We have never been ‘new experimentalists’: On the rise and fall of the turn to experimentation in the 1980s

Abstract: The 1980s, it is often claimed, were the decade when experimentation finally became a philosophical topic. This was the responsibility, the claim continues, of one particular movement within philosophy of science, ‘new experimentalism’. The aim of this article is to complicate this historical narrative, and to argue that in the 1980s **,** the study of experimentation was not so much carried out by one movement with one particular aim, but rather in a very diverse and open-ended way, by people with very different aims and backgrounds. We will then argue that, from the late 1990s onwards, this diversity disappeared and made room for disciplinary divisions: some questions concerning experimentation became philosophical, others sociological, etc. The reason for this, we claim, was that science and technology studies, philosophy of technology, and philosophy of science, all took over certain aspects of the 1980s study of experimentation. In this way, we will argue, these elements became instutionalized, whereas others were forgotten. The importance of this process of institutionalization will then be illustrated by means of a discussion of other, similar approaches to the philosophy of experimentation that have not been able to ensure continuity, because they did not find an institutional home.

1. Introduction

The 1980s are often characterized as the period in which philosophers of science discovered experimentation. Theodore Arabatzis, for example, opens his lemma on ‘Experiment’ in *The Routledge Companion to the Philosophy of Science* as follows: “Although experimentation has been a staple feature of modern science since the seventeenth century, it was only recently, during the 1980s, that experimental practice attracted the attention of philosophers of science” (Arabatzis 2008, 159). Similarly, Friedrich Steinle writes that “[o]nly in the 1980s, did philosophy of science again take up the question of experiment” (Steinle 2003, 409), a claim he has recently repeated in an overview article written with Uljana Feest (Feest and Steinle 2014, 274). Such claims are not new: already near the end of the 1980s, Ian Hacking claimed that before the 1980s “there was almost no reflective philosophy of experiment” (1988, 147).

Hacking is also one of the authors who is mostly credited for this turn to experimentation. Arabatzis, for example, states that “[f]ollowing Hacking’s, by now, classic *Representing and Intervening*, experimental activity became a subject of philosophical scrutiny” (2008, 162). Even more, Hacking’s work is sometimes presented as having established a particular experiment-focused movement within philosophy of science, dubbed ‘new experimentalism’ (a term first put forward by Robert Ackermann in his 1989 review of Allan Franklin’s *The Neglect of Experiment*). Such claims can be found, for example, in the work of Deborah Mayo (1994), Steinle (2003, 409), Michela Massimi (2004, 36-37), Andrea Woody (2014, 124), Mieke Boon (2015, 59) and Jutta Schickore, who further specifies that “[t]he sorry state of [the realism-]debate around 1980 was one of the motivations for Ian Hacking to develop his ‘new experimentalism’ in *Representing and Intervening*” (2016, 20).

Recently, Massimiliano Simons and Matteo Vagelli (2021) have argued that it can be questioned whether philosophers of science really only discovered experimentation in the 1980s. A more thorough look at the history of philosophy, they claim, reveals that systematic philosophical schools reflecting on experimentation existed earlier in the 20th century as well, for instance surrounding the work of Gaston Bachelar or Hugo Dingler. These approaches are, however, mostly neglected in the history of recent philosophy of science. Such neglect, we argue following Fons Dewulf (2021), arises from a common conflation of philosophy of science in general with one of its institutionalized forms, namely Anglo-American Philosophy of Science. This has occurred in the history of philosophy of experimentation as well, since the above mentioned historical overviews restrict themselves to Anglo-American philosophy of science, whereas, as has been suggested by David Gooding, "[p]hilosophical theses about experiments' contribution to scientific knowledge have been stimulated primarily by work outside the mainstream of Anglo-American philosophy of science" (2000, 123). In line with the work carried out by Simons and Vagelli, and Gooding (and Dewulf's work more generally), the aim of our paper is to point out certain of these blind spots, and to argue that the history of philosophy of experimentation should not restrict itself to the claims made by those we now identify as philosophers of experimentation (or as 'new experimentalists'), but equally well should pay attention to how the field's identity and boundaries were discussed, challenged and negotiated.

Our starting point is a question raised by Simons and Vagelli (2021): if experimentation was already on the philosophical radar before the 1980s, was there still something special about the 1980s study of experimentation, and if so, what precisely? Whereas Simons and Vagelli (2021) suggest a number of factors to map the case of Ian Hacking, our paper aims to answer these questions about philosophy of experimentation more generally. Regarding these questions, it seems that there is little agreement: for some, the 1980s were the moment when experimentation really became a topic for mainstream philosophy of science; for others, what was unique was the realization that experimentation could provide new insights concerning specific philosophical topics (e.g. scientific realism, as Schickore claims, or the discovery-justification distinction, as she has argued together with Steinle (2006, vii)). Still others believe that the 1980s really did not deliver much. According to Mayo (1994, 271), for example, "New Experimentalism has come up short" in rectifying decades of philosophy of science purely focused on theory. And Hans Radder writes that "the expectation that the study of experimetnation would become a major issue within received traditions in philosophy of science has not been fulfilled" (2003, 2).[1]

Our discussion will show that these disagreements are not new. There was no real agreement among those who studied experimentation in the 1980s either on the topic's significance, and their motivations for studying experimentation were rather diverse and open-ended: while some studied experimentation to contribute to existing discussions, others saw it as offering a way to

[1] If one looks at the publication record, the result seems to be rather mixed: though a significant number of books on the philosophy of experimentation have been published (Gooding, Pinch and Schaffer 1989; Heidelberger and Steinle 1998; Radder 2003), and many companions to the philosophy of science include a lemma on experiments (e.g. Gooding 2000; Arabatzis 2008; Feest and Steinle 2014), it remains remarkable that many introductory books or companions to philosophy of science (e.g. Ladyman 2001; Okasha 2002; Godfrey-Smith 2003; French and Saatsi 2014; Baberousse, Bonnay and Cozic 2018) lack any substantial engagement with the topic. One very early exception is Alan Chalmers' *What is This Thing Called Science?*, which already included a chapter on experiments in the original 1976 edition (most likely due to his background as an experimental physicist). In more recent editions, Chalmers also included a chapter on new experimentalism.

open up new philosophical questions or even to radically challenge the practice of philosophy of science in general. It will turn out that not much was shared, beyond a rather vague and general commitment to the study of experimentation as a constructive activity, and that, with regards to the 1980s context, experimentation is to be understood as a sort of *minimally shared object*: while all parties agreed that experimentation should be an object of concern, they disagreed on why this concern was warranted and on what should follow from it.[2]

That experimentation formed a sort of minimally shared object in the 1980s will be argued for as follows. We will start by pointing out that, in response to what was perceived as a one-sided focus on theory and representation within mainstream philosophy of science, many students of experimentation started emphasizing that experimentation was a constructive activity, i.e. that experiments involve active interventions from the side of scientists on their object of study. This was primarily just a shared emphasis, however, which when developed further, led to very diverse views regarding what was philosophically significant about experimentation. We will show this by discussing how this emphasis on experimentation as a constructive activity was used to overturn three specific philosophical topics – the discovery-justification distinction, epistemological individualism, and the politics of science –, and how this resulted in many different, sometimes opposing views on why experimentation should receive more attention. This diversity manifested itself not only in terms of content, moreover, but equally well in dialogues and discussions between people with very different backgrounds (philosophy, sociology, anthropology, cultural studies, etc.).

Because our claim is that the 1980s study of experimentation was so diverse and multi-faceted, we refrain from narrating it too much in terms of 'new experimentalism'. Using this terminology already carries with it certain philosophical assumptions about what is significant about experimentation, and about who counts as a philosopher of experiment. Nowadays, for example, the title of new experimentalist is mainly given to Hacking, Allan Franklin and Peter Galison. Others, such as e.g. Robert Ackermann, Hans Radder, Karin Knorr-Cetina, Harry Collins, Andrew Pickering, David Gooding and Bruno Latour, are less easily grouped under this nomer: if they are discussed at all, they are mainly distinguished from new experimentalism as e.g. sociologists of science (see e.g. Woody 2014, 124). However, such distinctions were not prevalent at the time: Hacking (1988), for example, all grouped them together as philosophers of experiment; many of them (e.g. Collins, Gooding, Knorr-Cetina and Pickering) saw their work as concerned with classical philosophical questions (e.g. realism, induction, evidence, etc.); and all of them often appeared together in collected volumes on experimentation (Gooding, Pinch and Schaffer 1989; Pickering 1992; Galison and Stump 1996). For this reason, we have decided not to employ 'new experimentalism', and to opt instead for the more neutral terminology of a 1980s interest in, study of, or turn to, experimentation.

In the second part, we will then argue that, from the late 1990s onwards, this diversity disappeared and was replaced by disciplinary distinctions between different topics and students

[2] We draw the notion of 'shared object' from the work of Susan Leigh Star (with James R. Griesemer 1989; 2010) on the concept of 'boundary object', which similarly focuses on cases where one object of concern is shared by different groups, who do not necessarily share the same goals or interpretation of that object. We, however, did not opt for 'boundary object', since the term suggests that there are fixed disciplines and boundaries between which the object lies. Our point is that in the 1980s study of experimentation these disciplinary boundaries were precisely also unclear.

of experimentation. This was the result of how certain fields and disciplines – science and technology studies, philosophy of science and philosophy of technology – co-opted and incorporated certain aspects of the 1980s, while abandoning others.

In line with Dewulf (2021), we will then argue that this process of disciplinary division is to be understood in terms of the institutionalization of certain ideas, approaches and questions. This notion of institutionalization is interpreted here in a rather broad sense, as a set of tools, such as journals, societies, conferences, university curricula and job positions, that offer a carrier for views concerning, in this case, experimentation and its significance. The importance of such institutionalization will be emphasized in the last part of the paper, where we discuss certain forgotten approaches to the study of experimentation. That these are no longer continued, while others are still considered significant, cannot be explained merely in terms of their content, since in that respect they are all very similar. The difference rather arises from whether or not they succeeded in finding institutional carriers for their views on experimentation. This suggests that the history of philosophy of experimentation should be concerned not only with the claims and ideas made within what is commonly seen as the philosophy of experimentation, but equally well with how the field's identity, its boundaries and its relations to other ways of philosophically reflecting on science were conceptualized, challenged and institutionalized.

2. Emphasizing Experimentation as a Constructive Activity

1980s students of experimentation often started by distinguishing themselves from the view, which they took to be dominant within mainstream philosophy of science, that science was primarily a theory-focused representationalist activity. Thus, in 1981, Karin Knorr-Cetina contrasted her work with what she described as the objectivist view that "the world is composed of facts and the goal of knowledge is to provide a literal account of what the world is like. The empirical laws and theoretical propositions of science are designed to provide those literal descriptions" (1981, 1). Similarly, Ian Hacking's motivation for studying experiment derived from the observation that "the philosophers' conception of observation[,] the notion that the life of the experimentation is spent in the making of observations which provide that data that test theory […] plays a relatively minor role in experiments" (1983, 167). Harry Collins (1985, 2) contrasted his own approach with what he saw as the prevalent algorithmic conception of science, according to which science consisted in following a set of explicit rules that would lead to true claims representing untouched nature. Robert Ackermann (1985, 8) saw himself as correcting the idea that "[a]fter the knower studies the object of knowledge, it remains unaffected, and the knower changes primarily by adding information about the object to the stock of his scientific basis". David Gooding (1990, xi) saw himself as arguing against "received philosophies of science [which] focus so exclusively on the literary world of representations". And Deborah Mayo (1994, 270) summarized how she saw the main aim of 1980s studies of experiment as the attempt to "clear away the obstacles created by old-style accounts of how observation provides a basis for appraisal (via confirmation theory or inductive logic)".

By distinguishing themselves in this way, many 1980s students of experiment at the same time also started emphasizing what they saw as ignored: that scientific experimentation was primarily a constructive activity, i.e. that experimentalists intervene on, and often change, the reality they are interested in. Bruno Latour and Steve Woolgar, for example, wrote that "[their] very specific interest in laboratory life concerns the way in which the daily activities of working scientists lead to the construction of facts” (1979, 40). Knorr-Cetina similarly argued that most regularities studied by scientists “are *created* in the laboratory” (1981, 3). By means of a discussion of how the Hall effect and the Josephson effect did not exist before the apparatus required to produce them was created, Hacking argued that “to experiment is to create, produce, refine and stabilize phenomena” (1983, 230). A year later, Andrew Pickering wrote that “agency belongs to actors not phenomena: scientists make their own history, they are not the passive mouthpieces of nature” (1984, 8). According to Collins (1985, 2), “experimentation is a matter of skillful practice”, and Ackermann (1985, 124) stressed that “it is more revealing to consider scientific facts as constructions than as mere careful observations”. Gooding, finally, argued that “modern, especially analytical, philosophy [had largely neglected] the agency of observers and the way their observation of nature is mediated by their interactions with each other, with their instrumentation and with the natural world” (1990, xii).

Nowadays, this 1980s emphasis on experimentation as a constructive activity is often read in terms of the realism-debate (e.g. Psillos 1999, 247; Chakravartty 2007, 30; Woody 2014, 124; or the essays by K. Brad Wray, Matthias Egg, and Hasok Chang in Saatsi 2018). The work by Hacking and others is seen, more specifically, as a proposal for an alternative to the then dominant theory-focused forms of scientific realism: according to Schickore, for example, “[o]ne important driving force for the turn to experimentation was the impasse that had been reached in the debate about scientific realism” (2016, 20). However, this was definitely not the only, or even primary, motivation for many studies of experimentation in the 1980s. Hacking himself, for example, even though he indeed put forward a realist proposal, did not see the realism-question as the most interesting or pressing one: “[d]isputes about both reason and reality have long polarized philosophers of science. […] Is either kind of question important? I doubt it. We do want to know what is really real and what is truly rational. Yet you will find that I dismiss most questions about rationality and am a realist on only the most pragmatic of grounds” (1983, 2). He primarily discussed realism in so much detail, it seems, because he wrote *Representing and Intervening* as a textbook, and this focus allowed him “to organize my introductory topics in the philosophy of science” (1983, 2).[3] Similarly, Knorr-Cetina (1981, 3), Pickering (1984, 404), Ackermann (1985, 30), and Gooding (1990, 186) all claimed that their aim was not so much to argue either for or against (a specific form of) scientific realism, but rather to shift the focus away from this debate and the representationalist assumptions underlying it.

In what follows, we will show how this 1980s emphasis on experimentation as a constructive activity was used to open up and challenge very different philosophical discussions, concerning the distinction between discovery and justification, epistemological individualism and the politics of science. This will show that this constructive emphasis was not restricted to the realism debate, but rather should be read in a very broad sense: some used it to elaborate new positions within existing philosophical debates, others used it to introduce new philosophical questions, and still others used it to radically challenge how philosophy of science in general was to be conducted.

2.1 Overcoming or refining the context-distinction

[3] In fact, in the second, experiment-focused part of the book, realism takes up only one section, while most of it is concerned with all kinds of other philosophical topics, such as observation, modeling, the creation of phenomena, measurement and microscopy.

The distinction between the context of discovery and the context of justification, as Steinle and Schickore put it, "[f]or several decades [...] dictated what philosophy of science should be and how it should proceed" (2006, vii). Its influence even reached beyond philosophy, according to Gooding: many approaches in e.g. artificial intelligence as well "work with the impoverished notion of discovery still favoured by analytical philosophy" (1990, 5). Generally taken to have been first formulated by Hans Reichenbach (1938), Pickering summarized the distinction as follows:

> It asserts a clean separation between theory testing (the context of justification) and theory construction (the context of discovery). Theory testing, philosophers argue, is (or should be) amenable to explication according to the canons of formal logic. It is an impersonal, ahistorical, culture-neutral process, and hence the proper locus for philosophical inquiry. Theory construction, on the other hand, is held to be immune to philosophical explication. It is seen as essentially private and personal, and hence to be relegated to the realms of psychology. (Pickering 1984, 414n5)

Against this clean distinction, Knorr-Cetina argued that "what happens in the process of construction is *not* irrelevant to the [scientific] products we obtain" (1981, 5). In the design, operation, and evaluation of an experimental procedure, scientists are required to make certain decisions, concerning e.g. the materials to use, the disturbances to control for, and the results to exclude as outliers, and these decisions influence the end-produce: "[t]o view scientific investigations as constructive rather than descriptive is to see scientific products as highly internally constructed in terms of the selectivity they incorporate" (1981, 7). Similarly, Hacking emphasized the constructive nature of experimentation as an argument against the idea that the activity of scientists could be characterized in terms of discovery: "phenomena are hard to produce in any stable way. That is why I spoke of creating and not merely discovering phenomena" (1983, 230). And Gooding (1990, 7) argued, after having delineated no less than six forms of constructive interventions in the production and evaluation of experimental knowledge (concerning which measurement points to retain, how and where to present one's results, how to structure one's arguments, etc.) that these significantly problematized Reichenbach's distinction:

> Reichenbach's distinction between the contexts of discovery and justification has little regard for the process of constructing arguments: it made a divorce of convenience which implicitly justifies a highly selective approach to what scientists produce. Of course the justification of a claim can often be separated without difficulty from its 'generation' or 'discovery', but that separability reflects the conclusion of at least [three constructive processes]. (Gooding 1990, 7)

As such, experimentation was often characterized as a constructive activity to argue against the claim that scientific practice neatly divided into two different contexts. The conclusions drawn from this, however, were less in agreement. Some, such as Knorr-Cetina (1981, 7-8), Pickering (1984, 6) and Gooding (1990, 7-8) took it to mean that there were no real contexts to be distinguished: a scientific result could always be questioned, and hence it could never be considered as definitively justified in the sense presupposed by the supporters of the distinction. The claim that a result was justified was rather to be understood as saying that the result had been produced and evaluated in line with the standards governing the scientific community at the time. As Collins put it: “[i]t is not the regularity of the world that imposes itself on our senses but the regularity of our institutionalized beliefs that imposes itself on the world” (1985, 148).

Others, however, rather concluded that the distinction had to be sharpened. Franklin, for example, stated that he was mainly interested in the role played by experimentation in the context of justification, i.e. in the role it played in “theory choice or confirmation and the validation of experimental results” (1986, 5). Engaging with the work of Pickering and Collins then led him to argue that, while it was not the case that justification was purely socially determined (Franklin 1990, 2), it was necessary to distinguish a third context, the context of pursuit, which concerns the further investigation of a theoretical or experimental hypothesis after it has first been suggested in the context of discovery. In this way, Franklin tried to incorporate some of the social factors put forward by Collins and others: “the reasons offered by social constructivists [for accepting or rejecting a scientific claim], that is opportunity for future work, consistency with community commitments, recycling of expertise, and career interests, do play a role in pursuit. I also believe that the history has shown that these reasons do not influence what the experimental results are, the acceptance of these experimental results, or their use in justification” (1999, 179). Similarly, Hacking used Galison’s work to argue that Reichenbach’s distinction should be refined, rather than be replaced with social factors:

> A decade ago, ‘social construction of scientific facts’ philosophers implied that there is no such distinction between justification and discovery, and that evidence is a social product; experiments end when people have worked out their differences. Galison is neither Reichenbachian nor constructionalist. He denies that evidence has a purely logical content used in justification. Not only are data produced in material circumstances, but what counts as evidence is the product of historical traditions of experimentation and instrumentation. But there are strong nonsocial determinants of inquiry. (Hacking 1990, 103).

As such, we see that, while the emphasis on experimentation as a constructive activity led to a sort of shared belief that the discovery-justification distinction was not tenable as a neat dichotomy, there was less agreement on what this entailed: some inferred from it that the distinction could be abandoned, and that justification was merely a social phenomenon, while others took it to mean that the distinction needed to be refined to incorporate the role played by social factors. These views at the same time also embodied specific positions with regards to how philosophy of science in general was to be practiced, given the centrality of the distinction within mainstream philosophy: rejecting its significance meant advocating for a radical revision of philosophical practice in terms of a strong focus on social factors, whereas refining it suggested a more moderate position with regards to the status quo.

2.2 Epistemological Individualism

While it was hence open for discussion whether justification was completely social or not, all involved allowed the social some role to play. Hacking, for example, stressed in different places that “[e]verything I call a representation is public” (1983, 132) and that “[a] phenomenon, for me, is something public” (1983, 222). He contrasted this public nature of representations and phenomena with the view that private impressions, ideas and sense-data could act as scientific representations or as the foundations of scientific knowledge claims. Something could only be a scientific representation if it was constructed in such a way that it could be recognized by others as such: "[r]epresentations are intended to be more or less public likenesses. I exclude Kant's *Vorstellungen* and Lockeian internal ideas that represent the external world in the mind's eye" (1983, 133).

Gooding similarly targeted those epistemological views that "are expressed in the individualistic and mentalistic view of the scientist as a knowing subject" (1990, xiii). In opposition to this, Gooding argued that experimental knowledge is inherently public and social "because making experience intelligible is an active process in which observers often need to make sense of their own behaviour in relation to phenomena in order to communicate it to others" (1990, xiii). Contrary to the representationalist view, experimental knowledge is thus not obtained just by passively observing the world, but rather results out of constructive interactions between humans and parts of the world.

A similar view was put forward by Ackermann, in response to what he saw as "the root failure" of philosophers studying the scientific method, namely that they attempted "to trace scientific knowledge to the epistemological activities of the individual scientist" (1985, ix). Against such epistemological individualism, he emphasized that the establishment of experimental knowledge could only be understood when one included an analysis of the social structure of science. For Ackermann, this meant taking on a dialectical point of view, according to which it is only through the interaction between social and material elements that scientists are able to construct what he calls 'data domains', i.e. sets of empirical phenomena that have been stabilized and isolated, and over which scientists agree that they are real and relevant. A central role in the emergence of such domains is played by instruments, since it is through their functioning that representations and phenomena can become public: "[i]nstruments function to break off the influence of assumption on personal observation. If they did not exist, the fact of the influence of theory on perception might mean that shared data would be impossible. Where they do exist, a level of objective scientific fact [...] is more likely to be achieved" (1985, 129).

While there was thus quite some agreement that the constructive nature of experimentation problematized the epistemological individualism underlying the representationalism of mainstream philosophy of science, there was less agreement about what to infer from this. Collins took it to mean that the study of experimentation should be concerned primarily, or even solely, with the level of the community: "[f]or most purposes an individual's thoughts *qua individual* are of no interest. The most useful way of thinking about the goals of members of the core set is by thinking of those members as 'delegates' from the disciplines or other social and cognitive institutions which form their background" (1985, 148). Similarly, Joseph Rouse claimed that "[a]ttributions of knowledge are […] more like a characterization of the situation knowers find themselves within rather than a description of something they acquire, possess, perform, or exchange" (1996, 133). For Franklin, on the other hand, the public nature of scientific knowledge entailed that one should focus on its primary public manifestations, i.e. the published record:

> I think that the information acquired by an experimenter, by any means, is essentially that contained in the published work, and I think that the published reasons given both for the motivation of the experiment and for the acceptance or rejection of hypotheses are those that in fact determined the course of the work. Whatever an experimenter's private

> reasons for believing in a result, I think that only those that the author is willing to state publicly should be considered in discussing the validity of those results. (Franklin 1986, 5-6)

This again shows how a shared emphasis on the constructive nature of experimentation could lead to very different stances with regards to the level on which experimental knowledge was to be situated, and how consequently it was to be studied. And, again, it also led to very different positions with regards to the practice of philosophy of science: while Franklin claimed that philosophers should continue to study published papers, others advanced the more radical claim that philosophers should completely switch focus, to either the scientific community, as Collins claimed, or to the role of instruments, as Ackermann suggested.

2.3 The Politics of Science

In line with how the emphasis on experimentation as a constructive activity led many to the belief that social factors played at least some role, it also put to the fore questions concerning the relationship between science and politics. In part, these questions were already present in work predating the 1980s. This can be seen, for example, in Jerome Ravetz' *Scientific Knowledge and its Social Problems* (1971), which Hacking (1988, 148) described as one of the first to study experimentation, and which Ackermann (1985, ix) saw as a work that "decisively shifted the appropriate philosophical perspective on science toward a more historical and sociological standpoint". One of Ravetz' central points what that in recent times, the nature of science had shifted significantly: no longer primarily a detached endeavour concerned with merely describing the world, "[a]pplied science has now become the basic means of production in a modern economy" (1971, 21). Consequently, philosophy of science had to shift its primary concerns as well, from the epistemological to the political:

> If we are to achieve the benefits of industrialized science, and avert its dangers, then both the common sense understanding of science and the disciplined philosophy of science will need to be modified and enriched. As they exist now, both have come down from periods when the conditions of work in science, and the practical and ideological problems encountered by its proponents were quite different from those of the present day. (Ravetz 1971, 9-10)

While many 1980s students of experimentation approvingly referred to Ravetz' work and his claim that philosophy of science should pay more attention both to the social nature of science and to its place within society, there was less agreement on what was to be inferred from this. Some, such as Hacking (1992a, 10), believed that while the emergence of experimentation as a style of reasoning was the result of "little microsocial interactions and negotiations", over time experimental results had become autonomous from society in some sense. As he put it:

> I have said nothing about the most important ingredient of an experiment, namely, the experimenters, their negotiations, their communications, their milieu, the very buildings in which they work or the institution that foots the bill. […] This is […] because I am concerned with elements that are used in the experiment. But that is weak, because experimenters use money, influence, charisma, and so forth. We can nevertheless to some extent hold on to the difference between what the experimenters use in the experiment

> and what is used in order to do the experiment or in order to further its results. (Hacking 1992b, 51)

Hacking did not dispute that those factors he described as external played a role in experimentation, but the experimental sciences had in fact developed themselves in such a way that their functioning and products could be ascribed a rather autonomous position. Hence, one could give a philosophical account of them without explicitly including such external factors.

A different position was put forward by Ackermann. For him, studying the constructive nature of experimentation, i.e. what he called the dialectics between instrumentation and social structure, allowed philosophers to discuss and criticize the politics internal to scientific practice, as he illustrated by means of a discussion of recent Big Science. The instrumentation used there is of such a nature that it requires the working together of scientists with many different backgrounds, who are not always able to evaluate each other's contribution. This threatens what Ackermann saw as an essential aspect of the social structure of science, i.e. its polycentricity. This comes down to the idea that science works best when scientists are in a position of meaningful disagreement, i.e. when they have a mutual understanding of what the different positions within the community are, and where one's own position is to be situated (Ackermann 1985, 55). Important to the stability of this system is that the reward system within science is also polycentric, i.e. that reward is primarily achieved through recognition by one's peers within the scientific community (Ackermann 1985, 56). In Big Science, however, one is primarily funded and rewarded not by one's peers, but rather by a big bureaucracy. In this way, Ackermann's philosophical analysis of science leads him to the direct political claim that "the threat to polycentrism posed by the funding of big science has serious consequences for the dynamics of scientific progress" (1985, 57).

A third stance with regards to the relation between science and politics was provided by Collins. While his work primarily focused on a very specific issue within scientific practice - experimenters' regress situations -, he believed that studying them "can tell us things about culture as a whole - while at the same time this new perspective demystifies the role of scientific expertise" (1985, vii). His analysis indicates, he claimed, that scientific practice is not to be conceptualized in terms of an algorithmic model, according to which science is a complex system that produces scientific knowledge by following formal rules, and in which individual scientists are mere cogs in this machine. It should rather be seen in terms of an enculturation model, according to which science consists of local communities with differing social norms concerning what counts as knowledge, and which one enters in a way that cannot be written down as a formal procedure (Collins 1985, p. 159-160). The importance of this difference in philosophical conceptions, Collins then argued, lies in the fact that they have very different societal consequences:

> For the future *citizen* the [algorithmic] model of science and the natural world that is developed through normal scientific teaching is positively dangerous for democracy and for the long-term future of science itself. The model allows the citizen only two responses to science: either awe at science's authority along with a total acceptance of scientists' *ex cathedra* statements, or rejection - the incomprehending anti-science reaction. This is the citizen's interpretation of flip-flop logic. Where scientists' *ex cathedra* statements are

> found wanting - as they inevitably will be from time to time - then the most likely reaction is disillusion and distrust. (Collins 1985, 161)

Hence, it seems that there was some agreement regarding the claim that emphasizing experimentation as a constructive activity, especially in contemporary applied science, entailed that science could not be seen as completely divorced from society. At the same time, there was less agreement on what this meant exactly: while for Hacking, scientific practice had, in part, become autonomous from society, Ackermann stressed that there was equally well politics to be found within science, and Collins argued that we should not neglect the societal role played by images of science. This disagreement, again, went hand in hand with different positions regarding the practice of sscience more generally: while Hacking suggested that philosophers could continue focusing on internal epistemic factors, Ackermann stressed that they had to include internal political factors, while Collins put forward the more radical suggestion that philosophically studying experimentation was just another way to study broader cultural and societal phenomena.

3. The Importance of Institutionalization

The previous section has shown that the 1980s emphasis on experimentation as a constructive activity allowed different philosophers to question, challenge and reformulate certain issues that were dominant within mainstream philosophy of science. It has also shown that beyond this emphasis, there was little agreement on what should be taken as philosophically significant about experimentation. Moreover, these disagreements concerned not only specific philosophical topics, but equally well how philosophy of science more generally was to be conducted.

We take these disagreements around a shared, rather vague notion of experimentation to indicate that in the 1980s, the study of experimentation formed not a single philosophical movement with a particular aim, but rather a diverse group with different goals and a future that was still open-ended: it was not yet determined whether experimentation would be added to the list of mainstream philosophical topics, or would radically challenge how philosophy of science was conducted. In other terms, discussions on experimentation in the 1980s also concerned the identity of philosophy of science: while for some, experimentation offered a way to challenge the identity of what they saw as mainstream philosophy of science, for others it allowed them to confirm and refine what they saw as the established boundaries of those questions that were properly philosophical.

To the claim that these disagreements suggest that the 1980s study of experimentation was very diverse and open-ended, one could reply that they in fact show that, already in the 1980s and early 1990s, there was a significant dichotomy present within the study of experimentation: between those who put more emphasis on the role of social factors in science, and those who argued that their importance should not be exaggerated. This would then indicate that, contrary to our claim, the study of experimentation in the 1980s was in reality carried out from two different disciplinary perspectives: the sociology of science on the one hand, and the philosophical new experimentalists on the other.

This reply does not do justice to the disciplinary situation at the time, however, for as we have pointed out already in the introduction, all the philosophers discussed above interacted with each other in significant ways. They published together in collected volumes (Gooding, Pinch and Schaffer 1989; Pickering 1992; Galison and Stump 1996). They discussed each other's work at conferences and workshops: Pickering, Hacking and Galison shared a session at the PSA in 1988 on the philosophy of experiment, and Pickering, Ackermann and Michael Lynch organised a session at the PSA in 1990 on Allan Franklin's *The Neglect of Experiment* (Franklin himself could unfortunately not be there because of a car accident, see Ackermann 1990). They all saw themselves as working on philosophical topics: Collins (1985) started with an extensive discussion of the problem of induction and the work of Ludwig Wittgenstein and Nelson Goodman; Knorr-Cetina (1981) argued for her claims by engaging with the work of Thomas Kuhn, Paul Feyerabend, Steven Toulmin and Mary Hesse; Gooding (1990) saw his work as a reply to Reichenbach's context-distinction; and they all attempted, as we have seen, to elaborate a philosophical view on science that could replace what they saw as the mainstream one. Moreover, they were also often seen as philosophers of experimentation. They all made Hacking's (1988) list of philosophers of experimentation, and Ackermann grouped them together as follows in the 1990 PSA symposium:

> The point of [Franklin's new book] (in conjuction with well known studies by Latour and Woolgar, Pickering, Lynch, Hacking, Knorr-Cetina, and others) is to rescue the notion of experimentation that seems so crucial to an understanding of science from the disembodied form that it took in older empiricisms where experimentation was regarded as a mechanism for producing data regarded as factual assertions that could be used to thest the truth claims of theory. (Ackermann 1990, 451)

In what follows, we will argue that such disciplinary distinctions between sociologists and philosophers mainly emerged later on, from the late 1990s onwards. What happened there, we will argue, is that the 1980s study of experimentation transformed into different approaches and positions, and this more or less along existing or emerging disciplinary lines. We will then argue that these boundaries were the result of a process of institutionalization. We interpret institutionalization here in a rather broad sense, as a set of tools, such as specific journals, societies or conferences, university curricula and job positions, that in a sense offer a carrier for such theories or movements. Applied to the study of experimentation, this leads to two specific claims: certain parts have survived insofar as they have been integrated in existing or emerging disciplines as Science and Technology Studies (STS), philosophy of technology or philosophy of science; and a great set of students of experimentation did not succeed in having their concepts, theories and points of view become a part of a discipline, and in this way, have been either forgotten or marginalized.

3.1. Science and Technology Studies

One of the question at play during the 1980s, we have seen, concerned the role played by social factors in the production and evaluation of experimental knowledge claims. While all involved allowed them to play some role, there were disagreements about how big their role was. Those who emphasized the social more in their accounts, among whom Knorr-Cetina, Pickering and Collins, often drew their inspiration from the work of David Bloor and the Sociology of Scientific Knowledge (SSK), which was first put forward in *Knowledge and Social Imagery* (Bloor 1976).

Bloor, Knorr-Cetina, Pickering and Colllins all saw their work as contributing to the philosophy of science: they drew inspiration from, responded to, and engaged with, the work of Wittgenstein, Kuhn, Hesse, Goodman and others (see e.g. Bloor 1983 or Collins 1985), and they saw their work as offering a philosophical alternative to the theory-focused representationalism that they took to dominate mainstream philosophy of science. Others, however, soon came to see their work as an attempt to 'colonize' philosophy by sociological methods, i.e. as carried out by sociologists who for some reason considered themselves capable to address philosophical questions with the methods of the social sciences. Michael Friedman, for example, argued that SSK was shaped by an "explicitly philosophical agenda" which entailed the claim that "all there ultimately is to the notions of rationality, objectivity, and truth are local socio-cultural norms conventionally adopted and enforced by sociocultural groups" (1998, 239-240). This was perceived as a purely negative approach, which tried to reduce all philosophical-normative questions to purely sociological factors, and in this way, Friedman continued, SSK overstepped its bounds: "by insisting on such negative philosophical conclusions[,] defenders of SSK adopt an explicitly philosophical agenda which itself goes beyond the bounds of purely descriptive empirical research" (1998, 244).

At around the same time, some philosophers of experimentation as well started to distinguish philosophy of science proper from what they saw as sociology. Mayo, for example, claimed that by elaborating in more detail the role played by experiments in constraining what counted as epistemologically acceptable in scientific debates, philosophers of experiment could provide "sticks with which to beat the social constructivists" (1996, 61). Franklin, similarly, started claiming that those he called social constructivists – among whom Collins and Pickering – were no different from postmodernists in their claims that "science does not provide us with knowledge" and that "experimental evidence never seems to play any significant role" (1999, 4). Hacking, as well, it seems, saw a need to push back a bit on the idea that 'everything is socially constructed', by writing a book called *The Social Construction of What?*, in which he lamented what he saw as an emerging dichotomy:

> Even in the narrow domains called the history and the philosophy of the sciences, observers see a painful schism. Many historians and many philosophers won't talk to each other, or else they talk past each other, because one side is so contentiously 'constructionist' while the other is so dismissive of the idea. […] You almost forget that there are issues to discuss. (1999, vii).

Similarly, those who were more interest in the social factors at play in scientific practice became aware of emerging disciplinary boundaries. Very much in line with Hacking's book, Latour equally well wrote a book in 1999, *Pandora's Hope*, which was concerned, as he put it, with 'The Reality of Science Studies'. He opened the book with a few questions posed to him by someone he called a highly respected psychologist: 'Do you believe in reality?', 'Do we know more than we used to?', and 'Is science cumulative?' (Latour 1999, 1). Latour's response indicates that these questions derived from an emerging schism and dispute between sociological and philosophical accounts of science:

> I could not get over the strangeness of the question posed by this man I considered a colleague, yes, a colleague (and who has since become a good friend). If science studies has achieved anything, I thought, surely it has *added* reality to science, not withdrawn

> any from it. Instead of the stuffed scientists hanging on the walls of the armchair philosophers of science of the past, we have portrayed lively characters, immersed in their laboratories, full of passion, loaded with instruments, steeped in know-how, closely connected to a larger and more vibrant milieu. (Latour 1999, 2-3)

A few years later, however, even this recognition of emerging dichotomies has more or less disappeared. While the concern of many was still with the production of scientific and experimental knowledge, they no longer framed their accounts in terms of any opposition. The 'other side' was rather not even mentioned anymore at all. Collins, for example, started describing his work as part of what he called the third wave of science studies, with the first wave taking place "[i]n the 1950s and 1960s, [when] social analysts generally aimed at understanding, explaining and effectively reinforcing the success of the sciences, rather than questioning their basis", whereas the second wave was that of social constructivism in the 1970s, which showed that extra-scientific factors played an important role in the closure of scientific debates (Collins and Evans 2002, 239). Recently, a third wave had started, which, according to Collins and Evans, in part started with Collins' (1985) work: it concerned what they called "the 'expert's regress', by analogy with the 'experimenter's regress'. Because of the experimenter's regress, the class of succesful replications of an experiment can be identified only with hindsight; because of the expert's regress, the class of experts can be identified only with hindsight" (Collins and Evans 2002, 240). As such, the main concern of the third wave was to understand how "science is granted legitimacy in the political, legal, or other spheres" and "why science *should* be granted legitimacy because of the kind of knowledge it is" (2002, 241). The period in between the second and third wave, i.e. the 1980s, was only referred to in passing by Collins and Evans as "the science wars", without any further elaboration (2002, 237). A similar account can be found in Latour's Actor-Network Theory handbook *Reassembling the Social* (2005), where he traced back this study of the role played by science in the stabilization of social configurations to earlier work within the sociology of science, without any mention of philosophical contributions.[4]

These reconceptualizations by Collins and Latour of the history of their own work went together, moreover, with the formation of certain networks. In 2007, Collins launched The Expertise Network, which aimed at investigating, in an empirical way, how subjects experienced expertise (Collins, Evans, Ribeiro and Hall 2006), and which resulted in a series of workshops between 2007-2012 and a 2011-2016 ERC grant.[5] Similarly, in 2011 Latour lainched the AIME-project, which aims at conducting an empirical study of how actor-networks expand, diminish or stabilize over time. This project has resulted in a digital platform on which contributors from all over the world are encouraged to connect their researches on science and society.[6] At the same time, STS more broadly has also succeeded in becoming an institutionalized discipline: it has its own journals (such as *Social Studies of Science*), handbooks (*The Handbook of Science and*

[4] Very similar narratives are to be found in interviews with, among others, Collins, Knorr-Cetina, Michael Lynch, Latour and Woolgar on the history of STS, carried out for *Engaging Science, Technology, and Society* (published in 2018, accessible at https://estsjournal.org/index.php/ests/issue/view/10). While they trace back their work to Wittgenstein, Kuhn, SSK and other elements from the 1960s and 1970s, there is almost no mention of the 1980s study of experimentation. They rather turn immediately to the formation of STS as a discipline proper in the 1990s and 2000s.

[5] For an overview of The Expertise Network, see https://sites.cardiff.ac.uk/harrycollins/all-see-the-expertise-network/.

[6] For the project's platform, see http://modesofexistence.org.

Technology Studies), conferences and societies (such as the Society for Social Studies of Science 4S conference), STS departments and master programs.

In this way, we see how, from the late 1990s onwards, a disciplinary dichotomy emerged within the study of experimentation, between those interested more in the social factors at play and those who believed that their role was more limited. While at first, this dichotomy was palpable, over time it disappeared from how the history of science and technology studies is narrated: now, the narrative rather focuses only on the sociology of science. This disappearance, we have then argued, went together with the institutionalization of STS as a proper discipline.

3.2. Philosophy of Technology

Ackermann (1985), we have seen, argued that the study of instruments would allow philosophers to obtain more insight into the dialectics between the social and the material at play in the production of experimental knowledge. This idea was soon critically picked up by Don Ihde, who, in a review of Ackermann's book, argued that "Ackermann seems either to be almost totally unaware of [existing phenomenological-hermeutical work] or somehow avoids it, in keeping with the tradition of deliberately ignoring a countertradition" (1986, 126). Ackermann completely ignored the work of Patrick Heelan and that of Ihde himself, Ihde complained, and that was a shame, because otherwise the book "might have been greatly enriched" (1986, 127).

Ihde himself soon started elaborating these connections in more detail. At first, he did so under the label of 'Instrumental Realism', which he saw as a genuine 'school' converging around "a consensus regarding the role of the technological embodiment of science's mode of knowledge" (1991, xii) and "a critique of the extant and dominant forms of philosophy of science" (1991, 81). The list of people who, according to Ihde, belonged to this school was quite diverse, ranging from Hacking and Ackermann to Latour, Heelan and Hubert Dreyfus. This label, however, was never taken up and soon abandoned, including by Ihde himself.

Soon, Ihde started working under a different nomer, namely postphenomenology (see e.g. his 1993, 2009 and 2015). The central idea was that "all science, in its production of knowledge, is technologically embodied" (2009, 45), and hence, one could conclude, "[n]o instruments, no science" (2009, 35). Technology and instruments are interpreted as part of the embodiment of the scientists, and thus as an expansion of their lifeworlds. In this way, postphenomenology attempted to combine many elements of the 1980s study of experimentation with insights and concepts from phenomenology. In contrast to his earlier instrumental realism, this attempt was rather successful, since, as Robert Rosenberger and Peter-Paul Verbeek pointed out, from the early 2000s "postphenomenology has been quickly gaining influence in discussions on technology in the Philosophy of Technology, Science and Technology Studies, and other fields", bringing "together an international group of scholars working within a number of disciplines, including anthropology, sociology, cultural studies, media studies, as well as philosophy" (2015, 1).

In a sense, one could thus say that Ihde, and postphenomenology more generally, tried to overcome any disciplinary distinctions that could divide the study of instruments and technology. As we will see later one, there were other, similar attempts to do this (e.g. by Joseph Rouse), but whereas Rouse was not really succesful in this endeavour, Ihde was, and this precisely because postphenomenology was able to transform itself into an institutionalized approach within philosophy of technology. In 1998, Ihde turned what was first an informal reading group into “a technoscience research seminar” which “was made a permanent part of our Stony Brook doctoral program” (2015, xiv). This program produced a steady number of students who spread out across the United States, and at the same time received many visitors from abroad (mainly from Europe, see Ihde 2015, xiv).

One of the regular visitors was Peter-Paul Verbeek, who soon embraced the label of postphenomenology to describe his own projects (Verbeek 2005). At the University of Twente, he created an institutional setting very similar to Ihde’s in Stony Brook: postphenomenology was integrated in a consortium MA program and has produced a significant number of PhD students ever since. In this way, as Ihde put it, “participants from both Stony Brook and Twente plus other Asian, European, and North American Universities began to be a recognizable group, today over one hundred participants” (Ihde 2015, xv). It has also given rise to a set of textbooks and edited volumes (e.g. Rosenberger and Verbeek 2015) as well as to book series such as the ‘Postphenomenology and the Philosophy of Technology’ series with Rowmann & Littlefield.

The case of postphenomenology clearly indicates that the history of the study of experimentation is not to be conceptualized purely in terms of a dichotomy between philosophical and sociological approaches: there were different attempts to overcome such disciplinary dichotomies as well, and to bring together all kinds of approaches and disciplines. In a certain sense, one could even suggest that postphenomenology, at least in how it presented itself, attempted to uphold the diversity that, we have argued, characterized the study of experimentation in the 1980s. Our main claim here has been, however, that it has only been able to do so because it was able to turn itself into an institutionalized approach.

3.3. Philosophy of Science

From the late 1990s onwards, philosophy of science as well took over certain elements of the 1980s study of experimentation. One of them is the notion of ‘exploratory experimentation’. While the notion of exploration was already to be found in the work of e.g. Hacking (1983, 225) and Pickering (1984, 9), it was in particular Gooding (1990) who elaborated it. For Gooding, the notion of ‘exploration’ captured something very general about scientific practice, since it was part of any knowledge claim’s trajectory towards becoming established:

> With the transition of exploratory experiment into demonstrative ones comes a change in the status of techniques, phenomena and in the reality of effects, relationships and entities. Statements about these empirical facts become meaningful in a broader sense - they have been made part of a cultural repertoire of facts, arguments and theories. (1990, xiv)

Gooding then used this notion to challenge the idea that there was a principled way to distinguish discovery from justification (1990, 7-8) and to argue against any kind of epistemological individualism: the stabilization of experimental knowledge could only be understood by equally well taking into account negotiations between opponents (1990, 19-24). In a sense, one could thus say that Gooding’s account of exploration incorporated many aspects of the 1980s study of experimentation, and that he used it, in part, to challenge how mainstream philosophy of science was conducted, since he took it to entail an anti-representationalist view of knowledge as socio-cultural in nature.

When one looks at how later philosophers of science have conceptualized exploratory experimentation, one sees a significant difference: exploration is no longer conceptualized as a notion that captures something general about scientific practice, nor is it seen as inherently embodying socio-cultural aspects. Rather than offering a way to radically revise how we see the functioning of science, it has been limited to a very specific part of scientific practice: it now is used as a contrast class to experiments that aim at testing theoretical hypotheses (Franklin 2005, 888), which was an aspect of experimentation that, according to Steinle (who took over the term from Gooding), simply "deserves much more attention in philosophy of science than it has hitherto received" (1997, 73). In doing so, however, the notion was stripped of many of the characteristics that made it interesting for Gooding: rather than offering a radically different view of scientific practice in general, it has now become just one of many aspects of science that philosophers can investigate. This change becomes particularly clear when one sees how almost none of the contemporary philosophical studies of exploratory experimentation even refer to Gooding's work anymore (see e.g. Burian 1997, 2007, Elliott 2007, Franklin 2005, O'Malley 2007, Schickore 2016 and Waters 2007). Especially Schickore's (2016) work illustrates how philosophers of science have overtaken and transformed Gooding's notion. According to her, the notion of exploration primarily has philosophical significance insofar as it can be incorporated within the traditional discovery-justification distinction:

> [M]aking the case that exploratory experimentation is a theme for philosophy of science requires more than showing that some new theories were in fact generated through exploratory research. It requires showing either that exploratory experimentation has a *justificatory* function or that the scope of Popperian philosophy of science is too narrow and that philosophy of science has to include descriptions of actual practices of knowledge generation. (Schickore 2016, 21-22)

Philosophy of science, Schickore assumes, is concerned with conceptualizing how scientists arrive at justified theories, a practice she explicitly distinguishes from that of history and sociology of science, which are concerned with what she calls the description of actual practices of knowledge generation. This illustrates how the notion of exploration, which Gooding in a sense conceptualized to challenge such disciplinary boundaries between history, philosophy and sociology of science, is now seen as only significant insofar as it can offer something explicitly philosophical, and not merely historical or sociological. The concept's philosophical value, in other words, is nowadays evaluated by reference to the standards of traditional philosophy of science: can it produce justified knowledge?

This evolution is the result, it seems, of an incorporation of certain aspects of the 1980s study of experimentation within philosophy of science. This process went together with the formation of certain societies (e.g. Integrated History and Philosophy of Science, the Society for Philosophy of Science in Practice) that, in part, aimed at bridging the gap that could arise between more traditional philosophy of science and the 1980s study of experimentation if it were to go a more sociological direction.[7] It equally well went together with the establishment of certain conference

[7] The mission statement of the Society for Philosophy of Science in Practice, for example, explicitly situates itself between traditional philosophy of science, which is sometimes too theory-focused, and

series and workshops (&HPS, launched in 2007 at the Center for Philosophy of Science in Pittsburgh, and SPSP, which started in 2006 at the PSA) and found its way into already existing departments (e.g. Pittsburgh, Cambridge, Bloomington, Minnesota) and journals (e.g. *Studies in History and Philosophy of Science*). This again illustrates how certain aspects of the 1980s study of experimentation could survive because they were incorporated within existing or emerging institutionalized disciplines. This in contrast to other aspects, which, as we will see now, never really found such an institutional home.

3.4. Alternative Futures

We have argued that in the 1980s, the study of experimentation was characterized by a great amount of freedom to explore and to propose alternative ways in which philosophy of science could be practiced. Most of those early projects would never gain the traction that they have aimed for, and have disappeared. Despite the fact that he coined the term 'new experimentalism', no one has really followed Ackermann (1985) in conceptualizing science in terms of the dialectics of data domains, for example. Similarly, Radder's (1998) proposal to capture experimental natural science in terms of its 'material realization', or Robert Crease's (1993) plea to speak about experimentation in terms of 'performance', never really gained traction.

Strangely enough, one could argue that a similar faith has befallen even prominent philosophers of experimentation such as Hacking, Galison and Franklin. Though often referred to as the main examples of new experimentalism, books such as Franklin's *The Neglect of Experiment* (1986) and Galison's *How Experiments End* (1987) have not produced any specific concepts or debates that have been extensively picked up in the broader philosophy of science literature. At first sight, Hacking's *Representing and Intervening* (1983) seems the exception, for instance with regards to his 'entity realism'. But, again, one could argue that the reason that this point by Hacking was taken up, was that it could nicely be translated into a position within the already institutionalized realism debate. Hence, our claim that only those aspects of the 1980s study of experimentation survived that were successful in finding a place within existing or emerging institutionalized disciplines, fields and debates.

Acknowledging this allows us also to rediscover forgotten projects of new experimentalism that have not been taken up, because they failed on the level of institutionalization. Take the work of Gooding (1990). To grasp the activity of constructing the right representations, he introduces a new concept and verb, namely 'construing'. This activity consists in organizing concepts and instruments in such a way that certain experiences can be made expressible and shareable. The results of this activity are neither untouched phenomena nor abstract theories, and hence he introduces the term 'construal'. There is no intrinsic reason why it would not have been possible that a whole field of studies of construals would blossom, taking Gooding's book as a paradigm. This, however, did not happen. Gooding's books failed to conquer a spot at the institutional table.

A similar failed attempt to form a school is found in the work of Joseph Rouse, who from the 1990s on aimed to bring together the diversity that had characterized the 1980s under the banner

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social studies of science and technology, which run the risk of "wilfully disregarding the world except as a product of social construction" (accessible at https://philosophy-science-practice.org/about/mission-statement).

of a ‘cultural studies of science’ (Rouse 1996). He used the term broadly “to include various investigations of the practices through which scientific understanding is articulated and maintained in specific cultural contexts and translated and extended into new contexts” and he did so in order “to highlight some important issues that might reshape the terms of interdisciplinary science studies” (Rouse 1996, 238). While Rouse’s cultural studies incorporated many elements that were part of the 1980s study of experimentation – emphasis on the materiality of scientific practices, attention for the social and political aspects of science, etc. –, ‘cultural studies of science’ never became a genuine institutionalized field, even though Rouse is still fighting for this label (e.g. Rouse 2015).

4. Conclusion

This article has been concerned with the question what, if anything, was special about the philosophical study of experimentation in the 1980s. We have shown that, insofar as there was something shared between different students of experimentation at the time, it was mainly the wish to distance themselves from mainstream representationalist philosophy of science, and a shared emphasis on experimentation as a constructive activity. This notion of experimentation is to be understood as a minimally shared object: beyond the claim that experimentation is constructive, not much was shared. Rather, when they started elaborating this claim further, disagreements and discussions soon arose, both concerning specific topics within philosophy of science and concerning how philosophy of science in general was to be conducted. As such, insofar as there was something peculiar about the 1980s study of experimentation, it was that it was a very diverse and open-ended dialogue between students of experimentation with very different backgrounds and aims.

In the second part of the article, we have then argued that this situation changed from the late 1990s onwards. Instead of diversity we saw the emergence of disciplinary divisions, which over time transformed themselves into a certain neglect for other fields concerned with the study of experimentation. This was a consequence of how certain existing and newly emerging disciplines, fields and approaches succeeded in adopting and institutionalizing certain parts of the 1980s study of experimentation. We have emphasized the importance of this institutionalization by then pointing at very similar approaches to the philosophy of experimentation that have not been successful in ensuring continuity, because they were unable to institutionalize themselves in the same way.

Our aim with this has been to complicate the common narrative regarding the history of the philosophy of experimentation, which mainly presents the history in terms of a particular movement – new experimentalism – that put experimentation on the agenda. By arguing that there was not one movement, but rather a diversity of positions, backgrounds and aims, we have tried to show that this common narrative is not a neutral rendering of the history of philosophy of science, but already carries an implicit answer to what philosophy of science is concerned with, and how it is to be distinguished from other projects also concerned with science and experimentation. By constructing our alternative narrative, our aim is to provoke a debate on what the aim of philosophy of science should be. This aim is not naturally given, but the object of a set of debates and institutionalizations.

We have not speculated too much on what the supposed causes were for the changes in how experimentation was studied in the 1980s and 1990s. Part of the reason, as Simons and Vagelli (2021) have argued, was the fact that the discussions within philosophy of experimentation soon became part of the science wars. At the same time, other, related factors can also have played an important role. One could speculate about the influence of shifts within the social sciences, whereby sociologists and historians felt the need to 'colonize' domains and questions, previously linked to philosophy of science, or one could think of the increased specialization and fragmentation of philosophy of science, similarly provoking questions of identity. Equally well at play could be factors concerning the funding of specific studies of science, experimentation, etc.

Searching for such possible causes brings about a second, related question, namely whether the process we have illustrated here – in which philosophers of science raise questions about, and disagree on, how their own practice is to be conducted and what it should aim for, and how these questions are settled over time through a process of institutionalization – is really that specific to the 1980s study of experimentation. Similar examples from the recent history of science, it seems, can easily be found. One is historical epistemology, first of all in its international sense, associated with Jürgen Renn, Lorraine Daston and Hans-Jörg Rheinberger. Here as well, its moderate success has been that it had a clear institutional backing, namely the Max Planck Institute for the History of Science in Berlin, which it has used to organize a set of conferences and to create a number of research groups related to historical epistemology from the 1990s on. This has also casued some groups in Paris to center around the label, such as the Research Network on the History and the Methods of Historical Epistemology (see Braunstein, Diez and Vagelli 2019). This movement has, moreover, also attempted to recuperate the work of Hacking as part of its movement, linking it back to earlier French figures such as Gaston Bachelard.

A similar story is being told about the institutionalization of philosophy of science in the United States. Several scholars have pointed out how, due to the Second World War, several members of the Wiener Kreis emigrated to the United States. Though they brought logical positivism with them and helped institutionalize philosophy of science, what the aim and scope of that novel discipline was supposed to be was a constant object of debate in the 1950s and 1960s (see Reisch 2005, Dewulf 2020, 2022). The same could be said about the history of Kuhn's *Structure of Scientific Revolutions* (1962) and the subsequent ebates with Karl Popper, Imre Lakatos and Paul Feyerabend, or the history of feminist philosophy of science, which is characterized by a radicality not unlike that of some 1980s students of experimentation. Moreover, feminist science studies has seen a similar development, where parts of it have been institutionalized either in analytical philosophy of science (e.g. the work of Helen Longino) or in fields such as STS (e.g. the work of Sandra Harding, Donna Haraway, or Karen Barad).

In all these examples, the question of what the aim of philosophy of science is supposed to be was central. The history of philosophy of science is full of such critical moments where its identity was challenged, and either was restored or diverted to new places. The HOPOS society and journal can play a prominent role in unearthing these forgotten debates and revamping them, since, as our examples indicate, there is no reason to expect that the future of the history of philosophy of science would be any different.

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