

CRITICAL-RETROSPECTIVE ESSAYS

The Study of the History of Sociology and Neighboring Fields

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Hannah Arendt, Totalitarianism, and the Social Sciences, by **Peter Baehr**. Stanford, CA: Stanford University Press, 2010. 248 pp. \$57.50 cloth. ISBN: 9780804756501.

The Politics of Academic Autonomy in Latin America, edited by **Fernanda Beigel**. Burlington, VT: Ashgate, 2013. 290 pp. \$119.95 cloth. ISBN: 9781409431862.

Zygmunt Bauman: Why Good People Do Bad Things, by **Shaun Best**. Burlington, VT: Ashgate, 2013. 152 pp. \$104.95 cloth. ISBN: 9781409435884.

Social Science in Context: Historical, Sociological, and Global Perspectives, edited by **Rickard Danell, Anna Larsson, and Per Wisselgren**. Lund, Sweden: Nordic Academic Press, 2013. 288 pp. \$52.95 cloth. ISBN: 9789187351044.

A Transatlantic History of the Social Sciences: Robber Barons, the Third Reich and the Invention of Empirical Social Research, by **Christian Fleck**. Translated by **Hella Beister**. London: Bloomsbury Academic, 2011. 406 pp. \$75.00 cloth. ISBN: 9781849660518.

Becoming Mead: The Social Process of Academic Knowledge, by **Daniel Huebner**. Chicago: University of Chicago Press, 2014. 349 pp. \$35.00 paper. ISBN: 9780226171401.

Auguste Comte: Die Macht der Zeichen, by **Wolf Lepenies**. München: Carl Hanser Verlag, 2010. 208 pp. € 19,40 paper. ISBN: 9783446235724.

Identities and Social Change in Britain since 1940: The Politics of Method, by **Michael Savage**. New York: Oxford University Press, 2010. 282 pp. \$44.95 paper. ISBN: 9780199587667.

The Frankfurt School in Exile, by **Thomas Wheatland**. Minneapolis: University of Minnesota Press, 2009. 440 pp. \$39.95 cloth. ISBN: 9780816653676.

What do the following three have in common: East Europeans (according to Friedrich Engels), Ronald Reagan (according to his doctor), and sociology (according to this author)? Two things at least. They can be studied by sociologists, and all three have (or had) only an incomplete, fractured awareness of their own history. Engels and other Western European Marxists claimed that particular people living to the east did not have a history of their own because they lacked agents of nation-building. An increasing number of elderly people suffer from Alzheimer's disease, as former U.S. president Reagan did; and due to their growing dementia they cannot remember their own lives. In the case of sociology, the causes of the alleged Alzheimerian status have yet to be discovered. But the diagnosis is clear: the discipline's memory is rather limited.

Both sociologists as a group and sociology as a discipline are less interested in the past of their peers and institutions and less concerned about changes in the status of their field over time than other disciplines. A simple indicator for measuring a discipline's interest in its own past is whether there exist specialized journals for its history. We can find such publications in biology, economics,

medicine, philosophy, psychiatry, and psychology, but not in sociology. An attempt to start one in the United States failed in the 1980s, and similar stories could be told for French and German attempts.¹ The survivors are only journals with a broader range—"behavioral sciences" or "human sciences"—or highly specialized ones like *Durkheimian Studies/Études Durkheimiennes*, now in its twentieth year, or *Max Weber Studies*, now in its fourteenth year, but not the *Simmel Newsletter*, renamed in 1999 after nine years to *Simmel Studies* and discontinued in 2003.

Missing journals are the direct consequence of an insufficient number of potential contributors. Analogous to the non-evolution of a nationalistic discourse in environments lacking both surplus income and educated individuals who could start studying and inventing the past of their compatriots (which was the explanation for the backwardness of the so-called people without history), in sociology too few members are committed to studying their own discipline and its past continuously. Obviously, one reason for this disinterestedness lies in the lack of representation of the history of sociology (HoS) in sociology's curricula. Not only in the United States, but also in Europe, one only rarely finds courses on the history of sociology beyond the level of Classical Sociological Theory. Lack of teaching opportunities produces lacunae on the textbook side. Admirers of Humboldt and his plea for unity of research and teaching would be delighted to come across this instance of their hero's pedagogy, even if it is an inverted one.

However, this assessment is not the complete truth; there are small enclaves where research on HoS is accomplished. What follows is a helicopter view of HoS's

publication record over the last five years. As much as possible I will omit both journal articles and edited volumes. To systemize the discussion, I will arrange the literature according to subjects. On a not-too-generalized level one could differentiate contributions to the history of any scientific discipline as studies of one (or more) of the following aspects: people, ideas, instruments, institutions, and contexts. Analytically, I prefer to inspect the books from a methodological point of view. This means that the "how" is more central than the "what." In connection with the perceived disinterest of sociologists in sociology's past and the dementia diagnosis, answers to the "who" question are of particular interest. HoS is no closed shop; therefore, a critical-retrospective essay has to look beyond the territory reserved for sociologists.

People

The most popular unit of analysis addressed by HoS authors is people, in particular, individuals. Echoing the genius ideology, a single man or, since a generation ago, woman is onstage. The result is a biography, sometimes with an addendum like "intellectual." The format is mostly unspectacular and old-fashioned, and the hero's life is presented in conventional order: parents, birth, childhood, early experiences, continuing chronologically down to death, followed by the afterlife called reception and legacy. Readers of such biographies cannot recognize from the text whether the author is a sociologist or not; the discussion of theories and other insider topics seldom transcends advanced textbook coverage, most probably due to the publishing houses' pursuit of buyers.

A majority of the scholars in this area devote their whole career to their hero. Mary Pickering started her affair with Comte as a doctoral student in history at Harvard in the 1980s and finished the third volume of her biography in 2009. Dirk Kaesler wrote his first paper on Weber before he earned his Ph.D. and finished four decades later his authoritative portrait of the "Prussian, thinker, and mama's boy" (as the subtitle has it) in 2014. The nature of the relationship between the biographer and the subject is seldom as disinterested as the scientific

¹ See Jack Nusan Porter, "The Journal of the History of Sociology: Its Origins and Scope." *The American Sociologist* 35(3) (Fall, 2004), pp. 52–63. *Sociological Origins's* last issue dates from Fall, 2010. The last issue of the *Revue d'Histoire des Sciences Humaines* dates from 2011, after 25 volumes; *Tracés* started in 2002 and is still running with three issues per year. The German *Jahrbuch für Soziologiegeschichte* stopped appearing in 2007 and restarted recently under a new title: *Zyklus*.

ethos would call for. Like long-term imprisonment, long-term research changes the captive. Why does one sociologist receive the attention of biographers while others do not? From a casual reckoning, we have now at least four biographies of Weber, three of Theodor W. Adorno and C. Wright Mills, and two of Alfred Schutz, but none on Vilfredo Pareto, David Riesman, or Talcott Parsons.² Writing a biography is something that needs justification if the resulting book wants to belong to sociology. One life and one biography remain always just one case, but case of what? and why this case? Sociologists-as-biographers seldom address these questions. On the other hand, since biography writing is a time-consuming task, it would be unfair to ask authors to develop a comparative approach and use it for several cases. Lewis A. Coser's still highly readable *Masters of Sociological Thought* ([1971] 1977) could be named here as the exception which proves the rule.

A third particularity lies in the fact that one can write a biography only if the author under investigation left enough stuff behind, as did Karl Marx and Friedrich Engels. The better organized the *Nachlass*, the more probable it is that a biography, or more than one, will appear. Therefore we never will get a full-fledged biography on Georg Simmel, because his papers were looted when his son Hans fled the Nazis. If admiration is one prerequisite for writing a biography, well-organized—or, at least, available—papers are another.

It is not only narrow-minded sociologists who could mock this kind of literature as essentially non-sociological because it focuses on an individual instead of social entities like groups. However, one should keep in mind that producing books is seldom done by writers alone; the role of publishers and their marketing specialists have to be considered as factors as well. Marcel Fournier, biographer of Durkheim (2013) and Mauss

(2005), hinted at the fact that both his French and British publishers vetoed titles with no individuals' names in them.³ It is to Fournier's credit that both his books embed their main characters in their personal networks and social environments as far as possible.

Similarly rich descriptions are offered by a series of recent biographies, including Jeremy Adelman's biography of Albert O. Hirschman (2013), Lawrence Scaff's *Weber in America* (2011), and Jonathan Sperber's *Karl Marx* (2014). Joseph A. Schumpeter loved to read biographies in bed (as some of his now half-dozen biographers reveal), and he would have been happy with these because they make good reading, even if one recognizes that biographies seem to be a country for old men. I prefer not to share my experiences with catastrophic examples of biographies, but at least one observation needs to be made: the authors of the aforementioned books are all historians by education, whereas the failed biographies have been produced by sociologists proper.

Much less often one comes across studies on groups of sociologists. Some schools, in the broad meaning of people devoted to the same worldview forming what Ludwik Fleck (unfortunately, no relation) called a "thought collective," are covered more prominently than their rivals for no obvious reason. One virtually over-researched group is the one under the leadership of Max Horkheimer: the Frankfurt School of Critical Theory. Quite recently, the American historian Thomas Wheatland added some new insights in *The Frankfurt School in Exile* (2009) by investigating the Columbia University archives; I examined other American sources in *A Transatlantic History of the Social Sciences: Robber Barons, the Third Reich and the Invention of Empirical Social Research* (2011). Wheatland emphasizes that Horkheimer's group was seen as leaders in empirical social research by Columbia's authorities and therefore received an offer to complement the theory-heavy department there. The

² It would go beyond the scope of this essay to cover the genre of *Festschrift* for HoS. Obviously each of them reveals much about the celebrated, like status in the discipline and reputation among disciples; and the lists of contributors could be used to start investigating communication networks.

³ Marcel Fournier, "Biography in the Social Sciences: The Case of Marcel Mauss," in *Knowledge for Whom? Public Sociology in the Making*, ed. by Christian Fleck and Andreas Hess, Burlington, VT: Ashgate, 2014, 19–26; and personal communication.

newly arrived German mandarins declined the invitation cordially and chose a looser bond with Columbia. Ten years later, the same group of exiles was in financial turmoil, and Horkheimer was forced to change his mind. He lobbied the American Jewish Committee to hire him and his entourage. Forced to work like an ordinary white-collar employee made Horkheimer sick. He flew to Santa Monica and took Adorno with him as his personal assistant. With AJC money they hired Californian psychologists who then designed and administered the study published as Adorno et al. *The Authoritarian Personality*. The alphabetical sequence of the authors' names attributed, by implication, much more credit to Adorno (born Wiesengrund) than to the three others (who were kept away from any communication with the AJC bosses in New York). Both recent studies on the "Frankfurtists" (Bertold Brecht's malicious label) challenge the conventional wisdom that bemuses the Critical Fans; most likely, however, they will not change the myths surrounding this group.

Larger units than groups the size of schools or departments are studied only rarely. Pursuing collective biographies, what historians call "prosopography," would make it possible to go beyond the highly visible, well-established, tiny minority of "top dogs." Despite the difficulties of collecting enough comparable data, the outcomes are both revealing and rewarding. An analysis of two "generation units" (a Mannheimian term) of German-speaking social scientists—one going into exile because of the rise of the Nazi movement, the other remaining in their places of origin—reveals that on average the exiles climbed up the career ladder more quickly than the "homeguards" (Fleck 2011, Chapter Four).

In HoS, conventional social-research techniques are applied on rare occasions only. Sociologists seem to avoid bothering their peers with surveys and questionnaires, and as a consequence we do not know even basic socio-demographics about the members of our discipline: father's occupation, religion, and marital status are secrets, and nowadays not even data on age are at hand. Autobiographies become the only sources where such data are revealed occasionally, but even in the age of the chattering class only a minority

participates in this activity.⁴ The question whether today's sociology professoriate (at the local, national, or international level) is socially different from that of the interwar period (or any other period) cannot be answered. Stephen P. Turner (2014), who claims that U.S. sociology today is in the hands of Sociologists for Women in Society (SWS) activists, does not provide supportive data that meet sociological standards. Perhaps the data are not available; but as long as there is no chance to prove his hypothesis, one cannot suppress the feeling that Turner got something wrong: the obviously growing number of female sociologists might go hand in hand with an increasing amount of pressure from groups such as SWS and lip service (or more) to their demands, but anecdotal observations are no proof of a conspiracy of women. Historically informed people should recall similar allegations vis-à-vis Jews when their numbers in academia rose sharply.

Ideas

For easily discernible reasons, many HoS publications belong to this corner. Since the first appearance of the sociology of knowledge nearly a century ago, practitioners of this approach still prefer to study highbrow products like theories. Sociological theorizing proceeds not by the generalization and abstraction of empirical data and lower-level propositions, but by interweaving personal ideas and brainwaves with the close reading of any sample of contributions produced over the last century and a half. Therefore, a text which contains massive quotations from and references to G. H. Mead or Hannah Arendt, for example, does not reveal on first sight whether it is a HoS investigation of past authors or a contribution to

⁴ The production of autobiographies by sociologists is quite uneven. Besides shorter invited contributions to edited volumes (e.g., Deflem 2006) or to the *Annual Review of Sociology*, some sociologists write this kind of text and others do not. Three recently published interesting examples are from Edith Kurzweil (2007), Renée C. Fox (2011), and Arthur J. Vidich (2009); all contain extensive coverage of the subjects' social backgrounds.

contemporary debates. This is no criticism toward this kind of work. The only thing I want to highlight is that it is not always easy to judge publications fairly because of the difficulty of locating them properly in the increasingly confusing field of the social sciences. There are no rules separating contributions to contemporary theory from those belonging to HoS. In the spring of 2014, German television stations celebrated the 150th birthday of Max Weber and aired 90-minute debates held in the halls of the *Bayerische Akademie der Wissenschaften* in Munich. One of the debates was titled, referring to a famous Weber chapter heading, "Prefatory remarks on the process of rationalization—150 years after Max Weber," and the late M. Rainer Lepsius tirelessly argued in favor of Weber's contemporary relevance (for example, as the idol of postwar Germany's political elite). In his voluminous biography, Kaesler (2014) argues persuasively against bringing Weber and Weberian concepts too much into the present day, in very much the same way that Sperber (2014) portrays Marx as a nineteenth-century thinker. Weber is just the most telling instance of a man dead for nearly 100 years but alive like a coeval, at least for his admirers.

Peter Baehr's *Hannah Arendt, Totalitarianism, and the Social Sciences* (2010) is an excellent study about Arendt, her view on sociology (unfavorable!), and her position vis-à-vis others on the then heatedly discussed topic whether there are similarities or not between Hitler's and Stalin's dictatorships. Examining Arendt's communication with contemporaries and contrasting her viewpoints with other authors form Baehr's analytical strategy; sometimes he quotes data from unpublished sources, but his dominant methodology is close reading. He does not conceal his conviction that Islamism in the present day is another instance of totalitarianism, and therefore Arendt's insights are fresh even forty years after her passing. From Baehr we can learn that at times analyses of past texts are even better than using standard primary data collection.

Daniel Huebner's *Becoming Mead: The Social Process of Academic Knowledge* (2014) does not aim to influence present-day controversies, but it is a thoroughly academic

study inside the walls of HoS. Confronted with the puzzle that someone became "known in a discipline in which he did not teach for a book he did not write" (Huebner's characterization of Mead), Huebner chose the best approach by going into the archives to reconstruct the making of one of the most multifaceted American philosophers. Huebner excavated hitherto unknown manuscripts, originating from public lectures Mead gave over the span of two and a half decades; some of them became small articles published in unusual outlets, like a piece on hypnotism that was published in *The Dental Journal* in 1895 (Huebner found it thanks to digitalization). Later this year Mead's most famous book, *Mind, Self, and Society*, will come out in a revised edition due to Huebner's effort to locate additional notes taken by cohorts of Mead's students over a period of nearly forty years.

I do not want to be forced to choose between the two styles of research for which Baehr and Heubner function here as examples (and I am sure both are able to pursue the approach of the other); more work from either orientation would be useful. Studies like those by Baehr and Huebner support their arguments by examining sources beyond what is available in print, usually materials stored in archives accessible to specialists only. In the digital age, the wall between the unpublished and the published has become increasingly porous. One would expect that HoS researchers would have adapted to this new situation. But the truth is, they have not. Digitalized sources and text collections such as JSTOR offer new paths into the unknown and provide options for analyses unthinkable a generation ago. To give just two illustrations of potential new paths of research in HoS: Scholars interested in historical and sociological semantics can locate early appearances of new concepts without even leaving their desks (or their online computers); and the dissemination of sociological terminology into the larger public, which is sometimes accompanied by adopting sociological insights out there, can now be studied in such detail that comparative research (between disciplines, countries, thought collectives, etc.) is a sound option—but the number of people picking

up on these opportunities is very small, tending toward zero. In 1995, Robert K. Merton and Alan Wolfe published a small piece on “consumers” of sociological knowledge by counting how often words coined by sociologists were used in major U.S. newspapers. One must recall that when this research was executed, the Internet was invented but not used widely. Today such a study could be done on a much broader scale, but over the last two decades the Merton & Wolfe paper from 1995 did not get any resonance: it has been quoted only five times in Web of Science and twenty times according to Google Scholar.

I would not go so far as to let studies off the hook when they avoid the new opportunities. But of course, there are some new studies that deserve to be taken seriously even though they were executed very bookishly. In March of 2007, a celebrity sociologist's past caught up with him when a German-Polish historian made public in a German newspaper that Zygmunt Bauman was “Agent Semyon” in his youth, an informer for the military intelligence and a member of a counter-insurgency unit devoted to killing anti-communist resistance fighters. This was not news for the Poles, but outside Poland Bauman was seen as a victim of Polish anti-Semitism. On the Internet, the revelation traveled quickly, and since then no one could claim not to know about the affairs Bauman was involved in—but no one cared. Bauman reacted like all who have a skeleton in their closet: he blamed the bearer of the unwelcome news. He denied any wrongdoing and has received award after award since then. Very weird, and clearly a topic HoS researchers should attend to. Shaun Best took it up, using a traditional methodology but with appealing results. The subtitle of his *Zygmunt Bauman* makes his perspective clear: *Why Good People Do Bad Things* (2013). The author was interested in how Bauman became the PoMo pillar-saint by downplaying his Stalinist youth and reinventing himself after his 1968 forced exile. Best did pure library research, and he does not explain why he did not do more than that (such as archival research in Poland or elsewhere). Nevertheless, the book contains a revealing story, and as such it makes good reading.

Instruments

Whereas nobody could be surprised to come across studies on people and ideas in HoS, one needs to search harder to find investigations of scientific instruments. It is not really astonishing that the study of instruments is not well developed in sociology. Some people might even question the existence of instruments in sociology if one wants to go beyond paper and pencil, typewriters, or nowadays notebooks and smart phones. Of course, we do not have colossal machineries like CERN's Large Hadron Collider, and we do not even participate in functional magnetic resonance imaging studies as some of our neighbors from economics departments started doing some years ago. Even compared with psychologists (at least before brain scanning became *le dernier cri*), our sociologists' toolbox contains much smaller and cheaper instruments.⁵ But there are some, and these few require at least some time if one is to command them sufficiently well.

Andrew Abbott (2011) argues persuasively that the only tool humanists need is a well-stocked library, one that offers direct access to huge numbers of publications assembled according to a not-too-arbitrary system of classification. Sarah E. Igo, a historian by education, was not primarily interested in the development of social research instruments when writing *The Averaged American* (2007), but more in the consequences of their results on wider audiences. Nevertheless, her widely acclaimed work contains more on social research instruments than most books written by professional sociologists about the same period. Igo's narrative of the field trips of the Lynd couple to Muncie, Indiana, the victory of the Gallup surveys over the *Literary Digest's* postcard returns, and the celebration of Kinsey's expeditions into bedrooms does not reveal much for readers of the original or secondary literature. But Igo developed an understanding that empirical social research techniques did something to those under investigation after they became readers of the

⁵ The Center for the History of Psychology at the University of Akron houses a remarkable collection of artifacts and runs exhibitions. To my knowledge nothing similar exists for sociology.

aggregated presentation of their own stories. Knowing how often the average American practiced particular sexual activities transformed a number into a norm. Whether it is correct that these studies established a “mass public” must not be discussed here in detail, but what is astonishing when reading Igo is her nearly complete ignorance about what happened in sociology (and neighboring disciplines) at the very moment when *Middletown in Transition* (1937) or *Sexual Behavior in the Human Male* (1948) and *Sexual Behavior in the Human Female* (1953) came out in print. Neither the SSRC-sponsored *Studies in the Social Aspects of the Depression* (a dozen volumes in 1937) nor Riesman’s *Lonely Crowd* (1950) are even mentioned by Igo.

Mike Savage is not only a sociologist by profession but is one of its leading exponents in the United Kingdom today. His *Identities and Social Change* discusses what happened in the then still-immature discipline of sociology in his country from 1940 onward. He starts by analyzing the very British Mass Observation (what should one call it?) movement by going to its archive and examining both the instructions given to volunteers and the reports they sent in. Then Savage does something similar with the field reports of interviewers from one of the big studies in the fifties and the Goldthorpe/Lockwood study on Affluent Workers from 1963, plus some studies less known outside England. Here and there a non-British reader gets lost and finds it difficult to follow Savage’s argument, but overall this is a model of reflective investigation of what happened in sociology some decades earlier by applying today’s sociologists’ toolbox. My favorite finding is from page 100, where Savage characterizes Charles Booth’s data collection technique as “wholesale interviewing” because this pioneer distrusted ordinary people and spoke instead with members of the local elite. (Savage should have revealed the originator of the expression and could have added a footnote indicating that Weber did the same when he investigated the life conditions of rural workers east of the Elbe.)

Institutions

A broad definition of instruments could include even such abstract entities as

paradigms, either in the loose understanding of Kuhn or applying the more refined and clear-cut version elaborated by Merton. Paradigms of all pedigree function as instruments because they mold what one wants to say about the subject under investigation. The role paradigms played in the history of the sciences and the history of the “inexact sciences” (a term coined in 1958 by the philosophers of science Olaf Helmer and Nicholas Rescher that did not become widely used, let alone famous) is researched much less frequently than the catchword is used. Joel Isaac, a British intellectual historian, reconstructs in his *Working Knowledge: Making the Human Sciences from Parsons to Kuhn* (2012) the decade before *The Structure of Scientific Revolutions* appeared and almost immediately became the beacon of the anti-positivist mood. For his Ph.D. thesis, Isaac examined the papers of Parsons, Kuhn, and others stored in the archives at Harvard and MIT, and he identifies rightly the “Harvard complex” as the place where much of the development happened. Isaac is able to relate many new things, in particular about the Pareto Circle at Harvard that previously had become known through autobiographical revelations only.

But Isaac’s concentration on the papers stored in the Boston area narrows his view. He completely underestimates scholars with very close relations to Harvard, like Merton, and he does not care about career patterns and prejudices. Kuhn is presented as a reader, thinker, and discussant, but not as someone in need of the next job, angry about affronts, or potentially a victim of anti-Semitism. The reader therefore learns Kuhn’s reading list from 1949, but Ludwik Fleck is never mentioned in Isaac’s book. Similarly, Kuhn’s connection with the neopositivist International Encyclopedia of the Unified Science, where *Structure* came out initially as the last issue, is one-sided. Papers of the Unity of Science Movement and of Charles W. Morris at Chicago tell a story which is a bit different from the one Isaac extracted from archives in Cambridge.

Instead of listing some more minor dubiousness, I would like to take Isaac’s case as an illustration of the difficulties and hindrances one encounters in analyzing institutions. Focusing on one place, even if it is as

diverse and populated as the Boston area, forces one to ignore others. Academic micro-environments seldom are monads, but when interconnected with other small worlds, they form a discipline—with annual meetings, committees, panels, and journals, operating a labor market with its prestige order, financial resources, and so on. Intellectual historians regularly underestimate these forces, whereas sociologists exaggerate the non-cognitive side of scholarship sometimes. Research on HoS, including the literature from neighboring disciplines, is anything but well-organized and does not profit from a division of labor, a.k.a. cumulative research. Of course, a Ph.D. candidate cannot examine all potentially relevant archives or publications, but it seems to require no justification to say that more collaboration would be advantageous. Only seasoned scholars like Lawrence Scaff are in a position to draw their conclusions from the broad foundation of former consultations of dozens of different archives in different cities.

Again, there are some small examples where what one demands is executed. Fernanda Beigel's edited volume *The Politics of Academic Autonomy in Latin America* is such an exception. Under her directorship, a group of graduate students and young post-docs examined the South American landscape of academic research and higher education. The results transcend HoS, but what the Argentinians have to say should get the attention of the Northerners. Academia is seen here truly embedded in larger affairs such as diplomatic competition between neighboring countries. Foreign aid and intervention apparently shaped South American societies, and therefore also the academic world. Whereas European and North American contributions to HoS very often ignore state and inter-governmental agencies, Beigel and her group examine both well-known institutions like UNESCO's midwifery role for the *Facultad Latinoamericana de Ciencias Sociales* (FLACSO), a unique transnational research organization, and also the lesser-known, like the World University Service-UK that helped refugee scholars to return to Argentina when the generals stepped down. Despite the turbulence, the authors stress that even under dictatorship there were options for

academic autonomy. The Beigel book conveys to us Gringos at least the lesson that we should demonstrate more openness toward research from other parts of the world.

Contexts

One of the trendiest ideas in all parts of the history of science and scholarship is the plea for context and contextualization. On closer inspection, this postulation starts to become slippery. Peter Galison (2008) tried to clarify whether context could function as an explanation of anything. Pointing out different modes of context offers no resolution but could help organize future research: for example, textual vs. non-textual and further differentiation of the non-textual into political, institutional, industrial, and ideological contexts. Making use of these classifications and referring to them explicitly might at least help readers know what authors want to highlight.

One of the finest contextualizers is the German sociologist Wolf Lepenies, a stylist and independent thinker whose scholarly home lies somewhere beyond the small world of ordinary disciplines. His latest book, *Auguste Comte: Die Macht der Zeichen* (The Power of Signs), sets the founder of sociology in one particular context: his marketing machine. Lepenies adds some revealing findings to the well-known facts and trivia (Brazil's flag, the mirror in front of Comte's desk). One of Comte's concerns was his portraits; one could say he was obsessed with the dissemination of his visual presentation, and this around 1830 when even daguerreotype had not been invented (he lived to see the first daguerreotypes of himself in the 1850s). He commissioned series of busts, lithographs, and medals, thought about the appropriate color his movement should use for flags (green, as the color of hope, won), and created slogans to unite his supporters like "L'amour pour principe, l'ordre pour base, le progrès pour but" (love as principle, order as foundation, progress as goal). Lepenies always keeps a distance and never stoops to an oppositional attitude, which would be very easy to do when studying a whimsical eccentric and founder of a new religion.

The chapters of *Social Science in Context: Historical, Sociological, and Global Perspectives* transcend the boundaries of HoS both conceptually and with regard to the subjects covered. Sociologists and historians of science and ideas build the core of the group of mostly young scholars who are the authors. Their chapters address topics ranging from “gendered co-production of social science” to the “use of behavioural science in post-war Sweden”; they examine the “cultural history of the social sciences’ politico-didactics” and “newspaper enquêtes 1900–1920”; and they discuss the fate of German sociology under Nazi rule and the formation of scholarly specialties like business administration, geography, men’s studies in educational research, and the indigenous epistemologies of Sámi reindeer herding. Beyond these details, the book is of interest here because of its fruitful transgressions of conventional wisdom and disciplines. To thumb one’s nose at conventions feels good and sometimes provides new insights. Per Wisselgren studies the first public lectures by early sociologists in Stockholm, with a focus on how their publics were recruited and composed; Jonas Harvard describes the transfer of social research techniques into newspapers. Richard Danell covers international citation patterns and detects an increasing internationalization of citations in the social sciences: inhabitants of large countries tend to look inward more than researchers living and publishing in small countries. If this result is valid, it is still an open question whether it would hold for HoS, too. My fear is that the answer will be negative because, as a specialty, HoS is not integrated enough; it is, for the majority of its practitioners, not the paramount and constant field of activity, and lines of communication are less advanced than would be desirable. As a consequence, people affiliated with HoS know each other and each other’s work, but cannot cite each other because the topics of study are too far apart.

Concluding Remarks

Publications on HoS do not execute an ascribed function but do have two advantages at least. The whole historiography of

the 120+ years-old discipline has not influenced the identity of the discipline; insofar as it needs a shared picture of its trajectory, those who draw the picture do not utilize the publication record of the small group of HoS scholars. One of the advantages of this situation is that it is easy for authors to report something new, to tell an unearthed story or to point to hitherto ignored aspects of our discipline’s past. Second, in most cases the narratives avoid jargon and fancy pseudo-theories; their authors tend to stick to an old-fashioned epistemological realism and positivism of facts without any reservations. It is quite refreshing to read a whole book without encountering phrases about the social construction of everything or similarly overused insights.

On the other hand, there are also quite a few shortcomings. First, the number of sociologists doing HoS is too small, at least in the United States, to establish the field as respectable specialty. Second, instead of collaborating more closely, those who do HoS studies work on topics too distant from each other. As a consequence, the field under investigation is diffuse, and every newcomer looks for parts of the site yet to be mined. Some of the most superfluous contributions are sentimental tales about neglected predecessors and sidelined members of the discipline: the only thing you could learn from these kinds of texts is that the vast majority of members of any discipline sink into oblivion, sometimes even during their own lifetimes. So what? Third, in carrying out their research, HoS scholars of all disciplines do not make use of available sociological data-analysis techniques, not to mention that both HoS and the much larger group doing history of science do not examine the options of Big Data or digitalization in any detail. Fourth, the mutual influences between sociology of science/social studies of science and historio-sociological studies of the social sciences (HoS could be seen as part of this group) are weak: some might join me in feeling good about not encountering actor-network theory or other gonzo statements; but on the other hand, the neglect of what happens in neighboring thought collectives cannot be to the advantage of a still underdeveloped field.

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The Climate Moment: Environmental Sociology, Climate Change, and the Left

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Marx and Nature: A Red and Green Perspective, by **Paul Burkett**. Chicago: Haymarket, 2014. 318 pp. \$20.00 paper. ISBN: 9781608463695.

This Changes Everything: Capitalism vs. the Climate, by **Naomi Klein**. New York: Simon and Schuster, 2014. 566 pp. \$30.00 cloth. ISBN: 9781451697384.

The Capitalism Papers: Fatal Flaws of an Obsolete System, by **Jerry Mander**. Berkeley, CA: Counterpoint, 2012. 257 pp. \$26.00 cloth. ISBN: 9781582437170.

The Collapse of Western Civilization: A View from the Future, by **Naomi Oreskes** and **Erik M. Conway**. New York: Columbia University Press, 2014. 91 pp. \$9.95 paper. ISBN: 9780231169547.

The Wrath of Capital: Neoliberalism and Climate Change Politics, by **Adrian Parr**. New York: Columbia University Press, 2013. 216 pp. \$29.50 cloth. ISBN: 9780231158282.

On September 21, 2014, the largest climate march in U.S. history took place in New York City, as more than 300,000 protestors signaled to UN delegates arriving for climate talks that more desperate measures were needed to protect humanity and other species. The massive demonstration, though representing a wide array of social and political viewpoints, had its origins on the Left. The radical intellectual thrust of the movement was apparent the day prior to the march, when a vast "People's Summit/Teach-In" was led by two organizations—Global Climate Convergence and System