Conceptual Analysis in the Philosophy of Science[1]

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Conceptual analysis as a method of inquiry has long enjoyed popularity in analytic philosophy, including the philosophy of science. In this article I offer a perspective on the ways in which the method of conceptual analysis has been used, and distinguish two broad kinds, namely philosophical and empirical conceptual analysis. In so doing I outline a historical trend in which non-naturalized approaches to conceptual analysis are being replaced by a variety of naturalized approaches. I outline the basic characteristics of these approaches with illustrative examples, arguing that recent developments in the philosophy of science show that in order to achieve a more adequate understanding of scientific endeavour we need to prioritize the naturalized accounts of the method.

Keywords: conceptual analysis; empirical philosophy of science; naturalism

1. Introduction

The philosophy of science established itself as an academic discipline in the first half of the twentieth century. At its inception the way in which the philosophy of science was practiced was highly influenced by the developments of that time. In particular, the logical analysis of

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the language of science was thought to be the key to understanding the structure of scientific theories, the bearing of evidence on a proposed hypothesis, the role of experiment, and the explanations provided by scientific theories. As in other fields of analytic philosophy, philosophers of science have also relied on conceptual analysis while analysing issues such as the above. In this article I offer a perspective on the ways in which the method of conceptual analysis has been used, and distinguish two broad kinds: philosophical and empirical conceptual analysis. In so doing I outline a historical trend in which non-naturalized approaches to conceptual analysis are being replaced by a variety of naturalized approaches.

I will characterize philosophical conceptual analysis by way of drawing attention to three key features: (i) analysing meta-scientific concepts independently of science; (ii) the unjustified tendency to use methods of formalization, and (iii) the requirement to analyse concepts in terms of necessary and sufficient conditions. Philosophical conceptual analysis is best seen in the schools of logical positivism and empiricism, which ruled for decades until they began losing influence under the weight of emerging internal issues prone to increasingly severe criticism. Despite the introduction of numerous attempted modifications intended to resolve the problems (see, e.g., Suppe, 1974 for an exhaustive overview), the project was ultimately abandoned. However, we may find traces of the logico-positivistic approach to conceptual analysis in schools that were formed as an antidote to the positivistic picture of science, as well as in some contemporary schools and debates. That said, the trend seems to be such that newly emergent approaches to the study of science are gaining in popularity. I categorize these other approaches, jointly referred to as 'empirical conceptual analysis', into four kinds: the case-study approach, applied approaches, qualitative approaches, and quantitative approaches.

As stated, the purpose of this paper is to characterize the method of conceptual analysis used in the philosophy of science. Ultimately, I will argue that we should prioritize the empirical ways of using this method if the goal is to achieve a better grasp of scientific endeavour. I will substantiate these claims by showing the strengths and weaknesses of these methods– both in comparison with one another and with traditional approaches. I will also illustrate some of the gains that have been made using empirical conceptual analysis, which might otherwise have been impossible to achieve through the application of traditional methods..

The structure of this article is as follows. Section 2 introduces philosophical and empirical conceptual analysis as two general accounts (i.e. non-naturalized and naturalized), followed by the provision of more detailed characteristics and illustrative examples (Sections 3 and 5). Section 4 serves as a link between the two extremes, illustrating the existence of combined approaches, thus supporting my view that it is not necessarily a question of either/or, but rather that middle-ground positions may and in fact do exist. Despite this fact, in Section 6, I present evidence that suggests we would do best to cut down on the non-naturalized aspects of the method of conceptual analysis, and to commit more fully to naturalized versions instead.

A final introductory remark concerns the way in which I shall retain a tight, specific focus. There is an ever-growing literature on the value of conceptual analysis in the context of the emergence of 'experimental philosophy' (see Knobe and Shaun, 2017 for an overview). I have little to add to it and will restrain myself from further comment. I believe this is justified, since my aim here is different from that which those engaged in experimental philosophy have in mind. Usually, what is at stake there is the use of an intuition-driven justification of philosophical thought experiments.[2] Furthermore, it is often couched in terms of the *a priori* and *a posteriori* distinction, with the experimental philosopher objecting to *a priori* methods

[2] This is in no way exhaustive of the concerns of experimental philosophy, see, e.g., Sytsma and Buckwalter (2016).

of justification. Although there are some implicit similarities with some of the issues discussed here, my concerns are somewhat different and do not necessarily address the justificatory use of intuitions, nor the context of thought experiments.

Because of space constraints I also cannot discuss the Canberra plan[3] in any detail. The only remark I wish to make is that the Canberra plan is, in my view, appropriately placed in the non-naturalized camp because of its top-down nature and because it exhibits all three features of philosophical conceptual analysis (see below).

2. Two approaches to conceptual analysis in the philosophy of science

There is little disagreement on the point that philosophers employ conceptual analysis as a method of inquiry; however, it remains controversial what this method actually comes down to. Here I attempt to distinguish two broad accounts of the method, which I call *philosophical conceptual analysis* and *empirical conceptual analysis* respectively. I associate the former with a non-naturalized approach, whereas the latter is introduced as naturalized. In Section 3, I go into greater depth on this topic to further illuminate and distinguish these categories. Before going into more detail, let me first make a couple of general remarks on what I take conceptual analysis to be. Conceptual analysis is understood as the analysis of concepts in terms of other concepts, and any reflection on concepts can be construed as conceptual analysis.[4] However, the exact methods by which this analysis of concepts proceeds are manifold.

2.1 Philosophical conceptual analysis as a non-naturalized approach

Traditionally–exceptions notwithstanding–philosophical inquiry has aimed at analysing

[3] See Papineau (2015, Section 2.3) for a brief overview and a source of references.

[4] Thus, not only philosophy but also science heavily relies on such analysis. Generally, any introduction to a scientific discipline is focused on conceptual understanding and is often high-yield at the start.

concepts in the form of addressing the question, 'What is X?'. In the context of the philosophy of science, by 'X' one usually means a meta-scientific concept such as *science, scientific explanation, law of nature, theory, model, experiment, values in science, scientific reasoning, reductionism,* etc. The goal of such an inquiry is to specify conditions under which a given concept applies to a given case. This means that in order for something to count as a genuine case of, for instance, scientific explanation, it has to satisfy the specified criteria: otherwise it is not a case of scientific explanation. For example, according to the deductive-nomological account of scientific explanation, an explanation must meet several criteria: the explanandum must be a logical consequence (in the strict logical sense of the term) of the explanans, the explanans must contain general laws which must be required for the derivation of the explanandum, and the sentences constituting the explanans and explanandum must be true (Hempel and Oppenheim, 1948; see also, e.g., Godfrey-Smith, 2003, ch. 13 for discussion). This implicitly presupposes a certain methodological approach to studying concepts–one that, as I will argue, proves to be problematic.

Such an approach takes a concept as a starting point, usually with considerable philosophical baggage, and proceeds to define this concept in terms of various (potentially specious) normative criteria. A long-favoured assumption (with little justification) was to think of scientific explanations in terms of deriving conclusions (statements about observations or empirical laws) from more fundamental laws.[5] Thus, in analysing concepts, the starting point is often located within an inherited theoretical background against which analysis takes place, and as a result, a meta-scientific notion is investigated through a

[5] The idea that the laws of nature play an indisputable role in explanation was questioned early on (Scriven, 1962), though for the most part in the context of ordinary explanations. It took some time before philosophers became aware of various explanatory practices within science which do not rely on the laws of nature in any substantial sense.

philosophical prism. Such an approach can be construed as a top-down approach: the philosophical prism sits at the top while a philosopher peeks down through it at the science.[6] One danger lurking in the shadows as a consequence of this approach is that science may end up distorted in ways that fit preconceived philosophical categories. I call this approach *philosophical conceptual analysis* to reflect the metaphor of the philosophical prism.

2.2 Empirical conceptual analysis as a naturalized approach

The other broad approach may also target the question, 'What is X?' The difference is thus not necessarily one of subject matter, but of the way in which the question is addressed. Rather than first proposing criteria for the applicability of a concept (with an intent of locating anything that seems to satisfy these criteria within contemporary scientific practice), *empirical conceptual analysis* takes as its starting point the concept as it already is employed within science. Hence, the nature of the approach is bottom-up: a philosopher studies what scientists take to be paradigmatic examples of explanations and proceeds to offer careful generalizations based on these, thus forming (philosophical) accounts of scientific explanation. The notion of the 'empirical' in 'empirical conceptual analysis' serves to suggest that the starting point is empirical, rather than philosophical in nature. The main point here is that conceptual analysis cannot be detached from studying the actual uses of concepts (and the ways in which we learn concepts). One may worry that, in this way, philosophical inquiry is reduced to a merely descriptive endeavour, whereby philosophy must sacrifice any claim to the normative dimension of a theory of science. It is beyond the scope of this paper to sufficiently address this objection; however, a few remarks are in order.

First of all, the idea of reducing the philosophy of science to a merely descriptive project is not necessarily a self-undermining enterprise, since the philosophy of science would

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[6] A common worry raised by numerous authors is that such an approach is an exercise in the rational reconstruction of science, which leads to spurious results.

still have its place among disciplines that take science as their subject matter. This is because, among other things, scientific disciplines are in the business of doing science rather than providing elaborate 'meta-scientific' theories. Furthermore, disciplines such as the sociology of science or the psychology of science focus on different aspects of scientific practice, i.e. the social dimension of science, including the institutional foundations of the research, funding, and publication system, and the cognitive dimension of engaging in scientific inquiry, respectively.[7] Second, it is not the case that the philosophy of science would thereby give up on any normative claims. Rather, the relationship would be much more complex. I will further discuss empirical conceptual analysis in Section 5, describing in more detail various empirical methods and examples.

2.3 Two caveats

Before I proceed any further, a couple of caveats are in order. The two broad approaches to conceptual analysis specified above are not incompatible, strictly speaking. One may rely heavily upon one or the other, but one may also employ a combination of the two (see Section 4). This means that the two approaches ought to be construed as approaches that allow for various degrees of use. For example, a particular analysis may be predominantly based on the first approach while also somewhat drawing on the second.

From what I have said thus far one may be under the impression that both the top-down and bottom-up approaches are straightforward. This is not my claim, and it is important to further elaborate this point. Just as there are degrees to which one may rely on either of these approaches, there are varying degrees of strictness with which an inquiry follows either the top-down or bottom-up approach. For instance, it would be rather naïve to construe the

[7] This is not to suggest that there is no possibility of fruitful collaboration between the disciplines. Indeed, it very well may be that the contrary is true. The point is only that, on average, philosophers tend to focus on different aspects from sociologists or psychologists.

bottom-up approach as a simple matter of extrapolation, i.e. broadly generalizing from a small set of cases. Yet, piecemeal, step-by-step generalizations stemming from the *detailed* analysis of several cases seem promising, as long as one is careful enough to draw constrained and limited inferences. There has been awareness of this issue for some time (e.g., Shapere, 1987), but to what extent it is feasible, and which modifications in the process of generalizing are required, remain matters of controversy, indeed the focus of an ongoing debate (see, e.g., Chang, 2012; Wagenknecht, Nersessian, and Andersen, 2015b). Thus, I implore the reader to approach the notions of top-down and bottom-up as useful shortcuts for the purposes of this discussion–rather than as (1) matters of simple deduction from a general theory to a particular case, or (2) induction from a small set of examples to a general theory, respectively.

3. Characterizing philosophical conceptual analysis

Three main features of philosophical conceptual analysis shall be detailed and illustrated through examples: the analysis of meta-scientific concepts independently of scientific practice; heavy investment in formalizing procedures without any clear justification for so doing; and the requirement to provide necessary and sufficient conditions for defining concepts.

3.1 The analysis of meta-scientific concepts independently of science

Meta-scientific concepts usually designate high-level concepts which scientists implicitly employ. Examples of such concepts are easy to find, and include the notion of science and its demarcation from non-science and pseudo-science, the notion of scientific explanation, the notion of the laws of nature, and so on. Scientists intuitively use these notions, usually without explicitly providing a precise characterization or explanation. One of the goals of philosophical inquiry is to render explicit what is implicit: this includes elaborating on the nature of the aforementioned concepts. The question now is how to proceed.

An especially popular approach in certain circles is to think of these concepts as something that can be investigated relatively independently of actual scientific practices. This stems from the worry that were we to take into account the actual practices of individual scientists we would end up with a subjective view of science. We are told that a proper philosophical analysis provides an epistemological grounding for meta-scientific concepts, and such an analysis needs to be free from subjective and historically contingent aspects. After all, epistemology, as opposed to psychology, is about objective relations, or so the argument goes (Schickore, 2018).[8] The practice of proceeding in this way has often relied on describing everyday examples, accompanied by only a handful of introductory science textbook examples. The limited array of examples and their inability to represent larger, more complex explanatory strategies should give pause to the extent to which one may apply the results. That said, the outputs of such analysis have often been presented in an overly generalized manner, suggesting that this is how science in general works. For instance, this is how the work of Hempel and Oppenheim (1948) on scientific explanation has been criticized by some (Scriven, 1962).

The view that an 'understanding of what science is (or does) must be independent of the content of any specific scientific beliefs' (Shapere, 1987, p. 7) turns out to be problematic, and has been criticized by Shapere himself, among others. The main issue concerns the likelihood of distorting the picture of what science is, what it does, and how it does do whatever it does. It is not clear whether we can sensibly characterize what is, for instance, a scientific explanation if we do not investigate various advanced instances of scientists giving

[8] This is an echo of the famous distinction between the context of justification and the context of discovery (Reichenbach, 1938). According to this distinction, philosophical analysis, when properly conducted, belongs to the justificatory realm, whereas actual practices in which particular subjects engage are deemed psychological, and thus not a target for philosophical inquiry.

explanations. Rather, what is more likely is that instead of developing an account of scientific explanation, we start with what we take to be an explanation, based on a source other than scientific practice (e.g., ordinary everyday examples, superficial introductory textbook examples, etc.). However, there is no guarantee that this preconceived account of explanation has something interesting to tell us about scientific explanations. It might turn out that we have fallen prey to using a philosophical construct that purports to offer ‘the account of scientific explanation’. However, such a designation is arbitrary because, in fact, it is an account of scientific explanation only in name, not substance.

As suggested, the independence of the analysis may not be absolute. Indeed, as illustrated above by Hempel and Oppenheim, analysis often progresses through a process of browsing scientific examples. One may thus wonder whether this is an instance of a bottom-up, rather than top-down approach. There are a couple of things to note. First, recall from the previous section that these approaches are to be construed as a matter of degree; hence, it may be possible to view such attempts as a bottom-up approach to some small degree (because the depth of these examples is usually rather shallow). Second, notwithstanding the first point, it still fits better with the top-down picture (to some degree) given the skewed picture such an approach often provides. Browsing through selected examples in a rapid fashion runs the risk of falling prey to the (philosophical) prism by which one has been previously influenced.[9]

3.2 Tendencies in formalizations

Valued by many, formalization has been construed as a tool that is put to work to help with clarifying various concepts. Formalization has received considerable attention in the context of the development of modern logic. Turning sentences of natural language into logical formulae has successfully eliminated many of the ambiguities in ordinary language, resulting

[9] This risk is perhaps ever present, included in any strong bottom-up leanings; again, the difference lies in the degree to which one is exposed to such a risk.

in end-products that exhibit clear structure, and thereby provide greater insight. Similarly, many of the scientific disciplines that have proven extremely successful are steeped in mathematical formalisms of various kinds. The success of formalization has had a significant impact within science and logic (philosophy) , leading some to think that, perhaps, formalization may be the backbone of critical thought. Although formalization is a tool which has the undeniable capacity to clarify certain areas of inquiry, it is not self-sufficient, and as always, one has to carefully evaluate the merits and limits of using this method.

Whenever various means of formalization have been introduced into the social sciences, many have voiced their dissent.[10] Perhaps the most pressing issue concerns the question of whether by mimicking some of the features of well-established scientific disciplines we maintain reasonable epistemic standards for good practice–e.g. we avoid importing form that is devoid of meaningful content. Indeed, with the expansion of formalization we may run the risk of engaging in cargo cult science rather than actual science. A striking example illustrating some such issues is the ‘second law of criminal behaviour’, discussed in some length by Haack (2003). Originally proposed by David Abrahamsen, the law states that a criminal act (C) is equal to the sum of personal criminal tendencies (T) plus one’s total situation (S), divided by the amount of one’s resistance (R). Thus, we end up with a neat equation in the form of $C = (T+S) / R$. Such an equation seemingly provides a way to quantify an aspect of human behaviour. However, it is far from clear how to make sense of

[10] Numerous reasons have been given in support of the claim that formalization (mathematization) may introduce unforeseen problems. It is outside the scope of this paper to enter into these debates, or even to provide an overview. However, in no way do I want to leave the impression that formalizing the social sciences is a fool’s errand. Quite the contrary, I am committed to the view that various approaches to formalization have proven extremely fruitful within the social sciences, and do not believe this claim is undermined by particular attempts that have been misguided.

the equation, since the quantified notions lack both the necessary rigour (e.g. the notions of ‘personal criminal tendencies’ and ‘one’s total situation,’ etc., remain vague) and a method for measuring them. Arguably, the social sciences have spent considerable time discussing such methodological pitfalls.

Coming back to the philosophy of science, even a brief overview of the literature provides examples of similar practices dangerously close to cargo cult science. A case in point is Larry Laudan’s notion of the ‘problem-solving effectiveness of a theory’, which, according to him, ‘is determined by assessing the number and importance of the empirical problems which the theory solves and deducting therefrom the number and importance of the anomalies and conceptual problems which the theory generates’ (Laudan, 1978, p. 68). Since no precise way of counting the ‘empirical problems’, ‘anomalies’, and ‘conceptual problems’, nor any way of establishing a measure of the ‘importance’ of these problems have been provided, Laudan may be charged with engaging in contentious attempts to provide rigour where none is to be found. Although this is different from employing straightforward formalism, it nonetheless shares the rudiments.

Yet another related problem is found in a recent paper by Fiora Salis. In a debate over metaphysics and the semantics of fictions in the context of scientific models she argues that,

> when we say that Zaphod is more narcissistic than Morris we say (or imply) that there are certain degrees of narcissism i and j such that i > j, Zaphod has narcissism to degree i, and Morris has narcissism to degree j

and she continues by claiming that

> this is problematic because Zaphod and Morris do not exist [Zaphod and Morris are fictional characters], so they cannot really have degrees of narcissism i and j. In other words, Zaphod and Morris do not instantiate the relevant degrees of properties (Salis 2016, p. 248).

This problem, Salis suggests, is solved by an appeal to mathematical entities in a way that the above statement now takes the following form: “There are some degrees of narcissism, i, j, such that i > j, according to *The Hitchhiker's Guide to the Galaxy* Zaphod has narcissism to degree i, and according to *Changing Places* Morris has narcissism to degree j" (Salis, 2016, p. 249). Here the problem with formalization somewhat differs from the two cases discussed above, since in this case we already understand what the relevant concepts are (i.e. narcissism), and, perhaps, we also have an intuitive understanding of how to 'measure' these (this last point is contentious, but let us grant it here for the sake of argument). Arguably, such formalization does not advance our knowledge because it provides no new information beyond the ideas it recapitulates. To be fair, Salis' debate is about metaphysics, and so I should say more about how it relates to the deeper issue I have chosen to examine. In Salis' argument, and in similar discussions, I contend that formalization plays a misleading role and distracts us from discussing an important issue related to the human cognitive power of imagination. Rather than encouraging reflection, such formalizationt presents us with a problem in metaphysics that has spurious means of resolution.[11] Thus, formalizationserves merely as a deceptive intermediary, diverting our attention from a respectable problem to one which is questionable.

Finally, Hempel and Oppenheim (1948) went to great pains to provide a formalization of what they perceived as the logical structure of scientific explanation. Couched in the language of first order logic, they believed their account could provide much needed rigour. In doing so, they relied on the strict logical notion of derivation. However, as noted by Teller (2001), among others, the way in which derivation in science (even in highly mathematized fields, such as physics) often proceeds is a far cry from the notion of derivation familiar to

[11] Note that appeals to mathematical realism are considered by many to be highly contentious (Field, 1989).

logicians.. It is hard to see how an account of scientific explanation could be based on such a strict logical notion of derivation if actual explanatory practices are not.[12]

I hasten to add that my quarrel here is not with the methods of formalization. Again, it is an undeniable fact that various formal methods play a key role in many of the sciences. Nor do I wish to claim that certain phenomena cannot, by their nature, ever be approached from the perspective of some formal method, including those addressed in the philosophy of science. Rather, the claim here concerns only the way in which some of these formal methods have been put to work. I argue that in many instances the attempts of philosophers are questionable, insofar as they provide little justification for choosing and using that particular method.[13] Indeed, it may very well be that using formalization to account for certain aspects of science is an inadequate way to conduct an inquiry, and promoting formalization in these instances would be ill-advised. In other words, we must first consider whether employing a formal method may help us to learn something useful, and then make sure the method is accompanied by content, so that we avoid falling into the trap of cargo cult research.

3.3 The search for necessary and sufficient conditions

The third feature of the philosophical conceptual analysis discussed here is the requirement to

[12] There are many more examples of spurious formalization tendencies. For instance, an attempt by Lewis (2001) to construe the pessimistic induction argument in the debate on scientific realism in terms of a base rate of true and false theories has been criticized by Saatsi (2005). More specifically, Lewis' view that the characterization of the reliability of a test in the context of this debate should be construed in terms of the rates of false positives and false negatives has been the target of criticism.

[13] For instance, think of the 'raven paradox' discussed in the literature on confirmation theory. It proved misguided to analyse confirmation by using the apparatus of predicate logic. That does not mean, however, that all formal approaches are doomed to fail, as, for instance, some Bayesians have argued. Indeed, Bayesian statistics is a tool that is used in confirmatory practices in some domains, including medicine.

provide necessary and sufficient conditions for defining concepts. Thus, to understand a concept according to philosophical conceptual analysis is to possess a set of necessary conditions that are jointly sufficient for applying a given concept. Meta-scientific concepts should then be defined in terms of those conditions that specify whether the concept genuinely applies to a purported instance of that concept. For instance, according to the deductive-nomological (DN) account of scientific explanation, a scientific explanation consists of deriving whatever is to be explained from the premises, i.e. from a set of laws of nature and initial conditions. Furthermore, the premises must be true, so that the derivation is not only valid, but also sound. This set of premises plus the requirement of truth provides the defining feature of what it means to have a scientific explanation of a given phenomenon.

The driving force behind this idea is that having a set of necessary and sufficient conditions will provide a demarcation of concepts, so that we may achieve precision and clarity in applying them. This is echoed throughout the history of the philosophy of science and is favoured by many to this day. Consider the debate on scientific representation. Gabriele Contessa claims that "according to the interpretational conception, a vehicle is an epistemic representation of a certain target (for a certain user) if and only if the user adopts an interpretation of the vehicle in terms of the target" (Contessa ,2007, p. 57). Analogously, Adam Toon says that

> theories of depiction aim to state conditions that are necessary and sufficient for something to be a depiction. Similarly, if possible, we want to provide a set of conditions that are both individually necessary and jointly sufficient to establish an instance of each form of model-representation that we identify (Toon ,2012, p. 23).

The sentiment is shared by Roman Frigg and James Nguyen:

> the problem is: what turns equations and structures, or fictional scenarios and physical objects into representations of something beyond themselves? It has become customary to phrase this problem in terms of necessary and sufficient

> conditions and throughout this review we shall follow suit (Frigg and Nguyen, 2017, p. 51).

Because I return to this issue in Section 5.2, I shall not comment any further here.

3.4 Summary

Let us now summarize what has been said thus far. In thinking about various aspects of science, philosophers employ the method of conceptual analysis. I first provided a rough distinction between two ways of construing this method: philosophical conceptual analysis and empirical philosophical analysis. The former can be characterized by three key features: (1) the analysis of meta-scientific concepts being pursued independently of actual science, (2) it often relies on unjustified formalization and (3) it seeks to define concepts in terms of a set of necessary and sufficient conditions, i.e. in an essentialist manner. For these reasons, philosophical conceptual analysis may be construed as a non-naturalized methodology, i.e. one that is not properly informed by science. Before providing the characteristic features of empirical conceptual analysis, I shall consider approaches that stand somewhere in between, which draw on both the philosophical and empirical aspects.

4. Combining philosophical and empirical conceptual analysis: semi-naturalized approaches

Earlier I noted that philosophical and empirical conceptual analysis may both be practiced to various degrees. That would suggest the possibility of identifying approaches that lie somewhere between the two extremes. Since I use the terms ‘naturalized’ and ‘non-naturalized’ to refer to positions on the opposite ends of a spectrum, it is appropriate to call those that are somewhere in-between ‘semi-naturalized’. I shall now briefly discuss two such semi-naturalized approaches: the historical school of the philosophy of science, as exemplified by the works of Thomas Kuhn, and the social constructivist movement of Bruno Latour and Steve Woolgar.

Thomas Kuhn's *The Structure of Scientific Revolutions*, originally published in 1962, is now considered a classic. Kuhn's success lay in the novelty of his approach and in his engaging prose. He had both enthusiastic supporters and sworn critics. In the second edition of *The Structure*, Kuhn (1970) attempted to address some of the most pressing issues surrounding his notion of incommensurability, which had proven troublesome and hard to swallow for the many who accused Kuhn of e relativizing science to the extent that science would no longer enjoy the status of a privileged way of knowing. According to these detractors, Kuhn had failed; he subsequently spent the bulk of his career trying to develop his ideas into a non-relativistic philosophy of science.

Although Kuhn was not the first to realize that psychological and sociological factors play an important role in everyday scientific practice, he arguably had the most profound and catalyzing impact on the ensuing development of this idea. Indeed, a significant portion of the philosophy and sociology of science is informed by Kuhn. While Kuhn's recognition of the psychosocial aspects of scientific practice is consistent with models that are taken for granted in the more empirically-minded circles of the philosophy of science, it was widely rejected during the heyday of logical positivism.

However, as noted above, Kuhn has additionally been criticized by a number of commentators (see, e.g., Shapere, 1964) for clinging to certain aspects of logical positivism. One of the most controversial notions Kuhn introduces is the thesis of incommensurability, which states roughly that scientists working within different paradigms (both historical and contemporary) cannot understand each other; they employ different vocabularies, and even when they use the same word, the word has a different meaning for each of them. Kuhn's incommensurability thesis relies on the descriptivist[14] theory of reference a philosophical

[14] According to this account, the reference of a term is fixed by its descriptions. If the descriptions change then the referent changes as well. To pick one of Kuhn's examples, consider the concept of mass. In Newtonian physics, mass is an intrinsic property of objects and as such stays invariant. However, in the Special

account of how terms allegedly refer. However, he entirely fails to justify his choice of preferred philosophical theory. As noted by Shapere (1964), a bizarre consequence of the incommensurability thesis (based on the descriptivist theory of reference) is that physicists belonging to different 'paradigms' could not possibly understand each other, something that is clearly not the case. Not only can physicists understand each other with respect to current competing theories, they can also understand their colleagues and rivals across time, i.e. they understand, or make sense of, most of the physics that was developed long before their time.[15]

Thus, it seems that Kuhn fell prey to the philosophical prejudice that biased his construction of a theory of science. There is no a priori reason why the philosophical theory of reference should be privileged over observation when considering the question of whether scientists from different paradigms can understand each other.[16] , Kuhn is consequently guilty of engaging in philosophical conceptual analysis, insofar as he analyzes meta-scientific concepts independently of empirical scientific methods. I should quickly add that this does not define the whole of his work, but only certain aspects, as his notion of incommensurability arguably illustrates. Indeed, Kuhn's work represents a mixture of approaches: on the one hand, he clearly demonstrates his leaning toward a more empirical construction of a philosophical theory by providing historical case studies and by acknowledging the role of

Theory of Relativity, mass actually acquires another meaning, one that is dependent on the relative velocity of the object (STR effectively distinguishing between 'rest mass' and 'relativistic mass'). According to this view, when scientists from the two paradigms speak about mass they mean completely different things.

[15] Of course, no one means to suggest that, say, a theoretical physicist can understand every detail of what an experimental physicist does, or vice versa. Nor can a theoretical physicist in one particular field understand every detail of another theoretical physicist's work from a different field. However, the reason here is different from what Kuhn suggests, i.e. it is a lack of familiarity with domain-specific details that inhibits understanding, rather than an inability to communicate ideas due to the lack of a shared (scientific) discourse.

[16] Note that what is at dispute is understanding each other rather than agreeing with each other.

psychological and sociological factors in actual scientific practice; on the other hand, his theory is ensconced in a prior philosophical framework that distorts certain of its aspects.

The second example concerns the social constructivist school of the philosophy of science, the strong programme in the sociology of knowledge (Latour and Woolgar, 1979). Latour's and Woolgar's *Laboratory Life* shows great promise in introducing empirical methods into the philosophy of science – in their case, the ethnographic method.[17] I will not delve into the prospects of the ethnographic method here, but will note that it may offer helpful insight into the daily business of scientific research, among other things, by examining what scientists do and how they interact in lab meetings and in other contexts. Despite the potential merits of the ethnographic method, it is necessary to acknowledge the philosophical prejudice that skewed Latour and Woolgar's observations.

Latour and Woolgar observed the scientific process through the philosophical prism of relativism, a theoretical lens that was neither derived from, nor justified by their observations (which would represent an empirically-minded bottom-up approach). Latour's and Woolgar's work represents an instance of forcing science into a pre-established philosophical framework–in this particular case, the framework of relativism (a top-down approach). Intuitively, such a relativistic stance seems foreign to the basic principles of science. While an intuitive appeal is far from a decisive argument, it is enough to raise doubts about the strong conclusions of relativism. These doubts serve to indicate that Latour and Woolgar must provide a stronger, evidence-based argument in order to demonstrate convincingly that scientific practice is, indeed, steeped in relativism. One may object that the truth of relativism can be established on merits other than what the scientific community thinks, which is surely true. However, in this case, one would arrive at relativism wholly independently of science,

[17] See Section 5.3 for more on the results the ethnographic method has brought to contemporary research.

and it is unclear what source of knowledge could demonstrate the truth of relativism, p especially given the radical nature of this claim. Thus, the position of relativism seems to be nothing more than a philosophical construct.

5. Characterizing empirical conceptual analysis

Historically speaking, there appears to be an ever-growing movement among scholars that is characterized by a clear tendency to profess an inclination to a naturalistic approach toward the philosophy of science–in other words, an approach that is grounded in empirical knowledge. The precise nature of this approach, and the kind of empirical grounding that is required, however, remain contentious issues that philosophers have slowly started to address in greater detail. Below, I offer a brief characterization of several different approaches that seem to be gaining in popularity, with a special focus on what these methods of inquiry uncover.

In Section 2.2, I discussed empirical conceptual analysis in terms of a bottom-up approach. The starting point of such analysis is a concept as it is used in the sciences. The hope is that, by analysing the context in which this and similar cases emerge, we may find ourselves in a position to say something more substantive about that particular notion, and perhaps clarify it by so doing. I contend that there are four main branches of empirical conceptual analysis, each based on a different method. These include the case-study approach (Section 5.1), the applied approach (Section 5.2), and the qualitative (Section 5.3) and quantitative approaches (Section 5.4).[18]

The reason for ordering the discussion in this way is purely historical, i.e. philosophers first paid attention to scientific practices as exhibited in experimental research or as discussed

[18] In scientific disciplines the case study approach is usually conceived as an instance of a qualitative method (Berg, 2001, ch. 10). In the philosophy of science, however, I believe it makes sense to keep them separate for historical reasons. In any case, nothing of substance hangs on this point.

in scientific papers, and only later have they espoused methods of a qualitative and quantitative nature.[19]

In the following sections I will provide an overview of several examples to further illustrate all four kinds of methods mentioned above and to show the points of departure from the traditional analyses as discussed in Section 3.

5.1 The case-study approach

The case-study approach aims to analyse particular examples of scientific practice, or particular historical periods, from the perspective of the history and philosophy of science; as such, it differs from an approach of merely using examples to illustrate one's point. While examples are often superficial and do not take up more than a paragraph or two, case studies consist of much deeper analysis and offer much deeper insight. Based on this distinction, it should be clear that I have illustrated my claims throughout this paper by enumerating examples, rather than examining case studies. Arguably, in his most celebrated work, Kuhn has also relied on examples, rather than case studies. Now, it is important to note that the distinction I am drawing is not determined solely on the basis of length, nor on an individual's acquaintance with the subject matter being discussed. The crux of this distinction lies instead in the following insight:, the case-study approach, notably, is consistent with the bottom-up approach discussed above (rather than the top-down approach). Indeed, according to the case-study approach, it should (for the most part) be particular cases that drive philosophical theorizing, and not the other way around. These cases are not limited to the subject matter of published scientific papers; the empirical realm of investigation includes scientific practices and findings derived from various sources.

[19] This is not to say that such a development is straightforward. Indeed, as we have seen in Section 4, some used qualitative methods early on. However, such few exceptions do not pose a threat to what is otherwise a strong trend.

The case-study approach is embodied by the ‘new experimentalism’ movement that started in the 1980s, notably in the works of Ian Hacking, Allan Franklin, Peter Galison, Robert Ackermann, and Deborah Mayo, among others.[20] Philosophers of this strand revolted against what they perceived as a theory-centric view of science that had become popular during that time. According to the popular theory-centric view, scientific research is principally a matter of formulating general theories in terms of sets of propositions, and experiment functions only to test the validity of a theory. The new experimentalists parted ways with the received view once they started investigating actual experimental practices, with the intent of providing accounts of what was taking place in working laboratories. Soon enough, these philosophers re-conceptualized the notion of experiment in terms of real-world experimental practices and the roles experiment plays in scientific practice generally. They started reporting on the equipment used, maintained, and calibrated in various sorts of labs; on the data-gathering processes, data-analysis processes, and the practices of intervening in a system; on cross-checking results, controlling for error, and the elimination of noise. Thus, thanks to the ‘new experimentalism,’ experiment has been re-conceived as something that “has a life of its own” (Hacking, 1983) and is often independent of a high-level theory. Indeed, experiment often functions differently from how tradition views it. For instance, experiment has an important exploratory function, something akin to plain old curiosity that may open up a completely new field of research. By removing the noise, experimental manipulation is also key to creating new phenomena and, as such, is most visible within the sphere of modern technology. Without paying attention to actual experimental practices, it would have been difficult for the new experimentalists to show the extent to which the received view provided only a very limited account of the nature of experiment.

[20] See especially Hacking (1983).

Beside the new experimentalism movement, the ‘new mechanism’ movement has gained in prominence since the early 2000s.[21] The roots of the new mechanism movement stretch as far back as the early 1990s, most notably to the publication of Bechtel and Richardson’s *Discovering Complexity*, originally published in 1993 (Bechtel and Richardson, 2010). It arose in the context of the study of cellular and molecular biology and soon spread to neuroscience, cognitive science, and the social sciences. In introductory remarks such as, “the account of mechanism (…) was motivated by particular examples of research in the biological sciences” we are again reminded that the method of inquiry is of the bottom-up sort, and that the starting point is scientific practice itself (Bechtel and Richardson, 2010, p. xx).[22] Research in many fields can plausibly be construed as a search for the underlying mechanisms responsible for observed phenomena, as evidenced by the actual usage of the notion of mechanism by scientists (Craver and Tabery, 2015). This is not to say that the concept of mechanism has an agreed upon meaning, nor that any single definition is sufficient to capture it. Indeed, there is no need for an exhaustive definition, since different disciplines and research projects may function according to slightly different variations of the more general notion.[23] Instead, we are given a qualitative characterization of the notion as it applies in

[21] See Machamer, Darden, and Craver (2000) for an early paper. A point of note is that this paper is the most cited paper in the recent philosophy of science, with over 2,000 citations. This further hints at the fact that the tides are changing, and such approaches are gaining in strength. See also Glennan and Illari (2018) for an authoritative overview of the cutting edge of the movement.

[22] The notion of ‘example’ here is intended to be used interchangeably with ‘case study’. That said, elsewhere Bechtel and Richardson do use the notion of ‘example’ in the same way as discussed in the present article, and they distinguish it from ‘case study’.

[23] This much has been well recognized, though there seems to be some value in using a generalized notion of a mechanism that captures most of the otherwise disparate usages (see Illari and Williamson, 2012; Glennan, 2017).

specific research contexts, illustrating the way in which the new mechanists have abandoned traditional approaches of studying concepts.

Clearly, both the new experimentalism and the new mechanism movements revolt against all three features of the philosophical conceptual analysis discussed in Section 3. Their intense focus on actual scientific practice suggests a method of theoretical analysis that tries to be faithful to the process and principles of science. Within each of these movements there is very little formalization to speak of, and the quest for defining concepts in terms of necessary and sufficient conditions is deemed unilluminating.

Construed this way, we may now see the difference between the case-study approach, as exhibited by the new experimentalism and new mechanism movements on the one hand, and the philosophical conceptual approach on the other hand. Recall the point from Section 3 when I briefly discussed Hempel and Oppenheim's approach. Their starting point consisted of mostly superficial examples, on the basis of which they drew far-reaching conclusions, which they subsequently formalized. I argued that, to a certain degree, such an approach is better viewed as top-down, rather than bottom-up. We may now appreciate the point in greater detail: the case-study approach concerns advanced practices. It, moreover, requires substantial analysis of these, which significantly affects the philosophical conclusions that are drawn.

5.2 The applied approach: the case of the cognitive-historical method

Having discussed the case-study approach, let us turn to what I call the 'applied approach'. By 'applied' I mean to suggest that philosophers draw heavily on existing results, especially from the cognitive sciences, in order to address a particular issue. Hence, it may be said that these philosophers apply empirical results from previous research to their own investigations. In practice, the applied approach is intertwined with the case-study approach.[24] Rather than

[24] In practice, the applied approach is aimed at a particular case. However, the reason for keeping the applied and the case study approaches distinct is that the case-study approach does not need to

presenting a general (and inevitably vague) analysis, I will briefly describe a particular instance of such an approach.

Nancy Nercessian has championed what she calls the ‘cognitive-historical’ method for investigating the ways in which certain scientific concepts have changed over time (Nercessian, 2008). As the name of the method suggests, it combines both cognitive and historical approaches. Let us look in some detail at what this method has to offer.

The traditional approach to studying concepts consisted of searching for the essential properties, that is, in writing down the set of necessary and sufficient conditions that define a particular concept (see Section 3.3). This tradition has been challenged on at least two fronts. First, Wittgenstein (1953) argued that it is difficult, if not downright impossible, to define a great number of concepts in terms of necessary and sufficient conditions. We are unsuccessful in defining concepts in this way because some concepts are inherently fuzzy, with boundaries that seem to withstand attempts at sharpening them. Instead, Wittgenstein coined the term ‘family resemblance’ in order to capture the network of characteristics that relate particular instances to a given concept. Second, some cognitive scientists have also taken issue with the tradition, basing their opposition on research data. Rosch and Mervis (1975) conclude that concepts tend to have a graded structure: there are typical cases of a given concept and there are also cases which are more or less similar to them. For instance, while robins seem to be a typical example of the concept of birds, penguins and ostriches, though still birds, are less obvious examples of this concept. In connection with this, one typical characteristic of birds is that they have the capacity to fly, but clearly not all of them can.[25] Given these findings,

draw on evidence from a specific branch of the sciences–for example, the cognitive sciences– in order to investigate a given case.

[25] Results coming from the cognitive sciences have greatly improved theories of concept formation and theories of reasoning (e.g. bounded rationality). Kornblith (2002, 2014) is a good source for

Nersessian argues for a more nuanced approach to studying the historical development of concepts, such as the field concept in the works of Faraday, Maxwell, Lorentz, and Einstein. In the construction of the notion of a field, pictorial visualization and analogy played a key role in the works of Faraday and Maxwell, as Nercessian documents.. Nercessian's aim is thus to closely track all the small changes and the various cognitive tools that have aided in the construction of broad scientific concepts.[26]

In a similar fashion, Shapere notes that "what is important in trying to understand scientific concepts is, (…), the way they develop; and to this task traditional theories of meaning, and indeed the whole program of studying meanings, is a hindrance rather than a help" (Shapere, 1987, p. 34). He goes on to claim that

> philosophical theories of meaning, such as those which seek necessary and/or sufficient conditions of application of terms, cannot do justice to the development of such concepts as 'force', 'energy', 'field', 'electron', 'particle', or, indeed, any scientific concepts at all, including allegedly 'metascientific' ones. For in scientific cases, the emphasis is on developing concepts which are adequate, in the light of what we have learned, for thinking and talking about nature and for understanding it (Shapere, 1987, p. 10).

Nercessian thus refrains from succumbing to the necessary and sufficient camp. Scientific practice is of utmost importance to her, and she uses no, or very little, formalization.

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an overview of the literature on the research into concept formation, with a discussion of its philosophical significance.

[26] It should be noted that the 'prototype' approach to concepts favoured by Rosch and Mervis (1975) is not the only game in town, as there are a few contenders. This means that Nercessian's method may be also based on not as strong a footing as originally thought. However, for my purposes here nothing of substance depends upon whether the specific details of Nercessian's account are correct. Rather, my point is to illustrate that some such approach is necessary to account for the conceptual development of scientific and meta-scientific notions (see below), and that it is empirically-oriented in spirit.

5.3 Qualitative methods

I have argued that some philosophy of science movements turned away from the traditional, non-naturalized way of inquiry. The earliest attempts, eventually gaining prominence, were fuelled by case studies, and by developments in cognitive science. More recently, however, philosophers of science have begun drawing even more on various empirical ways of conducting inquiry.

A vast array of qualitative methods is at the disposal of philosophers of science. Before moving on, some methodological worries need to be addressed. Some have argued that qualitative methods are suspicious–best to be avoided–because they rely on subjective reports or other criteria that are far from the rigour of quantitative methods.[27] Although this objection does carry some weight, I believe it may be addressed by realizing the proper domain of application, the particular goals of inquiry, and the fact that every method, including quantitative, has both limits and merits. Ethnographic research may allow a deeper insight into the workings of a small community; however, the price one has to pay is the highly constrained possibility of generating any broader (and broadly meaningful) generalization. Quantitative methods, e.g., survey methods, on the other hand, may allow generalization, but surveys cannot provide the kind of intimate insight made possible by ethnographic research.

The most popular methods used in the philosophy of science include ethnographic research, (mostly semi-structured) interviews, textual analysis (including textbooks, published papers, lab notes, grant proposals, etc.), and some combination of all of these. To fully appreciate how these methods may contribute to our understanding of science, let us examine some examples.

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[27] For more details see, for example, (Brysbaert and Rastle 2009, ch. 10).

Using the ethnographic method in combination with several others, Kevin Dunbar has analysed scientific causal reasoning during interactions among scientists in weekly lab meetings (see, e.g., Dunbar, 2002). The purpose of these meetings is to address and attempt to solve problems that arise during research, many of which concern unexpected findings. Dunbar claims that "scientists have a clear set of cognitive strategies for dealing with these findings" (Dunbar, 2002, p. 157) by first categorizing their findings with respect to their current knowledge, and second, drawing an analogy with a related experiment. This may eventually lead either to the realization that there was something wrong with the experiment, or that, perhaps, a new discovery has been made. Though, as Dunbar emphasizes, causal reasoning is multifaceted, the method of analogy enjoys some popularity. In this context, Dunbar claims that

> it is important to note that analogies based upon superficial features are very useful and help solve problems that the scientists frequently encounter. For example, if a scientist is obtaining uninterpretable data and another scientist makes an analogy to an experiment that worked in which the temperature was different, and suggests changing the temperature, this can potentially fix the problem. Many of the superficial analogies were of this type and solved the problem. These types of analogy are very frequent, but were rarely mentioned by the scientists in their interviews and never appeared in their lab books. When asked why they did not mention these analogies, the scientists thought that they were so obvious that they were not worth talking about. Despite the obviousness of these analogies, they are one of the most frequent cognitive workhorses of the scientific mind (Dunbar, 2002, p. 159).

Dunbar also reports on the comparative differences among Canadian, American, and Italian immunology labs. Although these labs were of similar size, used the same types of materials, cells, and equipment, and published in the same leading journals, and although members of these labs even attended the same conferences, the reasoning during lab meetings somewhat differed from lab to lab in accord with the norms of the wider culture (Dunbar 2002, p. 163).

Importantly, all these findings are impossible to uncover by any method other than the participatory, i.e. ethnographic method, because as Dunbar explicitly states, these things are omitted from scientists' published works and lab notes.

Thus, using the qualitative approach, Dunbar has enriched our understanding of the concept of scientific reasoning. He approached the question of 'What is X?', i.e., what is scientific reasoning, from the perspective of investigating how scientists do, in fact, reason. Dunbar does identify the traditional deductive and inductive inferential strategies, but what his analysis brings forth is the way in which such characterizations of scientific reasoning remain only partial, and must be complemented by a whole array of other reasoning practices. Accordingly,we must enrich our notion of scientific reasoning.

Similarly, Susann Wagenknecht has combined the empirical methods of participatory observation and interview to investigate the ways in which scientific knowledge is generated in interdisciplinary labs (Wagenknecht, 2016). Her aim was to uncover how practitioners handle the 'epistemic dependence' on one another, and how 'epistemic trust' is established in the working environment of interdisciplinary teams,–more particularly, in a small planetary research group and a large molecular biology lab. The epistemic dependence stems from the fact that in collaborative efforts scientists with different backgrounds must rely on each other, since it is impossible for them to verify every detail of each other's research; this leads to the cognitive division of labour. Thus, trust is very important if researchers are to form a group that is based on epistemic dependence. Hence, these methods allow Wagenknecht to elaborate on the nature of the notions of epistemic dependence and epistemic trust.

Most recently, also drawing on the participatory observation ethnographic method, Rebecca Hardesty investigated a group of neurobiologists whose research focuses on the genetic underpinnings of Down syndrome (Hardesty, 2018). This group has developed a new mouse model ('GCDS') that they regard as 'the gold standard' of all Down's syndrome genetic research. However, there are severe practical limitations regarding the use of this model. These include the costs ($15,000 for each mouse), the short life-expectancy of the strain that is ill-suited for this particular research (i.e. age-related cognitive degeneration as it occurs), and the low chance of successful breeding (about 11%). Consequently, the lab uses a different mouse (TS65Dn) which they regard as inferior to their own. The goal of Hardesty's research was to answer questions such as, 'how does the lab justify working with what they believe to be an inferior mouse model?' (Hardesty, 2018, p. 15). In Hardesty's own words

> lab X has developed a new 'genetically correct' Down syndrome mouse model (GCDS) that it rarely uses. Instead, it uses a mouse which it considers to be an inadequate genetic model (TS65Dn) of the condition; however, the Lab has performed a series of experiments in order to show that the inferior Ts65Dn mouse is genetically equivalent with respect to APP in the GCDS mouse, therefore, Lab X has a new justification for the old mouse. Yet, Lab X has not revealed this equivalency explicitly in the papers that came out of these experiments; instead, they only published the data on APP in the Ts65Dn line (Hardesty, 2018, p. 20).

Furthermore, Hardesty argues that

> by solving this practical problem of not being able to use the mouse line and resolving this tension between their values of good practice, the lab made the GCDS mouse a standard against which one could evaluate other DS mouse models. This is an epistemic success for Lab X and a philosophically relevant feature of modeling practice that was not apparent without participation/observation fieldwork (Hardesty, 2018, p. 21).

The above is intended to demonstrate that the traditional philosophical conceptual method has limits with respect to its ability to illuminate important aspects of scientific practice. Furthermore, these examples[28] should also illustrate the way in which qualitative methods provide unique insight into scientific minds. Such insight may not be achievable by other

[28] Some further examples can be found in a recent volume edited by Susann Wagenknecht, Hanne Andersen, and Nancy Nercessian, which contains a discussion of various qualitative methods as well as their application to particular research questions (Wagenknecht, Nersessian, and Andersen, 2015a).

methods, including other empirical methods, such as quantitative methods. This is because, as Dunbar notes, scientists may not mention all of the practices they rely on, including the kinds of analogies they use in their interviews or lab notes. Thus, qualitative methods may catch one's special attention only in participatory research. Let us now turn to quantitative methods.

5.4 Quantitative methods: the case of the OPTIMIST project

Quantitative methods are used to first collect data and then further analyse them using various statistical techniques. One form of the quantitative method, favoured in this type of empirical research in philosophy, is the survey. Participants are asked to fill in a questionnaire by indicating with which of the pre-defined answers they identify the most. Depending on the nature and purpose of the survey, the participants may also be asked about their gender, academic position, or other general information about themselves. This allows the gathering of a great deal of data in a standardized way, but by its design it requires some simplifications to be made, which may obscure seemingly minor details that later could turn out to be of some importance. This trade-off further illustrates the need to use a combination of methods to form an overall picture.

The OPTIMIST Project constitutes a recent example of employing quantitative research methods in the philosophy of science .[29] The acronym OPTIMIST stands for 'Optimization Methods in Science and Technology' and is a collaborative effort of researchers from the University of Belgrade and the Ruhr-University Bochum.[30] One branch of the project concerns itself with better understanding the motivations and preferences of scientists, and with gaining insight into their working conditions with respect to their

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[29] More precisely, the project is an interdisciplinary effort, and as such, includes researchers from disciplines such as computer science, psychology, science policy and the philosophy of science. More broadly, it also belongs to the field of 'social epistemology' (see below).

[30] For details visit http://www.ruhr-uni-bochum.de/optimist-survey/.

environment and communication–all with the hope of providing some guidance in helping to maximize their efficiency. In order to address these issues, members of the project have launched a survey for researchers in laboratories such as Fermilab and the Rutherford Appleton Laboratory. This sort of reflection on values in science provides much-needed clarification of the concept of values and the various roles they play in scientific practice.

However, other quantitative methods, such as various data-mining methods (rather than surveys), have been put to work. Previously published results of the OPTIMIST project addressed the question of the connection between the efficiency of a series of high-energy physics experiments run in Fermilab, measured by publication and citation rates, and the team size, the time it takes to complete an experiment, and the number of teams per experiment (Perović, Radovanović, Sikimić, and Berber, 2016). Using data envelopment analysis, distance-based analysis, and analysing other contributing factors to team and project performance, the authors conclude that "[small teams] very simply proportionally widely outperform the bigger ones in terms of citations and publications" (Perović, Radovanović, Sikimić, and Berber, 2016, p. 102). Building on these results Sikimic, Radovanovic, and Perovic (2018) further address the question of the extent to which time spent on a particular project correlates with its outputs. They show that there comes an 'epistemic saturation point', after which the number of most significant results usually drops, whereas the number of less significant publications increases. They suggest that this may have a bearing on the rational organization of research with respect to the decision of whether to continue a project or not.

A possible objection lurking behind is whether this sort of project still constitutes philosophy, an objection sometimes raised in debates on experimental philosophy. I have attempted to address this worry in Section 2.2, albeit in a limited fashion. I believe it is worth noting that the project, broadly construed, belongs to the field of social epistemology, which is an emerging field in philosophy, one that addresses the social nature of knowledge. As such, it is a legitimate field within philosophy (see, e.g., Goldman, and Blanchard, 2015 for an overview).[31]

6. Concluding remarks

A general distinction has been proposed between two ways in which conceptual analysis has been employed in the philosophy of science. On the one hand, I described philosophical conceptual analysis, exceedingly popular (not only) in the logical positivism movement, by reference to three characteristic features. These include, firstly, the conducting of analysis independently of the details of scientific practice. The purported justification is that by incorporating various actual details, the analysis of science would be in danger of succumbing to subjectivism, something which must be avoided in the philosophy of science, which concerns itself with the context of justification. Secondly, the notion that formalization is an adequate instrument for gaining insight into and clarifying meta-scientific concepts. Thirdly, that the analysis and genuine clarification of meta-scientific concepts requires providing a set of necessary and jointly sufficient conditions for the employment of a given concept.

This approach is partially inherited by movements that struggled to show the inadequacy of logical positivism, as I discussed in connection with Kuhn's historical school of the philosophy of science. Furthermore, we saw that traces of those features are still prevalent in some contemporary debates, including the debate on scientific representation. I argued that these features can be characterised as consistent with a non-naturalized methodology, because they do not pay due respect to actual scientific practice. However, as noted above, none of these features is inherently flawed. Rather, I argued that it is the way in which they have been used that is inappropriate.

[31] This is in line with how Sikimic (personal communication) defends the philosophical aspect of the research.

The second general view of conceptual analysis, I suggested, is empirical. Empirical conceptual analysis is a bottom-up approach, rather than top-down; i.e. it takes scientific practice as its starting point. I described multiple methods employed by various movements and showed how they improve upon previous methods. Indeed, some of the interesting results would be difficult (if not impossible) to obtain through more traditional methods. This is one of the main reasons why philosophical conceptual analysis, even if practiced in a relatively modest way, (i.e. avoiding extremes discussed in Section 2.1), is being slowly replaced by more empirically-oriented methods–as, indeed, it ought to be.

With the advent of new research methods and their introduction to the philosophy of science, many authors have begun to pay greater attention to how their approach differs from that of others. It is not a rare sight to lay one's eyes on passages in which philosophers voice critical opinions of non-naturalized methods. For instance, with respect to the debate on scientific explanation, Collin Rice claims that "unfortunately, (…) philosophers have often inappropriately attempted to apply their primarily *a priori* approaches to explanation to these unique instances in biology rather than first considering how these models are actually used by scientists to provide explanations" (Rice, 2015, p. 590-591). Similarly, with respect to the bottom-up and top-down approaches, the remarks of Bechtel and Richardson reflect many of the ideas laid out in this paper. For instance, they claim that

> it is worth noting that the history of science can be approached in two ways, as a reservoir of examples to illustrate a prior conception of science or as a source of insight into features of science that an adequate philosophical account of science needs to incorporate. Our use of the history of science is in the latter tradition. (…) We then built the account around the cases rather than selecting cases to illustrate the account. As we have said, our focus was on what counted as 'reductionistic' research in these fields. We soon discovered that these fields could not be naturally fitted to existing philosophical models of reduction without doing serious damage to the sciences and to their actual historical development (Bechtel and Richardson, 2010, p. xx).

The form of naturalism exemplified by the empirical conceptual analysis discussed here is perhaps different from some of its other incarnations. For example, in some sense, this may be a stronger commitment to naturalism than found in some of Quine's work, as suggested by Bechtel, when he claims that

> although Quine made claims about scientific practice (e.g., about the role of nonepistemic factors such as simplicity in the evaluation of scientific theories) and about the limitations on scientific knowledge (e.g., the underdetermination of theories by all possible evidence), he generally did not engage science as a naturalist. For example, he did not study the investigatory strategies or reasoning employed by particular scientists or the specific explanations they advanced (Bechtel, 2008, p. 8).

Although, for reasons described above, I favour empirical conceptual analysis in general, I do not think philosophers should commit to one particular method. Every method provides access to a specific aspect, or set of aspects of scientific investigation, but not to others. Every method allows one to answer some sets of questions, but not others, as shown in Section 5, where I discussed the characteristics of and differences among the various methods, including the case-study approach, the applied approach, and qualitative and quantitative methods. In order to gain a complete picture of scientific practice, different methods of investigation must be used and the results then integrated.

The continuous shift toward empirical conceptual analysis as a method, or rather, a set of methods, is further evidenced by the establishment of international societies such as the Society for Philosophy of Science in Practice (SPSP) and the rate at which these communities have grown. We are also witnessing the emergence of collaborative and interdisciplinary projects such as the OPTIMIST project, or the Evidence-Based Medicine Plus movement (EBM+). The focus of these projects often extends beyond 'mere' analysis and has more practical objectives in mind. These include policy-making regarding, for example, the organization of research or clinical practice in medicine. Consequently, philosophers are now starting to jointly publish articles with scientists in both philosophical and scientific journals.

All this suggests that we are witnessing a truly naturalistic shift with respect to the method of inquiry in the philosophy of science.

Let me close by addressing a legitimate worry one may have at this point. The literature on naturalism and various empirical methods in philosophy has become enormous. I have not addressed many of the pressing issues people have been discussing, but I would maintain this is because my purposes here target different aspects of the debate at large. It would prove impossible to address all that has been said on this theme. l Yet, I believe I have managed to bring focus to a particular problem within the philosophy of science in a way that furthers discussion and contributes to the growth and vitality of the discipline. I must admit that much of the discussion in the present article is sketchy at best and deserves further elaboration that is beyond the scope of this paper. I hope that such elaboration is made possible through discussion, debate, and meaningful engagement with the ideas I have proffered throughout this article.

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