research could perhaps be informed by the methodological framework developed by Michèle Lamont, investigating how the meaning of “good social science” was negotiated among those deciding about project proposals (cf. Lamont 2009; and the chapter in Social Knowledge in the Making, coauthored with Katri Huutoniemi).

Comparable to Social Knowledge in the Making, the chapters collected in the volume edited by Solovey and Hamilton Cravens, Cold War Social Science, are also united by a distinct intellectual leitmotif, which in this case is the presentation of case studies of “Cold War Social Science” and the lessons to be learned from these studies about the concept itself. Unlike Social Knowledge in the Making, however, Cold War Social Science has no explicit interest in consolidating or establishing a field. Its chapters are ordered in three parts to reflect the overarching orientations of the articles, but also to “illuminate central three themes whose significance extends beyond the Cold War era” (Solovey, in Solovey and Cravens 2012, 6): knowledge production, liberal democracy, and human nature. It is assumed (and partly shown) that contemporary debates on these three themes are still informed by positions that emerged under the influence of the Cold War. This applies to scientific disciplines like linguistics (treated in chapter 4 by Janet Martin-Nielsen) or theory traditions like decision theory (treated in chapter 6 by Hunter Heyck); but it also extends to discourses about human reason (as in Heyck’s chapter) or to discourses about the human
nature and human–machine interaction (treated, for instance, in chapter 10 by Edward Jones-Imhotep).

The trajectory of linguistics, as Martin-Nielsen shows, was inseparably tied to the development of the computer. Previously only a minor subfield at English departments, its pursuit of formalization helped linguistics achieve the reputation of being the most advanced of the social sciences. That nurtured the hope for automated language translation, a technology that had the potential to decisively shift the paradigm of Cold War politics. The computer played an important role in the rise of linguistics, yet, as Martin-Nielsen concludes, computers have not been part of the discipline’s official history.

Heyck’s chapter focuses on decision science, a field of research booming in North America of the early decades of the Cold War. He argues that the theorizing of choice within the SSH was embedded in a wider discourse about reason and democracy. In decision science, this embeddedness also informed the turn away from the individual to the decision as the basic entity in research. “Instead of asking whether people were rational creatures, the question should be, what is the best system for producing rational choices? The object of study needed to be the choice, not the chooser” (Heyck, in Solovey and Cravens 2012, 100).

Another chapter that immediately takes up a concern familiar to STS scholars is Jones-Imhotep’s on the trustworthiness of complex technological systems. He claims that making machines trustworthy involved both shaping the material object and the human user. Within this perspective, Jones-Imhotep explores the reliability crisis, a series of events starting in the early 1950s that shattered the trust of US officials in its weapons systems. Increasingly, human–machine interaction became an issue of concern. Even if the fear that soldiers operating weapons might behave wrongly, irrationally, or against their commands was not new, “the drive to synthesize [human actions] into one overarching system of reliability was unprecedented. The Cold War lent urgency to the search for a disciplined technology” (Jones-Imhotep, in Solovey and Cravens 2012, 190).

The quality of the chapters in Cold War Social Science varies; some provide interesting and novel insights; others are inconclusive, superficial, or contain errors. All texts attempt to establish links from their specific cases to Cold War politics (and, to a lesser degree, culture), and the ordering of the chapters into three blocks is persuasive. It is a pity that the collection comprises exclusively articles that describe the American part of the story; perspectives from the Soviet bloc or from peripheral countries would surely have contributed to the book’s aim of assessing the value of the “Cold War Social Sciences” concept. An even more problematic feature of some
chapters in *Cold War Social Science* is that they implicitly or explicitly follow the narrative, briefly alluded to above, that positivist thought and research massively dominated the social sciences at large (and not only Cold War Social Science) in the first two decades of the Cold War. Hamilton Cravens, for instance, asserts that “the social sciences were of a piece,” most notably because “they were all positivistic” (Cravens, in Solovey and Cravens 2012, 131). It should be emphasized that the authors of *Cold War Social Science* do not concur, but nevertheless in most cases follow a regression narrative which supports the image that SSH in the 1950s and 1960s, under the spell of American military and government funding sources, streamlined their epistemologies. Social scientists and humanities scholars thus produced results that might have matched the natural scientists’ conceptions of science, but are disappointingly naive and misled about the true nature of human beings. Though widespread (e.g., see Robin 2001; or, most recently, Erickson et al. 2013), this narrative insinuates that other, non-positivist or non-scientific forms of social science have not coexisted—an assumption which, of course, is factually wrong, both with regard to Cold War Social Science and to the SSH at large during the second half of the twentieth century. As Solovey notes in *Shaky Foundations*, there was a vibrant critique of behavioralist positions and projects within and beyond academia. Jamie Cohen-Cole’s (2014) recent book, *The Open Mind*, also discloses the regression narrative’s blindness to the multitude of other segments of SSH that did not follow scientific or positivistic forms of scientific thinking, but criticized them.

**Conclusion**

Studies of the past and present of the social sciences and the humanities do not yet form a field. Scholars working in these areas do not converge around a gravitational point—theoretically, methodologically, or organizationally, in journals and associations. Some disciplinary associations, both national and international, have sections dedicated to history, but to a lesser degree are there sections concerned with the present state of the discipline. There is a multitude of more specialized organizations, like *Cheiron: The International Society for the History of Behavioral and Social Sciences*, or its European offspring, the *European Society for the History of the Human Sciences*; again, the focus here is on historiography. STS would be a natural place for adding the missing perspective on contemporary structures and knowledge practices in the SSH. Here, however, the past decades have seen only reluctant approaches to studying SSH.
The task of writing a field review on this heterogeneous amalgamation of cultures and approaches is thus easier to complete with a broader vision of the field, one that does not assume the existence of a single academic tribe but is open to the phenomenon that the products of one tribe are acknowledged and used in others. Since one way to describe fields in this perspective is to focus on debates, let me sum up what I see as recurrent themes within contemporary studies of SSH. I see three of them and think that they will be addressed with even more emphasis in the near future: the debate on positivism in SSH and its “epistemological others” (Steinmetz 2005); the impact of the Cold War on the gestalt of the SSH; and, finally, the adequacy of STS approaches to describe techniques and practices in the SSH.

First, the question whether the SSH at large have been homogeneously positivistic in the middle decades of the twentieth century is still receiving a broad range of answers, even if one narrows it by referring only to specific disciplinary or regional contexts (e.g., sociology in North America). Among scholars studying the past and present of SSH, positivism still appears to be a non-neutral term, charged with a wide range of resentments that undermine sober debates about its historical relevance. Apparently, we have not yet made peace with positivism. In some arguments, as in Cravens’, cited above, positivism still functions as an umbrella for what is now deemed stupid, misled, and even worse, potentially dangerous SSH. This, of course, is an unhealthy form of presentism. But chances are that a debate about the role of positivism in past and present SSH will straighten out some of the distortions and leave more space for assessing what the scientists studied really tried to achieve. The books by Isaac (2012) and Solovey, and several contributions to the Cold War Social Science volume (e.g., by Isaac, Heyck, Howard Brick, and Michael Bycroft) contain useful facts and thoughts on this topic.

Second, a theme pulsating in many studies on the recent history of SSH is the impact of the Cold War on the epistemological and organizational shapes of the disciplines. This is related to the issue with positivism, but there are other aspects to it that justify treating it as a separate point. However, one can see a comparable trend to precipitately attribute changes in disciplinary settings to “the Cold War.” Often, however, such accounts do not clarify their understanding of “the Cold War.” Expanding a classification proposed in a review of Cold War Social Science by Will Thomas (2012), it appears useful to discern four instances of Cold War (Social) Science: (1) research done directly in support of military and global political activities, most of it under contract with the military; (2) research not directly related to military and global problems, but of which it was assumed that it might lead to insights relevant to research in category
(1), and therefore was funded; (3) research neither directly nor potentially linked to Cold War political and military activities, nor funded by the respective patrons, but was nevertheless supported by a rhetoric that however vaguely linked it to the national interest; and (4) research that happened to take place in the political and cultural context of the Cold War and, despite not being related to any political or military objectives, bears the marks of this context, either by tacit adoption or outspoken rejection of some current intellectual propositions. This categorical scheme could structure the debate in a way that allows for a more differentiated conversation and fewer misunderstandings among the scholars involved. One further lacuna, however, that limits the development of this line of research is that most work that is made accessible through publication in international outlets—and for the most part, this implies in English—is concerned only with the North American side of the conflict (notable recent exceptions are Gerovitch 2002; Engerman 2009; Smolkin-Rothrock 2010; Aronova 2011).

Finally, third, one debate that is not yet opened broadly, but might become so in the near future deals with whether available STS concepts can be usefully applied to describe techniques and practices in the SSH. Solovey, for instance, applied the concept of boundary work; Lemov, in her contribution to Social Knowledge in the Making, relies on the work of Susan Leigh Star and Geoffrey Bowker on classification to describe attempts to set up anthropological archives since the 1920s; and Igo transfers the idea of an alignment of objects from the natural sciences into public polling. Some other concepts, for example, Latour and Woolgar’s inscription devices, can also be expected to travel easily into studies of SSH. But this probably will not apply to all concepts. Until now, considerations on the adequacy of STS concepts are buried in the methodology sections of some studies of SSH, but it appears not unlikely that a debate will emerge that sets out to clarify this and related issues on a more general, theoretical basis.

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Notes

1. “Scientists work from models acquired through education and through subsequent exposure to the literature often without knowing or needing to know what characteristics have given these models the status of community paradigms” (Kuhn 1970, 46).

2. The reader interested in the transformations of the very idea of the Cold War is advised to turn to a volume recently edited by Joel Isaac and Duncan Bell (2012), *Uncertain Empire: American History and the Idea of the Cold War*. This book, however enlightening, was not included in this review because it focuses on a meta-debate in historiography and describes mostly developments and events in politics and popular culture; only the final two chapters, by Peter Mandler and Sharon Ghamari-Tabrizi, concern the social sciences.

3. Mark Solovey provides links to a multitude of reviews of the book on his homepage at the University of Toronto (http://individual.utoronto.ca/solovey/solovey/Cold_War_Social_Science.html, accessed May 7, 2014).

References


**Author Biography**

Christian Dayé is a post-doctoral researcher at the Department of Sociology of the University of Graz, Austria. His research interests include the history of the social sciences in the 20th century, sociological theory and methodology, and science studies. He is currently finishing a book on the development of social scientific techniques of prospection during the first decades of the Cold War.