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Abstract

This paper argues that in order to inform current debates, the historiography of the social sciences and humanities can derive methodological principles from historical epistemology. Broadly conceived, this line of epistemological reasoning consists of two interrelated movements of thought. The historization of epistemology, on the one hand, understands epistemological principles and notions as historical products. Thus, they can change their meaning over time. The epistemologization of history, on the other hand, approaches the history of science with systematic interests. In order to inform contemporary debate, it aims at evaluating past science. This concept of a normative historiography of science has been most radically formulated in the work of Gaston Bachelard, who also developed the concept of epistemological obstacles. However, most work in the tradition of historical epistemology has focused on the natural sciences. The paper discusses three exemplary studies to show how principles and approaches from historical epistemology can be fruitfully applied in the historiography of the social sciences and the humanities.

Keywords: Historical Epistemology - Gaston Bachelard - Epistemological obstacles - History of Social Science - Epistemic hopes

1 Introduction

The idea - or hope - that the history of the human sciences should be written in a way that renders it useful to contemporary debates and practices is not new. In American sociology, it surfaced recently in a note by Richard Swedberg, then Chair of the History of Sociology section of the American Sociological Association, which was published in two parts in the section's newsletter *Timelines* (Swedberg, 2012, 2013). Swedberg proposes that the history of sociology has two tasks: the first is to produce the history of sociology in a more traditional sense; the second is to ensure that practitioners within the discipline are “aware of the knowledge of the past that is necessary to carry out a sociological study of good quality” (Swedberg, 2013: 1). In short, this second task is to provide the working memory of sociology. And while “historians of sociology may well have focused a bit too much on the former”, it might be wise to put more emphasis on the second task, above all because it concerns not only the historian, but all sociologists.

A recent article by German-speaking sociologist Frank Welz (2010) parallels this claim. Welz discerns three functions of the history of sociology, and related to these functions three types of historians: the “identity engineer”, the first of these types, attempts to establish and promote a canon of texts and ideas that are central to the discipline. Given the competitive structure of the academic field of social theories and research objects, the function of such historiographical work is to stabilize a disciplinary identity. In reaction of the identity engineer’s attempt to selectively construct a disciplinary core, the “collector of ideas” aims at exploring sociological research directions and ideas that have been virtually forgotten. Giving a voice to those who are at the danger of being lost in the social - and not always rational - processes of scientific recognition, the function of this approach is to countervail tendencies of disciplinary closure, of mainstream building, and of theoretical or methodological domination. Finally, the hope of the “trace-tracker” is to show that historical studies are of use to contemporary researchers. By providing knowledge on the roots of the ideas and concepts central to these debates, historiography can provide knowledge about the intricacies, complexities, dead ends and tricks of concept formation which, in turn, can inform current debates in sociology.¹

¹ Welz continues by arguing that there is a historical logic inherent in this threefold scheme. The use of history to construct an identity has been important at the beginning of the disciplinary establishment; when this had been successfully accomplished, the shortcomings of a canonization had to be remedied by reference to a variety of directions suppressed by it. Given the contemporary state of the discipline, Welz argues that the most important tasks were those of the track-tracer: to explore the roots of concepts used in contemporary debates and inform these by providing „genealogical explorations“ (Welz, 2010: 20; my translation; see also the ensuing debate, Dayé, 2012; Welz, 2012).

For Welz, as for Swedberg, to inform current debates is a vital task of the history of sociology. And although Swedberg mentions that historiography conceived in this way implies taking a normative stance, it is Welz who establishes the relation between the role of the trace-tracker and the philosophical discourse in which a constructive and normative relation between the history of a field and its current practices has been discussed most comprehensively: historical epistemology.

Historical Epistemology is a label for a line of methodological reasoning in the philosophy and historiography of science usually related to French authors like Gaston Bachelard, Alexandre Koyré and Georges Canguilhem, but also to Ludwik Fleck, Thomas S. Kuhn and Michel Foucault (Rheinberger, 2010a). Differences notwithstanding, all these authors approached the history of science(s) with the aim of reflexively informing contemporary debates within these sciences. If one thus seeks to write the history of the social sciences and humanities in a way informative to current debates, one reasonable source for methodological knowledge is this tradition in the philosophy of science.

Unfortunately, however, this is about where the argument usually stops. The text of Welz is a good example for this point: it claims that the historian of sociology should do historical epistemology, but does not explore how this can actually be achieved. To be sure, this question is not trivial: almost all studies in the tradition of historical epistemology have been concerned with the natural sciences. This paper is concerned with exploring whether and how two basic ideas and some selected concepts from historical epistemology can be transferred into the historiography of the human sciences. It does not challenge the claim made that to inform current debates is a desirable function of historiography, at least not directly. Instead, evaluating the feasibility of this claim, it discusses whether and how this might actually be achieved by following principles of historical epistemology. Section 2 offers a brief description of the project and the trajectory of historical epistemology in general. Basically, the project consists of two interrelated movements of thought, the historization of epistemology and the epistemologization of history. I then turn to a more detailed discussion of the positions of Gaston Bachelard, who most radically developed the idea of a normative historiography of science (section 3). The aim of this paper cannot be to reconstruct the discussion on historical epistemology in a way adequate to a philosophical informed reader. Rather, the claim can only be to seek for thoughts and concepts from historical epistemology that can inform methodological debate within the field of the history of the human sciences. I proceed with a discussion of three exemplary works from the recent historiography of the social sciences that explicitly or implicitly follow principles of historical epistemology: Peter

Baehr's (2010) study on Hannah Arendt's concept of totalitarianism is presented as an example for the epistemologization of history (section 4.1); Heinz Steinert's (2010) analysis of Max Weber's famous essay on the Protestant ethic as an example for the historization of epistemology (section 4.2). Finally, the adjacent section 5 discusses the development of the Delphi method as a case where the analysis profits from a conceptualization inspired by the writings of Gaston Bachelard, most prominently by his concept of epistemological obstacles. In drawing these lines together, I conclude that historical epistemology in general as well as the writings of Bachelard more specifically provide methodological positions and concepts for the history of the human sciences that help producing insights that can inform for current debates. In fact, of course, they are already present in some works. However, following these positions necessarily entails a form of normative historiography not every historian would want to follow (section 6).

2 Historical Epistemology: A Warren of Narrow Alleys

In general, historical epistemology is used to characterize a line of thought in philosophy of science which Hans-Jörg Rheinberger (2010a) sees to emerge in the works of Ernst Mach, Émile Boutroux, Henri Poincaré and others and which he sees continued in the works of authors like Ian Hacking, Bruno Latour, Andrew Pickering, Stephen Shapin - though he could also refer to his own studies (Rheinberger, 1997, 2010b). Rheinberger conceives of historical epistemology as comprising two movements of thought: firstly, the historization of epistemology (or philosophy of science); and secondly, a process "that can be described as the epistemologization of the history of science." (Rheinberger, 2010a: 3) To historicize epistemology means to understand principles of scientific thinking and doing as historical products. They are no fixed notions, but, sometimes subtly, and sometimes in purportedly revolutionary upheavals, change their meaning over time. To epistemologize history, on the other hand, means to approach the history of (a) science with a systematic interest. Historical analysis in this line of thinking addresses not genuinely historical problems, but rather problems in theory, methodology, and logics and philosophy of (scientific) knowledge. Both movements of historical epistemology are theoretically interwoven and aim at informing contemporary discussion. Unlike for instance the 'new sociology of ideas' (Camic and Gross, 2001), historical epistemology conceives of historiography not as an end in itself. Rather, it has a function for contemporary scientific practice.

Perhaps more markedly than other currents of epistemological thinking, historical epistemology does not comprise a homogenous core of systematically interrelated tenets, but

instead heavily varies in its claims from author to author. This might be caused by the fact that many early authors that are now deemed important for the development of this tradition of thought did not interact, neither in personal encounters nor in published debates. They were scattered geographically, temporally, and hierarchically. Some of these authors were not inside academia; others were working at universities, but were natural scientists and found no easy access to academic philosophy. In the 1930s, this not very hospitable situation was worsened by “the intellectual migration forced by National Socialism” which “tore up existing traditions” (Rheinberger, 2010a: 90). After the end of World War II, positions of historical epistemology were debated and developed in parallel and apparently irreconcilable strands of discussion, one in French, the other in Anglo-American philosophy, which shared not much more than its problematization of some core assumptions of positivism (Brenner, 2006).

Seen in this context, the novelty of “post-positivism”, whose proponents often present Kuhn’s *The Structure of Scientific Revolutions* as marking a theoretical watershed, is not so much on the theoretical but rather on the social level. Most ideas for which Kuhn’s book became the *locus classicus* have already been around, and many of them, as Joel Isaac (2012) has recently shown, were vividly discussed within the circles at Harvard Thomas Kuhn frequented. Clearly, Kuhn’s theory of the development of sciences is novel. But based on a lack of knowledge about earlier works in the same direction, the novelty of the epistemological claims was exaggerated. This could happen because no canon had yet been established. The ideas had been formulated, but they were not on the screens. They had not yet been structured and allied to form a “field”, and were known only to specialists.²

This lack of an established field is reflected in recent attempts to institutionalize historical epistemology academically. Several projects have been described as adhering to the principles of historical epistemology without offering a detailed description of how these related to the earlier approaches. For those informed about the work of earlier authors, most often those cognizant of the French tradition of philosophy of science, it is hard to see the relations and linkages between the newer projects and the tenets of the older authors. Arguing, for instance, from a close reading of the works by Gaston Bachelard and Georges Canguilhem, Cristina Chimisso (2003: 298) claimed that newer programs, e.g. those formulated by

² Who of the earlier European authors Kuhn had read is debated. Alexandre Koyré had repeatedly visited the United States. It is also known that, by citing his work as influential on his own thinking, Kuhn introduced Ludwik Fleck to his Anglophone readers. But at the same time, he omitted or ignored the work of other European authors, both earlier ones, like e.g. Gaston Bachelard, or contemporary ones, like e.g. Georges Canguilhem or Michel Foucault: “Kuhn montre une préférence pour les auteurs de la génération antérieure: si Meyerson, Brunschvicg et Metzger sont cités, Canguilhem et Foucault sont passé sous silence.” (Brenner, 2006: 116)

Lorraine Daston and Jürgen Renn, “share the name but not the methods and aims with the doctrines” of historical epistemology developed by earlier authors.

Indeed, Gaston Bachelard (1884-1962) developed a radical and provocative version of historical epistemology that implies a normative approach to the historiography of science (Chimisso, 2013). The next section attempts to outline some of Bachelard’s main positions and concepts. It attempts to describe his ideas both on the historization of epistemology and the epistemologization of the history of science. It should be clear that the focus is on Bachelard’s contribution to historical epistemology. Bachelard has been a prolific writer in many philosophical fields, and the discussion presented here is not comprehensive or representative of his thinking at large, but selective with regard to his understanding of a normative historiography of science.

3 Gaston Bachelard and the Normative Historiography of Science

Bachelard developed a sophisticated position on how to reach an epistemologization of the history of science. The history of science should be analyzed in a way that provides insights which can be used by contemporary sciences in re-considering, re-orienting, and calibrating their epistemologies. Bachelard (and Canguilhem) used the notion “epistemology” not with the meaning it usually takes in Anglophonic philosophy, where it denotes a systematic theory of knowledge. Instead, they understand it as an active way of reflecting “on the historical conditions under which, and the means with which, things are made into objects of knowledge.” (Rheinberger, 2010a: 2) In contrast to the Anglophonic (but also the German) tradition, such an understanding of epistemology is interested in scientific practices.

It must be emphasized that for both Bachelard and Canguilhem, the historization of epistemology and the epistemologization of the history of science could not be thought of separately. For Bachelard, to historicize epistemology means that the philosophy of science has to give up its *a priori* and normative stance towards the sciences and to begin to reflect *ex post* on scientific practices. In his view, philosophy of science has to become an *ancilla scientiae*. Philosophy is not able to dictate ex cathedra how science is conducted properly. Rather, philosophy of science has to adapt its terminology and its structure in order to comprehend the flexibility and rapid evolution of contemporary science (Bachelard, 1984: 10, 1974: 16-29). The idea of a primacy of philosophy of science which would allow for acting towards science as a corrective or vetoing voice is not justified. Rather, because it so crucially depends on the results of the scientific disciplines, philosophy of science should

understand that it is “la seule *philosophie ouverte*”, the only open philosophy (Bachelard, 1966: 7). Its aim should be to approximate the dynamism in the empirical sciences, become permeated by it and thus enter the constant and rapid flux of events in the sciences to become itself a dynamic field. “In no way is this meant to imply a humiliation of philosophy; on the contrary, philosophy should be elevated to the height of the sciences.” (Lepenies, 1987: 13; my translation) The principle of the new, dynamic philosophy of science would be to conceive of its products as historically changing alongside the results of contemporary science.

The claim for an epistemologization of the history of science, on the other hand, must be seen in relation to Bachelard’s concept of epistemological obstacles. These are caused by the psychological inertia of the cognitive mind. Epistemological obstacles are usually habits of thinking that are used everyday and thus remain unreflected for the most part of our lives. In science, however, these obstacles can divert the cognitive processes from reaching the truth. What is thus required, Bachelard claims, is a psychoanalysis of the scientific mind. For him, epistemological obstacles can be caused by a variety of factors. As an example, Bachelard discusses primary experience. When a human being first experiences a thing, this experience is place “before and above that criticism which is necessarily an integral part of the scientific mind.” (Bachelard, 2002: 33) Another example is universalization, i.e. the attempt to transfer observational sentences into general statements of the highest order (e.g., scientific laws). The idea of generalization, Bachelard claims, has repeatedly obfuscated the process of science and thus functioned as epistemological obstacle in the history of science (Bachelard, 2002: 65):

According to our opponents, according to philosophers, the greatest generalities should be made the basis of scientific culture. The basis of mechanics is that all bodies fall. In optics, it is that all light rays are propagated in a straight line. And in biology, it is that all living beings are mortal. Thus at the threshold of every science great first truths would be set in place, intangible definitions that shed light on a whole theory. The opening paragraphs of pre-scientific books are in fact cluttered up with these attempts at preliminary definition, as we can see in eighteenth-century physics and twentieth-century sociology alike. And yet the question can be raised as to whether these great laws constitute truly scientific thoughts or, what amounts to the same thing in our eyes, thoughts that suggest other thoughts.

First experiences and universal statements in sciences thus can lead to obstacles hindering scientific progress. These two examples show that the effect of epistemological obstacles can be followed both on an individual and on a collective level. On the individual level, everyday knowledge and weltanschauung dull the researcher's eyes towards the facts. He adopts extra-scientific stocks of knowledge without further examination and let it influence his research. But epistemological obstacles are also effective on the collective level of scientific disciplines. Here, they undermine the shared aim of maintaining and enlargening the stock of scientifically true knowledge. For Bachelard, to conceive of general definitions as the starting point of a new science and to begin with positing laws instead of describing the empirical nature of the relevant phenomena can crucially inhibit this science's development. Further, specific myths effectively confine, on the individual level, the range of conceivable approaches to a phenomenon and thus can, if common within a scientific community, obviate collective epistemic progress; a process that Bachelard extensively discussed with regard to fire (Bachelard, 1964).³

For Bachelard, the way to true insight is a step he calls epistemological rupture. Scientists have to get rid of everyday knowledge and pre-scientific myths. Individually, this means to draw a radical break between everyday and scientific insight. Every statement must be checked for implicit, non-confirmed assumptions. On the collective level, Bachelard claims for a dynamization of the notion of insight. To him, science as a collective enterprise is not simply cumulative. If a scientific insight degenerates into a doctrine and scientists remember only its immediate content, but not its context of relevance - i.e. the problem the insight was able to solve -, further cognitive progress is inhibited. Scientific thinking consists in a continuous critique of doctrine, and scientific progress is always an epistemological rupture with the body of knowledge already available.

Though similarities exist, it should be clear that compared to for instance Alexandre Koyré's (1957) concept of a scientific revolution or the change of paradigms that Thomas Kuhn (1970) saw to occur in such revolutions, Bachelard's epistemological break denotes a different kind of discontinuity. Both Koyré and Kuhn are concerned with science as a collective and do not include the scientific individual in their analyses. And while epistemological ruptures for Bachelard are radical on the individual level and necessarily have to be in order to allow for scientific thinking, they are always partial on the collective level and do not embrace the totality of a (scientific) weltanschauung, as in Koyré's *From the Closed World*

³ One example of a myth that Bachelard sees influential across all scientific disciplines is the myth of the digestion, functioning as the implicit justification of seeing the truth of a thing to be found deep in its interior and that to fully grasp it, one has to internalize it (Bachelard, 2002: 172-184).

to the Infinite Universe (1957). Also, in Bachelard's writings, we find nothing comparable to Kuhn's notion of incommensurability, a term by which he denotes a break in the development of a science which makes any epistemic continuity impossible. And Bachelard's claim that ideas degenerate into a doctrine if not continually questioned does not fully match with Kuhn's notion of normal science.

Bachelard uses the notion of an epistemological obstacle also to define the distinction between a traditional history of science and epistemology. "A fact that a whole era has misunderstood remains a fact in historians' eyes. For epistemologists however, it is an obstacle, a counter-thought." (Bachelard, 2002: 27) It is not sufficient to historically document the development of philosophy: in order to fully capture a scientific thought, it must be evaluated "from the standpoint of reason", but not of contemporary, but moreover of actual, "developed" reason, "for it is only now that we can really judge the errors of the mind's past." (Bachelard, 2002: 27). While the traditional history of science by principle - or *prima facie*? - negates any attempt to evaluate the historic events it deals with, the task of the epistemologist is to evaluate scientific ideas (Canguilhem, 1981a). Bachelard sometimes used the metaphor of a courtroom, or a tribunal, of the history of science (Canguilhem, 1994a; Chimirro, 2003). It passes judgment on past scientific ideas. However, this metaphor requires an important qualification: though historical epistemology produces judgments, it does not aim at sanctions (Canguilhem, 1994a: 14; my translation):

A judgment in this case is neither a purge nor an execution. The historiography of science is no backward-oriented history of progress [i.e., no Whig history], not a representation of episodes that, from the vanishing point of today's truth, are obsolete. Rather it wants to investigate and to make intelligible to what extent notions, attitudes or methods which are nowadays obsolete were themselves innovations at their time and to what extent, as a consequence, the obsolete past remains the past of an activity which still deserves to be called scientific. It should not become intelligible why something had been dismantled, but also how it was first constructed.

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⁴ „Un jugement, en cette matière, n'est pas une purge, ni une exécution. L'histoire des sciences ce n'est pas le progrès des sciences renversé, c'est-à-dire la mise en perspective d'étapes dépassées dont la vérité d'aujourd'hui serait le point de fuite. Elle est un effort pour rechercher et faire comprendre dans quelle mesure des notions ou des attitudes ou des méthodes dépassées ont été, à leur époque, un dépassement et par conséquent en quoi le passé dépassé reste le passé d'une activité à laquelle il faut conserver le nom de scientifique. Comprendre ce que fut l'instruction du moment est aussi important qu'exposer les raisons de la destruction par la suite.” (Canguilhem, 1994a: 14)

In an attempt to evade traditional relativism, the historiography of science applies the normative criteria provided by epistemology to write both a history of obsolete insights and a history of confirmed insights. For Bachelard, there is no doubt that science progresses, and that correct and wrong theories can be discerned. That there is progress in science is undisputable; this is a fact withdrawn from any discussion.⁵ A science, in other words, has a destiny, not just a chronology (Canguilhem, 1994b: 175). And it is precisely because of this destination that from a history of science, if done from a philosophical standpoint, a philosophy of science can emerge.⁶ This is, again, the core idea of historical epistemology.

In order to understand scientific progress, it is imperative to write both histories, the obsolete and the confirmed, and to bring them in a dialectic relation. Episodes or ideas that are obsolete from today's perspective might have destroyed epistemological obstacles at the time of their introduction. Progress in knowledge is not simply the occupation and structuration of the previously unknown, but to a much larger extent the replacement of the previously known. The realization of truth is inhibited not simply by non-knowledge, but by wrong pre-knowledge (cf. Schmidt and Tietz, 1980: 10). The history of science is a history of breaks, of ruptures with ideas previously deemed true. Any attempt to display of science as a continuously and cumulatively growing field leads into a blind alley, into non-science, non-philosophy. To repeat one of Bachelard's own examples, nobody could seriously claim an intellectual linkage between alchemistic transformation and nuclear transformation (Bachelard, 1974: 76)⁷

To sum up, the Bachelardian version of historical epistemology engenders a normative historiography of science that is not only concerned with a Rankean *wie es eigentlich gewesen*, but discerns between correct and false ideas and analyzes the interplay between these ideas with the aim to inform current scientific practice. Such normative historiography emphasizes the importance of epistemological obstacles, of habits of thinking that are of an origin external to science and hinder the scientific progress. It is basically a historiography concerned with breaks and ruptures instead of continuities. However, even from this brief and, from the philosophical point of view, admittedly unsophisticated sketch of the Bachelardian system of thought, it becomes clear that one cannot simply "do" a Bachelardian historical

⁵ "On peut discuter sans fin sur le progrès moral, sur le progrès social, sur le progrès poétique, sur le progrès du bonheur; il y a cependant un progrès qui échappe à toute discussion, c'est le progrès scientifique dès qu'on le juge dans la hiérarchie des connaissances, en son aspect spécifiquement intellectuel." (Bachelard, 1966: 21)

⁶ "Une science a son destin et non seulement une chronologie. De l'histoire de la science, philosophiquement questionnée, c'est-à-dire quant à la formation, à la réformation et à la formalisation des concepts, surgit une philosophie de la science." (Canguilhem, 1994b: 175)

⁷ The same concept of rupture characterizes Michel Foucault's "archaeology" of the human sciences (Foucault, 1994, 2002).

epistemology of human sciences. The new scientific spirit is a spirit of the natural sciences, and the enduring difficulties of interpreters of Bachelard's thinking to establish the relations between his philosophy of science and his writings on aesthetical and literary issues indicate that ... His system is embedded in a specific theoretical debate, and to take it out of its context engenders the inevitable damage of either damaging it or disconnecting its parts.

4 Historical Epistemology and the History of the Social Sciences

This section and the next explore how principles and ideas of historical epistemology can be used in the historiography of the human sciences. Historical epistemology was developed with almost exclusive regard to the natural sciences (exceptions are Canguilhem, 1981b; Lepenies, 1981); and in its attempt to be a most general philosophy of science, it was far more concerned with the history of science as a whole than it was with the history of scientific disciplines, specialties, or fields (Lepenies, 1978). This makes it wiser not to claim to wholesale adopt a selected approach, but rather to transfer carefully selected elements into the historiography of the human sciences.

Instead of theoretically spelling out the problems implied in transferring elements from historical epistemology into the historiography of the social sciences, in this section I discuss two historical studies which provide examples for the fruitfulness of such an approach. I will thus not systematically treat every point mentioned in the preceding discussion of historical epistemology, but instead introduce exemplary work in the hope that the reader will find it sufficiently suggestive to assess the potential of historical epistemology in the history of the social sciences. In addition, because none of the two authors discussed in this section refers to historical epistemology, I explain why I think these studies follow principles of historical epistemology.

Though it is true that the two movements of thought that make up historical epistemology, the historization of epistemology and the epistemologization of the history of science, are intrinsically interwoven, it is also true that scholars who follow one of these must not necessarily follow the other. This first study discussed, Peter Baehr's *Hannah Arendt, Totalitarianism, and the Social Sciences*, appears to be an example of how the historization of epistemology could be translated into the history of the social sciences. In turn, the second study, Heinz Steinert's *Max Webers unwiderlegbare Fehlkonstruktionen*, is an example of what results from an epistemologization of the history of the social sciences.

4.1 The historization of concepts: Peter Baehr on totalitarianism

In general, the project of the historization of epistemology is to conceive of epistemological concepts and ideas as inherently historical products. Epistemology does not deliver concepts that transcend time and space; it will never be able to establish *the way to truth*. Rather, to historicize epistemology is to investigate how epistemological notions and ideas are conceived, used, transported and modified. For instance, Lorraine Daston and Peter Galison (2007) investigated how the concept of objectivity changed its meaning since its emergence in the early 18th century. Objectivity, they claim, is no constant in the history of science. Rather, it has a history.

Translated into the historiography of the social sciences, the range of notions which could be investigated changes. Of course, the history of general terms like objectivity or positivism had in the social sciences could be investigated. But in addition, central concepts like power, the public and even society itself could step into focus.

In his recent book-length study on *Hannah Arendt, Totalitarianism, and the Social Sciences*, Peter Baehr (2010) pursues two objectives. The first is to historically reconstruct the shaping of the concept of totalitarianism in the thinking of Hannah Arendt. It investigates how Hannah Arendt modified the concept of totalitarianism in the decades prior and after the publication of her famous *The Origin of Totalitarianism* (Arendt, 1973) in 1951. It does so by pinpointing three instances on which her theorizing on totalitarianism was discussed, criticized, or even frankly dismissed. The first is a series of letters with Daniel Riesman in the 1940s, in which the limits of the concept of totalitarianism were discussed. The second are reviews of Arendt's works on totalitarianism written by sociologist Raymond Aron. And finally, Baehr discusses the debate on the secular religion thesis that evolved between Arendt and the French sociologist Jules Monnerot.

Underlying all these debates was Arendt's devastatingly critical and apparently unalterable view of the intellectual project of the social sciences. In Arendt's view, the social sciences were unable to capture, let alone explain, phenomena like the mass cruelties of the totalitarian regimes in Nazi Germany and Communist Russia. This was caused by three issues she saw as fundamental to the social sciences (cf. Baehr, 2010: 10-34). The methodological principle to reach objectivity by attempting to retain a normative distance to the subject of study - *sine ira et studio* - led social scientists to depict concentration camps and other cruelties in a way that suggested normality. Further, the methodological move common in the social sciences to compare virtually unrelated things in order to see similarities on a higher level - a strategy Arendt criticized as underlying "functionalism" - would lead social

scientists to name Nazi camps in one stroke of breath with other forms of internment; Erving Goffman's *Asylums* (1961), published a decade after Arendt's critique of the social sciences had appeared, was indeed not the only example, but rather a later one in a series of social science works she condemned. Finally, "the related issue of social science's tendency to become trapped in analogies and ideal types ... impeded its ability to confront [the] historical novelty" (Baehr, 2010: 16) of the systems of mass murder that undergirded both totalitarian regimes in Germany and Russia.

However, Baehr claims to follow not only a historical interest. He writes: "If the first objective of this book is to retrieve debates that have been largely forgotten, the second objective is substantive: to distill from these disputes a series of issues that continue to tax the modern mind." (Baehr, 2010: 5) Baehr aims to show that a detailed study of the debates on totalitarianism and the capacities of the social sciences that were carried out in the 1940s and 1950s indeed further the analysis of current radical Islamism and violent jihadism. In the final chapter of his book, Baehr (2010: 124-44) addresses two questions which immediately relate to the historical analysis contained in the preceding chapters. The questions are: "To what extent is radical Islamism similar to, and different from, 'totalitarianism'? Is it an unprecedented phenomenon and danger, as some earlier observers claimed totalitarianism to be?" (Baehr, 2010: 127)

This is not the place to reproduce Baehr's answers. What we should note however is that Baehr attempts to use the increased historical knowledge of one specific, albeit very sophisticated intellectual trajectory of the concept of totalitarianism - Hannah Arendt's - to analyze aspects of the actual social world. By historizing the concept, he achieves a higher sensibility towards the theoretical implications and intricacies of the totalitarianism concept. The historization of a sociological concept thus allows for an analysis that appears to be more thoughtful, comprehensive, and sophisticated than other analyses that lack the sensibility gained through exposing the biography of the main concept.

4.2 The epistemologization of sociology's history: Heinz Steinert on the "Weber Thesis"

In general, the main thrust of the epistemologization of the history of science is to retrieve normative knowledge about how and how not to do science. Major parts of Heinz Steinert's recent study *Max Weber's Irrefutable Misconceptions* (Steinert, 2010; my translation), the example I discuss in this section, critically reassess the claims Max Weber made in his now

classic sociological text *The Protestant Ethic and the Spirit of Capitalism*. As Steinert comments, the methodological and theoretical shortcomings of Weber's text have been repeatedly exposed in the last decades. Thus, Steinert has much material to draw upon. But it remains open why these criticisms have not led to a dismissal of the *Protestant Ethic* as a "classic". Many contemporary sociologists treat Weber's propositions as proven. Steinert's main objective is to explain why this is the case. As before, I will not present Steinert's study in its own right, but as a showcase of an epistemologization of the history of social sciences.⁸

Given the bulk of earlier studies critical of Weber's thesis, Steinert (2010: 20, emphases in original; my translation) claims that Weber's idea

to make capitalism an unintended consequence of religious estrangement from the world is nice, but wrong. [...] [The study's] scientific value was contested from the beginning. As regards the methodology, the paper could probably serve best as an example of how *not* to design a socio-historic investigation. As regards the content, the research carried out in the century after the "Weber thesis" had been presented [...] could *not* confirm its details or its main proposition; many issues were refuted.⁹

Steinert reconsiders each step of Weber's argumentation in a very detailed and informative manner. He attempts to read Weber's text without the prejudices usually held towards a "classic". This means to approach the old text with the same set of questions with which one would approach a contemporary sociological or socio-historical study: "How precisely does the argument proceed? What is the evidence? Has the author collected new data? If yes, how? What is the relation between empirical data, assumptions and conclusions? Is this relation solid? What of this is innovative?" (Steinert, 2010: 12; my translation)

⁸ It must be emphasized that this is not Steinert's terminology. Steinert calls his study a "historization of Weber". But Steinert's understanding of what it means to historicize a classic text is broad and combines both steps of historicization and of epistemologization. As I hope to make clear in this section, to treat the study as an epistemologization of a classic is justified.

⁹ „Max Webers aparte Idee, Kapitalismus zu einer unbeabsichtigten Nebenfolge von religiöser Weltabgewandtheit zu machen, ist zwar hübsch, aber historisch falsch. Die 1904 bis 1906 erschienene Reihe von Aufsätzen [...] war damals [...] Vielen plausibel (besonders den Protestanten, aber ihr wissenschaftlicher Wert war von Anfang an umstritten. Methodisch könnte man die Abhandlung ohnehin am ehesten als Beispiel dafür verwenden, wie man eine sozialhistorische Untersuchung *nicht* anlegen sollte. Inhaltlich wurde die ‚Weber-These‘ in dem Jahrhundert an Forschung, seitdem sie dem staunenden Publikum der vorletzten Jahrhundertwende im Wilhelminischen Deutschland präsentiert wurde, in praktisch allen Einzelheiten und als Gesamtaussage nicht bestätigt, in vielen Punkten widerlegt.“ (Steinert, 2010: 20) Steinert uses "Weltabgewandtheit" instead of the original notion of "Weltfremdheit" (Weber, 2002: 154). My translation of this term follows Stephen Kalleberg's suggestion to use "estrangement from the world" (Weber, 2009: 64) instead of Talcott Parsons's "other-wordliness" (Weber, 1992: 7).

This first step thus consists in a detailed and critical reading of the text in search of its inherent scientific, not didactic qualities. For Steinert, such a reading of the *Protestant Ethic* results in a collection of “irrefutable misconceptions”. For instance, the relation between religion and economic prosperity that Weber describes on the first pages of his essay is neither supported by contemporary nor by present data. There is no evidence corroborating the claim of an intrinsic relation between Protestantism and prosperity which operates regardless of region or other variables. Also, Weber is not sufficiently systematical in his treatment of Protestantism. As Weber acknowledges, Protestantism is no homogeneous organizational structure. Instead, it comprises a multitude of different currents of theological thinking and living. In some parts of his text, Weber is very cautious to discern, say Calvinists from Lutherans. But when it comes to the main argument, this caution vanishes, and Weber refers to Protestantism as a whole without any justification.

In a second step, Steinert attempts to find the reasons for the success of Weber’s text. In short, his argument is that although scientifically unsound and contested, the “Weber thesis” appeared plausible to the audiences addressed by Weber - mainly to the circles of white, well-educated, protestant males that entertained the idea of a cultural Protestantism (*Kulturprotestantismus*) and discussed the possibility of Protestantism disposed of the clerical structures and open toward some of the innovative forces in modern society, foremost toward science. As Steinert (2010: 52; my translation) puts it,¹⁰

[o]ne can imagine these circles perhaps in this way: Here met academic notabilities, males with beards (almost all) and scars from duels (some), a good part of them members of the upper upper class, at any rate all sharing the feeling of being the “intellectual aristocracy”, covertly bothered by the economic crisis and the upsurges of social democracy, and mutually assured each other at good food, wine and cigars of the occident’s and, particularly, of Protestantism’s superiority.

The continuing success of Weber’s essay cannot, Steinert (2010: 272) concludes, be explained neither by the text nor by the personality of its author. Instead, it must be understood by reference to the audiences. The *Protestant Ethic* essay continued and still continues to serve a function, as it did for the “cultural Protestants”. A scientific community

¹⁰ „Man kann sich diese Runden vielleicht so vorstellen: Da trafen sich die akademische Honoratioren, Herren mit Bärten (fast alle) und Schmissen (manche), zum guten Teil Mitglieder der Ober-Oberschicht, die sich aber jedenfalls als ‚Geistesaristokratie‘ fühlten, heimlich beunruhigt von der wirtschaftlichen Krisenlage und von den Umtrieben der Sozialdemokratie, und versicherten einander bei gutem Essen, Wein und Zigarren der Überlegenheit des Okzidents und besonders des Protestantismus.“

established itself around the work and life of Max Weber and continues to receive a money and infrastructure to maintain its existence. It was this community who claimed to award Weber the status of a sociological classic. Would not Parsons have showed an interest in his work, Weber as a classic would probably have remained a German phenomenon (cf. Steinert, 2010: 267).

Steinert's primary aim is to dismantle the myth that the classic status of the *Protestant Ethic* essay is justified by the scientific quality and sophistication of its argumentation. A major way to achieve this is by critically assessing the essay's methodological and epistemological premises. Steinert deliberately ignores the classic status of Weber's essay, and attempts to evaluate its scientific claims on their own rights. This leads to a series of considerations on "how *not* to design a socio-historic investigation." (2010: 20, emphasis in original; my translation) Steinert's book epistemologizes the history of sociology, because it draws lessons from past research. It approaches the history of social science from a normative perspective. It assesses the quality of an intellectual contribution in relation to both contemporary and nowadays scientific standards. And it attempts to explain sociologically - and not, in a Bachelardian manner, psychologically - why a wrong theory survived.

5 Epistemic Hopes in the Development of Delphi

While the two examples discussed above were both concerned with the history of concepts and theories, it is important to note that historical epistemology offers also a valid approach to the history of research methods, techniques, and tools. This holds true also for the human sciences, as can be demonstrated with the example of the history of the Delphi technique, a "technique of prospection" (cf. Mallard and Lakoff, 2011) developed during the first decades of the Cold War in one of the most infamous U.S. research organizations of the period, the RAND Corporation. Delphi is a procedure that consists of several rounds of interrogation, usually done by questionnaires. It aims at establishing a consensus amongst experts on future events and developments. The questionnaires that are sent to the participating experts virtually ask the same questions again and again, providing in each round new information feedback, almost always the aggregated results from the previous rounds. This iterative procedure should lead to a convergence of the estimates over a "reasonably" narrow range that can then be interpreted as "consensus" of experts.

The Delphi was invented and developed during the 1950s and 1960s by a group of mathematicians and logicians - most notably, Olaf Helmer and Norman C. Dalkey - at the RAND Corporation, a think tank based in Santa Monica, California, which at that time was mostly financed by the US Air Force and the Pentagon. It became a leading method in the then nascent field of futurology. Helmer and Dalkey expected their method to have a series of positive outcomes, which I propose to describe as their “epistemic hopes”: that it would improve rational decision making by establishing more stable estimates in areas dominated by uncertainty; that it would open political decision making to the rationality purportedly incorporated in social scientific methods, etc.

At the core of Delphi methodology is the concept of the expert. The expert is expected to be better able than lay people to estimate future trends and developments in her or his field of expertise. However, though they were carried out by the same group of people, the studies in the early period of Delphi at the RAND Corporation (1948-1968) show a crucial ambivalence towards the concept of the expert. In brief, two concepts of the expert could be discerned: the concept of the expert-as-predictor and the concept of the expert-as-evaluator. To conceive of experts as predictors in the Delphi framework basically means to make them come up with estimates based on their implicit knowledge without any attempt to elicit their reasoning. From round to round, the experts were informed about the median values of all the answers given in the former round and asked whether they desired to change their answer in face of the median. In contrast, the concept of experts-as-evaluators attempts to make the reasoning behind the experts’ estimates accessible to all the participants. The questionnaires contain not only items for the estimates, but also items questioning the individual expert’s reasoning and items that ask them to indicate which information they would need in order to come up with better estimates. The iterative design thus leads to a growing set of evidential materials and information which is made available to all participants. The task of the expert thus is not so much to predict, but rather to evaluate the given material with regard to the main question or hypothesis.

Historically, we can identify a precursor study from 1948 which applied the expert-as-predictor concept (Kaplan et al., 1950); then, the first Delphi study from 1950 applying the expert-as-evaluator concept (Dalkey and Helmer, 1963); and finally, a study from 1963 which became a paradigmatic case for the use of Delphi outside the RAND orbit but which relied again on the earlier concept of the expert-as-predictor (Gordon and Helmer, 1964; Helmer, 1966). The silent return of the predictor has had a severe but unacknowledged consequence for the Delphi methodology, because the interpretation of the convergence of

the estimates as “consensus” was based on the expert-as-evaluator concept. Lacking the shared set of evidential materials, there was not much the experts could agree on.

There is thus a striking discontinuity in the history of Delphi: though the Delphi inventors took some effort to establish the expert-as-evaluator concept as valuable and epistemologically stable (Helmer and Rescher, 1958, 1959), the arguably less sophisticated expert-as-predictor concept dominated most of the method’s trajectory since the mid 1960s. The proposed interpretation establishes a discontinuity that hitherto went unnoticed in the literature on Delphi. What is more, it also evaluates this discontinuity in a Bachelardian manner.

Still, the question remains open how this discontinuity can be explained. Given that Olaf Helmer, who had spent considerable effort on establishing the expert-as-evaluator concept in the 1950s, was also involved in the study that silently dismantled it and returned to the expert-as-predictor concept (Gordon and Helmer, 1964), this discontinuity is all the more astonishing. No simple answer seems plausible. Moreover, several factors might help understand the silent return of the predictors. Probably together with (or even on the initiative of) the Delphi researchers, RAND management had decided around 1963 to take concerted steps to disseminate Delphi more widely. The method had hitherto been used at RAND only. Trying to match the contemporary requirements of statistical stability, Delphi researchers might have felt the urge to conduct Delphi studies with larger samples than before (the first Delphi study had a panel of eight experts), which fostered the adoption of a less elaborate and expensive feedback procedure.

But this, in my view, explains only part of the silent return of the predictors, mainly because of the severe methodological consequences of this return. The omission of a collaborative compilation of a set of empirical evidence and information undermined one of the central claims of Delphi, namely the claim that the convergence of expert opinions can legitimately be treated as “consensus.” Lacking the collaborative composition of a set of evidence that evolves and alters from round to round, the “consensus” produced by the long-range forecasting Delphi had no substantial backbone. Since they could not agree on the epistemic value of a set of evidential material, what were they expected to consent upon? Or, put it the other way round, what is the likely psychological effect of being repeatedly asked to revise one’s answers in the face of the opinion of the majority? Does such a procedure result in consent? Or, rather, in a mixture of annoyance and fatigue? Without the experts-as-evaluators concept, the convergence that should be a rational result of the method becomes an artifact. The expert-as-evaluator concept offered a justification for the iterative procedure as a means of establishing consensus which the expert-as-predictor concept lacked. If the

latter is nonetheless applied, then the interpretation of the convergence as consensus has no methodological justification.

It is unlikely that this was unnoticed by the inventors of Delphi. Above all, Helmer, Dalkey, and Rescher had received training as philosophers of science. Also, Delphi has been confronted with severe criticism, and several critics have raised the issue of fatigue. What is thus interesting about the Delphi case is that its proponents failed to identify its final form as a dead end. Even in the face of heavy attack, they continued to believe in their method's inherent quality and capacity. Thus conceived, "epistemic hopes" is not only an analytic category that helps to describe what historical proponents hoped to achieve, but also an explanatory category that helps to understand why they resorted to less sophisticated ideas. Positive expectations towards the capacity, the productivity, or the efficiency of one's own ideas can prevail over concern, doubt, or uneasiness with potential flaws inherent in these ideas. Epistemic hopes can turn into epistemological obstacles, and in the case of Delphi, they did. They lead the RAND researchers to believe in the value of their approaches even in the face of an apparent flaw - in our example the fact that the silent return of the predictors in principle voids any serious attempt to interpret the convergence of estimates as "consensus".

There are, however, decisive differences between Bachelard's epistemological obstacles and epistemic hopes as I propose to define them. Above all, the concept of epistemic hopes emphasizes the motivational effects triggered by positive expectations - hopes give a mission, something that makes it sensible to draw arms even against an overwhelming opposition. This is so because unlike Bachelard's obstacles, which are culturally widespread phenomena (e.g., myths), epistemic hopes are attached to the innovative, the non-familiar, the previously unthought.

A second major difference between epistemic hopes and epistemological obstacles is that Bachelard was mainly interested in subjective processes of cognition in science. Collectives intermediating between culture and the individual do not feature prominent in his thought. However, without any theoretical problem, the concept of epistemic hopes can be applied to social collectives like, e.g., research groups, theoretical schools, or academic tribes. This allows for linking this concept to important insights from social psychology, most importantly perhaps to the theory of cognitive dissonance (Festinger et al., 1956). Hence, the historian of science can analyze the means with which their historical actors attempted to reduce the dissonance extant between their claims and reality, one such means being the invocation of epistemic hopes. In our case, for instance, one formulation that was repeated

almost mantra-like is that knowledge about the future is so crucial to rational policy making that even imperfect methods constitute an improvement.

6 Conclusion

Historical epistemology can offer fruitful starting points for the historiography of the social sciences. This holds true for the more general movements of thought - the historization of epistemology and the epistemologization of history, both combined and separated. But this holds also true for the more radical ideas of Gaston Bachelard and his vision of a normative history of the sciences that informs current scientific practice. The research topics Swedberg (2013: 20) outlines in his note - “to establish exactly what from the past is being transmitted” and what not, and to investigate the reasons for this selection; to investigate “[w]hat has been forgotten of the past but deserves to be revived and incorporated in the working body of current sociology”; “to research exactly how knowledge is being transmitted, distorted and improved”, or to assess the importance of “tacit knowledge” in the development of sociology - offer several links to perspectives of historical epistemology as described above.

The examples presented in this text, of course, are examples. I do not claim to have systematically assessed or even adequately addressed all those ideas from historical epistemology that offer valuable inspiration to methodological debate within the historiography of the human sciences. However, an initial list that can be derived from the examples discussed includes the following research designs:

- To investigate in depth past debates in order to construct a terminological system deemed relevant to the analysis of contemporary phenomena
- To evaluate classic or historic texts with the same - technical, methodological, theoretical - scrutiny you would apply when reviewing coeval texts
- To seek for discontinuities in the progress of scientific knowledge and look whether these discontinuities can be explained by factors rooted in the (social) psychological situation of the proponents.

If one decides to do historical studies of the social sciences in a way informative for current debates, as both Richard Swedberg and Frank Welz hope more historians will do in the future, the historicists’ tenets do not provide a useful starting point. Their strength is in the identification of alternatives, of contingencies, of paths taken and paths not taken; but this usually leads to some kind of relativism. Such relativism diminishes the ability of historicist

studies to inform current debates. If one expects the history of the social sciences to be of use for current debates within the disciplines, then a normative approach to historiography appears indispensable.

In the view of Bachelard, the discontinuous development of science is caused by epistemological obstacles; habits of thinking that remain unreflected for the most part of our lives. As they obscure the truth, it is important for researchers to break up with these habits, which is not easy. Regresses in the development of science can be explained by reference to epistemological obstacles, while progresses always can be understood as resulting from a successful break with formerly held habits of thinking. The identification of epistemological obstacles is thus a central task of the historian of science; and this is a normative undertaking. Clearly, the evaluation of older works along standards is at odds with a strict version of historicism.¹¹ The evaluation of events and ideas in history is more virulent if one applies nowadays standards, and one could not finish a sentence in this direction without being accused of writing “Whig history”.

However, it should be emphasized that the Bachelardian system replaces this simple dichotomy between historicism and presentism by a processual understanding. In his view, out of a philosophical questioning of the history of science, a philosophy of science might emerge. This clearly does not deny the relevance of historically sound, “historicism” research; rather, it adds to a phase of historiography a further phase of philosophical questioning. And it is this second phase in which the value of various conceptualizations and ideas is systematically assessed and evaluated. Even if one could argue that this separation will never be strict, the basic understanding of historiography and systematical evaluation as two separate phases of work breaks a simple dichotomy between historicism and presentism and sets the two lines of thinking in a potentially fruitful relation.

¹¹ The historicist position was put forth most outspokenly by Robert Alun Jones (Jones, 1977, 1983a, 1983b), who refined it in a debate with Charles Camic (1979, 1981; Jones, 1981) and some years later in what came to be called the „historicism controversy“ (Jones, 1985; Seidman, 1985; Turner, 1985; Warner, 1985). Presentist uses of historical figures still abound in contemporary debate; see, e.g., Pūras (2014).

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