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FORMAL METHODS IN THE PHILOSOPHY OF NATURAL SCIENCE

What is the proper place of formal methods in philosophy of natural science, or in philosophy more broadly speaking? The idea that philosophy should proceed formally ("more geometrico", as in the title of Spinoza's *Ethica*) has been around for some time, but both the attitude towards formal methods and the understanding of formal methods itself has changed. Mathematical logic has succeeded geometrical demonstration as the paradigm of formal precision, and in technical areas such as foundations of mathematics and logic, Frege's and Russell's logicist programmes indicate early peaks of the application of these methods. The idea of employing such formal-logical methods in philosophy more generally was championed by the logical empiricism of the 1920s and 1930s. Wrestling with the methodological foundations of their discipline in an attempt to exclude what they perceived to be nonsense, some at the time even sought recourse in a purely formal-logical foundation for philosophy. Frege's student Carnap in his programmatic paper on "the old and the new logic" (Carnap, 1930, 26) put the matter thus: "To pursue philosophy means nothing but: clarifying the concepts and sentences of science by logical analysis."[1]

As the philosophical sub-discipline of philosophy of science is to a large extent historically continuous with logical empiricism, it is no wonder that the newly emerging field of philosophy of science – which mostly meant: philosophy of natural science – in the 1950s centered around an array of formal-logical methods.

This attitude towards formal methods has not remained unchallenged: the 1960s saw a historicist turn in philosophy of science that has led to a fairly critical attitude towards formal methods. As Kuipers (2007, viii) remarks, "many philosophers do not like to be associated with the logical empiricists". In this paper I will argue that the availability of new formal methods and an increased sensitivity for the uses and limitations of formal approaches makes possible a fresh case for the usefulness of formal methods in philosophy of science and particularly in the philosophy of natural science. Formal methods also play an integral part in the methodology of conceptual modeling that lies behind a number of recent success stories in that and in related areas of philosophy. Individual contributions of the ESF Network's Team A, which centers on formal methods, all testify to the usefulness of that methodological outlook.

Before arguing for these claims starting in section 2, I will set the stage by expanding a bit on the historical background of the question of the place of formal methods in philosophy of science.

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[1] German original: "Philosophie betreiben bedeutet nichts Anderes als: die Begriffe und Sätze der Wissenschaft durch logische Analyse klären."

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1 PHILOSOPHY OF NATURAL SCIENCE IN THE 20TH CENTURY

Depending on one's outlook, philosophy of natural science can be viewed as an old subject, or as a rather new one. Certainly philosophical reflection accompanied the development of the New Science in the early modern period, and there are good reasons for viewing philosophy of science as a historically unified enterprise with roots in the 13th century, or even in Aristotle. This historical lineage is the subject of the flourishing field of history of philosophy of science. On the other hand, the current academic sub-discipline of philosophy of science is a development of the late 19th and the early 20th centuries – Ernst Mach in 1895 was the first person to hold a chair in philosophy of science at Vienna, and the *Verein Ernst Mach*, subsequently the *Vienna Circle*, together with the *Berlin Circle* in the 1920s and 1930s were the birthplace of logical empiricism, which played a key role in forming and establishing the discipline of philosophy of science. As already mentioned, this more recent historical lineage is crucially important when it comes to the role of formal methods.

Logical empiricism was, broadly speaking, an attempt at turning philosophy into a respectable scientific discipline. In the eyes of the propounders of this doctrine this meant to abolish metaphysics, where no clear scientific standards were discernible, and instead to embrace strict standards of reasoning, the strictest of which, apparently sufficient even for strengthening the foundations of mathematics, were made possible by the development of modern formal logic. As the quote from Carnap given above indicates, logical analysis of science would be all that was left of serious philosophy.

On the other hand, the idea of a formal study of science can also be linked to the widespread formal self-understanding of science. The idea that proper science needs to be mathematical has been strong since the 17th century – witness Galileo's image of the book of nature being written in the language of mathematics, or Kant's later pronouncement that a purported science was a science only insofar as it was mathematical.[2] It appears only natural that such a subject should be approached by tools equally mathematical or formal. The use of formal methods in 20th century philosophy of (natural) science thus appears as a confluence of two mutually supporting ideas: the logical empiricists' idea of logical analysis as *the* tool of philosophy, and the commonsensical idea of studying that which of itself is formal by formal means. Based on the demand for the unity of science characteristic of logical empiricism, the deployment of formal methods was then assumed to spread to other areas as well. The messy details of actual science notwithstanding, unified science was to be rationally reconstructed using the formal methods of logic – and that of course meant: of the logic of the time.[3]

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[2] Cf. Galilei (1623) and Kant (1786).

[3] The history is of course more tangled than this sketch suggests. It should not be forgotten that the "left wing" Vienna Circle besides Carnap also included philosophers like Neurath, who proposed a pragmatic approach to the philosophy of science including psychological and sociological studies, cf. Uebel (2001) on Neurath (1932). This idea

Philosophy of science developed as a subject proper mainly in the U.S., following the emigration of many of the leading logical empiricists due to the rise of Nazism.[4] In the 1950s, the field consolidated around a positivist orthodoxy, leading to compendia such as Nagel's *The Structure of Science* (1961). Formal accounts of explanation, confirmation, theory reduction, laws of nature, and other key concepts had been worked out by then. The cracks were however already beginning to show: the adequacy of those formal accounts appeared doubtful.[5]

Initially, logical empiricism could respond to criticisms about the descriptive adequacy of proposed accounts by pointing to their status as first steps in a research program. When the account of scientific concepts remained questionable vis-à-vis actual practice over decades, however, it appeared that the research program had failed to deliver. Historical and sociological studies of actual science such as Kuhn's (1962) *Structure of Scientific Revolutions* (published in the logical empiricists' own book series) were seen as more important than logical constructions that increasingly seemed to be built of thin air.

This sketch of the historical background may help to explain the generally critical attitude towards formal methods that is, or at least was, prevalent among many philosophers of science.[6] Determining the proper place of formal methods in philosophy of natural science nowadays means to be aware of this historical baggage, and to take up the challenge of showing how the criticism leveled against logical empiricism's deployment of formal methods can be met.

2 THE USES AND SUCCESS OF FORMAL METHODS IN RECENT PHILOSOPHY OF NATURAL SCIENCE

Despite the mentioned criticism, formal methods never vanished from philosophy of science. Many of the early formal-logical accounts – e.g., the deductive-nomological account of explanation – have always remained important for the field, not at least in teaching the subject, and not just because of their historical significance, but also because they remain systematically significant due to their clarity and exactness.

There are however also many *new* success stories of the deployment of formal methods in the philosophy of natural science. I will argue that nowadays, formal methods have their proper place right in the center of philosophy of science, and that we can identify two factors that explain their successful return: the develop-

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however had little impact on the development of the subject of philosophy of science in the years after the Second World War.

[4] The historical context of logical empiricism is described in detail in the essays of Stadler, Hoffmann and Reisch in Richardson and Uebel (2007).

[5] Cf., however, Feigl (1970) for a dissenting view on the relevanceof actual scientific practice.

[6] For a more detailed overview, cf., e.g., Richardson (2007).

ment of new formal methods on the one hand, and the adoption of the methodology of conceptual modeling on the other.

2.1 Conceptual modeling

The philosophy of science of the 1950s focused on a mostly static view of the metamethodological embedding of formal methods. Explication of key concepts was considered to be a matter of logical analysis of what was there. More recent applications of formal methods however mostly occur in a dynamic setting. This move is usefully described in Kuipers (2007), who tells a story of refined ways of concept application. In a similar vein, but from a broader perspective, I would like to describe the respective metamethodological change as a move towards *conceptual modeling*.

In science and engineering, *mathematical modeling* has long been seen as one of the most fundamental methodologies, and one of growing importance. Mathematical modeling presupposes quantitative and computational methods. However, a slight generalization of the same methodology that may be called *conceptual modeling* is ubiquitous also in non-quantitative research areas. This methodology and its uses are described in more detail in Löwe and Müller (2009). Briefly, conceptual modeling is an iterative process through which a stable reflexive equilibrium is reached between a concept or a collection as concepts, X, as explanandum and a (somewhat) formal representation of it. Each iteration towards the equilibrium involves three steps:

1. *Formal representation.* Guided by either a pretheoretic understanding of X or the earlier steps in the iteration, one develops a (more or less) formal representation of the explanandum.

2. *Phenomenology.* With a view towards step 3, one collects evidence in the range of the explanandum that is ideally able either to corroborate or to question the current formal representation.

3. *Assessment.* In the light of the results from step 2, one assesses the adequacy of the representation. If this assessment is positive, the modeling cycle is left – no further iteration is necessary since an equilibrium has been reached. Otherwise, the representation has to be changed, and a new iteration is started at step 1.

This method obviously covers mathematical modeling as employed in the sciences and in engineering, where the formal representation typically comes with a numerical mathematical model that allows for quantitative predictions. In the case of philosophy, the scheme usefully generalizes the methods of "conceptual analysis" or of "logical analysis" as invoked by Carnap: it leaves room for a dynamical, iterative approach, and it is not confined to a fixed set of formal means of representation. The examples from philosophy of natural science given below testify to the usefulness of that method.

2.2 Example success stories: new formal methods

Formal methods are nowadays not limited to the traditional field of formal logic – which by itself has expanded vastly, providing for modal, temporal and other logics and giving much formal insight into the important notion of a model, or a structure. The methods also include a significant amount of probability theory and aspects of game theory, graph theory, computer simulations and other techniques of formal modeling. It should also be emphasized that in this development, philosophy of science does not play the merely passive role of employing off-the-shelf techniques developed in other disciplines, but has also led to the development of new techniques.[7]

In the following short descriptions of formal success stories, the contrast is always between the way matters were seen within the original paradigm of logical empiricism focusing on inferential relations among sentences and logical analysis, and new approaches based on an extended array of formal methods and pursued in a modeling framework.

No originality is claimed for the accounts of the employment of formal methods given here. These accounts are rather meant to illustrate my main point, which is that we are witnessing a return of the fruitful employment of formal methods in philosophy of science. Consequently the following sketches will be rather brief. Other examples connected with the work done in the ESF Network's Team A could easily be added, e.g., work on Bayesian methods in confirmation (Fitelson and Hawthorne, 2005; Huber, 2005), or on social aspects of science (Hartmann and Bovens, 2008; Dietrich, 2006; Pigozzi, 2006).

Reduction vs. intertheoretic relations What is the relation between a scientific theory and the theory that historically takes its place – like, e.g., the Newtonian theory of universal gravitation superseding Galileo's law of falling bodies? The new theory should at least account for the same empirical facts as the old one. Thus, within the logical empiricist paradigm of theories as collections of general statements, it seemed that some relation of logical derivability or reduction would be appropriate: the new theory should allow one to derive all empirical statements of the old one, plus some more. It is easy to see that this idea breaks down even in the case of the example of Galileo vs. Newton (ironically used as an illustration by Nagel (1961)): In the earth's non-uniform gravitational field, the Galilean law is only an approximation to what Newton's theory predicts.

The move to present-day probabilistic methods has proved to be promising. Rather than focus on the "reduction" of one theory by another, a wider picture of intertheoretic relations emerges. That picture also includes the data the theories account for and thus remains much closer to actual scientific practice (Batterman, 2008; Hartmann, 2008). Methodologically, the move from theory reduction to a

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[7] Cf., e.g., Leitgeb (2009), who also echoes the earlier programmatic paper of van Benthem (1982). Cf. also Horsten and Douven (2008) for a state-of-the-art survey.

Bayesian account of intertheoretic relations exemplifies concrete work in conceptual modeling.

Quantum logic: old and new The quantum logic of Birkhoff and von Neumann (1936) was an attempt at reading off a "new logic" from the mathematical structure of quantum mechanics. Initially the idea was to find an interpretation of propositional connectives like conjunction and negation that would be a formal counterpart to operations on the set of subspaces of a Hilbert space that constitutes the state space of a quantum system. A fascinating possibility was that the "true" logic could turn out to be different from classical propositional logic – and for empirical reasons.

Present-day logic paints a different picture, and again, the conceptual modeling paradigm captures this development. Quantum logic never came to replace classical logic (signaling inadequacy in the assessment step) – but the logic community has also become much more open towards the idea that there could be different logics, each suited to a specific domain.[8] Furthermore, there are new tools within logic that can be fruitfully employed in a study of quantum mechanics (there are more options for a fresh start of the modeling cycle). In fact dynamic logics seem to be very well suited for a description of quantum operations studied in quantum information theory (Baltag and Smets, 2008). Thus, advanced formal methods allow one to leave old normative questions (about "the" logic) behind and work towards a better understanding of science as actually practised.

Determinism and indeterminism of theories The question of whether a given scientific theory is deterministic or not, was approached mostly informally before Montague (1962) introduced a model theoretic approach. In this field many advanced methods of mathematical physics have been employed, and the formal technical level of discussion is very high (witness Earman, 2007). In fact here the deployment of formal methods has significantly advanced other discussions, too, in that the importance of precise definitions of, e.g., the notion of state has been recognised. Questions of theory determinism or indeterminism are furthermore relevant not just for philosophy of science, but also for science itself.

2.3 A proper place for modality in the philosophy of natural science?

In the sketches just given I have stressed the involvement of new formal methods that go beyond the traditional toolbox of logical empiricism, and the importance of a broadened understanding of what one is doing in employing formal methods via the method of conceptual modeling. I will now take a closer look at my last example, viz., determinism and indeterminism or, more broadly, the involvement of modality in the philosophy of natural science.

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[8] Carnap's Principle of Tolerance (Carnap, 1937, 51f.) already points in that direction.

Determinism is a modal notion: it signifies the absence of open possibilities. Modality arguably plays a role in many other concepts of science, too: laws of nature, essences and natural kinds, causation and intervention, and probability. My suggestion is that the time is ripe for taking modality seriously in philosophy of natural science.

Even though modality is studied formally nowadays, this was not so in the early days of logical empiricism. From that doctrine's point of view, there were two problems about modality in science. Firstly, modality was interpreted as *logical modality*, where logical possibility just means the absence of formal contradiction – but this is not the notion of modality that is needed to analyse the mentioned scientific concepts. The notion of logical possibility is too broad: many things that are physically impossible are still logically possible (think, e.g., of going faster than the speed of light). Secondly, modality apparently has poor empiricist credentials. This continues to stand in the way of a fruitful employment of modal notions in philosophy of science. After all, mere possibilities – possibilities that are not actualised – are empirically inaccessible because they are unreal, so how could they be important for empirical science?

The first important step towards an employment of modality in philosophy of science is to take a lead from the discussion about different modalities. This discussion developed out of formal research into the semantics of modal logic since the 1950s. Initially one may view this semantic enterprise as a quest for a formal representation of *the* meaning of "possibly" and "necessarily". The semantics that was established, the so-called Kripke semantics that spells out the modalities in terms of relations among possible worlds, showed however that there is much leeway in specifying different modal logics with different semantics. The initial assessment of this fact was rather critical: among all those options, it seemed that one still had to find the right one to specify what "possibly" and "necessarily" *really* meant. This assessment has changed in the meantime, and the many options for a semantics of modality are now seen as a good thing. It has become common to acknowledge a number of different kinds of modality: there isn't just logical modality, but there are various other kinds of modality that may have different formal properties and a different metaphysical status. In terms of the modeling paradigm, this means that a larger range of formal ways of spelling out aspects of modality has become available. It will be best to explain some of these options in terms of possibility; the consequences for the dual modality of necessity follow immediately.[9]

As mentioned, there is logical possibility: the absence of formal contradiction. This notion is rather broad. Famously Ramsey pointed out to Wittgenstein that his *Tractatus* theory, which relied on logical possibility in postulating the independence of elementary propositions, was flawed because it could not, e.g., account for the rather straightforward impossibility of the same patch's being both red and green – no formal contradiction is involved here, since "red" and "green" just fig-

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[9] Possibility and necessity are dual in the following sense: It is necessary that p if and only if it is not possible that non-p.

ure as two different predicates, and it is logically possible for one and the same thing to fall under any number of different predicates. The colour overlap in question is however clearly impossible in another sense. It has become common to speak of *metaphysical* (im)possibility here, and to base philosophical arguments on metaphysical rather than logical modality. For philosophy of science, however, a notion of *physical* possibility seems to play an even more important role. Physical possibility is often taken to be what laws of nature express, and insofar as science is a quest for the laws of nature, science is really about physical possibility. Determining the place of modality in philosophy of science thus comes down to modeling physical modality.

2.3.1 Modeling physical modality

Questions about the interrelation of various kinds of modality are notoriously difficult to resolve. There are arguments in favour of modal monism (the claim that there is one single fundamental modality, to which all other modal notions can be reduced), but also in favour of modal pluralism (the claim that there are different irreducible modalities). Thus, the question of whether physical possibility is just a restricted version of logical or metaphysical possibility has been debated: e.g., Fine (2005) argues convincingly that physical and metaphysical modality are independent and indeed believes that they are both fundamental, thus providing an argument in favour of modal pluralism.

My conviction is that physical possibility is not fundamental, and that a fruitful explanation of the use of possibility in philosophy of science needs to refer to a different notion of possibility: *real possibility*, also known as historical possibility because of its link with temporality.[10] The peculiarities of that notion of possibility are best explained via some of its specific formal properties.

2.3.2 The formalities of real possibility

The formalities of real possibility have been worked out since the 1950s. Prior's *Time and Modality* (1957) set the agenda for research into the interrelation between modality and tense, whose formal similarities as sentence-modifying operators had by then just been recognized. Prior (1967) and subsequently Thomason (1970) developed models for so-called "branching time" in which the tempo-modal notion of an open future serves as the basis for a semantics of both the tenses and the modalities of real possibility and real necessity. In a model of branching time, possible courses of events, also called *histories*, are maximal linear subsets of a

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[10] Fine, in the mentioned work, explicitly excludes real ("historical") modality from his discussion, but gives no reason for this (cf. Fine, 2005, 237n4). This strikes me as odd, since he himself has contributed to the development of the formalities of real possibility; cf. Prior and Fine (1977).

branching tree of open possibilities.[11] A modern description of the branching time framework is given by Belnap et al. (2001, Chap. 6-8).

In terms of formal properties, real possibility is special because of its interaction with the tense operators. We will employ the standard formalisations of "F" for the future operator "it will be the case that" (the past tense "it was the case that" is accordingly symbolized as "P"), and "$\Diamond$" and "$\Box$" for the modal operators "possibly" and "necessarily", respectively. A specific aspect of real possibility is the satisfiability of the formulae

$$\Diamond p \,\&\, \mathrm{F}\neg\Diamond p \tag{F1}$$

and

$$\Diamond p \,\&\, \neg\mathrm{F}\Diamond p, \tag{F2}$$

which express the temporality of real possibility. (F1) says that some p that is now possible, will at some future point in time not be possible any more – a fact that we know all too well, as witnessed by the fact that we sometimes complain about missed opportunities. (F2) is even stronger, saying that p, which is now possible, will cease to be possible immediately in the future – it's now or never, so to speak. Instances of this are also well known.

These formulae are *not* satisfiable if "$\Diamond$" is read as logical or as metaphysical possibility; those modal notions are abstract, without any link with the passage of time. What is logically possible now will remain so forever, and has in fact always been logically possible – if those temporal determinations make any sense at all.[12] For further formal properties of real possibilities based on branching time, cf. again Belnap et al. (2001).

The mentioned formal framework of branching time has been extended in order to overcome one of its major shortcomings: While real possibility is possibility in a concrete and thus concretely localised situation, branching time does not capture that *spatial* aspect. In the extended formal framework of *branching space-times* (BST; Belnap 1992) this aspect is explicitly recognised, as histories (possible courses of events) in that framework do not have the form of a single temporal chain of events, but of a single space-time. In BST it is therefore possible to express the fact that something that is possible here now, is not possible

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[11] Technically, a history in a model of branching time is a maximal linear subset of the tree, i.e., a subset in which any two elements are comparable and which is maximal with respect to that property. Such a set corresponds to a complete path through the tree.

[12] This question is mirrored in the case of mathematics, where there are different opinions as to whether "It is now the case that $2+2=4$" makes any sense at all. – Do not be misled by the fact that, e.g., a logical possibility may be *instantiated* as a real possibility, which then *is* temporal. E.g., it is logically possible that crows fly, and it may be really possible that a certain concrete crow that is now before you should fly within the next five minutes. This, however, is not the same as the mentioned abstract logical possibility, but also depends on many local and temporal factors, e.g., the state of the crow's feathers and the air pressure.

now somewhere else.[13] Belnap's BST is the most advanced formal framework for studying real possibility available to date, and it has been used in a number of applications to problems of metaphysics, philosophy of language, and philosophy of physics.[14]

2.3.3 A model for physical possiblity based on real possibility

Physical possibility, the modal notion that determines the laws of nature, belongs to the same group of abstract, a-temporal modalities as logical and metaphysical possibility: what is physically possible now, has always been physically possible and will remain so forever.[15] Real possibility, on the other hand, is possibility in a concrete, indexically specifiable situation: it is right there before us. The main question about the interrelation of real vs. physical possibility is how scientific practice, which is based on real, concrete experiments and observations, can help us gain access to abstract physical possibility. This question is similar to the question about the interrelation of theory and observation in the sciences, but phrasing it in terms of possibilities gives it an importantly different twist.

Real possibilities rule in the lab and in scientific work generally: Every concrete run of an experiment reveals one of the outcomes that are *really* possible in the given, concrete situation – including, in almost all cases, the real possibility that the experiment may fail due to some sort of interference. Even though experiments thus primarily reveal something about real possibilities, they can sensibly be seen as probes of physical possibility, too. At least that is what experiments are designed for: Generally speaking, in an experiment one wants to find out not about the really, but about the physically possible outcomes, together with their probabilities, of an experimental set-up with given, experimenter-controlled initial conditions. One will therefore disregard certain runs as not pertinent to the question about physical possibility (e.g., because somebody kicked the apparatus), even though the pertinence of these runs for the issue of real possibility cannot be questioned. One will also smooth out the observed distribution of results in various ways. Details vary by case – here a connection with Bogen and Woodward's (1988) data/phenomena distinction suggests itself: physical possibilities appear as phenomena distilled from real possibility figuring as data, with all the well-known

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[13] In view of BST's compatibility with relativity theory, the "now" of course has to be taken with a grain of salt. Technically, possibilities are linked to space-time locations in BST, in a manner that is fully compatible with the absence of a notion of absolute simultaneity in special relativity theory.

[14] Cf., e.g., Belnap (2005) for causation, Weiner and Belnap (2006) and Müller (2005) for objective single-case probabilities, Müller et al. (2008) for modal correlations, Placek and Müller (2007) for counterfactuals, and Müller and Placek (2001) as well as Placek (2009) for Bell-type correlations.

[15] At least this is so if one disregards scenarios in which the laws of nature change over time. I will ignore such scenarios in what follows. The point about abstractness would remain in any case.

idiosyncracies of that step. It is generally acknowledged that there is no formal way of inferring phenomena from data.

Physical possibilities as summed up in laws of nature and physical theories are thus determined via the notion of real possibility that has primacy in scientific practice. In concrete runs of experiments, real possibilities are actualized. Both the concrete initial situation of the respective runs and the concrete outcomes are then described via a number of variables, giving rise to stable, repeatable phenomena. The aim of the experimenter in such a description is to record all salient variables, not everything at all. Physical possibilities (which in a given case may be physical necessities) are then arrived at from real possibilities: so-called laws of nature are established as generalisations covering many experiments, and considerations of saliency again play a crucial role here, as in any case in which phenomena are inferred from data. Statements about laws of nature on this account have an unquestionable modal content: they simply report what is physically possible or necessary, and they are based in real possibilities.

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