

Title: The Institutional Stabilization of Philosophy of Science and its Withdrawal from Social Concerns after the Second World War

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Abstract: In this paper I criticise the thesis that value-laden approaches in American philosophy of science were marginalized in the 1960s through the editorial policy at *Philosophy of Science* and funding practices at the National Science Foundation. I argue that there is no available evidence of any normative restriction on philosophy of science as a domain of inquiry which excluded research on the relation between science and society. Instead, I claim that the absence of any exemplary, professional philosopher who discussed the relation between science and society sufficed to narrow the focus of philosophers of science, given the institutional stabilization of the domain within professional philosophy from 1959 onwards.

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1. Introduction

The withdrawal of American philosophy of science after the Second World War from topics concerning the role of science in society has received much attention in the literature. (Giere, “The Structure, Growth and Application of Scientific Knowledge”; Howard, “Two Left Turns Make a Right”; Reisch, *How the Cold War Transformed Philosophy of Science*; Edgar, “Logical Empiricism, Politics, and Professionalism”; Douglas, *Science, Policy, and the Value-Free Ideal*). Reisch construes this withdrawal as the result of the broader anti-communist climate of post-war America, whereas Scott Edgar ties it to the emergence of restrictive professional standards for philosophy of science. More recently, Vaessen and Katzav proposed two institutional mechanisms as the causes of the withdrawal: a shift in the editorial policy of *Philosophy of Science*, associated with Richard Rudner as the new editor in 1959, and the funding decisions at the “History and Philosophy of Science” (HPS) subprogram of the U.S. National Science Foundation’s (NSF) Social Science program (Vaesen and Katzav, *The National Science Foundation and Philosophy of Science’s Withdrawal from Social Concerns*).

These accounts all take for granted that there was a substantive shift from a pluralist approach in American philosophy of science which included research into the relation between science and society to a more restricted approach which excluded such research. Given this characterization, the central question becomes which actors actively restricted the domain and why. In this paper I argue that this characterization of the historical shift is mistaken for three reasons. First, there is no evidence that any actors in the 1950s wanted to exclude questions about science in society from philosophy of science. Second, few philosophers of science were investigating these questions by the end of the 1950s to begin with. Third, the shift away from these questions during the 1960s coincides with an important institutional change for philosophy of science: it becomes a recognised

subdiscipline in philosophy disconnected from the sciences themselves. This institutional change has not yet been properly recognised as a potential influence on the content of philosophy of science. I argue that at the crucial moment when philosophy of science found institutional stability within professional American philosophy, there were no successful exemplars for the domain to guide both teaching and research on the relation between science and society. This led to a neglect of the topic in the 1960s when the research for the domain became standardized.

2. The Restrictive Nature of the Value-free Ideal

Previous accounts of the developments within American philosophy of science after the Second World War have discussed the impact of logical empiricist ideas, especially Reichenbach's, on the domain (Douglas, *Science, Policy, and the Value-Free Ideal*, 48-49; Edgar, "Logical Empiricism, Politics, and Professionalism", 179; Reisch, *How the Cold War Transformed Philosophy of Science*, 284, Vaesen and Katzav, *The National Science Foundation and Philosophy of Science's Withdrawal from Social Concerns*, 74). They all agree that at least a subsection of logical empiricist philosophers believed philosophy of science should not be concerned with the relation of science to society. Vaesen and Katzav call this the value-free ideal for philosophy of science. Supposedly, it functioned as a normative restriction on philosophy of science as a domain of intellectual inquiry, by excluding all questions concerning the role of science in society from the domain. Reichenbach's distinction between the context of discovery and the context of justification is commonly associated with the thesis that philosophers of science should not deal with any questions concerning the relation between science and society (Reisch, *How the Cold War Transformed Philosophy of Science*, 355–56; Douglas, *Science, Policy, and the Value-Free Ideal*, 48). Such restrictive interpretations of Reichenbach in particular and logical empiricism in general are mistaken.

Reichenbach believed that philosophers of science *could* focus on the logical structure of scientific theories and an evaluation of their acceptance in abstraction from the social context in which these theories were developed. However, Reichenbach never claimed that philosophers of science should *only* be concerned with the logical reconstruction of science (Richardson, “Science as Will and Representation”). In fact, in 1952, Reichenbach advised the newly-founded NSF on a philosophy of science subprogram and explicitly characterized philosophy of science as a richer domain. In August 1952, Raymond Seeger, a physicist and head of the Division of Mathematical, Physical and Engineering Sciences at the NSF, asked Reichenbach for advice on how the NSF might further the development and organization of philosophy of science.¹ Reichenbach replied that the best institutional location for philosophy of science was a research institute, much like the German Max Planck Institute, that operates independently from the concerns of particular universities and could act as an intermediary between scientists and philosophers through seminars, conferences and research stays. Reichenbach also discussed the important topics for the field:

Logical analysis of science. Clarification of scientific concepts; studies in scientific methodology; research in mathematical logic and its application to the sciences, to linguistics, to engineering problems; training of scientists in logic and methodology.

Relations between science and society. Studies concerning the impact of science on social structure and its influence on human relation. Applications to education

¹ Seeger to Reichenbach, 8 August 1952, Hans Reichenbach Papers, Box 38, folder 2 (HR 38-2), Archives of Scientific Philosophy (ASP), Hillman Library, University of Pittsburgh.

and social ethics; training of scientists in transferring their methods to the social field.²

The letter is useful to understand both Reichenbach's ideas concerning the institutional location of philosophy of science and his ideas about the scope of its domain. Reichenbach did not conceive philosophy of science as a subfield of professional American philosophy and neither did he adhere to a restrictive value-free ideal. The relations between science and society, including applications to education and ethics, belong to philosophy of science. As Thomas Uebel has argued, all logical empiricist philosophers accepted that the pragmatics of science, which focused on these kinds of questions, was a valid part of the domain of study besides the logic of science. Within the logical empiricist movement only a division of labour was made between these different approaches (Uebel, "Knowing Who Your Friends Are", 163). Such division is also present in Reichenbach's letter to Seeger.

Unlike what Katzav and Vaessen claim, there was also no restrictive value-free ideal operative when the NSF decided to support research in philosophy of science. In the 1958 official NSF report on the start of the Social Science Program, written by its director Harry Alpert, the addition of the HPS division is defended, because "it can make science self-conscious and aware of the significance of what it is doing in relation to other human activities".³ Three years earlier Alpert had organized a conference in Philadelphia to discuss the potential value of such a program. Henry Margenau represented philosophy of science, Bernard Cohen the history of science and Bernard

² Reichenbach to Seeger, 12 September 1952, HR 38-2, ASP.

³ "Annual Review of the Social Science Research Program", 1 July 1958, Ernest Nagel Papers (EN), Box 29, folder "National Science Foundation", Rare books and Manuscripts Library, Columbia University.

Barber the sociology of science. Philipp Frank and Herbert Dingle gave comments on the addresses, and Harry Alpert, Raymond Seeger and Alan Waterman, president of the NSF, concluded the conference with their views (Shryock, “The Nature of the Conference”, 327). By the end of the gathering it was officially decided that the NSF would establish a framework for an advisory panel for the History, Philosophy, and Sociology of science. Such combined panel would “serve to give unified direction to any efforts in these areas” (“Appendix”, 352). This unified direction implied the study of the relation between science and society, especially in a democratic society.

In the summary of the conference, Margenau and Frank’s view are described as a unified plea for philosophical and historical perspectives on science, “in order to adjust scientists to the social environment in which they operate” (Shryock, “The Nature of the Conference”, 327). In his published address Frank defended that the history, philosophy, and sociology of science can make scientists less ignorant in the ways of man (Frank, “Summarizing Remarks”, 350). Although Frank claimed that one can reconstruct any scientific theory on a logical basis, he did not believe that a philosophical understanding of science ends with such a reconstruction (Frank “Summarizing Remarks”, 351; Reisch, *How the Cold War Transformed Philosophy of Science*, 208–33). No normative restriction on the domain of philosophy of science can be found in his text, and it is equally absent from the motivation that Alpert and Seeger invoke for supporting the HPS subprogram.

Even if the conditions under which the HPS subprogram was initiated in 1958 did not favour a restrictive value-free ideal, the philosophers who operated as advisors to the program could still have brought a restrictive ideal into effect. Vaesen and Katzav (“The National Science Foundation and Philosophy of Science’s Withdrawal from Social Concerns”, 76) defended that once the NSF

funding for the HPS subprogram was set up, the advisors used it to promote value-free approaches at the expense of value-laden ones. This claim, however, faces two problems. First, like Reichenbach, the advisors most likely did not have a restrictive idea about the field. Second, there is no evidence that the applications of a value-laden subgroup were downplayed.

First, let us look at the first five panel-members for the HPS subprogram: Ernest Nagel, Max Black, Sidney Morgenbesser, Grover Maxwell and Wesley Salmon. In the introductions of Nagel's courses on philosophy of science, the relation between science and society is consistently included into the domain of philosophy of science. In the introduction of his 1964 course he states that philosophy of science has to "study the relation of science to social conditions which make the former possible, and perhaps give an interpretation and adjustment of scientific conclusions in relation to contemporary culture."⁴ Moreover, on Nagel's worksheet for the NSF meeting of 6 and 7 March 1959, it is possible to see who applied for NSF funding in philosophy of science in 1959, and who Nagel rejected.⁵ The four proposals for philosophy of science included projects by Herbert Feigl (Concepts of physical science), Abraham Edel (Social Science variables), Richard Montague (Metamathematics) and Robert Mulligan (Evolution of Man). Mulligan and Edel's project could be labelled as value-laden projects. However, on the document Nagel rejected Feigl's and Mulligan's. It is unclear which criterion Nagel applied to cut up these projects into rejection/acceptance, but it was not by using a restriction on value-laden topics as the boundary.

⁴ Ernest Nagel, "Philosophy of Science 1964", EN 24 – folder "Teaching Materials Philosophy of Science New School 1954".

⁵ "Advisory Panel for Anthropology and History and Philosophy of Science", EN 29 – "National Science Foundation".

For their discussion of Grover Maxwell, Vaessen and Katzav investigate the volume *Current Issues in the Philosophy of Science*, co-edited by Maxwell and Herbert Feigl. As they point out, the volume does not contain any contribution that discusses societally engaged philosophy of science. Yet, in the summer of 1960, Feigl and Maxwell, started working on a new project, entitled “Problems of Ethics for our Age of Science”. Although no published work apparently came out of it, the five pages long project-outline, which was sent to several philosophers of science including Wilfrid Sellars and Carl Hempel, discussed value-laden issues: “issues concerning the justification of moral value judgments; value judgements within the domain of science; critical survey of the dependence of moral value judgments on variations in human nature”.⁶ Thus, no restrictive ideal should be ascribed to Feigl and Maxwell.

Wesley Salmon, Sidney Morgenbesser and Max Black, who were the other advisors to the NSF between 1959 and 1965, never published on the social aspects of science. However, this does not imply that they would discard work on such topics as irrelevant to the field, as Vaesen and Katzav have claimed (“The National Science Foundation and Philosophy of Science’s Withdrawal from Social Concerns”, 76). Given the lack of evidence that Salmon, Morgenbesser or Black had a restrictive view on philosophy of science, evidence is needed that they effectively downplayed projects concerning the relation between science and society. Such evidence is hard to come by, since the NSF meetings were not publicly recorded and most of the actors involved did not preserve the reports that they wrote to the NSF.

⁶ Feigl to Hempel, 16 December 1960, Carl Hempel Papers (CH) 16-1, ASP. Feigl to Sellars (including *Science & Ethics Program*), 19 April 1960, Wilfrid Sellars Papers (WS) 181-14, ASP.

3. Scarcity of value-laden projects

The idea that the NSF advisors actively downplayed value-laden projects in the 1960s, or indirectly dissuaded scholars from entering such projects, presupposes that there were a lot of scholars working in the domain of philosophy of science on value-laden topics who would have been willing to submit value-laden projects. Ever since Howard's overview of value-laden work published in *philosophy of science* in the 1940s and 1950s ("Two Left Turns Make a Right", 67-70), it has been taken for granted that there were a lot of philosophers still available in the 1950s who engaged value-laden topics in philosophy of science and were only subsequently excluded from the field. Vaesen and Katzav ("The National Science Foundation and Philosophy of Science's Withdrawal from Social Concerns", 77) have attempted to list them individually in order to show that they were never supported by the NSF. However, their list is deceptive, since it is difficult to recognize most of these philosophers as *philosophers of science*. Sidney Axinn, Rollo Handy, Jack Kaminsky and Dale Riepe all published one value-laden paper in *Philosophy of Science* in the 1950s (Axinn, "Two Concepts of Optimism"; Handy, "Personality Factors and Intellectual Production"; Kaminsky, "Can 'Essence' Be a Scientific Term?"; Riepe, "Flexible Scientific Naturalism and Dialectical Fundamentalism"). These papers do not refer to each other, are not integrated into other research of the time and were never cited by anyone else. Moreover, their other work focuses on different aspects of philosophy.

Vaessen and Katzav also list philosophers who never published in *Philosophy of Science*: Errol Harris, Gail Kennedy, Paul W. Kurtz, R.W. Sleeper, H.S. Thayer and Paul Schmidt. Similar to the aforementioned four authors, none of them took part in any contemporary discussion in philosophy of science, neither do they refer to each other. Those works which are cited by Katzav and Vaessen were never discussed by any other philosopher of science. Moreover, these philosophers never

attempted to play any role in the formation of philosophy of science as a discipline in the 1950s or 1960s. In their publications, these authors were not developing anything that could be meaningfully interpreted as a distinct approach to philosophy of science. They do not argue how their views matter to philosophy of science as a domain. Thus, there is no evidence that they could have been identified at the time as a group of intellectuals engaged in philosophy of science, neither by themselves, nor by others.

This leaves four philosophers of science from Vaessen and Katzav's list: Lewis Feuer, Sidney Hook, Philip Wiener, and David Miller. Feuer and Hook share a similar profile of a politically engaged philosopher. However, there is no evidence that their work was marginalized - at most, it was not widely read by professional philosophers of science in the 1960s and Feuer's work was never reprinted as representative of philosophy of science in the 1950s. This indicates that his contemporaries did not consider his work exemplary for the domain as such. However, the reason for this need not be the exclusion of a value-laden approach, given that excerpts from Sidney Hook's book *Reason, Social Myths and Democracy* were consistently reprinted, both in the volume *Readings in the Philosophy of Science* edited by Feigl and Brodbeck, and in a volume with the same title edited by Philip Wiener. Still, by the end of the 1950s Hook had become a purely politically oriented philosopher who had no research interests in philosophy of science. Wiener is a representative of a socially-oriented philosophy of science, but there is no evidence that Wiener was marginalized. Instead, he shifted his focus. From the 1940s onwards he was institutionally tied to the emergence of the history of ideas and the history of philosophy as domains of inquiry, especially through his editorship of the *Journal of the History of Ideas*. This leaves David Miller - to most, an unknown figure. Miller published in *Philosophy of Science* from the 1930s until the 1950s and opposed the empiricist, anti-realist interpretation of science by logical empiricists. Miller

was an opponent of formal logic as an analytic tool, but this should not be a reason to classify him as value-laden philosopher of science in particular. Miller's work was marginalized: it was never reprinted and almost never referred to by anyone. However, there is no evidence that Miller was marginalized because of the value-laden nature of his work. Thus, contrary to what previous accounts have presumed, there was no abundance of philosophers of science in the 1950s who dealt with questions about the role of science in society who could have been subjected to marginalization.

4. *Philosophy of Science*: from journal to discipline

Previous accounts have not properly related the withdrawal from social concerns to the struggle over the institutional identity of philosophy of science that occurred during the 1950s. Once this institutional struggle is integrated into the historical narrative, it becomes clear that no active exclusion of value-laden discussions was required to downplay certain topics once the domain became institutionally stable as a subdiscipline in philosophy.

To investigate the institutional instability of the domain I will focus on the journal *Philosophy of Science*. According to Howard, the removal of value-laden topics from the journal should be understood within the context of a shift in editorship, from West Churchman to Richard Rudner (Howard, "Two Left Turns Make a Right", 70). Editorial policy determines what kind of work gets published and thus a shift in such policy is crucial to understand a shift in a discipline (Katzav and Vaesen, "On the Emergence of American Analytic Philosophy"). However, one should not conclude that any shift in the direction of a domain of inquiry can be reduced to a shift in editors and a shift in who got to decide which work to publish or which project to fund. A change in the function of a journal and the institutional location of its domain of inquiry can also affect its

content, and the choice of editors for a journal is often as much an effect of these changes as that it is a cause. This is what happened in the case of *Philosophy of Science*.

To make my case, I discuss two editorial transitions in *Philosophy of Science*, from William Malisoff to West Churchman in 1948 and from Churchman to Richard Rudner in 1959. In both transitions, the status of both the journal and the domain of inquiry in relation to American professional philosophy was at stake. After the second transition, the journal definitively changed its identity: it aimed to function as the outlet for the best available work in philosophy of science, now seen as a new subdiscipline in American philosophy. This is the function that it still serves today. Given this transition, the kind of papers that Howard identified as value-laden were no longer candidates for publication, since they were testament to the older function of the journal, not as a disciplinary outlet, but as an outlet for all reflections on science, including partisan reflections of a non-academic style.

When William Malisoff launched the first edition of *Philosophy of science*, there was no recognizable subdiscipline “philosophy of science” within American professional philosophy. Malisoff did not use “philosophy of science” as a term to delineate an *academic* domain of inquiry. In his first editorial, Malisoff wrote that philosophy of science was “the organized expression of a growing intent among philosophers and scientists to clarify, unify, the programs, methods and results of the disciplines of philosophy and of science” (Malisoff, “Editorial”, 1). The journal was conceived as a meeting ground to discuss the significance of science in contemporary society. Moreover, the style of the journal would “allow the greatest latitude”. When Malisoff wrote a second editorial in 1944, “philosophy of science after ten years”, he boasted that many writers for the journal “had hoped to carry on in its pages a fight against the rising tide of obscurantism and its not too unfriendly appeasers” (“philosophy of science after ten years”, 1). Despite his zeal to

continue the diverse publication record, he noted in 1944 that many papers had only been of minor importance and that the record “on the whole had been highly unsatisfactory”, not because most contributions were academically insignificant, but because they had been “just barely intelligible”.

Malisoff urged his contributors to avoid trade jargon of certain schools of philosophy and to abandon the barriers of a “secret language”. When he presented his journal to Reichenbach in 1936, he made clear that it was not the American counterpart to *Erkenntnis*, the German research journal edited by Reichenbach and Carnap. His journal focused on a diverse audience, made up of philosophers, scientists and the interested lay-man alike: “my advice to contributors is to discuss their assumptions and results but omit the formal proofs which they can send to mathematical journals.”⁷ In 1946 Malisoff reaffirmed that philosophy of science had to be “the model of democracy” (Malisoff, “A Science of the People, by the People and for the People”).

Academic philosophers dealing with science were not opposed to Malisoff’s democratic intentions, but they also wanted an outlet for their academic work, and Malisoff’s journal did not serve this purpose. Ever since *Erkenntnis* was shut down in 1938, Hans Reichenbach had been on the lookout to recreate this research journal within his new American context. After the death of Otto Neurath in 1945, the International Institute of Unified Science, which was the formal institution that had organized conferences and published the *Encyclopedia of Unified Science* faded into administrative non-existence (Reisch, *How the Cold War Transformed Philosophy of Science*, 294). But by July 1947, Philip Frank, Charles Morris, Rudolf Carnap and Hans Reichenbach joined forces to create an American counterpart: the Institute for the Unity of Science. This institute had the goal “to encourage the integration of knowledge by scientific methods; to conduct research in

⁷ Malisoff to Reichenbach, 14 february 1936, HR 3-23, ASP.

the psychological and sociological backgrounds of science; to compile bibliographies and to publish abstracts and other forms of literature with respect to the integration of scientific knowledge.”⁸ Just as all previous institutions of logical empiricists, it was not meant to confine philosophy of science to the icy slopes of logic and neither did it situate its activity within professional philosophy. In its initial by-laws Reichenbach and Carnap were appointed as editors of the Institute’s future periodical. When Malisoff unexpectedly died in November 1947, Morris advised Reichenbach to attempt a take-over of the journal⁹. Reichenbach promptly contacted the publisher Williams & Wilkins and emphasized that a take-over would significantly increase the academic reputation of the journal within the scientific community.¹⁰

The publisher replied that the decision was in the hands of the *Philosophy of Science Association*, which had never gathered, but was made up of all subscribers to the journal. Given Malisoff’s death, this group would meet in Chicago on 27 and 28 December to decide the fate of the journal.¹¹ When Reichenbach learned that West Churchman, at the time professor in philosophy at the University of Pennsylvania and a collaborator of Malisoff, had momentarily taken over the editorship, he was alarmed and wrote to Frank: “the man is a real danger for the philosophy of science.”¹²

⁸ “By-laws of the Institute for the Unity of Science”, HR 18-34-11, ASP.

⁹ Morris to Reichenbach, 18 November 1947, HR 18-33, ASP.

¹⁰ Reichenbach to Williams & Wilkins, 21 November 1947, HR 38-16, ASP.

¹¹ Gill to Reichenbach, 25 November 1947, HR 38-16, ASP.

¹² Reichenbach to Frank, 13 December 1947, HR 18-34, ASP.

Among Carnap, Morris, Frank and Reichenbach, a strategy for the Chicago meeting was devised. Since Reichenbach could not be present, Frank would attempt a compromise between Churchman's group and the supporters of the Institute for the Unity of Science.¹³ Meanwhile, Herbert Feigl, not yet a formal member of the Institute for the Unity of Science, but a strong advocate for logical empiricist philosophy, tried to persuade Ernest Nagel to act as a "compromise" editor.¹⁴ However, Nagel was unwilling to become editor and the pressure was entirely on Frank's capacity of negotiation. When the meeting started, Frank, Feigl, Morris and Carnap realized that they were outnumbered by the subscribers which Churchman had gathered. As Frank reported to Reichenbach, Gustav Bergmann, philosophy professor at the University of Iowa, also showed up unexpectedly.

During the meeting, a new governing board was formed "according to a proposal of one of the Churchman group". It consisted of Churchman himself, F.S.C Northrop, Bergmann and two direct supporters of Churchman, Littauer and Cowan. As a compromise, Frank was also added to the board. When this new board met, Frank was accused of attempting a "coup d'état" on the journal. In order to prevent future conflict between Churchman and the Institute for the Unity of Science, Northrop and Bergmann convinced Frank to take the role of president of the governing board, even though he himself did not like this "delicate position".¹⁵ The presidency of Frank became a purely formal matter: he had no impact on the editorial board in whatever way and Churchman was eventually confirmed as new editor. After the meeting, Bergmann proudly admitted to Wilfrid

¹³ Morris to Reichenbach, 3 December 1947, HR 18-33, ASP.

¹⁴ Feigl to Nagel, 15 December 1947, EN 1 – "Herbert Feigl".

¹⁵ Frank to Reichenbach, 6 January 1948, HR 18-34, ASP.

Sellars that he had “succeeded, temporarily, in keeping *Philosophy of Science* a free outlet by keeping it out of the hands of Carnap and Reichenbach.”¹⁶ The results were threefold.

First, Reichenbach decided that their Institute should not collaborate with Churchman’s journal, nor with the association. To Frank, he wrote: “it is now imperative for us to build up our own journal, we should not waste our prestige with this group.”¹⁷ This decision ensured that, for the next ten years, there was no cohesive research community for philosophy of science as a domain of inquiry: logical empiricists shunned the journal (e.g. Hempel and Nagel only became a subscriber to the journal in 1959) and the conferences organized by these philosophers under the heading of section L of the *American Academy of Arts and Sciences* would be disjoint from the *Philosophy of Science Association*.

Second, Churchman reprinted Malisoff’s 1944 editorial and promised that “the present editor has every intention of continuing this policy”. In 1949 Churchman reaffirmed in his own editorial that philosophy of science “should play the liberal role within science today”. Churchman did not want philosophy of science as a domain of inquiry to transform into an academic discipline.

The field, and its journal, cannot and should not become "respectable" in the eyes of the "competent experts" [...] Its function is to keep alive the conflicts of general viewpoints that may give rise to a more powerful and fruitful science of the future. For this reason, philosophy of science is not professional philosophy,

¹⁶ Bergman to Sellars, 5 January 1948, WS 158-10, ASP.

¹⁷ Reichenbach to Frank, 13 January 1948, HR 18-34, ASP.

nor professional science, which are both in the main conservative in their outlook. (Churchman, “Editorial”)

This created a third effect: within broader academic circles, the credentials of the journal became suspect. Just after Churchman seized his role as editor-in-chief, the sociologist George A. Lundberg wrote to Hempel that he had been “depressed by the low quality of some of the papers that appeared in recent years in *Philosophy of Science*.” Lundberg had received a paper from Churchman for comments “of such appalling incompetence as to raise the gravest doubts on my mind as to Churchman’s fitness for the editorship.”¹⁸ Hempel replied in agreement that “a much stricter editorial policy for *Philosophy of Science* would be most desirable all around.”¹⁹ In 1951 Adolf Grünbaum equally complained to Reichenbach about Churchman’s editorial policy to remove all technical material from submitted manuscripts.²⁰

Churchman was a biased editor. In an article from 1950, he argued on pragmatic grounds that logical reconstructions do not contribute to the improvement of science and, therefore, should not play an important role in the philosophy of science (“*Logical Reconstructionism*”, 166). If Churchman was to succeed in presenting an alternative to formal approaches through his journal, he needed good alternatives. Given the lack of any cohesive research alternative that Churchman’s policy engendered, this became an important problem. Bergmann confessed to Sellars that he was worried about the credentials of the journal in Churchman’s hand, and he advised Sellars to send Churchman good manuscripts, especially in philosophy proper, “and rather technical (not

¹⁸ Lundberg to Hempel, 8 July 1948, CH 25-11, ASP.

¹⁹ Hempel to Lundberg, 16 July 1948, CH 25-11, ASP.

²⁰ Grünbaum to Reichenbach, 27 May 1951, HR 37-24, ASP.

unnecessarily too, too, of course).”²¹ Throughout the 1950s, Churchman’s editorial policy became increasingly problematic: publications were not widely read, neither by scientists nor by philosophers. The many disconnected papers in the journal on the role of science in society which were cited by Howard and Katzav and Vaessen are evidence of this problem.

On 29 December 1956 the governing board of the *Philosophy of Science Association* met to discuss its financial situation. When the physicist Henry Margenau, who had taken over the presidency from Frank, suggested to dissociate the membership from the subscription to the journal, Ernest Nagel took over the meeting and heavily criticized the quality of the journal. He suggested that “the association be more active in running the periodical, that a publication committee be appointed, that the journal should be run in a more business like manner.”²² Nagel’s protest resulted in the appointment of a reform committee which both reorganized the journal and the association by adopting new by-laws. These new by-laws effectively transformed the journal into a professional research journal for philosophy of science as a domain - the very shift that Malisoff and Churchman had opposed. The by-laws now stated the following:

The objects of this association shall be the furthering of studies and free discussion from diverse standpoint of the field of philosophy of science, and the publishing of a periodical devoted to such studies in this field.²³

²¹ Bergman to Sellars, 5 January 1948, WS 158-10, ASP.

²² “Meeting Notes”, 4 January 1957, Richard Rudner Papers (RR) Box 1 – “Margenau”, Rare books and Manuscripts Library, Washington University.

²³ “By-laws adopted by the reorganization committee”, 13 November 1957, RR 1 – “Margenau”.

In 1958 C.J. Ducasse took over the presidency of Margenau and immediately focused on the execution of these by-laws. In collaboration with Ernest Nagel, he began searching for a candidate to replace Churchman: “the journal needs something of a rebirth.”²⁴ Although Nagel was the preferred candidate of the board, he again refused to take up editorial responsibilities. He wrote Ducasse that his own preference would be for a distinguished practicing scientist, who would have the confidence of other scientists as well as of philosophers.²⁵ Although Nagel wanted the journal to improve its academic credentials, he did not think that the domain itself should be part of professional philosophy. However, no scientist was found to be both willing and able. During the search Ducasse made it clear to Churchman that the journal should end the “liberal” policy:

I think there should be room in the Journal not only for formalized articles, and for articles on methodology, but also for articles on such topics as, say the naive tacit metaphysics of scientists who repudiate having any metaphysics; on the psychology of logicians and of scientists in general; on the sociology of science; on such scientific status as sociology, or history, etc., may or may not have; on such a question as that of the relation between philosophy of science and theory of knowledge; on the relation between proving scientifically, and convincing; and so on. The one thing I would insist on, however, in such articles no less than in formalized ones, or methodological or specialized ones, would be competence at least, and preferably, high quality — for such articles, as well as formalized

²⁴ Ducasse to Nagel, 22 June 1958, EN 29 – “PSA”.

²⁵ Nagel to Ducasse, 13 June 1958, EN 29 – “PSA”.

ones, can have this. We should have no half-bake or amateurish stuff; and we should try to have some articles of men of high reputation fairly often.²⁶

Ducasse's main concern was to transform the journal into an outlet that could drive professional research in philosophy of science, as the new by-laws stated. In his description of the field, the sociology of science was not excluded from the domain. The impact of science on society and of society on science could be a legitimate part of the field. Nagel responded with complete agreement to this letter which was circulating among the board members.²⁷ After much deliberation, the board eventually nominated Arthur Burks first, Richard Rudner second and Wesley Salmon third.²⁸ Since Burks turned the offer down, Rudner, who was not Nagel's preferred candidate, became editor in 1959.²⁹ Rudner quickly decided to remove the inactive editorial consultants that Churchman had gathered. In order to comply with Nagel and Ducasse's wish for more professionalism, he replaced them with Adolf Grünbaum, May Brodbeck, Carl Hempel, Wesley Salmon and Richard Madden – all of them professionally trained philosophers.³⁰

²⁶ Ducasse to Churchman, 21 September 1958, EN 29 – “PSA”.

²⁷ Nagel to Ducasse, 23 September 1958, EN 29 – “PSA”.

²⁸ Ducasse to Zerby, 6 October 1958, RR 1 – “Ducasse”.

²⁹ Initially, Nagel had doubts whether Rudner would steer the journal in the right direction. Richard Brandt who had been a colleague of Rudner at Swathmore College suggested to Nagel that Rudner would not be very influential with scientists. Brandt to Nagel, 28 October 1958, EN 29 – “PSA”.

³⁰ Rudner to Ducasse, 30 June 1959, RR 1- “Ducasse”.

5. Professionalism and The Absence of Exemplars

Historical narratives about developments within philosophy of science cannot presuppose that there was a professional discipline which institutionally represented philosophy of science as domain of inquiry before 1959 and which subsequently became “impoverished” by the exclusion of certain topics. It is only because philosophers decided to reorganize the journal *Philosophy of Science* and its association, that the professional discipline that we now recognize as philosophy of science was able to emerge in the 1960s to begin with. As Alan Richardson pointed out, the important disciplinary infrastructure for the domain was only formed in the 1960s: the first separate PSA conference in Pittsburgh in 1968 and the crucial centers of research in Pittsburgh, Boston and Indiana (Richardson, “Occasions for an Empirical History of Philosophy of Science”).

For European logical empiricists, it had been a constant concern to distinguish their new approach from traditional philosophy, and, consequently, also from its traditional institutional embedding as philosophy (Richardson, “Scientific Philosophy as a Topic for History of Science”). The Institute for the Unity of Science was never meant to serve as a professional institution for philosophers. Reichenbach equally believed philosophy of science should ideally be located in an institution which was aimed at scientists and was distinguished from the philosophy department in universities. Even Ernest Nagel wished in 1958 that the editor of *Philosophy of Science* would not be a professional philosopher. During the 1955 conference for the NSF, Margenau and Frank also made it clear that philosophy of science as a domain of inquiry was not part of academic philosophy. Margenau interpreted philosophy of science “as a component of science” in need of similar funding as science (Margenau, “Present Status and Needs of the Philosophy of Science”, 337). According to Frank, professional philosophers of science should aim to provide both scientists and science teachers with “a deeper understanding of science and its place among other

human activities” (Frank, “Summarizing Remarks”, 351). Thus, in the 1950s most senior actors did not want philosophy of science to be anchored in philosophy departments.

However, once Rudner took over the editorship and gathered only professional philosophers in his editorial team, the journal’s function, and eventually the PSA’s function decidedly moved towards an institutional embedding of the domain of inquiry within professional philosophy. Rudner’s editorial board was made up purely of philosophers, none of whom had any direct scholarly concern for the place of science in society, and all of whom were interested to further develop the dominant, contemporary research interests, on explanation, induction or theory change. This shift effectively lay to rest the broader social significance that logical empiricist philosophers had attributed to their movement and their activities. It also removed the “liberal”, “non-professional” intentions of Malisoff and Churchman from the domain. From Rudner’s editorship onwards, the intention of the journal and the field to form relations with, or even be part of scientific disciplines faded away.

At this crucial moment of institutional change, there were few, professional philosophers who dealt with questions concerning the role of science in society. Otto Neurath had died in 1945 and left no one but Frank to replace his sociological and historical perspective on science within the network of logical empiricism (Dewulf, “The Place of Historiography in the Network of Logical Empiricism”, 2020). Most of Neurath’s work was not even available to the English-speaking world at the time. Although Philip Frank was promoting a sociological and historical perspective on science in the 1950s through his work (Frank, *Philosophy of Science*, Tuboly, “Philipp Frank’s Decline”) and attempted to support such research, e.g. by guiding the set-up of the HPS panel for the NSF, his influence was hampered in two respects. Not only was his competence questioned by the younger generation of professional philosophers (Richardson, “That sort of everyday image of logical positivism”), but his position at Harvard University, alongside his advanced age, also gave

him little opportunity to influence many graduate students in philosophy. Dewey's pragmatism had no representative within professional philosophy who played an active part during the institutionalization of philosophy of science, with the exception of Churchman (Giere, "From Wissenschaftliche Philosophie to Philosophy of Science", 349). Churchman's take-over of *Philosophy of Science* was a failed enterprise: the journal published too many unread papers in the 1950s and the journal's non-academic policy ensured that the activities and interests of logical empiricist philosophers would never be integrated with Churchman's concerns. Instead of creating a pluralist journal that incorporated both worlds, Churchman ended up isolating his own approach. Consequently, the papers and topics from his journal were not used in the classrooms of Hempel, Grünbaum, Salmon or Nagel. To make matters worse, after his failed editorship Churchman abandoned philosophy of science as a domain and focused on management studies instead.

At the crucial moment of professionalization, none of the available scholarly research that dealt with issues concerning the relation between science in society had become a successful exemplar for professional philosophers.³¹ The consequence was a lack of such issues in the emerging discipline throughout the 1960s. Work on the pragmatics of science had remained an ideal throughout the 1950s, but had found no representative that was capable of guiding new research, or of connecting the varied attempts at a discussion from the 1950s. Second-generation professional "logical empiricists", like Pap, Grünbaum, Salmon, Putnam, Morgenbesser or Suppes, who would

³¹ This is a contingent fact of the matter. There were certainly potential exemplars available by the end of the 1950s, but none actualized their potential within the emerging professional discipline of philosophy of science. E.g. Robert Merton was successful in promoting more research on the relation between science and society. Despite the fact that some of Merton's famous papers were published in *Philosophy of Science*, his influence on the philosophical community was limited. His graduate students did not pursue a career in philosophy.

set the agenda for philosophy of science in the 1960s and 1970s, had been attracted by the available exemplars of their field: Reichenbach's *Experience and Prediction*, Hempel's "Studies in the Logic of Explanation and Confirmation", or Carnap's *Logical Syntax of language*. Even though Reichenbach, Hempel, Carnap or Nagel never excluded a historical or sociological perspective from their domain of inquiry, they themselves never engaged in it explicitly. Their neglect of such perspectives was only reinforced when their intellectual successors who had no interest in developing the pragmatics of science themselves, founded philosophy of science as a professional discipline. As I argued in section 3, none of the value-laden work from the 1950s that Howard or Vaessen and Katzav point out, ever led to any discussion at all, not even among the cited authors themselves. Whereas Hempel's "Studies in the Logic of Explanation" was generally conceived by the end of the 1950s as an exemplar of professional inquiry in the logic of science, both in teaching and research (Salmon, *Four Decades of Scientific Explanation*, 4), the pragmatics of science had no comparative exemplar. By 1959, the division of labour between the logic and the pragmatics of science had resulted in a wide-spread professional neglect of the latter. This neglect sufficed to shift the content of philosophy of science as a domain of inquiry under the conditions of its professionalization in American *philosophy*, when only few philosophers of science were engaging questions about the role of science in society. No restrictive ideal of any kind was required.

If a subgroup of philosophers of science had enforced a restrictive boundary concerning value-related topics both through the journal and the NSF, there has to be evidence of such exclusion. None can be found. For both Ducasse and Nagel, the relation between science and society was a relevant topic for philosophy of science. Even Carl Hempel emphasized in the 1960s that the social consequences of theory-acceptance should be taken into account (Douglas, *Science, Policy, and the Value-Free Ideal*, 56–59). The boundaries that were drawn, especially during the editorial

transition of 1958, focused on the standard of professionalism solely. During this period other boundary-work did occur, but mostly it excluded *other* philosophical fields. When the PSA organized its first separate conference in Pittsburgh in 1968, the rejected papers mostly dealt with epistemology, metaphysics or logic.³² None of the papers that were submitted to the first PSA conference discussed value-laden topics. This confirms that value-laden interests within the philosophy of science community were scarce.

However, scarcity does not imply extinction. The community of philosophers of science throughout the 1960s and 1970s welcomed discussions of the relation between science and society. The entire fourth issue of *Philosophy of science* in 1962 was dedicated to the topic. During the PSA meetings of 1970 and 1972 Ronald Giere and Michael Scriven urged the discipline to give the topic more attention (Giere, “The Structure, Growth and Application of Scientific Knowledge”; Scriven, “The Exact Role of Value Judgments in Science”). By 1974 the social impact of science was already addressed in multiple contributions to the PSA meeting (Cartwright, “How Do We Apply Science?”; Kaye, “The IQ Controversy and the Philosophy of Education”; Skolimowski, “Technology Assessment as a Critique of a Civilization”). However, unlike formal approaches to induction, explanation or theory change, the science-society relation remained without any widely-accepted exemplar to guide both teaching and research for the professional discipline.

6. Conclusion

According to George Reisch, the current clarity of the conceptual difference between philosophy and politics in reflecting on philosophy of science as a domain should be understood as an artefact

³² “List of Accepted and Rejected Papers”, Wesley Salmon Papers Box 66, folder 7, ASP.

of certain historical and institutional conditions, especially during the 1950s (Reisch, *How the Cold War Transformed Philosophy of Science*, 369). Reisch suggested that the removal of the public intellectual from American universities was an important part of these conditions. This fits well with my representation of the developments in the 1950s: philosophy of science as a domain of inquiry did not find a stable, institutional location as (socially and intellectually significant) intermediary between philosophy, science and society, neither through Churchman's efforts, nor through the Institute for the Unity of Science. Given the institutional climate of the 1950s, philosophy of science as a domain only found stability once it decidedly reinvented itself as a subdiscipline of professional philosophy.

The contingent nature of this development reveals that our narratives within the history of philosophy should also focus on the institutional location of philosophical activity. The withdrawal of American philosophy of science from social concerns can only be understood against the background of the institutional shift towards a self-standing professional discipline. Against the traditional interpretation of the withdrawal, I have argued that there was no active exclusion of value-laden work from philosophy of science as a domain. The lack of any successful, exemplary, professional discussion of the relation between science and society sufficed to narrow the focus of philosophers of science, under the conditions of the institutional stabilization of the domain within professional, American philosophy from 1959 onwards.

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