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Formal Methods in the Philosophy of Science

Abstract. In this article, we reflect on the use of formal methods in the philosophy of science. These are taken to comprise not just methods from logic broadly conceived, but also from other formal disciplines such as probability theory, game theory, and graph theory. We explain how formal modelling in the philosophy of science can shed light on difficult problems in this domain.

Keywords: formalization, formal method, applied logic.

1. The logical method in philosophy

Rationalist philosophy was guided by the idea of *mathesis universalis*, which was encapsulated in the conviction that philosophical problems should ultimately be tackled in a mathematical manner. Ideally, all philosophical problems would have been solved using geometrical methods, but this has not happened, and the idea was gradually abandoned. In analytical philosophy during the first half of the twentieth century, there has been a somewhat analogous motivating credo—namely the rallying cry of what one might call *logica universalis*. The idea was that by using logical methods all philosophical problems can be solved or dissolved. It is largely recognized that this expectation also proved, to a large extent, to be unfounded. Today, many of the traditional philosophical problems are as pressing as they ever were, even though we have more insight into them now than we did in the past, and have come to a better understanding of why they are so hard to solve.

Still, many philosophers remain convinced that what is often called the logical method forms the methodological core of analytical philosophy. When confronted with a philosophical problem, one should address it as follows. First, the problem must be formalized, that is, it must be at least roughly expressed in the language of first-order logic. That in itself is of course not enough. If formalization is restricted to translation to first-order logic, then Wang's criticism applies:

> [W]e can compare many of the attempts to formalize with the use of an airplane to visit a friend who lives in the same town. Unless you simply

> love the airplane ride and want to use the visit as an excuse for having a good time in the air, the procedure would be quite pointless and extremely inconvenient. [49, p. 233]

So, more needs to be done. The key philosophical concepts in the formalization must be identified. As a next step, basic principles that express how these philosophical notions are related to other philosophical notions must be articulated, and again be (at least roughly) formulated in first-order logic. Also, pre-theoretical convictions involving these philosophical concepts must be spelled out. Then, a precise hypothesis concerning the philosophical problem is put forward. Subsequently, a determined attempt is made to logically derive the hypothesis from the basic principles and the pre-theoretical data. If this attempt is successful, then an answer to the philosophical question has been obtained. (This answer is then, of course, not immune to criticism.) If the attempt is not successful, then the exercise has to be repeated. Perhaps more or different basic principles concerning the key philosophical notions are required. It is also possible that more pre-theoretical data are needed. And our pre-theoretical intuitions are not sacrosanct either; some of them may be overridden by theoretical considerations.

The role of logic in this methodology is clear. Logical formalization forces the investigator to make the central philosophical concepts precise. It can also show how some philosophical concepts and objects can be defined in terms of others. If it emerges that certain objects are "constructed" as classes of other objects (using a version of the method of abstraction), ontological clarification is reached. Insistence on logically valid derivation, moreover, forces the investigator to make all assumptions that are needed fully explicit. As a result of this procedure, precise answers to philosophical questions are obtained. And if a conjectured hypothesis can not be derived from known basic principles and data, then there must be hidden assumptions that need to be explicitly articulated. For instance, in his formal investigation of Euclidean geometry, Hilbert uncovered congruence axioms that implicitly played a role in Euclid's proofs but were not explicitly recognized [23]. Naturally, given the completeness theorem for first-order logic, such results are often obtained by model-theoretic techniques. That is, in order to show that a given conjecture does not follow from a collection of premises, a model is constructed in which the premises hold but the conclusion fails.

A central thesis of the present article is that this view of the role of logic in philosophy is somewhat naive. The claim is not—note—that this picture is *wrong*. But we do think it is very incomplete in that it does not allow for an adequate explanation of the multifarious ways in which logic is able to shed light on philosophical problems.

The scope of our discussion in this article will be in one respect narrower and in another respect wider than these introductory remarks suggest. Although the view to be proposed is intended to apply to philosophical questions generally, we will focus on problems in the philosophy of science. On the other hand, we will not be occupied solely with logical methods *strictu sensu* but will consider other formal methods as well.

2. Logical empiricism

In the first half of the previous century, the philosophy of science came into its own as a subdiscipline of philosophy. This new philosophical discipline was dominated by the school of logical empiricism. Axiomatic formal logic had been developed shortly before by Frege and Russell (see [19] and [44]). While logical empiricism developed, logicians came to realize that they had a complete list of the axioms of first-order logic. The research methodology of the new discipline of philosophy of science consisted in applying formal first-order logic to philosophical problems. Russell and Carnap developed construction systems in which they sought to define such notions as that of a physical object and that of a physical concept [42, 8]. Logical empiricists sought to provide adequate logical definitions of central concepts from the philosophy of science, such as the concept of scientific theory, scientific explanation, confirmation, reduction, causation, and scientific law. Valiant attempts were even made to express the distinction between science and non-science in logical terms. It was expected that the main questions in the philosophy of science (such as: Is biology reducible to physics and, if so, how?) could be systematically answered once their key concepts were defined in first-order logic. Russell expresses the function of the method of logical analysis as follows:

> Although ... comprehensive construction is part of the business of philosophy, I do not believe it is the most important part. The most important part, to my mind, consists in criticizing and clarifying notions which are apt to be regarded as fundamental and accepted uncritically. As instances I might mention: mind, matter, consciousness, knowledge, experience, causality, will, time. I believe all these notions to be inexact and approximate, essentially infected with vagueness, incapable of forming part of any exact science. Out of the original manifold of events, logical structures can be built which have properties sufficiently like those of the above common notions to account for their prevalence, but sufficiently unlike to allow a great deal of error to creep in through their acceptance as fundamental. [43, p. 341]

Unfortunately, time and time again the logical analyses that were proposed proved to be grossly inadequate. This phenomenon is perhaps most dramatically illustrated by the history of the successive attempts to give a strictly logical definition of the concept of confirmation. The most famous proposals in this direction were the hypothetico-deductive account (see, e.g., [3]) as well as Popper's falsificationism (which comes to an endorsement of one "half" of hypothetico-deductivism; see [41]) and Hempel's positive instance account [21]. Each of these proposals was undermined by counterexamples. It always seemed possible to find cases of genuine confirmation that failed to be classified as such by the definition or of cases of spurious confirmation that were classified as genuine confirmation by the definition. The most general argument against logical accounts of confirmation—which became famous under the name "the grue paradox"—was offered in [20]; it convinced pretty much everyone that a strictly logical approach to confirmation was hopeless.

Though Carnap favored a rather different approach to confirmation from the start in that he thought that probability theory was the right framework for a theory of confirmation, at first, and actually for a very long time, he conceived of probability theory as a generalization of first-order logic: he saw probability theory as the logic of partial entailment and thought of probabilities as being uniquely determined by the logical structure of the language on which they were defined [9]. As it became clear that one cannot determine these supposedly unique logical probabilities in a nonarbitrary way even for very simple toy languages, and that, in effect, given Carnap's account of probability, one cannot even disqualify some ostensibly absurd probability assignments (see [7]), Carnap abandoned his logical conception of probability, and towards the end of his career even came to embrace a position on confirmation that he called a "subjectivist point of view" [10, p. 112]. It is clear, however, that this evolution could only be regarded as a defeat for the logical empiricist program.

For the logical empiricists, the distinction between "contentful" and empty ("metaphysical") questions was of the utmost importance [17]. Because of this, complete formalizability was a *conditio sine qua non* for logical empiricism. Resistance to first-order formalization of concepts and questions was conceived as a mark of meaninglessness. The empirical component of the logical empiricist program imposed an extra constraint. Ultimately, the formalizations had to be formulated in terms of empirically meaningful vocabulary.

But many of the concepts and questions that were investigated resisted complete formalization in empirical terms. Formalization in first-order logic itself was not really a problem. Rather, often a straightforward formalization did not appear to shed light on the philosophical problems at hand.

The key to the philosophical problems often did not appear to be found in logical relations between empirically acceptable propositions. Take the case of scientific explanation. Attempts to formalize this relation without using apparently non-empirical concepts, such as Hempel and Oppenheim's deductive-nomological account [22], faltered. It began to look as if at least for an important subclass of scientific explanations, the relation of causality played a crucial role [45]. But the relation of causality smacked of metaphysics. Using it in an explication of the concept of explanation was unacceptable from the logical empiricist point of view, at least until an adequate empiricist analysis of causation was available. Attempts to provide such an analysis were made (see, for instance, [34]), but these were unsuccessful. In fact, analyzing the notion of causation remains a notoriously hard problem (though see the next section for references to some recent promising approaches to causation that might well have been to the liking of the logical empiricists).

The weight of successive failures became hard to bear. In the 1960s, logical empiricism was considered as a package deal and rejected wholesale by more or less the entire philosophy of science community. Concomitantly, a widespread skepticism concerning the role of logic as an important tool for investigating the methodology and conceptual framework of science took hold, and the philosophy of science took a new direction. The newly accepted doctrine was that the salient structure of science is of a historical and sociological rather than of a logical nature ([27], [28]). In the later part of the twentieth century, this was the predominant view among philosophers of science. Today research in history and sociology of science is still regarded as absolutely indispensable for arriving at a sound philosophical conception of the nature and structure of the sciences. We dare to predict that this will remain so in the foreseeable future.

3. New formal tools

Meanwhile logicians were making progress in "pure" areas of logic that at first seemed far removed from the concerns of the analytical philosophers. Logicians early on in practice abandoned the identification of logic with axiomatic first-order logic. They did so despite the fact that most of them remained convinced that classical axiomatic first-order logic (with identity) contains a complete list of the laws of logic, a conviction that is maintained by most logicians (but not all) up to the present day. In other words, the scope of the field of logic has expanded.

New logical disciplines emerged and blossomed. The field known as *model theory* was shaped by the hands of Tarski and his collaborators. Through the work of Gödel, Church, Turing, and Kleene, the disciplines of *recursion theory* and *complexity theory* were formed. Under the influence of seminal work by Kripke, various branches of *intensional logic* gradually matured. Logicians started to investigate questions that were not traditionally taken to belong to the province of logic proper. Questions regarding the computability in principle of functions on the natural numbers, questions regarding the laws of metaphysical necessity, and questions regarding the complexity of mathematical truths expressible in a given formal language, all came to be regarded as, in some sense, logical questions. Soon, these evolutions in the field of logic made their way to standard textbooks. For instance, the authoritative and influential *Handbook of Mathematical Logic* [4] is divided into four parts: Proof Theory, Model Theory, Set Theory, and Recursion Theory.

For decades, mathematical logicians worked, to a large extent, in splendid isolation on their “pure” subjects. Increasingly, however, they came to regret it, as they came to appreciate that logic is an inherently interdisciplinary enterprise. The conviction took hold that when logical research remains detached from the concerns of other disciplines (mathematics, computer science, philosophy, linguistics, ...) it has a tendency to become anemic (both scientifically and financially).

In more recent times, logic has become more applied and in some sense more empirical. In the days of Frege and Russell, logic was dictating the norms of reason with which our inferential practices should comply. Logic was not much concerned with the way in which scientists and ordinary people actually reasoned. But this has markedly changed. Logic now tries to respect the reasoning practices in ordinary life and in the sciences. This does not mean that logic has abandoned its normative ambitions, but it does mean that logic has restricted its normative ambitions to investigating the normative consequences of the concepts that are actually inherent in practical reasoning, rather than imposing on the participants in the practice logical concepts that they ought to use.

This evolution can be illustrated on the basis of the history of the logic of conditionals. In the first decades of the twentieth century, logicians held that the logic of indicative conditionals was adequately explicated by the truth conditions of the material implication. In the second half of the twentieth century, by contrast, logical theories of conditionals were constructed using methods from intensional logic and from probability theory. These logics proved to be more faithful to the inferential relations that are actually operative in our conditional reasoning (see [1]).

A few decades ago, philosophers of science started to follow suit in acquiring a broader conception of logic. Whereas philosophers have long been discussing the virtues and vices of formalization in philosophy, today they discuss the value of *formal methods* (in the plural!) in philosophy. New logical tools and methods were absorbed by the philosophical community and were gradually applied to specific problems in the philosophy of science. This gave rise to a remarkable comeback of the use of logical methods in the philosophy of science. Examples are not hard to find. Methods from intensional logic were used to arrive at new theories of causation and scientific law [31, 32]. Techniques from recursion theory and complexity theory were used to arrive at new theories of scientific discovery [26]. And results from proof theory were used to shed light on the problem of underdetermination of scientific theories by observational evidence [11].

Aside from this, philosophers of science increasingly started to draw on formal methods that lie outside the scope of logic, even as it is liberally conceived. Probabilistic concepts and methods are nowadays used freely by philosophers of science who have no interest in contributing to the articulation of a logical conception of probability. Among the many notable examples in this connection are the probabilistic analyses of causation given in [16] and [40], Myrvold's Bayesian approach to theoretical unification ([36]; see also [46]), and the probabilistic accounts of coherence offered in, among others, [6, 18, 38], and [12]. Other formal methods and concepts that are increasingly applied to problem areas in the philosophy of science are those of game theory and of graph theory. Game theory has been applied, for instance, to philosophical questions regarding common knowledge (see [30]). Graph theory has been applied to the theory of causality and events [24]. A burgeoning area of research in which both strictly logical and probabilistic tools are being employed, and to which also philosophers of science are contributing, is that of judgment aggregation; see, for instance, [33, 39], and [13].

In making use of these new formal methods philosophers of science are merely treading in the footsteps of the great philosophers of science of the first half of the twentieth century (such as, most notably, Carnap) who were keen to make use of any of the (then) newest formal methods. It was merely a historical contingency that in this period axiomatic formal logic emerged as a new and exciting discipline full of promise for the future. When its value to the philosophy of science had been established beyond doubt, a kind of intellectual laziness set in: many adopted a habit of assuming uncritically that logic is the right framework for approaching every problem in the philosophy of science. This assumption proved to be ill-founded.

Computers have become widely available to and useable by scientists. Today, practical and powerful computational methods lie within the reach of every investigator. In particular, computer simulations have become an indispensable tool in the sciences. Philosophers of science are also increasingly making use of computer simulations. For some recent examples, see [5, 48], and [35]. Parallel to this development, there has been a growing interest among philosophers in the epistemological status of computer simulations; see [29] and the references given therein. Interestingly, Lehtinen and Kuorikoski argue that "the information [computer simulations] provide is epistemically just as relevant as the information provided by an analytical proof" (p. 325). Even if this should be an overly optimistic view on the value of computer simulations, there is nowadays widespread agreement among philosophers that such simulations serve more than merely illustrative or heuristic purposes.

4. Rethinking the role of formal methods in the philosophy of science

As a result of these developments, the toolbox of the philosopher of science is now greatly expanded. This has opened up a vast space of possibilities, but it also presents new challenges. As a rule, it is reasonable to assume that formal methods can shed light on just about any important problem in the philosophy of science. But for each specific problem, a fitting formal framework has to be actively sought. A crucial component of research into a problem consists in seeing what is a good formal framework for it and why, and what the limitations of the framework are. Axiomatic first-order logic is but one such framework, and a restricted one at that. Finding the right formal framework for a problem is a highly nontrivial task. There is no general recipe for it.

One should guard against exaggerated expectations concerning the role of formal frameworks. Formal methods can shed light on problems in the philosophy of science, but it would be unreasonable to expect that formal methods can, on their own, solve problems in the philosophy of science. This is because philosophical premises inevitably play a decisive role in the application of a formal framework to a philosophical problem. Consider, for instance, Bayesian confirmation theory. It may (and probably does) shed light on the problem of induction (though see [25]). But whether Bayesianism is the correct theory of confirmation can never be a purely formal question. It will ultimately have to be argued for (or against) on the basis of philosophical premises that may or may not be shared by all researchers in the field of confirmation theory.

We are now in a position to revisit and correct the incomplete story about the logical method with which we have started this paper.

The logical method as it was described there is by no means obsolete. It remains our main tool for uncovering hidden assumptions in theories. For instance, in recent decades it was discovered by the logical method that Newton's mechanics is not deterministic. Conservation principles need to be explicitly added to Newton's laws before a deterministic theory is obtained. This shows the error in textbook demonstrations that derive conservation principles from Newton's basic laws of mechanics [14]. In this issue, Andreka and her collaborators show how in Relativity Theory the formula connecting rest mass and relativistic mass can be deduced from postulates that are geometrical in nature [2].

But, as mentioned before, formal methods are no longer restricted to methods of formal logic in the axiomatic-semantic sense of the word. The scope of logical methods has been expanded, and extralogical formal methods are increasingly brought to bear on problems in the philosophy of science.

Ironically perhaps, Kuhnian ideas can be used to explain how formal methods can yield increased insight in a domain. Formal methods function as paradigms in the Kuhnian sense of the word in that they are used for modelling concepts and problems in the philosophy of science. As such, they function as spectacles through which we can look at these concepts and problems and in this way give us insight into them. In this respect, philosophy of science does not differ from the sciences themselves. The mathematical theory of analysis, for instance, functions as a paradigm in classical mechanics in the same sense in which the formalism of Bayesian networks functions as a paradigm in the recent study of causality. Even the logical framework in the narrow sense functions as a paradigm in this sense. It allows us to view a scientific theory as a finite object: a finite set of basic principles closed under logical deduction.

If formal methods function, in some sense, as paradigms in the philosophy of science, then it should not come as a surprise that for every formal method there comes a point of diminishing returns. When a formal method has been applied in one area of the philosophy of science, it is very natural to try to apply the same technique to other branches of the philosophy of science. But at some point the new applications begin to look forced and somehow unnatural: the formal method does not succeed in shedding (new) light on the conceptual problems at hand. When this stage is reached, it is better to continue looking until a better modelling technique is found. The pursuit of a strictly logical definition of confirmation, briefly discussed above, is, in fact, a case in point. Attempts to counter the problems that were uncovered for the logical accounts of confirmation piled epicycle on epicycle, with each new proposal facing new problems. There came an end to this chain of fruitless efforts only when subjective probability theory made its entrance in philosophy and enabled researchers to formulate a confirmation theory—the earlier-cited Bayesian confirmation theory—that appeared to solve with remarkable ease all, or at least most, of the problems that had beset the older confirmation theories [15]. In a way, Bayesian confirmation theory retained the valid kernel of the logical approach in that, at least in general, on this theory it also holds that a hypothesis is confirmed by evidence it logically entails and disconfirmed by evidence that is logically inconsistent with it. But it also handles naturally those cases that do not fall in either of the foregoing categories and that had appeared to be major obstacles for the earlier approaches to confirmation. It is at most a mild exaggeration to call this "probabilistic revolution in the philosophy of science" [37, p. 81] a veritable paradigm shift in the Kuhnian sense of this term.

One major lesson to be learned from this episode is that, as researchers, we should try to be as flexible as possible in our use of formal methods; another is that, as teachers, we should realize that the curricula most philosophy departments are offering, which contain the standard courses in logic but no introductions to any of the other formal methods discussed above, are, at least from a philosophy of science perspective, seriously incomplete.

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