Is Philosophy useful for Mathematics and/or vice versa?

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1 Preamble

Thinking about the subject of the conference, I was for some time unsure what to make of it. Since my background is in mathematics, I wanted to consider first the related question "Is Philosophy useful for Mathematics and/or vice versa". I will come back to the original question further below, if time permits.

But now I was faced with the following puzzle:

(1) Is the question about the interaction between philosophy and mathematics (or science) seen as two different disciplines (like eg chemistry and biology)? -

(2) or is the question whether mathematics as a discipline can be the object of philosophical scrutiny and whether the insights that could be gathered from this scrutiny could feed back into mathematical research? -

(3) or are we asking whether the reverse of (2) takes place, namely whether mathematical research can lead to new philosophical insights?

(4) or finally, is the question whether part of mathematical practice - but not mathematics itself - is actually driven by philosophical concerns? In other words, does Philosophy come into play when we try to define the historically shifting boundaries of mathematical research?

So in short, is the question about the overlap between mathematics and philosophy (1)? Or is it about the applications of philosophy to mathematics (2)? About the applications of mathematics to philosophy (3)? Or about mathematical practice, or culture as a natural turf for philosophical thinking (4)?

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To decide which option to choose, it is of course necessary to have working definitions of mathematics and philosophy as disciplines. This is actually difficult in both cases.

The problem of the definition of philosophy is of course a very old one, and this problem is of course paradoxically a philosophical problem as such. In fact, it is often when an honest attempt to negate the very existence of philosophy is made that new philosophical insights are gathered. In the context of this talk, I will consider that philosophy - very loosely - is an attempt to understand the boundary conditions of knowledge, or in other words what constraints our attempts to understand the world faces. This is certainly a problematic definition, and it might not even make sense ultimately because it is to a large extent self-referential - but here again, this kind of criticism would proceed from a typically philosophical thought process. The only concrete definition of philosophy one can give is probably historical rather than conceptual but I do not want to dwell on such issues in this talk.

Now as to the definition of mathematics, this is of course also a hotly debated topic - and it already was one of the main philosophical concerns of the Greeks (eg it is discussed at length in the 13th book of Aristotle's metaphysics). I propose here to see mathematics in the light of its relationship to experimental sciences. Scientific theories are always expressed in mathematical language. Once the theory is thus expressed, it becomes independent of the experiment which gave birth to it, and can be made the object of an independent investigation. Any further results about a mathematical theory expressing a piece of scientific knowledge are then of an objective nature and will hold irrespective of any further experimentation. So in this sense, mathematics is the ultimate goal of scientific knowledge because once a mathematical theory can be formulated, which models an experiment, the link with physical reality is severed.

With these provisional definitions in mind, I will now return to the questions (1), (2), (3) and (4). If you believe these definitions, both philosophy and mathematics have something to do with the very nature of knowledge. In both disciplines, one is concerned with knowledge outside a purely experimental context. However, a basic difference is that a mathematical insight can have real world content. Eg the solution of the differential equation describing the movement of a ball thrown into the air, although obtained in the non-experimental context of mathematics, expresses something that can be tested in the real world. On the other hand, philosophy will not say anything about the real world, but would rather eg be concerned with the conditions that guarantee that a given statement says something about the real world, or about the type of knowledge that we can hope to acquire about it. Interpretation (1) now seems problematic, because it looks like philosophy and mathematics, although somehow fundamentally related, do not operate at the same level (because mathematics can make experimentally verifiable statements, but philosophy cannot). Similarly (2) does not seem to be right tack either, because it superimposes philosophy on mathematics. If such an superposition were possible, mathematics would not be the final answer to scientific knowledge that it is, but it would have to be complemented by philosophy. More concretely, (2) would suggest that conceptual thinking about mathematical content could help in the practice of mathematics (and the solution of problems), and that this conceptual thinking is metamathematical. However, conceptual thinking about mathematical content is part of what mathematical practice is, and is therefore not of a philosophical nature. Eg understanding that the question "Are there any non complete metrics on the field of real numbers" has nothing to do with real numbers and is really a question about any set with cardinality the size of the continuum, is not a philosophical insight, but a mathematical one. Interpretation (3) is also problematic, because if mathematics could feed into philosophical thinking, then we would be in the situation where the solution to a mathematical problem could have philosophical implications. But mathematics is, technically at least, very self-contained and well-defined. The solution of a problem in a precisely defined logical context like, say arithmetics, cannot have implications for much wider questions about the boundaries of our knowledge. So we are left with (4), which is my interpretation of choice.

Since both mathematics and philosophy try to say something about knowledge itself, they must be somehow interact in their cultural and historical development. I will expand on this point of view in the rest of this talk. I would like to stress that I will focus on what I see as the interaction between philosophy and mathematics. I will not focus on the various interpretations of mathematics given by various schools of philosophy (like eg the ancient Greek schools, or German idealism [Kant], or the logicism of Frege-Russell, or Brouwer's intuitionism etc.).

2 A basic example: the axiomatic method

A fundamental example of the interaction between philosophy and mathematics is the introduction of the axiomatic method in geometry by the ancient Greeks. This method is nowadays so much part of our understanding of mathematics that one sometimes does not think that non axiom based mathematics even deserve the name of mathematics. However, as is well known, large parts of mathematics both European and non European, were developed with no axiomatic basis. Calculus was invented and then studied intensively for over two centuries before it was given a proper axiomatic basis. On the other hand, statements of calculus, even if they are given without the proper theoretical underpinning provided by an axiomatic system, still play the role expected of a mathematical statement, in the sense that they are established outside an experimental framework and can then be used to make predictions about the real world. An example here is again given by the differential equation for the movement of an object shot into the air, which was solved early on in the 17th century and has applications in eg ballistics, even though the statement that the solution is a parabola was then not derived from any first principles. The beginnings of the axiomatic method are shrouded in mystery, but whatever its origin, it was introduced to explain what we mean when we assert that we have managed to establish a non experimental, mathematical fact. The step that was taken in the 4th century BC to introduce axioms and proofs in mathematics (by Euclid) is not a mathematical step (because it does not amount to the introduction of new mathematical object, and it is also not a deductive step) but it had a fundamental impact on mathematical practice. I propose that such a step is a specifically philosophical one. If you believe in my description of philosophy as studying the boundary conditions of knowledge, it certainly seems to fall under that heading. Indeed, by introducing the axiomatic method, one tries to define what one means when one claims that mathematical knowledge has been gathered. This method is put forward as a boundary condition for such knowledge: anything asserted outside its framework will not be considered as established in a mathematical context.

As is well-known, the axiomatic method has a long and distinguished history. After the invention of calculus, it was for a time put on pause. In the early 19th century, the abstract problem of the independence of the Euclidean parallel postulate from the other axioms of Euclidean geometry led to the work of Lobachevsky, Bolyai (and also unpublished work of Gauss) on non Euclidean geometries, and later to the proof by Beltrami and Klein that the parallel postulate cannot be deduced from the other axioms (by establishing the existence of hyperbolic models of Lobachevskian non Euclidean geometry). This is a major new step in the history of the axiomatic method, because the question of the independence of the axioms of a mathematical system had (to my knowledge) never been asked or answered before - although I wouldn't be surprised if one discovered that Leibniz had foreseen this.

In the early 20th century, the axiomatic ends up pervading all of mathematics via the axiomatisation of set theory by Zermelo-Fraenkel, which proposed to provide an axiomatic basis for all of mathematics, and in particular for analysis. Later on, 1st order logic was introduced and axiomatic theories, together with independence problems and other questions about axioms, are studied in an abstract setting. This is the end of the journey for this method, which has now gone beyond being merely used, and has led to the creation of a new mathematical framework, namely 1st order logic.

3 Second example: intrinsic and extrinsic

Here is another example. At the end of the 19th century, a new approach to questions of commutative algebra emerged, which advocated the use of intrinsic as opposed to extrinsic methods.

An emblematic example of this is Hilbert's proof of what is now called "Hilbert's basis theorem." This theorem in particular implies that a ring of polynomials in several variables over a field is noetherian, meaning that an ideal of polynomials (ie a set closed under multiplication by any polynomials, and closed under addition) always has a finite set of generators (generators of an ideal are polynomials, such that any other ideal containing them also contains the ideal). It had been conjectured for a long time that this theorem should hold, but the methods used before Hilbert heavily depended on the structure of the ring of polynomials, and also insisted on being constructive. Hilbert's proof used only the noetherian property and was based on the insight that it was sufficient to show that a polynomial ring in one variable over a noetherian ring is itself again noetherian. He provided a proof for this, thus reducing the proof to a simple induction on the number of variables. Paul Gordan, a German mathematician who had worked on the problem for a long time (and provided partial proofs, for small numbers of variables) and who applied methods from a field called Invariant Theory, is said upon seeing Hilbert's proof to have cried out "This is theology, not mathematics." What is remarkable about Hilbert's method is that he insisted on using some specific structural properties, in order to increase the generality of the problem posed. Earlier approaches were happy to work in a very specific context (ie a polynomial ring over field, not any noetherian ring) and to avail themselves of some properties or facts that were only available in that context. While this had the advantage of providing more facts to work with, it also hid the central structural properties relevant to the problem. Hilbert's approach can be described as intrinsic, in the sense that it focusses on the structural properties relevant to a problem, whereas the earlier approaches can be termed extrinsic, because although they were concerned with the same structural properties, they would not refrain from making use of facts only available in a special case.

Here is a much simpler example. Suppose that one wanted to show that the map $x \mapsto \frac{x}{2}$ on the real line has a unique fixed a point. One way is to do is to notice that the equation $x = x/2$ is equivalent to $x(1 - 1/2) = x/2 = 0$ and thus that $x = 0$ (after multiplication by 2). This proof cannot be adapted to more general maps from the real line to itself and avails itself of all the specifics of the definition of the map. On the other hand, one could ask oneself in the same context whether any map from the real line to itself, which contracts distances, has a fixed point. This happens to be true and is a special case of Banach's fixed point theorem, which asserts that any map from a complete metric space to itself, which contracts distances, has a unique fixed point. The first method would be analogous the earlier approach to Hilbert's basis theorem, in the sense that it provides a solution to the problem of fixed points of contracting maps in a special situation using all the particulars of that situation, whereas Banach's fixed point theorem insists on giving a proof trimmed of any local details.

In the 20th century, a general shift from extrinsic to intrinsic methods can be observed in mathematics. This can be seen in the new definition of a manifold (eg in Bourbaki's treatise on Differentiable Manifolds), which is defined as a topological space with certain properties, rather than as a subset of some finite-dimensional real vector space (the vector space not being relevant to the geometry of the manifold). It can also be seen in the new definition of algebraic varieties by Grothendieck and his school, where ambient projective spaces do not play a role anymore. Finally, intrinsic methods are also at play in the development of commutative algebra (especially in the work of E. Noether), where the focus shifts from quotients of polynomials rings by ideals with given generators to general commutative rings with minimal finiteness properties.

This new distinction between intrinsic and extrinsic methods and concepts changed mathematical practice, although it is not in itself a mathematical step. One could argue that Hilbert's proof of the basis theorem is simply a solution to an old problem based on a new insight, and that in this sense it simply amounts to an understanding of what is needed for the proof. However, in general, intrinsic methods are not always more efficient than extrinsic ones (eg differential equations on a manifold are often best handled locally, in a coordinate chart, rather than through an intrinsic global language, which sometimes obscures the relevant points). The important point here is that it has now become standard to ask oneself whether a given notion depends on the specifics of a mathematical situation. The first reflex of a modern mathematician, when faced with, say, the definition of non-singularity of a complex variety in terms of Jacobian matrices in a coordinate chart, is to ask whether there is a definition of non-singularity, which does not in its formulation depend on the choice of a chart. In this sense, the distinction between intrinsic and extrinsic modifies mathematical practice, although the terms *intrinsic* and *extrinsic* are not mathematical terms. The introduction of this distinction should be understood as a philosophical development. It is certainly philosophical according to the loose definition I gave of philosophy above, since behind it is the idea that the dichotomy intrinsic/extrinsic draws something like a fault line across our mathematical knowledge, that it would be perilous to ignore.

However, as in the case of the axiomatic method, which ultimately led to a study of axiomatic systems in abstracto via 1st order logic, the strive for the intrinsic also led to a mathematical framework, which attempts to catch the essence of this notion: this is category theory. A category is a universe of objects in which one chooses to work. One then asks what general properties of the category suffice to guarantee that its objects have certain properties. Once one has chosen to work in a certain category, it is only possible to use its general properties, and extrinsic methods simply cannot be applied anymore. In particular, in the language of categories, one cannot look at a specific object. One can only talk about properties characterising certain classes of objects (so-called universal properties). In this sense, category theory cancels the very individuality of an object and therefore everything that could lend itself to an extrinsic reasoning. A categorical approach is eg at work in the modern approach to linear algebra. In that approach, one looks at a category with certain properties encapsulating the properties of finite-dimensional spaces and linear maps between them. When making a reasoning, one never looks at a specific vector space. This should be contrasted with the old approach, where linear algebra is about matrices. In that case, one always chooses to identify a finite-dimensional vector space over a field k with k^n for some $n \geq 0$. This identification requires a choice of basis, which is arbitrary.

4 The nature of the interaction between philosophy and mathematics

I would now like to make some comments on the nature of the interaction between philosophy and mathematics, basing myself on the two examples above. Before I start, I would like to point out that there are many other examples of such an interaction. For instance, one could consider the development of set theory as a basic language for mathematics. Another example, which is linked to the last one of course, is Cantor's study of the infinite. One could also look at the development of the concept of algorithm and of the notion of Turing machine. Finally, one could review the various attempts that have been made in 20th century mathematics to understand the notions of identity and equality in mathematics. I do not have the time here (and I probably also lack the competence) to give a more detailed account of all these examples here.

If one looks at the example of the axiomatic method, one can see three stages in the historical development of this method in mathematics. In the first one, it is introduced in the special case of Euclidean geometry. In that situation, no questions are asked about the method itself. It is simply used. In the second stage, non-Euclidean geometry, an abstract question about the axiomatic system of Euclidean geometry - the independence of the parallel postulate - is raised. This is the first sign of an attempt to understand this method in greater generality. Finally, a new mathematical framework to describe axiomatic systems is created - this is 1st order logic.

Something similar happens with the dichotomy between intrinsic and extrinsic. This dichotomy appears first in Hilbert's work on commutative algebra, when he consciously insists on building a proof relying only on basic structural properties. In the first part of 20th century, and in particular in the work of the Bourbaki group, it then becomes standard to require minimal structural definitions of new mathematical objects. Finally, in the wake of the work of Eilenberg and MacLane and under the impulse of Grothendieck, category theory was created and developed. As we have seen before, this is a mathematical framework in which intrinsic properties can be studied in vivo.

This suggests that a certain epistemological process is at work when philosophical notions take hold in mathematical practice and then develop. I propose that this process can be best understood in terms of an epistemological framework proposed by the neo-Kantian philosopher E. Cassirer. I will quickly recall its basic constituants below and I will then make some comments on its applicability in our context.

5 Cassirer's symbolic forms

E. Cassirer proposed in his book 'The Philosophy of Symbolic Forms' that human knowledge has a certain structure, which becomes apparent when one looks at its unfolding in history. According to him, knowledge has three phases: an expressive phase ('Ausdrucksfunktion'), a representative phase ('Darstellungsfunktion') and a semantic phase ('Bedeutungsfunktion'). When these three phases are completed, one reaches a 'symbolic' knowledge, where the three phases are one, but yet still feed each other. A historical instance of symbolic knowledge is then called a 'symbolic form'. To explain what Cassirer means by the words 'expressive', 'representative' and 'semantic', the best is to look at his foundational example, which is language. The 'expressive' phase of language is its onomatopoeic component, ie the fact that many words actually reproduce the sound of what they refer to. Its 'representative' phase is its purely representative function, ie the fact that many words refer to something by convention. These words might have have an onomatopoeic origin, but it has been lost. The 'semantic' function of language is its grammatical and logical structure, ie the fact that words have to be organised in sentences and according to certain rules. A fully developed human language is then a prime example of a 'symbolic' form. The 'expressive' phase is thus a phase of knowledge, where a residue of the reality that is represented is still present. The 'representative' one is a phase where a basic correspondence with reality is still present, but on the other hand, knowledge is already somewhat removed from reality. In the 'semantic' phase, knowledge reaches a kind of structural autonomy. The unity of the three phases, which Cassirer insists upon, is very apparent in language. Indeed, when we use language, all three phases are present at once. Cassirer calls full knowledge 'symbolic', because for him a 'symbol' carries all three phases in itself. For example, a word in a language is a prototypical symbol. It relates to the outside world, through its onomatopoeic origin, but it also more universal through the fact that its use in a social group is a convention, and then finally it can be used within a complex grammatical structure.

Cassirer's philosophy of symbolic forms should not be considered, in my view, as a systematic epistemology. The interest of his philosophy is that it provides a lens through which one can look at a large range of cultural developments. In a large part of his book, he sieves through historical and ethnological data to find examples of symbolic knowledge in various geographical and social contexts. This is unlike many treatises of philosophy, which spend more time constructing a theory than illustrating it.

Mathematics itself is a rich repository of symbolic forms. Here is a simple example: arithmetics. At the outset, arithmetics is a system of numeration which mimics our experience of the multiplicity of objects in the outside world. Eg small Roman numerals represent a multiplicity by groups of vertical bars. This is the expressive phase. The representative phase of arithmetic can be seen in the Arabic notation for numbers, where the numbers do not literally express multiplicity anymore. Finally, the semantic phase is the mathematical theory of integers.

But I shall not dwell on examples of symbolic forms in mathematics themselves.

What I would like to suggest is that the interaction between philosophy and mathematics follows the pattern mapped by Cassirer's philosophy. In fact, it looks like throughout its history, mathematics has become the object of a certain kind of philosophical knowledge, which unfolds through symbolic forms.

The axiomatic method is a prime example of this phenomenon. In their original incarnation, axioms in Euclidean geometry were understood to be self-evident statements, not statements arbitrarily chosen to be the starting point of the theory. This self-evidence had its root in the fact that Euclidean geometry was supposed to model part of the physical world, and the axioms reflected basic aspects of our experience of the spatial world. This is the expressive phase. On the other hand, in the development of Lobatchevskian geometry, one clearly moves away from physical geometry and the axioms are not immediately grounded in experience anymore. However, the axioms of Euclidean geometry outside of the parallel postulate are still retained so there is still a bridge to physical geometry. This is the representative phase. Finally, in the study of first order theories which is the object of mathematical logic, not even the historical link to Euclidean geometry that still existed in the work of Lobatchevsky and Bolyai persists. This is the semantic phase, where the notion of axiomatic system has acquired a kind of dialectical independence. So we see that the philosophical understanding of the notion of axiomatic system in mathematics historically develops as a symbolic form.

Similarly, the development of intrinsic methods in mathematics also follows Cassirer's pattern. In Hilbert's original 1890 paper "Über die Theorie der Algebraischen Formen", only polynomial rings over fields are considered, although the method can be generalised without much effort to general noetherian rings. So in this sense, Hilbert's understanding of "intrinsincness" is still rooted in the very rings that led him to think in this novel way. On the other hand, in her classic paper "Idealtheorie in Ringbereichen", E. Noether defines the notion of noetherian ring in full generality and the link with the original motivating polynomial rings is lost. However, a bridge to polynomial rings is still present, because these rings are noetherian rings. Finally, in a categorical context, the notion of intrinsic property has acquired the semantic independence that is characteristic of the last stage of a symbolic form.

6 The two faces of mathematics

I hope that I have made a convincing point of the fact that mathematics is a natural ground for philosophical thinking and that this philosophical thinking unfolds in ways that are similar to other epistemological contexts, like indeed the mathematical field itself. In our mathematical practice, we are thus under the dual pressure of mathematical problem solving on the one hand and of the paradigmatic shifting effected on mathematics by philosophical thinking. As I argued above, both aspects of mathematical culture correspond to different concerns however, and in this sense they cannot support each other. In other words, solving problems in mathematics will not answer any philosophical questions, and a paradigmatic shift in mathematics does not contain as such a solution to a mathematical problem. So what of the original question, "Is Philosophy useful for Mathematics and/or vice versa?"? My answer is that Philosophy is very important for Mathematics, because without it, the mathematical field would not expand. On the other hand, Mathematics is useful for Philosophy because mathematical culture is a fertile ground for the development of philosophical ideas.

However, is philosophical thinking around mathematics only a kind of intellectual luxury, from the point of view of mathematical practice, or is it essential for the development of the mathematical sciences? As I pointed out above, the field of mathematical research would be less wide without specifically philosophical thinking, but on the other hand, one could argue that the bulk of mathematics has not been much affected by this expansion. For instance, problems of applied mathematics do not require categories or 1st order logic. The analytical framework of the 18th century would often suffice for such problems. On the other hand, one could argue that the thrust in a problem of applied mathematics is not really mathematical: the aim here is to solve problems, which come from outside the mathematical field. There is nothing wrong with this, of course, and in fact, as I said above, it is in the context of real world problems that mathematics was borne. However, when considering an applied problem, one is not engaged in a purely mathematical pursuit. So in this sense, the insensitivity of applied mathematics to philosophical developments is not really relevant. There are however other parts of mathematics, which are definitely not motivated by real world problems, but where philosophical thinking does not seem to play much of a role either. One example of such a field is analytic number theory. Here again, the framework of early 19th century analysis is basically sufficient. Also, from its beginnings, unlike eg in the case of Commutative Algebra or Algebraic Geometry, the dialectical range of Analytic Number Theory has not expanded. The concerns of Dirichlet are very much the same as those of Hardy and Littlewood a century later, even if from a technical point of view, the Riemann Hypothesis has moved to the foreground. It is only the set of techniques which has expanded. The same could probably be said of the field of Graph Theory, and more generally combinatorics (but I confess that my knowledge is very limited here). So what does this say about the work of mathematicians working in various parts of mathematics? Is it so that in some parts of mathematics, in practice very often the parts with algebraic rather than analytic concerns, the general take on what mathematical practice is differs from the one of other parts? Is this dichotomy a kind of accident? I do not really know how to make sense of this but I think that this dichotomy reflects a very basic polarity in mathematical practice.

In the context of this question, I would like to quote a very interesting text by the mathematician T. Gowers, "The two cultures of mathematics", which addresses part of this issue. Here is an extract:

'... "The 'two cultures' I wish to discuss will be familiar to all professional mathematicians. Loosely speaking, I mean the distinction between mathematicians who regard their central aim as being to solve problems, and those who are more concerned with building and understanding theories. This difference of attitude has been remarked on by many people, and I do not claim any credit for noticing it. As with most categorizations, it involves a certain oversimplification, but not so much as to make it useless. If you are unsure to which class you belong, then consider the following two statements.

(i) The point of solving problems is to understand mathematics better.

(ii) The point of understanding mathematics is to become better able to solve problems.

Most mathematicians would say that there is truth in both (i) and (ii). Not all problems are equally interesting, and one way of distinguishing the more interesting ones is to demonstrate that they improve our understanding of mathematics as a whole. Equally, if somebody spends many years struggling to understand a difficult area of mathematics, but does not actually do anything with this understanding, then why should anybody else care? However, many, and perhaps most, mathematicians will not agree equally strongly with the two statements.'

In the context of this extract, I would like to suggest that a philosophical development in mathematics is very much related to the question of 'understanding' mathematics. A philosophical development clarifies in some way what it is that we mean when say that we 'understand' a part of mathematics. From this perspective, a part of mathematics where little philosophical development has taken place is likely to be more fuzzy as far as its general aims are, although these aims, if at all formulated, would not be specifically philosophical. It is striking that in some areas of mathematics (like combinatorics or analytic number theory), some problems seem to be put forward in a rather random fashion. Eg it is often difficult to understand what motivates the many elementary problems of combinatorics put forward by P. Erdos in his long career. Similarly, in analytic number theory, many results seem to coexist, with little conceptual unity. At the other extreme, one may consider algebraic geometry, which is completely dominated by the question of the existence of enough algebraic cycles, and the conjectural existence of a reasonable category of motives. Even if these questions are not as such philosophical, algebraic geometry in its present form would not exist without the important philosophical developments that led to the categorical notions which now lie at the foundation of this field.

So to summarise, the mathematicians who would believe that the point of solving problems is to understand mathematics better ((i) above) are likely to be involved in a part of mathematics which has undergone a philosophical paradigmatic shift. Whether (i) or (ii) is the better choice is of course not for me to say but it seems to be quite a fundamental dichotomy.

7 What about experimental sciences?

With the discussion above about the interaction between philosophy and mathematics in mind, one might ask whether this discussion has any bearing on the more general topic of the conference, which is about the usefulness of philosophy and science for each other. I do not presume to be able to speak competently about this question, but I would like to suggest that the situation of mathematics might shed some light on it. The reason for this is the fact that sciences are formulated in terms of mathematical theories, and thus anything that is said about mathematics must therefore feed into part of the general scientific discourse.

Here is an example of a situation, where physical, mathematical and philosophical concerns are all simultaneously present. Consider the problem of the formulation of the foundation of classical quantum theory. The classical formulation is the one given by J. von Neumann in his book 'Mathematical Foundations of Quantum Mechanics'. In this book, the basic object of study of quantum mechanics is defined to be a space of functions on the physical space $\mathbf{R}^3$, which are integrable in suitable sense. The various observables (energy, momentum, speed etc.) are then encoded as certain (unbounded) linear operators on this space. This formulation is very efficient and is constantly used by physicists and chemists. However, one could argue that there is a conceptual flaw in this formulation, because the space of integrable functions that it introduces is modelled on a classical (ie non quantum) description of space. From a physical point of view, the very space $\mathbf{R}^3$ corresponds to a classical and not a quantum point of view because it is modelled on our macroscopic (ie Newtonian) experience of the world around us, where we can compute the coordinates of an object in space precisely. So in this sense, von Neumann's description of quantum mechanics is a little topsy turvy, because its formulation uses an object whose appearance is justified by a non quantum experience of the world (quote from a paper by Isham and Butterfield on the foundations of quantum gravity: "... there is a danger of certain a priori, classical ideas about space and time being used unthinkingly in the very formulation of quantum theory..."). Many attempts were made later to reformulate quantum mechanics, for instance using instead the language of topoi introduced by A. Grothendieck (see eg paper by Isham and Butterfield). This development in mathematical physics can again be construed as a specifically philosophical one, because it is motivated by an epistemological concern, not a mathematical or physical one.

This suggests that philosophy can also work in tandem with other sciences, possibly still working on the platform of the mathematical theories that arise from these sciences, and possibly following the symbolic pattern outlined above.

One final remark.

One might ask whether there is a link between the uncanny collaboration between philosophy and mathematics which I described above and the notion of "paradigm shift" famously coined by Thomas Kuhn in his book "The structure of scientific revolutions". This would require a more in-depth discussion but I would like to suggest that Thomas Kuhn's "paradigm shift" is of a different nature. Indeed, a kuhnian "paradigm shift" is a change of practice in a science, which takes place as a result of accumulated experimental evidence. For instance, the introduction of atomic theory in the physical theory of matter is a paradigm shift, which resulted from the incapacity of pre-atomic theory to explain a whole range of phenomena. In that sense, a kuhnian paradigm shift operates on the same level as scientific research itself, it is not qualitatively distinct from it. By contrast, the interaction between philosophy and mathematics is an interaction between two disciplines with different concerns. The fact that each of these disciplines is ultimately relevant for the other one cannot, I believe, be explained mathematically, not can it be explained philosophically. It is a mysterious cultural reality, which can be witnessed but cannot be subsumed by another principle.