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MATHEMATICAL PHILOSOPHY?[1]

ABSTRACT

This article reflects on the scope and limits of mathematical methods in philosophy.

1. INTRODUCTION

Open a journal in chemistry at an arbitrary page, and you will see formulae. And it is the same for almost every other scientific subject. These formulae indicate that the article draws upon mathematics. It is like that if you open an issue of a journal that has just come out. But it is also like that if you open an issue of a journal that has appeared 50 years ago. Now open a general journal in philosophy at an arbitrary page. Chances are that you will see no formulae, but just text. This tells you that the article you are looking at does not draw on mathematical techniques or theories: it is written in a discursive style. This will certainly be the case if you open an an issue of a general philosophy journal that appeared 50 years ago. It is very likely also to be the case if you open a recent issue. Thus a discipline like chemistry is said to be a technical subject, whereas philosophy is said to be a non-technical subject.

This situation is currently changing rapidly. Until fairly recently, mathematical methods were used only in certain relatively specialised areas of philosophy such as philosophy of mathematics and philosophy of science. But in the last two decades, mathematical methods have become increasingly used in traditional areas of philosophy (such as epistemology and metaphysics).

It is time for a methodological reflection on this evolution. Until now, such a methodological investigation has not been carried out as far as I know. There has been some discussion on the use of mathematical models and methods in the philosophy of science ((van Benthem 1982), (Horsten and Douven 2008), (Muller t.a.), (Leitgeb t.a.), (Wheeler t.a.)). But there has been almost no systematic discussion of the use of mathematical methods in core areas of philosophy such as

1 This article is based on my inaugural lecture, which I gave in Bristol in December 2010. Thanks to Richard Pettigrew, Hannes Leitgeb, Neil Coleman, and Gregory Wheeler for valuable comments on earlier drafts of this article, and for stimulating conversations on the subject. Research for this article was partially supported by the AHRC project "Foundations of Structuralism" (AH/H001670/1).

H. Andersen et al. (eds.), *New Challenges to Philosophy of Science*,
The Philosophy of Science in a European Perspective 4,
DOI 10.1007/978-94-007-5845-2_7, © Springer Science+Business Media Dordrecht 2013

metaphysics and epistemology. Moreover, the methodological debate has focussed mainly on the use of *logical* methods.

This article is a systematic philosophical investigation into the role of mathematical methods in core areas of philosophy. I want to reflect on the scope and limits of mathematical methods in philosophy. I will argue that, while there are limits to what we can expect of mathematical methods in philosophy, mathematical methods can make a contribution to philosophy. I will not try to prove my case by giving you an annotated list of success stories and telling you what is so great about them. Instead, I want to look behind the examples. How is it that mathematical methods can contribute to philosophy? Which parts of philosophical inquiry necessarily have to be carried out in the discursive style?

I will not offer concrete methodological advice here: the concrete dangers and pitfalls of bringing mathematical methods to bear on philosophical problems have been discussed elsewhere ((Rota 1988), (Hansson 2000), (Horsten and Douven 2008)). And I also don't want to spend much time on the history of the use of mathematical methods in philosophy: a brief overview of this can be found in (Horsten and Pettigrew 2011).

2. Logical analysis and logical explication

Over the centuries it has occasionally been suggested that philosophy and mathematics are intimately related. Think of Plato's admonition "let no one destitute of geometry enter my gates", Spinoza's ideal of conducting philosophical research *more geometrico*, Leibniz' slogan "sedeamus et calculemus", etc. These sentiments were mainly fuelled by a craving for absolute certainty in philosophy. But in practice, philosophy has for most of its history been a discursive practice. This only slowly started to change in the beginning of the 20th century. Ironically, this change happened at the time when the search for apodictic certainty started to lose its grip on philosophy.

In their logical investigations of mathematics, Frege and Russell recognised that the *logical* form of certain sentences differs in important ways from their surface grammatical form ((Frege 1879), (Russell 1905)). Russell and the early logical positivists emphasised that this was in particular the case for many philosophical propositions. By bringing out the logical form of philosophical propositions, certain supposedly deep philosophical problems could be unmasked as *pseudo-problems*. For a while the opinion held sway in some circles that in this manner, all philosophical propositions will, after logical analysis, turn out to be either trivially true, or trivially false, or empirical. In other words, it was thought that after logical analysis, there would be no deep philosophical questions left.

This turned out not to be the case. Most of the age-old central philosophical problems turned out to be impossible to dismiss as pseudo-problems, even after logical analysis. Only philosophical problems that were somewhat suspect in the first place – they sounded a bit silly – could be dissolved by logical analysis. So the ambitions of logical analysis had to be scaled back.

Carnap thought that logic nonetheless had an important role to play everywhere in philosophy. In his view, philosophers should strive at giving *logical explications* of philosophical concepts (Carnap 1950). When presented with a philosophical proposition, the philosopher should first of all uncover its logical form (as in the method of logical analysis). This logical form will relate certain predicates, and is given in a formal language. Next, Carnap says, plausible basic *rules of use* should be spelled out. The aim is to decide the philosophical proposition by deriving it or its negation from the rules of use using the rules of logic alone. Of course there is absolutely no guarantee that we will be able to solve the problem in this way: our basic principles may be controversial, or they may simply be too weak to decide the philosophical question we are interested in.

The method of logical explication has been criticised by the ordinary language philosophers on various grounds. One of their critiques rests on the fact that we cannot jump outside natural language. They argue that it is an illusion to think that appeal to formal languages can be a decisive step forward if one wants to address a philosophical question. Suppose that we are indeed able to derive the philosophical proposition we were interested in from correct rules of use. Then this whole derivation can be translated back to natural language, and one would not have had to make the detour via formal languages in the first place (Strawson 1963).

This much is true: there is no formal substitute for philosophical thinking. By working meticulously in formal languages invalid argumentative leaps can be excluded, and the possibility of tacit assumptions that remain under the radar is eliminated. But if one is careful enough, this can also be achieved in ordinary language. In any case, the philosophical argumentation will centre around the question of the basic rules of use that will be proposed. And this is a discussion that will take place in informal English. In sum, for all that has been said so far, while methodologically useful, semi-mathematical tools such as formal languages do not touch the heart of philosophy: they are dispensable in principle.

3. The Dawn of Mathematics in Philosophy

In the late 1920s, Carnap started using mathematical models in philosophy. His ambitions were high: he wanted to construct the whole world (!) in terms of elementary sensory data and a similarity relation between those data. His models were set-theoretic in nature. In this programme, the colour red, for instance would turn out to be something like a set of sets of ... sensory data. And even physical space would turn out to be some such set. There is no need to go into the details of Carnap's "logical construction of the world" (Carnap 1928), because it was ultimately unsuccessful. Instead, let us look at a use of models that is generally regarded as at least somewhat successful.

The first kind of non-set-theoretical models that were used in philosophy are probabilistic models. Probabilistic models were the first examples of quantitative models in philosophy. Such models were used to shed light on the problem of confirmation in the philosophy of science. Suppose you have a scientific theory, and suppose you obtain a new piece of observational evidence. Then this piece of evidence can confirm or disconfirm the theory. Philosophers of science wanted to articulate a satisfactory philosophical theory of this support-relation between theories and evidence.

It turned out to be very hard for philosophers to find tenable basic principles of confirmation using Carnap's method of logical explication. The reason why the method of logical explication did not produce satisfactory results is that our intuitions about the confirmation relation are unreliable. By the beginning of the 1950s it had become clear that whenever we list principles concerning the confirmation relation that agree with our intuitions, they are invariably met with counterexamples. Somehow it seemed that a theoretical idea was needed.

In the late 1950s, philosophers of science took up the idea of modelling confirmation as probability-raising: evidence confirms a hypothesis if it raises the hypothesis' probability. This was the start of developing probabilistic models for studying the confirmation relation. Note that this development does not fit well with Carnap's method of logical explication: it is hard to imagine that the concept of probability somehow belongs to the logical form of all propositions involving the confirmation relation.

A whole machinery (known as Bayesian confirmation theory) has since then been developed to tackle problems in confirmation theory. And whatever one may think of it, this research programme was more successful than the approach to confirmation that came before, which was a version of Carnap's method of logical explication.

The probabilistic models contributed to our understanding of the confirmation relation by giving us an understanding of our intuitions concerning confirmation (Earman 1992). Before the advent of probabilistic models, we knew that our intuitions in this area are unreliable. But we did not really understand why. Probabilistic models provide compelling and integrated stories of why and in which situations our confirmation intuitions go astray. They show how our intuitions are shaped and sometimes deceived by our experience. The probabilistic models reintegrate and organise our intuitions.

For a model construction programme such as Carnap's (Carnap 1928), the ideal aim could be to find the unique correct model: the way the world actually is built up from experience. But the subjective probability-approach to confirmation never really aimed at uniqueness. From the start, the prior assignments of probability values were taken to be somewhat arbitrary, and were taken to irredeemably vary from person to person. Thus their theory was fundamentally a large ensemble of models rather than a unique intended model. This is very much in consonance with the model-theoretic or semantic view of theories in the natural sciences.

4. Recent uses of mathematical methods in philosophy

So probabilistic modelling plays an important role in the sub-discipline of philosophy of science that is called confirmation theory, whereas, at least until its very recent revival (Leitgeb 2007), the idea of set-theoretically constructing the world from experience was seen as a lost cause. There were a few other areas in philosophy where mathematical modelling played some role (such as philosophy of mathematics). But these are all somewhat specialised and relatively new areas of philosophy. Philosophy of science, for instance, came to its own in the first decades of the 20th century. In the core and more traditional areas of philosophy, such as general epistemology, metaphysics, and ethics, mathematical modelling was not done at all. And the mathematical methods used were in some sense 'logical'. Set theory is a part of mathematical logic, and some say that probability theory is somehow a 'generalised' form of logic.

This situation has begun to change in the past two decades. To an ever larger extent, mathematical modelling, as well as other mathematical techniques, are used even within traditional, core areas of philosophy. And the techniques and models that are used draw upon a large variety of mathematical fields (graph theory, mathematical analysis, algebra,. . .).

Let me mention some examples from epistemology and metaphysics. A fundamental epistemological question is: *why should our credences satisfy the standard laws of probability?* In recent work starting with (Joyce 1998), techniques and results of mathematical analysis have been used in the formulation and exploration of proposed answers to this question that involve distance-minimalisation. (De Clercq and Horsten 2005) have invoked techniques of graph theory to formulate identity conditions for secondary qualities such as colour shades. More examples could be listed, but instead I want to discuss one example (from metaphysics) in some more detail – of course this will be a highly simplified account.

Nominalists believe that the world, absolutely all there is, consists of concrete objects that stand in a part-whole relation to each other. Abstract objects *do not* exist, according to nominalism.

Nominalism is a philosophical theory if there ever was one. It is a metaphysical doctrine, dating back at least to the Middle Ages. But modern-day nominalists are enough of a naturalist to want their theory to be compatible with empirical science. The theories of the modern natural sciences use mathematics. So nominalism somehow has to find a way of recognising the truth of key principles of mathematics. Let us concentrate on the theory of the natural numbers: that is surely a key and basic mathematical theory.

There are two obstacles for the nominalist here. First, number theory seems at first blush about abstract entities. After all, in which museum is the number 7 held? Secondly, there is the question whether the world of the nominalist is large enough to accommodate the natural numbers. There are infinitely many numbers: who knows if there are infinitely many concrete objects?

In response to the first problem, it seems that the nominalist has to let concrete objects somehow play the role of the natural numbers: concrete objects is all she's got![2] In response to the second problem, the nominalist has to bite a bullet, and assume the existence of infinitely many concrete objects. This is perhaps not completely hopeless if space-time is infinite in some dimension – perhaps the time-dimension in the future direction.

From the 1950s onwards, nominalists (such as Nelson Goodman) have thought about precise principles governing the part-whole relation. In the 1960s, a minimal theory was gradually settled on, together with a list of possible extra principles that might also be true, but that are not universally accepted in the nominalist community (Niebergall 2011).

Now the following question emerges. Given that there are enough concrete objects to stand proxy for the natural numbers, can the basic axioms that govern the natural numbers somehow be validated? Roughly, this means the following: can the language of arithmetic somehow be translated into the nominalistic language of concrete objects and the part-whole relation, in such a way, that the basic principles of arithmetic are validated? In the light of the foregoing, it should be clear that from the present-day nominalist point of view, this is an elementary question that is of utmost importance. If it can be done, then a nominalist understanding and recognition of the laws of elementary arithmetic is possible.

Somewhat surprisingly, the answer turns out to be 'no' (Niebergall 2011). It has been shown in the past two decades that the nominalistic theories, minimal or extended by further principles that have been advocated, cannot validate even 'minimal' arithmetical theories. For the *cognoscendi*: they cannot even interpret the arithmetical system known as Robinson arithmetic, which is standard arithmetic without the axiom of mathematical induction. The proofs of this are in fact not really difficult: they only involve some relatively elementary facts about Boolean algebras.

Has the philosophical question of the viability of nominalism thereby been settled in the negative? Has a philosophical problem been laid to rest? No. For it is open to the nominalist to change her position and to say that not just the part-whole relation, but other, more complicated relations between concrete objects are nominalistically acceptable as belonging to the basic fabric of the world. When that is done, we have moved to the next round in the debate about nominalism.

But the point is that we will have advanced: we have made progress in this philosophical debate. We have not solved the question of nominalism; but we have shed light on it. And, coming back to the ordinary language philosophers, it is hard to see how this insight could have been obtained using the discursive methodology of ordinary language philosophy. In principle, Niebergall's proofs about the noninterpretability of Robinson Arithmetic in standard mereological theories can

2 An alternative for the nominalist is to develop a fictionalist position concerning mathematical objects. (Thanks to Neil Coleman for pointing that out.) But here I assume that indispensability arguments justify adopting a realist line on the question of the existence of mathematical objects.

be spelled out in ordinary English, just like any mathematical proof can. But it is unreasonable to think that Niebergall's arguments could have been produced in practice using the methods of ordinary language philosophy.

5. LIMITATIONS?

In the debate between ordinary language philosophy and 'formal' philosophy, objections against the methodology of logical explication have crystallised only gradually. The application of a wide variety of mathematical methods to central problems in philosophy is a very recent phenomenon. The opposition hasn't had time yet to get organised. In the years to come, that will probably happen. What follows is a glimpse of what their arguments might look like.

5.1 Philosophy and our conceptual world

One objection of the ordinary language philosophers that will undoubtedly re-emerge, is naturalistic in spirit. On the one hand there are the objects, properties, and relations in the world. It is the business of the sciences to describe what is out there in the world; philosophy had better not try to compete with them. On the other hand, there are our everyday concepts and conceptions, which latch imperfectly onto the world. It is the business of philosophy to describe our concepts: this is called *conceptual geography*. Our concepts are a fairly loose and gerrymandered lot. Now you can, using the process of logical explication, find substitutes for these concepts that are more structured, in the sense that they satisfy a small set of highly coherent and general basic principles. But when you have arrived at these substitutes, you have lost contact with our concepts as we live them in our experience. In Rota's words:

> The concepts of philosophy are among the least precise. The mind, perception, memory, cognition, are words that do not have any fixed or clear meaning. Yet they do have meaning. We misunderstand these concepts when we force them to be precise. (Rota 1988, p. 170)

This is not to say that there is anything wrong with trying to find mathematical structure in our conceptual world.[3] But it is somewhat unlikely that our conceptual world is mathematically structured in the way in which the physical world miraculously has turned out to be. And if it isn't, then it's no use pretending that it is. This is what Wittgenstein had in mind when he said that as soon as philosophy has produced a theory, you can bet on it that it is wrong (Wittgenstein 1956).

This is a deep and important point. The relation between ordinary language philosophy and phenomenology, on the one hand, and our concepts and our experience on the other hand, is somewhat like the relation between literary criticism

3 My former colleague Hannes Leitgeb emphasises that this is a valid objective of mathematical philosophy.

and literature. Literary criticism is somehow continuous with literature; discursive philosophy is continuous with our conceptual world. All of it belongs to our culture and will do so in the future. Moreover, discursive philosophy is not just an epiphenomenon of the culture and society we live in. It changes our conceptual world and our lived experience.

Mathematical philosophy has more in common with the particular part of our culture that we call science, which is much less continuous with our everyday conceptual world. Mathematical philosophy wants to play with the hard-hitting girls. Its ambition is not just to describe our concepts, but to capture properties and relations in the world. That is a tough proposition, but it seems to me that there is no way around it.

This is also where Carnap's insistence that the concepts that play a role in the logical explication must be *fruitful* is relevant. Carnap emphasised that the rule of use-principles need not be a completely faithful representation of the way in which the concepts involved are used in ordinary language. But, Carnap says, these formal substitutes for our ordinary concepts somehow have to be theoretically useful. And it is in this context that we should understand Strawson's critique of Carnap, when he says that Carnap's method is like offering someone a book on physiology when she asks (with a sigh): "who understands the human heart?" (Strawson 1963).

The hard-headed position that I am advocating here does not exclude that some of the properties that the mathematical philosopher wants to investigate, are subject-relative in some way. To take an example, consider confirmation again. As we have seen, many philosophers of science now think that the confirmation relation contains a subjective component. Nonetheless, the philosophers of science aim at more than describing our concept or concepts of confirmation; they aim at describing what it means for evidence to confirm a hypothesis.

It may be that in some areas, attempts to go beyond the geography of our everyday concepts are doomed to fail. Suppose, for instance, that not only all attempts to 'uncover the grounds of morality' turn out to be futile, but that even all attempts to derive most accepted moral maxims from a small and coherent number of principles fail. (This may, in so far as I can see, actually be the case.) This would then be an area where mathematical methods could never be applied fruitfully in the way that Carnap envisaged.

5.2 Models and instrumentalism

The following is often seen as an obstacle to playing with the big girls. In the natural sciences, sensory experience (observation and experimentation) is our ultimate touchstone. Theories are tested on the basis of their empirical consequences. Philosophical theories are also connected with sensory experience, but in a much less definite way, and their connection with the outcome of scientific experiments is even less clear.[4] How do we refute a philosophical theory, even if it is precisely formulated (as a class of models, say)?

It is often said that our common sense intuitions form the touchstone of philosophical theories.[5] The value of this depends on what the philosopher's aims are. If she wants to be faithful to our concepts and the relations between them, then our intuitions indeed occupy a privileged position. But if her aim is to latch on to an 'objective' relation or property, then our intuitions may well be unreliable – although they are unlikely to be even then massively mistaken: there is often deeper truth hidden behind intuitive falsehoods.

Indeed, success of a philosophical way of picturing the phenomena is not easy to define. It is a matter of shedding light on a subject, of providing insight, of showing how it all hangs together. The paucity of precise empirical predictions does not bar philosophy from obtaining objective knowledge. As Alonzo Church once said: the preference of *seeing* over *understanding* as a method of observation seems capricious (Church 1951). In other words, there may well be situations in which philosophers have good reasons to believe in the objective correctness of models that they produce: the key factor will be explanatory power.

This takes us to a fundamental difference between the role of models in the natural sciences and in philosophy. In the natural sciences, models can be valuable even if they are fundamentally unrealistic, not in the sense of making idealisations (such as the absence of friction), but in the sense of *intentionally* making fundamentally false incorrect assumptions (as is done for instance in modelling traffic as a fluid passing through a system of connected tubes). Even though such models do not really *explain* anything, they serve an important goal: they are connected to observational and experimental predictions. Even models that do not describe the world anywhere near correctly can be extremely powerful as a source of empirical predictions. Indeed, even an empiricist such as van Fraassen who is agnostic about the existence of unobservable entities, properties, and relations, is happy to acknowledge the value of models that postulate sub-atomic particles. An instrumentalist stance to models is always possible in science.

Philosophical theories do not typically make precise empirical predictions. Thus if one does not believe in the objective correctness of a class of models in philosophy (even granting the idealisations involved), then its value is much less clear. As intimated earlier, the way in which one can bring oneself to believing in the objective correctness of a class of models is in philosophy basically the same as in the sciences. It consists in success arguments. Ultimately, they are variants of *Inference to the Best Explanation*. Nonetheless, even classes of models in philosophy that are perhaps difficult to take seriously, such as the part-whole models discussed earlier, may have their value. They function as a conceptual

4 This point is emphasized in (Hansson 2000).

5 There is also the question who is meant with 'our' in this sentence. Experimental philosophers hold that many of the 'intuitions' on which analytical philosophy is built are generated by a quite unrepresentative sample of the population, and therefore suspect. I will leave this discussion aside here.

laboratory (van Benthem 1982). They give us insight in what metaphysically might have been, in a way in which theories of magnetic monopoles give us insight in what physically might have been.

5.3 *Informal concepts and the discursive style*

Classes of mathematical models are built using very precise concepts. This causes classes of mathematical models to have a certain *rigidity*: it is difficult to adapt classes of mathematically defined models to phenomena that they were not intended to describe in the first place. Classes of models that are generated by a mathematical technique are also very *stubborn*. Once a certain mathematical modelling technique has firmly taken hold of a field, it is very difficult to replace it with a new mathematical modelling technique or just to get rid of it if it doesn't work well. Genuinely new classes of mathematical models that are suitable for describing phenomena in a given field are very difficult to find.

Even though he was a great advocate of the use of models, Boltzmann pointed out that everyday concepts possess a *plasticity* that scientific concepts to a large extent lack (Boltzmann 1902).[6] Everyday concepts are in a sense like stem cells: they can become virtually anything. This is definitely a virtue when we are working in an area where we are still groping for clues, when we are still feeling our way around. In such a situation, the discursive method is the only way, for we still have to shape our concepts. And, as we know from medical science, it is important that we always keep a healthy supply of stem cells. You never know when you will need them. At some point, we may have to look for a new class of models, and then we simply have to start with our everyday concepts.

In some situations, stem cells take on a sharply defined shape: they commit themselves to a specific task and agree to a division of labour. This corresponds to the emergence of sharply defined models in science and in philosophy. At its best, models can have the effect of 'switching on the light'; at their worst they merely serve as the intellectual equivalent of wearing blinders. In any case, they are a prerequisite for having a theory that can really be tested. Precisely because models are precise, and somehow rigid, and somehow narrow-minded, they cannot easily dodge attempts at refutation.

Nonetheless, here again a difference with the use of models in the natural sciences emerges. Let us return for a moment to the example of nominalism. We have seen that the decision to take only the part-whole relation as fundamental can be and has been challenged. The debate about the correctness of taking the part-whole relation as the only basic relation is conducted in the discursive style. And this is in large part where the philosophical action is. More in general, philosophical disputes about the form and basic ingredients of the models must be conducted in the discursive style. There is no other way: conducting the discussion in the language of the model would beg the question. In this way, the discursive style

6 Frege also made this point (Frege 1879, introduction).

necessarily forms a constitutive part of any philosophical investigation. In the natural sciences, discussion about the basic ingredients of the models are less central. Again, this has to do with the fact that observational evidence forms the ultimate touchstone. Many scientists believe that as long as a class of models yields the right empirical predictions, there can be no legitimate cause for concern or criticism. Even if one does not believe that, there is much less at stake. As I have said before, even unrealistic models can be of utmost importance in science.

5.4 The bounded scope of mathematical methods

In the light of all this, we may ask: what then are the virtues of using logico-mathematical methods? Where is the pay-off?

Carnap's method of logical explication forces one to make the grammar and the structure of philosophical argumentation explicit. This is obviously a good thing: it is a question of intellectual hygiene. But its instances do not import an essential use of mathematical methods in philosophy. Rota puts it too harshly, perhaps, when he writes:

> Confusing mathematics with the axiomatic method for its presentation is as preposterous as confusing the music of Johann Sebastian Bach with the techniques for counterpoint in the Baroque age. (Rota 1988, p. 171)

In the case of nominalism, we have seen how mathematical methods can really enter into it. They can be used to prove limitative results, or *impossibility results* as they are sometimes called. In the case of part-whole nominalism, the principles turn out too be too weak to do significant mathematics.

But there are also situations where the principles we come up with are too strong. Think about the case of the theory of truth, where the liar paradox teaches us that intuitive basic truth principles lead to a contradiction. As a response to this, philosophers have tried to weaken the truth principles in such a way that the basic intuition behind them is still preserved as much as possible. In order to show that these weakened principles are at least consistent, one has to produce a model in which they are true. In this way, models can yield *possibility results*. Of course when one has produced a model, one has only a mathematical possibility result, and this falls far short of showing that the theory under investigation is a serious philosophical contender. Again, to substantiate the latter claim, a discursive story has to be told.

Mathematical models, such as probabilistic models in the case of confirmation, can *unify* a seemingly disparate array of intuitions. Carnap's method of logical explication can do this to some extent, but use of mathematical models and techniques are much more powerful in this respect. One reason for this is that a class of models can show what binds a collection of basic principles together, more so than a list of axioms can. A mathematical class of models gives us a way of looking at a class of phenomena in a unified way.

Models are ways of looking at something. Sometimes one can look at a phenomenon in different ways that are in some sense equally fruitful. Take the case of subjunctive conditional sentences: sentences of the form

If A were the case, then B would be the case.

One can look at subjunctive conditionals in a probabilistic way. That is, one can say (roughly) that a conditional sentence of that type are true (or acceptable) if and only if $Pr(B \mid A)$ is high. But one can also look at them in a 'topological' way. That is, one can say (roughly) that a conditional sentence of that type is true if and only if the situations in which A is true that are 'close' to the way things actually are, are also situations in which B is true. Now there are *representation theorems* which show (roughly) that for every 'probabilistic' model for subjunctive conditionals, there exists a 'topological' model that is equivalent to it, and, conversely, that for every 'topological model', there is a 'probabilistic' model that is equivalent to it (Leitgeb unpubl.). With equivalence is meant here that they classify the same subjunctive sentences as true. In other words, mathematical theorems can sometimes tell us that there is a sense in which two different ways of looking at something nevertheless in some sense yield the same results.

So the picture I want to suggest is the following. At the beginning, we have a philosophical hypothesis, informally expressed. In this form, its content is to a degree fluid and indeterminate. In order to understand the hypothesis, and eventually to assess it, we have to make it more definite and more precise. This can be achieved by associating with it a class of mathematical models. (This can of course be done in more than one way.) Only then mathematical techniques and results come into play. They allow us to *understand* the content of the models. They increase our insight into an interpretation of the philosophical hypothesis with which we started.

Nonetheless, there are limits to the power of mathematical methods in philosophy. As an essential but proper part of a philosophical account, mathematical models and methods can help shed light on philosophical problems. But even supposing that deep philosophical problems can in principle be solved: what mathematical methods can never ever do, is to single-handedly solve philosophical problems. This can never happen. For the reasons that I have given, philosophical theories will always remain more closely connected to our informal concepts and to our informal way of arguing than theories from the natural sciences. It would be folly to think that the discursive style of informal philosophy can ever be eliminated in any branch of philosophy. Use of mathematical methods will never be a substitute for philosophical thought.

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