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An Analysis of the Problem of the Applicability of Mathematics

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To my mother, my father, my brother.

Contents

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Introduction

We cannot literally take a number in our hands and 'apply' it to a physical object.

Patrick Suppes

If all you have is a hammer, everything looks like a nail.

Popular adage

The problem I am going to deal with in this work is often briefly summarized in the following way: why is mathematics applied and applicable to physics (and to science in general)? However, this is a very misleading way to set the problem. It gives the erroneous impression that we are handling two different areas of knowledge, mathematics and physics; that these two areas are mutually separated and independent; and that we are asking why the former can be profitably applied to the latter.

There is no doubt that mathematics is actually a field of knowledge independent from physics; but it is surely wrong to say the reciprocal for the physics. There is nothing like a 'nonmathematical physics' to which we 'apply' mathematics. Surely, we can provide the example of the Aristotelian physics, which was to all intents and purposes a "physics" (at least if by that we mean a complex of propositions aimed to describe and understand the emprical phenomena) and didn't make use of any mathematics at all. But nowadays physics is surely not *aristotelian physics + mathematics*. Indeed, the Scientific Revolution in XVII century had a deep impact on the conception and practice of physics — an impact that cannot be summarized by simply saying that physicists started to use mathematics.

The point that I want to emphisize is that, from the Scientific Revolution up to now, the *physical concepts themselves* are strictly interwoven with mathematical ones. So, in a certain sense, there is no "application" at all. Rather, we must account for the very fact that physical concepts are *mathematically shaped*. The mathematics operates *within* the physics; it is not something that is just sticked to it from the outside. As Dyson (1964) points out,

> For a physicist mathematics is not just a tool by means of which phenomena can be calculated, it is the main source of concepts and principles by means of which new theories can be created. (p. 129)

As a consequence of this strict twine between physics and mathematics, it often happens that a clear distinction between physical and mathematical concepts is impossible to draw in a precise manner.

> [T]here is no theoretical way of drawing a sharp distinction between a piece of pure mathematics and a piece of theoretical science. The set-theoretical definitions of the theory of mechanics, the theory of thermodynamics, the theory of learning, to give three rather disparate examples, are on all fours with the definitions of the purely mathematical theories of groups, rings, fields, etc. From the philosophical standpoint there is no sharp distinction between pure and applied mathematics, in spite of much talk to the contrary. (Suppes 1967*a*, pp. 29–30)

It follows that, if we want to take these quotations seriously, the problem cannot be seen as a problem only for the philosophy of mathematics. The topic has a boder-line feature. Its solution cannot depend only on a survey concerning the nature of mathematics. What we are trying to understand is the role of mathematics in the scientific enterprise; so, we are interested in those features of scientific enterprise that ask and justify the employment of mathematics.

In a very general way, we can distinguish between two contributions to our discussion. On the one side, we must point out (A) which features internal to mathematics are of some interest to the physicist. On the other side, we must show (B) how these features can be integrated in the representational production of physics, and hence how these features can help to reach this representing aim.

In contemporary philosophy of mathematics, the role of mathematics in science is often mentioned as a premise for drawing conclusion of various kinds. The importance and centrality of this role is so well emphasized — but it is notwithstanding still not well understood. And not only it is still not well understood, but it seems that the efforts to clarify this role aren't even proportional to the number of mentions. Just to give an example, there is a lot of works concerning the so called indispensability argument, whose fundamental premise says that mathematics has an *indispensable* role in our (best) scientific theories. But none of these works takes the trouble to go deeper into the question and to clarify *why* mathematics is really indispensable.

Thus, contemporary philosophers rarely dealt with the *real philosophical problems* that applicability of mathematics raises.[1] Such a circumstance is quite odd, since until Frege these problem were well present in the philosopher's discussions. The dismissal of such a problem is thus quite recent and one might legitimately ask why the problem has been dismissed.

In the next chapters I will deal with all these problems and I will try to offer some relevant contributions to their analysis. The present work is divided into three parts. The first two parts aim to understand the nature and the philosophical significance of the problem of mathematical applicability. Actually, one might raise the question: Is really there a problem here — a problem concerning the applicability of mathematics? And if there really is a problem, is it a *philosophical* problem? That Frege and many logicists thought, until the

[1] This is true *apart from some exceptions*, of course.

beginning of the 20^{th} century, that their philosophy of mathematics could offer a significant and satisfying answer to the question about why mathematics is so useful to science, and the fact that the problem has been dismissed after them, makes the previous questions even more urgent.

Thus, in the first part (HISTORICAL CONSIDERATIONS), I will deal with the historical problem of understanding why the applicability problem has been dismissed after Frege's and logicists' analysis (Chapter 1: *A Neglected Problem*). I will show that their answer is no longer satisfying and that such a dismissal was not due to a real overcoming of the problem.

The second part (PHILOSOPHICAL PROBLEMS) will be devoted to the analysis of the specific philosophical problems lying behind the applicability of mathematics. First I will discuss Wigner's (1960) famous analysis (Chapter 2: *Do Miracles Occur?*), and then I will deal with Steiner's (1998) fundamental work on the topic (Chapter 3: *Applicabilities of Mathematics*). As we will see, there are *many* philosophical problems concerning the applicability of mathematics. A further chapter (Chapter 4: *Applicability and Ontological Issues*) will be devoted to an analysis of the relations between ontological questions in mathematics and its applicability and effectiveness in science, in order to remove any misunderstanding about the possibility that the problems of mathematical applicability are nothing but a consequence of a certain ontological choice.

Finally, in the third part (AN ACCOUNT FOR MATHEMATICAL REPRESENTATIVENESS) I will offer an original account for one of the main roles played by mathematics in science: the *representative* role, which is at the very base of so many scientific discoveries and improvements in contemporary physics. First, (Chapter 5: *Structures and Applicability*) I will present my account in a purely theoretical way, and then I will offer some concrete examples in support of such an account (Chapter 6: *Some Concrete Examples*).

Part I

Historical Considerations

Chapter 1

A Neglected Problem

The philosophical reflection has always paid a fair amount of attention to the role of mathematics in the physical knowledge. From Plato to Frege, moving through Galileo, Berkeley, Kant, and many others, philosophy has always been interested in the relations between mathematics and the knowledge of the external world. However, if we give a look to the philosophical reflection from the middle of the 20$^{\text{th}}$ century up to now, we can notice that this problem has been progressively dismissed — that contemporary philosophy (both of science and mathematics) has forgotten the problem, and that it is only in the very last years that it has known a timid revival. During all this time, there has been no lack of physicians and mathematicians who expressed a vague wonder for the amazing effectiveness of mathematics in physics[1] — but no philosophical reflection followed up this wonder.[2]

Mark Steiner, in his (1998) underlines this neglect by pointing out that (Benacerraf & Putnam 1983) — which can be legitimately considered the most important 20$^{\text{th}}$ century anthology about philosophy of mathematics — com-

[1] See chapter 2.

[2] A relevant exception to this claim, which deserves to be mentioned, is Körner (2009 [1960]).

prises no article concerning mathematical application or applicability. Steiner reports that, when he reproached Benacerraf for it, Benacerraf answered that lack of material was the reason for this gap.[3] The aim of this chapter consists just in trying to find a historical reason for this philosophical neglect, and contextually in showing that this neglect cannot be overlooked any further.

This neglect seems to have a quite precise cronological collocation. It is a matter of fact that early in the 20$^{\text{th}}$ century the applicability problem was still, at least superficially, addressed. This is well testified by the quotations from Russell, Frege and Hahn that we shall read further on in this chapter. So, we can agree with Mark Steiner when he says that «the disregard by the philosophical community of issues of mathematical application is quite recent».[4] It is even more surprising if we think that it was just in the second half of the 20$^{\text{th}}$ century that mathematics gave its most important contribute to the physical inquiry.[5] So, what did it happen in the middle of the 20$^{\text{th}}$ century?

1.1 Carnap and the Logical Empiricism

Wilholt (2006) tries to give an answer to this question. He notices a suspicious coincidence between the disappearance of the applicability problem and the ascent and heyday of Logical Empiricism, and finds a cause for this neglect in the history of this philosophical current. By examining the idea of the early logicians who held that the analyticity of mathematics could account for its applicability, he points out that this idea has been transformed through Carnap's efforts to establish a consistent philosophy of mathematics within the framework of Logical Empiricism. This transformation concerned the notion of analyticity

[3] See (Steiner 1998, p. 14n). As Steiner remarks, this book was actully his «final atttempt to persuade my advisor and mentor, Professor Paul Benacerraf, that there really *is* a philosophical problem about the applicability of mathematics in natural science» (p. vii).

[4] (Steiner 2005, p. 625).

[5] I'm thinking here particularly to the introduction of algebraic methods in physics (theory of groups) and to the widespread practice of mathematical modelizing.

and the result of it was that the new notion was no more able to account for the applicability of mathematics — as the original was supposed to do.

Let's retrace with Wilholt this interesting philosophical development. The general tendency of the first logical positivists was to belittle the value of the applicability problem by levering on the tautological character of mathematical statements. In a paper published in 1933, Hans Hahn draws the following conclusion from it:

> [...] now one should realize how vastly far apart our view is from the old — maybe one may say platonizing — view that the world is constructed according to the laws of logic and mathematics [...] and that with our thinking [...] we have been given a means to grasp these eternal laws of the world. No! It is no reality that our thinking can grasp, no fact of the world that thinking can bring us lore about; it only refers to the way we talk about the world; it can only transform tautologically what has been said. (Hahn 1988 [1933], p. 160)[6]

Much can be said about this strategy of solution (or, it would be better to say, of 'obliteration') of the problem. But it is quite uncontroversial that this was the strategy widely adopted by the first logical positivists. Ayer (1956 [1936]) seems to be on the same line of thought when he says that «the truths of logic and mathematics are analytic propositions or tautologies» (p. 41):

> The power of logic and mathematics to surprise us depends, like their usefulness, on the limitations of our reason. A being whose intellect was infinitely powerful would take no interest in logic and mathematics. For he would be able to see at a glance everything that his definitions implied, and, accordingly, could never learn anything from logical inference which he was not fully conscious of already. But our intellects are not of this order. It is only a minute proportion of the consequences of our definitions that we are able to detect at a glance. Even so simple a tautology as '$91 \times 79 = 7189$' is beyond the scope of our immediate apprehension. To assure ourselves that '7189' is synonymous with '91×79' we have to resort to calculation, which is simply a process of tautological transformation — that is, a process by which we change the form of expressions without altering their significance. (p. 48)

This tautological character of mathematical statements echoes the doctrine of logicism, which had a great influence on the works of the first logical posi-

[6] Wilholt's translation from the original.

tivists. Frege and Russell were well aware of the fact that no account of mathematics can be considered satisfactory if it makes the applicability of mathematics a deep mystery.

> [W]e want our numbers — *Russell says* — to be such as can be used for counting common objects, and this requires that our numbers should have a definite meaning, not merely that they should have certain formal properties. This definite meaning is defined by the logical theory of arithmetic. (Russell 1993, p. 10)

Frege, particularly, was persuaded that the need to account for applicability of mathematics pushes us to say that mathematical statements have a cognitive content. The only difference between mathematical statements and, let's say, a formation of chess-pieces consists in the fact that the former are *useful* and can be profitably applied to other branches of science, while the latter don't. But *why* is it so? Because mathematical statements (e.g., arithmetical equations) express thoughts. Mathematics is not a mere formal game (as formalists want) because it is *useful* — and it is useful because it has cognitive content.

This "cognitive content" was supplied by Frege by means of a definition of mathematical concepts in terms of logical concepts. E.g., positive whole numbers are applicable to concepts,[7] such that, for every concept F, the number of F's is identical with the extension of the concept "equinumerous with F" — where "equinumerous" is in turn definable in terms of a one-to-one correspondence between extensions of concepts, which is surely a logical concept if the theory of concept extension is a part of logic. Arithmetical truths would be therefore analytical truths.

Russell and Whitehead provided a different version, couched in terms of classes and types rather than extensions of concepts, but the outcome was exactly the same: arithmetic is equally defined by means of logical terms, so that arithmetical truths are therefore logical (analytical) truths. Thus, in the logicist

[7]This is an important point in Frege's account of applicability of arithmetic. Arithmetical concepts apply to *concepts*, not to objects. As we shall see in chapter 3, this is the way in which Frege solves what Steiner calls the "semantic problem of applicability" for arithmetic.

account, the applicability of arithmetic (and, by extension, of mathematics) is justified by the purely logical character of the arithmetical concepts, so that their application is no more surprising than the application of a logical concept. The relevant point consists in the fact that the arithmetical statements are truths concerning *concepts*, not objects.

> Arithmetic thus becomes simply a development of logic, and every proposition of arithmetic a law of logic, albeit a derivative one. To apply arithmetic in the physical sciences is to bring logic to bear on observed facts; calculation becomes deduction. [...]. The laws of number, therefore, are not really applicable to external things; they are not laws of nature. They are, however, applicable to judgements holding good of things in the external world: they are laws of the laws of nature. (Frege 1980 [1884], p. 99)

It follows, as it is clear, that arithmetic will be applicable in every context where concepts (having a finite extension) appear. Thus, in Frege's original version of logicism the question "Why is arithmetic applicable?" gets the following response:

> (A-F) Arithmetic is applicable because it is a body of useful truths about concepts of finite extension; and we apply concepts whenever we make judgments about the external world; and a great many of them have finite extensions. (Wilholt 2006, p. 75)

In this account — it must be noted — there are two distinct elements that contribute to the success of the explanation: (A) the analyticity of the arithmetical truths and (B) the reduction of arithmetic to logical terms, namely, to (logical) laws of concepts. (A) guarantees that the application of arithmetic is conservative, i.e. that it does not lead to true premises to false conclusions; and (B) enables us to apply arithmetic everywhere the proper concepts appear. If these concepts rigthly apply to the objects of the external world that we are investigating (and if these concepts have a finite extension), then the laws of arithmetic rightly apply (can be applied) to these concepts.[8]

[8]Wilholt seems to link very closely (B) to the logicist reduction of arithmetic. «This reduction — he says — was the core of the original logicist explanation of the applicability of

So, when Hahn and Ayer invoke the analyticity of mathematical statements in order to explain their applicability, they are invoking exactly *this* kind of analyticity, I mean analyticity as the outcome of a (Fregean) logicist reduction.

However, as is well known, this logistic reduction of mathematics to logical terms encountered soon several difficulties: Frege's theory of extensions of concepts turned out to be inconsistent, and all the following attempts to avoid inconsistencies (theory of types, axiomatic set theories) had the problem of introducing premises or axioms (like the axiom of reducibility, the axiom of infinity and the axiom of choice) that seemed to be not purely logical truths. These difficulties ultimately determined the end of the logicist program.

The problem, at this point of the story, is paraphrased by Wilholt in the following way:

> [since] this reduction was the core of the original logicist explanation of applicability of mathematics [...] how could the view that the applicability problem was no longer a problem survive the demise of reductive logicism? (Wilholt 2006, p. 73)

In order to shed light on this, Wilholt suggests «to look at the account of that empirist who paid the most scrupulous attention to the intricacies of such question, i.e., [...], Carnap» (p. 73).

In his (2001 [1934]), Carnap formulates his famous *Principle of Tolerance*: «It is not our business to set up prohibitions, but to arrive at conventions» (p. 51). In other words, this principle says that there is no authoritative logic, but many possible logics; everyone is free to embrace his own logic, justifying it by means of its pragmatical utility. Among the possible languages that we can choose, there are also languages that contain the above mentioned axioms of infinity

mathematics» (Wilholt 2006, p. 73). However, we must distinguish two different components of (B): (B.1) the logicist reduction, namely the fact that arithmetic is reduced to logic; and (B.2) the fact that this reduction is made such in a way that the arithmetical concepts result to be concepts of concepts (and hence applicable only to concepts and not to objects). It is only the second point (B.2) that has an explanatory value regarding the applicability of mathematics. (B.2) can effectively be revived even in the absence of the point (B.1). That's what acutally Steiner proposes in (1998). Concerning this see chapter 3.

and of choice. Carnap formulates the rules and principles for such a language — that he calls "language II".[9] The idea of reducing mathematics to pure logic is thus abandoned, for there is no clear distinction between the former and the latter: both are equally constitutive of a form of language. It follows that Carnap cannot any longer defend a Fregean conception of analyticity. Mathematical truths are not analytic in the sense that they are reducible to logical truths; they are analytic simply in virtue of our adoption of a form of language.

In this way, Carnap manages to save the analyticity of the mathematical truths, and can answer the question "What makes arithmetical sentences *true*?" in the same way as Frege did: "The fact that they are analytic truths". But the abandonment of the idea of logical reduction makes for him impossible to answer the question "Why is arithmetic applicable?". The best that he can offer for answering to such a question would be:

> (A-C) Arithmetic is applicable because language II is a serviceable language to adopt. (Wilholt 2006, p. 75)

But this answer does not seem very satisfying, for one could ask why this language is a serviceable one. As Wilholt remarks,

> [...] it might be objected that to ask Carnap "*Why* is language II a serviceable language to adopt?" would be the same as to ask Frege "Why do we always use concepts when we relate to the external world?".
>
> But the difference between the two question is remarkable [...]. The universal applicability of *concepts* is a matter that one may plausibly accept as a brute fact, as part of the validity of pure logic. In contrast, the usefulness of language II, a system that explicitly incorporates a good deal of sophisticated mathematical content (like the axiom of choice), is no more acceptable as a brute fact than the applicability of arithmetic itself. (p. 75)

In order to better articulate Carnap's view on this point, Wilholt suggests to supplement (A-C) by means of (Carnap 1950). In the light of the distinc-

[9] To be more precise, the axiom of infinity does not need to be formulated in *language II*, since it is implicit in the syntax of number expression adopted by Carnap. The axiom of choice is expressed as the primitive sentence PSII 21, called "primitive sentence of selection". See (Carnap 2001 [1934], p. 92). Carnap explicitly says that «PSII 21 is Zermelo's *Principle of Selection* (corresponding to Russell's Multiplicative Axiom) in a more generalized form (applied to types of any kind whatsoever)» (Carnap 2001 [1934], p. 93).

tion between external and internal questions there sketched, and by drawing a parallel to the question "Are there numbers?", Wilholt concludes that Carnap

> would probably have scorned the question why language II is a serviceable language to adopt, insisting as he did that questions about the acceptance of a linguistic framework are practical rather than theoretical. (p. 75)

The question why the number language should be adopted falls within the practical sphere and cannot call for any theoretical account in terms of truth.

> This means that from the mature Carnapian perspective, the applicability problem was dropped from the philosophical canon, not because it was considered to have received an answer, but because all matters of applicability of frameworks were considered practical questions. A theoretical account of the applicability of mathematics was thereby ruled out as inappropriate. (p. 76)

Wilholt's paper continues with a final section in which he argues that the demise of reductive logicism determined the abandon of some aspects of the applicability problem and some of the Fregean ideas relating to its solution that shouldn't have been abandoned. I will talk later about these ideas and their importance for the applicability problem.[10] For the moment I want only to linger on the specific argument offered by Wilholt. To sum up, his idea is that Carnap chose to preserve the logicist tenets of reductive logicism, sacrificing its reductive characters; in this way he could answer the question about the general character of mathematical truth but relegated the question about the applicability of mathematics to the realm of pragmatical questions. This is the reason why Carnap dropped off the applicability problem and it is implicitly supposed that his influence on the following thinkers could and should explain why the problem was disregarded for such a long time.

I think that Wilholt's reconstruction is basically correct and that it surely explains why Carnap abandoned the problem. However, can it explain, *on its own*, the continuing disregard by a whole generation of philosophers? Surely, its

[10] See chapter 3.

explanatory power is correlated to Carnap's influence on the following philosophers — more precisely, to the fortunes of his notion of "analytic". Yet, it is well known that this notion, and the logical empiricism in general, began very soon to fall on hard times. So, there is a question that one could rise and that Wilholt leaves unsolved: Why wasn't this abandon reversed when these difficulties became more pressing, and why did it take such a long time to rediscover the problem of applicability? Wilholt sees the point but he gets rid of it by simply saying that «[it] is a different question» (p. 81).

These considerations suggest, on the one side, that Wilholt's account should be considered just as *a part* of the story, but not the whole story at all; and, on the other side, that we should take a look at the next development of the notion of analyticity. That's what I am going to do in the next section.

1.2 Analyticity and indispensability problem

As is well known, Quine put a hard strain on the notion of analyticity in his (1951). In this famous article, Quine indentifies two strong beliefs — two "dogmas", as he calls them — laying at the base of empiricism. The first is the belief that there must exist a precise demarcation between analytic and syntetic propositions, and that this demarcation can be made explicit. The second consists in the belief that each meaningful statement is equivalent to some logical construct upon terms which refer to immediate experience. These two dogmas are strictly interrelated and their refutation leads Quine to introduce his holistic theory.

The first dogma is refuted by Quine by means of an analysis of the notion of "analyticity". There are two distinct classes of analytic statements: those in the first can be called "logically true" and are exemplified by "No unmarried man is married". A statement of this kind «is true and remains true under all reinterpretations of its components other than the logical particles» (pp. 22-23). In other words, what makes it logically true is just his logical particles (like "no", "is", "un-") and in no way the meaning of its terms. The second class of analytic statements, on the other hand, is exemplified by "No bachelor is married". Statements of this class «can be turned into a logical truth by putting synonyms for synonyms» (p. 23). The problem, now, consists in specifying what a synonym is. Well, it seems that there is no way to define it in a non-circular manner. Quine tries to do it by means of the notions of "definition", "interchangeability *salva veritate*" and "semantical rules", but any of these attempts cannot break the circularity. In running through these attempts, Quine is unequivocally criticizing Carnap. The conclusion of this whole discussion is, in Quine's words, that

> It is obvious that truth in general depends on both language and extralinguistic fact. [...]. Thus one is tempted to suppose in general that the truth of a statement is somehow analyzable into a linguistic component and a factual component. Given this supposition, it next seems reasonable that in some statements the factual component should be null; and these are the analytic statements. But, for all its a priori reasonableness, a boundary between analytic and synthetic statements simply has not been drawn. That there is such a distinction to be drawn at all is an unempirical dogma of empiricists, a metaphysical article of faith. (pp. 36-37)

The second dogma concerns the two theories known as *verificationism* and *reductionism*. Verificationism is the theory according to which «the meaning of a statement is the method of empirically confirming or infirming it» (p. 37). As a consequence of it, two statements will be synonymous when they have similar methods of confirmation or infirmation. By extending this notion of synonymy to linguistic forms in general, we could define analyticity in terms of synonimy, simply by saying that a statement is analytic when it is synonymous with a logically true statement. So, it would seem that verification theory could save analyticity. However, there is still a problem to be removed: what do we mean when we say that two statements share the same method of empirical confirmation (or infirmation)? This question brings us directly to reductionism. For the most naïve view of the relation among a statement and the experience that confirms (or infirms) it is *radical reductionism*, the theory according to which «Every meaningful statement is held to be translatable into a statement (true or false) about immediate experience» (p. 38). Carnap tried to realize this project in (2003 [1928]) — unsuccessfully. But despite this failure, the dogma continues, according to Quine, to be influent, and it is particularly influent in the supposition that each statement, taken in isolation from its fellows, can admit of confirmation or infirmation at all.

The two dogmas are strictly interrelated, to the extent that Quine says they «are, indeed, at root identical» (p. 41). Against these dogmas, Quine sets his own holistic view, according to which

> our statements about the external world face the tribunal of sense experience not individually but only as a corporate body.
> [...] it is nonsense, and the root of much nonsense, to speak of a linguistic component and a factual component in the truth of any individual statement. Taken collectively, science has its double dependence upon language and experience; but this duality is not significantly traceable into the statements of science taken one by one. (pp. 41-42)

And also

> Any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system. Even a statement very close to the periphery can be held true in the face of recalcitrant experience by pleading hallucination or by amending certain statements of the kind called logical laws. Conversely, by the same token, no statement is immune to revision. Revision even of the logical law of excluded middle has been proposed as a means of simplifying quantum mechanics; and what difference is there in principle between such a shift and the shift whereby Kepler superseded Ptolemy, or Einstein Newton, or Darwin Aristotle? (p. 43)

Mathematical statements are potentially revisable too, just in the same way as logical ones are. As Putnam (1983) notices, the resulting notion of analyticity seems to be less close to the Kant's notion of "the predicate contained into the concept" than to the traditional notion of a priori.[11] So, not only mathematical

[11]Putnam (1983) writes: «[...] Quine also considers a very different notion: the notion

statements are not analytic, but they also are not a priori and can be revised in a possible future.

What does all this mean about the fate of the applicability problem? It is evident that Quine's criticisms to the notion of analyticity demolish Carnap's attempt to justify the truth of mathematical statements: if there is no room for analyticity (in any sense we mean it), we cannot say that mathematical statements are true because they are analytic, and it seems also that we cannot say any longer that the applicability problem is a pragmatical one, since this strategy rested, in Carnap, on his particular interpretation of the notion of analyticity. So, one could think of reverting to Frege's solution and trying to make it clearer and more coherent in its details. However, Quine's criticism, by completely abolishing the possibility of an appeal to *any* notion of analyticity, makes it impossible. Ideally, we are cast back to a 'pre-Fregean era', when the applicability problem was still problematic. It should have motivated a new reflection on the problem of applicability — but that didn't happen. So, the question is: why?

In order to answer this question, we must take into consideration another quinean theory, the so called "Naturalism". Naturalism is the theory according to which «it is within science itself, and not in some prior philosophy, that reality is to be identified and described» (Quine 1981*b*, p. 21). Naturalism is strictly connected to the above seen theory according to which every statement is in principle revisable. For if any statement is in principle revisable, on which of them should we rely? Quine's idea is that it is science that fixes the higher or lower degree of revisability of a belief (or better, of the corresponding statement). But what does he mean by "science"? His answer is very interesting:

of an analytic truth as one that is *confirmed no matter what*. I shall contend that this is the traditional notion of apriority, or rather, *one* of the traditional notions of apriority. [...]. And Quine's argument against *this* notion was not at all concerned with circularity of definitions» (p. 87).

> In science itself I certainly want to include the farthest flights of physics and cosmology, as well as experimental psychology, history, and the social sciences. Also mathematics, *insofar at least as it is applied, for it is indispensable to natural science.* (Quine 1995, p. 252; emphasis mine)

It seems that Quine does not include mathematics in the number of the sciences — at least, not directly. Mathematics can be considered a science only to the extent that it is indispensable to other sciences. It follows from this, together with the theory of the general revisability of the statements, that mathematical statements will be (probably) true only to the extent that they are indispensable to science.

We are so naturally lead to the famous quinean "indispensability argument". Since Quine cannot justify the truth of mathematical statements on the basis of their analyticity, he completely turns upside down the order of explanation and, instead of explaining the applicability of mathematics in terms of the truthfulness of mathematical statements (as Frege did), he argues that mathematical statements are true *because* they are actually indispensable in science. However, Quine gives this argument also a strong ontological connotation: he not only argues that mathematical indispensability justifies the truthfulness of mathematical statements, but also that it justifies our belief in the *existence* of mathematical *entities*.[12]

This strong ontological connotation given by Quine to his indispensability argument catalysed the focus of the following discussions — and indeed, until the 'revival' of the applicability problem, the indispensability argument was the only way in which applied mathematics was taken into consideration by philosophy. This, together with the above mentioned Wilholt's argument, can

[12]This argument is also known as the "Quine-Putnam argument", since it was independently suggested by both. However, the argument proposed by Quine and the argument proposed by Putnam significantly differs in terms of their premises and conclusions. In particular, and withouth entering into the details, Putnam's conclusion does not aim to having an ontological character, being limited to argue that mathematical statements are true. Nonetheless, it is clear that Quine's argument implies also Putnam's conclusion. For if mathematical entities exist, mathematical statements can be considered true (if they properly describe these entities). For more on the indispensability arguments in mathematics and on the relation between applicability and ontology of mathematics, see chapter 4.

help in explaining why the applicability problem knew so long an oblivion.

Yet, one could wonder about why Quine did not ask why mathematics is indispensable to sciences (provided that it is). I must confess I have no precise answer to this question. Actually, it seems that Quine takes the indispensability of mathematics as a matter of fact, something evident in scientific practice. I think the previous considerations could add something to the Wilholt's argument about why the problem of applicability was abandoned in the middle of 20th century, but it is probably too much pretentious to hope that this is a problem that can have a clear and precise answer. However, for our aims, it is interesting to note that the ensuing debate about the indispensability argument was less concerned about *why* rather than *if* really mathematics is indispensable to science. Field's (1980) attempt to refute the indispensability argument by showing that mathematics is actually *dispensable* to scientific practice is well known. But also on the opposite front the situation seems not to be very different, since the debate is mainly focused on what means for an entity to be "indispensable" (see Burgess 1983, Colyvan 2001*a*).[13]

1.3 Anything else?

As I have already noted, Wilholt's analysis shows a possible reason why during the 20th century the applicability problem was suddenly forgotten, but it is hard to believe that that is the *only* reason. In the previous section I tried to develop Wilholt's argument by considering the works of Quine, but this has not solved all the questions on the table. A precise and definite answer cannot probably be given and the best we can do is to put forward some incomplete ideas, tentative suggestions and fragmentary considerations. It is in this spirit that I want to conclude this chapter by suggesting some very general considerations about

[13] I will talk more extensively about these problems later, in chapter 4.

three facts that can have played a role in this neglect and can perhaps contribute to better — although not completely — account for it.

The first point is a typical character of the 20$^{\text{th}}$ century mathematics — of the practised one as well as its image in philosophical thinking: the particular idea according to which the "real" mathematics is pure and abstract mathematics. On the philosophical side, it must be noted that actually, during the time under consideration, not only the problem of applicability, but also the whole field of applied mathematics roused in general a scanty interest by the philosophical reflection. In this sense, the neglect of the applicability problem can be considered as a particular instance of a more general tendency in overestimating the importance of pure mathematics.[14] On the mathematical side, not very diffrently, this overestimate takes in some cases the shape of a Platonic conception of applied mathematics as a discipline less noble than pure mathematics — something with which the "pure" mathematician should not get his hands dirty.

A typical example of this kind of attitude, particularly important for his influence on contemporary mathematics, can be find in Bourbaki. Bourbaki is the pseudonym used by a group of (mostly) French mathematicians who wrote, from the 1935 until 80s, a series of books in advanced mathematics, the Elements of Mathematics (*Éléments de mathématique*) series, in nine volumes. The two foundamental ideas laying behind their works are so summed up by Atiyah (2007):

> One was that mathematics needed new and broad foundations, embodied in a series of books that would replace the old-fashioned textbooks. The other was that the key idea of the new foundations lay in the notion of "structure", illustrated by the now common word "isomorphism". (p. 1151)

This attempt has been variously celebrated, but it must be noted that

[14] We have just seen an interesting "counterexample" to this tendency: in Quine's naturalism, we attend to an inversion of the roles, for only *applied* mathematical entities permit us to say that mathematical entities (in general) exist. In this sense, applied mathematics seems to be much more (ontologically) relevant than pure mathematics. However, it must be noted that Quine talks about applied mathematics in a very general sense: he is just interested in the fact that *there is* an applied mathematics, not in the "how" that mathematics can be applied.

> [One] severe limitation of Bourbaki, no doubt conscious, was the restriction to pure mathematics. Applied mathematics is too messy and disparate to be included, and theoretical physics hovers on an uncertain borderline. One distinguishing feature of Bourbaki was the emphasis on clear and unambiguous definitions and on rigorous proofs. This was, as in algebraic geometry, a reaction against some sloppy treatments of the past, and it served a purpose in creating a firm platform for the future.Unfortunately, when taken to extremes, the requirement for total rigour excludes large areas of mathematics which are in their early creative stages. Had Euler worried too much about rigour, mathematics would have suffered. (Atiyah 2007, p. 1151)

This kind of attitude chances creating a gap between mathematics and other scientific disciplines which need mathematics in their development — as denounced, for example, by P. Germain already in 1953:

> it would be very interesting for us to know where [Bourbaki] puts the boundary which limits the field of properly called mathematics. But we can say that if Bourbaki insists on using the more abstract, and sometimes abstruse, method in every chapter of mathematics he would not show much interest in the development of applied mathematics.
>
> [...] many ideas in Bourbaki's books could be very useful, even for applied mathematicians. But the abstract, axiomatic and general form in which the courses are written may discourage people who are attracted to practical problems and who always like to see the physical significance and the relation to reality of mathematical ideas [...]. As a result there is the danger that a kind of barrier will be erected between mathematics and other sciences. (Germain 1953, p. 53)

The second point pertains the considerable focus deserved by contemporary philosophy of mathematics on foundational issues. Beginning around 1950, philosophers of mathematics focused almost entirely on foundational questions related to logic and set theory. One reason that fostered such researches was surely the realization that some questions in set theory (like the continuum hypothesis, just to make an example) are indipendent of the standard axioms for set theory and could not be answered by these axioms. All these questions does not relate to mathematics as it is actually used in science, and they are hence not tightly connected to the problem of applicability. However, it must be noted that the foundational role widely credited to set theory could have implicitly suggested an 'obvious' way to account for applicability: it is easy to apply set theory once we admit that sets can have non-mathematical objects as members, and if the whole mathematics can be founded on set theory we can hope to ultimately reduce all cases of mathematical application to application of sets. This idea, although quite naïve and not explicitly formulated, could have played a role in pushing philosophers of mathematics to think that, once we got clear on what sets are and how we know about them, then there would be no additional problem of applicability. Thus, we have here another possible motivation for which the applicability problem was delayed and progressively dismissed: we will be able to answer the question "Why and how is mathematics applicable and so effective in science?" only after we will have answered the question "What is mathematics?".

Finally, the third point consists in a typical character of the analytic philosophy: specialization — namely, the tendency to specialize in a limited number of particular problems concerning a certain discipline or a certain argument. Analytic philosophers approach these problems in a way that generally favours a small-scale analysis. The point that I want to emphisize is that this specialization, although usually positive, tends sometime to obstruct the interdisciplinarity. In the particular case at issue, the problem of applicability is clearly neither a philosophy of mathematics problem, nor a philosophy of science (or, of physics) problem, since it essentially bridges both. There is a sense in which the problem has not a precise home in analytic philosophy.

It is well known that philosophy of science and philosophy of mathematics have been and are characterized each by a quite definite list of problems. Those assigned to philosophy of mathematics has been, more or less, the following ones: foundation of mathematics, epistemological status of mathematcal statements (a priori/a posteriori, analytic/syntetic, true/not true, ...), ontological status of mathematical entities (realism/anti-realism), indispensability argument. On the other side, those assigned to philosophy of science has been, more or less, the following ones: logic of the scientific method, role of the inductive reasoning, distinction between observable and theorical entities, role of probability, etc... The applicability problem not only does not appear in any of the two lists (as I showed before, the indispensability argument does not deal directly with the reasons *why* mathematics is indispensable or applicable), but also there seems not to be a precise and appropriate collocation for it. It is a *boundary problem*. This is probably a consequence of the previous point, i.e. of the fact that the relations among physics and mathematics are considered as 'extrinsic' compared to what is considered the 'real' mathematics.

One might object by saying that the applicability problem is a 'higher level' problem: it presupposes that we already have a satisfying answer to more basic questions, like "What is mathematics?" and "What is physics?".[15] This is a popular point of view but I think that these questions cannot get a satisfying response without taking into consideration the concrete practice of physicists and mathematicians — and very often in these practices there is no clear distinction between physics and mathematics, applied and pure.[16]

[15]The second consideration I offered (p. 22) is just an example of such a strategy.

[16]In recent times, some philosophers have begun to consider philosophy of mathematics as too much disconnected from the mathematics actually studied in mathematical courses and practiced by working mathematicians. Thus, a new 'strand' in philosophy of mathematics has arisen whose aim consists in reconciling the philosophy of mathematics with the mathematics as it is really practiced. For a sort of manifest for this new philosophy of 'mathematical practice' see (Mancosu 2008). See also (Pincock 2009).

Part II

Philosophical Problems

Chapter 2

Do miracles occur?

In the previous chapter I dealt with the neglect of the problem of the applicability of mathematics during the second half of the 20$^{\text{th}}$ century by the philosophical reflection. However, even if such a problem was dismissed by the *philosophical* reflection, that does not mean that it was completely abandoned. Actually, a certain number of mathematicians and physicists never stopped to marvel at such a prodigious effectiveness of mathematics in science. Their wonder filtered out here and there in their more or less divulgative works. The very famous physicist Richard Feynman, for example, wrote in his (1967): «I find it quite amazing that it is possible to predict what will happen by mathematics, which is simply following rules which really have nothing to do with the original thing» (p. 171). Before him, but on the same line, Bourbaki already noted that

> mathematics appears [...] as a storehouse of abstract forms — the mathematical structures; and it so happens — without out knowing why — that certain aspects of empirical reality fit themselves into these forms, as if through a kind or preadaption. (Bourbaki 1950, p. 231)

In more recent times, the Nobel awarded physicist Steven Weinberg said that «It is positively spooky how the physicist finds the mathematician has been there before him or her» (Weinberg 1986, p. 725). And later he wondered again about the fact that

> mathematicians are led by their sense of mathematical beauty to develop formal structures that physicists only later find useful, even where the mathematician had no such goal in mind. [...]. Physicists generally find the ability of mathematicians to anticipate the mathematics needed in the theories of physics quite uncanny. It is as if Neil Armstrong in 1969 when he first set foot on the surface of the moon had found in the lunar dust the footsteps of Jules Verne. (Weinberg 1993, p. 125)

And also the mathematician Reuben Hersh in his (1990) wrote: «There is no way to deny the obvious fact that arithmetic was invented without any special regard for science, including physics; and that it turned out (unexpectedly) to be needed by every physicist» (p. 67).

Yet, all these expressions of marvel remained without a follow-up. None of these phisicists or mathematicians was pushed by these considerations to attempt an account of *why* mathematics is so effective in science. No blame for this, of course. After all, their main professional concern is not in explaining such an effectiveness, but in capitalizing at best on this effectiveness.

Notwithstanding, there is an interesting case in which this general tendency is contradicted. In 1960, the Nobel awarded physicist Eugen P. Wigner published an article in which he focused just on this effectiveness and sought to offer some philosophical reflections about it (Wigner 1960). The article comes to amazing and — as we will see in a little while — not completely satisfying conclusions, but it deserves to be mentioned and examined for three reasons. First of all, Wigner was one of the most important physicists of the last century, who gave very important contributions to particle physics by a wide application of group theory. He knows, in concrete terms, what it does mean to apply mathematics to physics, and he also knows how much amazing such an application can be. Thence, its voice deserve to be listened. Secondly, although his article is somewhat unsatisfying (even *frustrating*, in some passages) for a philosopher, it must be admitted that, if the applicability problem has known a timid revival in the past years, most of the credit must be ascribed to Wigner and his article: his unsatisfactory and provoking conclusions caused, in many cases, a philosophical reaction that fueled the debate. Hence, its contribution had a historical importance that cannot be disregarded. Third, the examination of his article, with all its gaps and difficulties, will give us some important cues from which we will be able to start our theorical coverage.

In the next sections of the present chapter I will first sum up in deatil Wigner's article, and then I will make some considerations about it. In the end, I will underline three points on which Wigner's account is not satisfying and on which we will have to focus our attention.

2.1 An unreasonable miracle

There are two points emphasized by Wigner:

> The first point is that mathematical concepts turn up in entirely unexpected connections. Moreover, they often permit an unexpectedly and accurate description of the phenomena in these connections. Secondly, just because of this circumstance, and because we do not understand the reasons of their usefulness, we cannot know whether a theory formulated in terms of mathematical concepts is uniquely appropriate. (p. 2)

But what does he mean by "mathematical concepts"? His answer is very simple: «mathematics is the science of skillful operations with concepts and rules invented just for this purpose. The principal emphasis is on the invention of concepts» (p. 2). Mathematical concepts, according to him, are *invented.* His claim is better clarified by his next words:

> Most more advanced mathematical concepts, such as complex numbers, algebras, linear operators, Borel sets (and this list could be continued almost indefinitely) were so devised that they are apt subject on which the mathematician can demonstrate his ingenuity and sense of formal beauty. [...]. The principal point [...] is that the mathematician could formulate only a handful of interesting theorems without defining concepts beyond those contained in the axioms and that the concepts outside those contained in the axioms are defined with a view of permitting ingenious

> logical operations which appeal to our aesthetic sense both as operations and also in their results of great generality and simplicity. (p. 3)

According to Wigner, the main characteristic of mathematics consists in its "being of some interest". Part of the concepts are comprised among (defined by) the axioms and part are instead invented by the mathematician only in order to satisfy her "sense of formal beauty". So aesthetics comes out to be the main, metatheorical leading criterion that mathematicians follow.

Of course, to use the word "aesthetics" in this context does not helps very much. Actually, we would like to know which these aesthetical criteria are and what is their role in invention. Anyway, Wigner does not give any satisfactory answer to these questions.

Having this conception of mathematics in mind, he passes to analyze the role of mathematics in physical theories. He points out two different roles:

1. evaluating the consequences of already established and already mathematically formulated theories; and
2. contributing to the (mathematical) formulation of physical theories.[1]

The first role is the role generally assumed by applied mathematics, where mathematics merely serves as a tool — probably no more than a calculus. That's what happens when, for example, we want to know the exact position of a star in the sky at a certain time t: by means of the appropriate astronomical theory, we make the relative computations and we find the wanted result. In this case, we already have a mathematically formulated theory and we use mathematics just in order to evaluate the consequences of this theory.

The second role is the most intriguing. It consists in the fact that physicists choose certain mathematical concepts for the formulation of the laws of nature. This is something that comes out with strong evidence even at a first glance to

[1] See Wigner (1960, p. 6).

the physicist's practice. But the very question is: Why does the physicist use these mathematical concepts to formulate the laws of nature?

> A possible explanation — *Wigner answers* — [...] is that he is a somewhat irresponsible person. As a result, when he finds a connection between two quantities which resembles a connection well-known from mathematics, he will jump at the conclusion that the connection is that discussed in mathematics simply because he does not know of any other similar connection. [...]. However, it is important to point out that the mathematical formulation of the physicist's often crude experience leads in an uncanny number of cases to an amazingly accurate description of a large class of phenomena. This shows that the mathematical langauge has more to commend it than being the only language which we can speak; it shows that it is, in a very reak sense, the correct language.(p. 8)

The three examples he gives for substantiating his words are: (A) the use of second derivatives in Newton's law of gravitation, (B) the use of matrices in elementary quantum mechanics, and (C) the quantum theory of the Lamb shift. These examples show with great incisiveness the «appropriateness and accuracy of the mathematical formulation of the laws of nature in terms of concept chosen for their manipulability, the "laws of nature" being of almost fantastic accuracy but of strictly limited scope» (p. 10). At this point, he proposes to refer to this fact as to the "empirical law of epistemology": it's an empirical law that we can use mathematics in science and that such an employment is so effective. This law, together with the laws of invariance of physical theories, is an indispensable foundation of these theories. Thus, according to Wigner's account, the astonishing ability of mathematical concepts in describing reality, and the amazing high degree to which it does that, is something that we should accept as an empirical fact — and that's all! His conclusions are finally expressed in the following famous, somewhat 'mystical' words:

> The miracle of the appropriateness of the language of mathematics for the formulation of the laws of physics is a wonderful gift which we neither understand nor deserve. We should be grateful for it and hope that it will remain valid in future research and that it will extend, for better of for worse, to our pleasure, even though perhaps also to our bafflement, to wide branches of learning. (p. 14)

Wigner's conclusion is ultimately aporetic: the effectiveness of mathematics in science is something misterious, a miracle that we cannot understand (we do not even *deserve* it!) and that we can just accept as an empirical law. We are just lucky that God, destiny, natural selection or whoever else gave us such a gift. We can just receive it and try to make it profitable, but ultimately we have neither a clear comprehension of it nor a direct control on it — to the extent that we can only «hope that it will remain valid in future research».

It is not completely clear what, in Wigner's argument, is really "misterious" and "unreasonable", and part of the critics is centered just on this point. Also the argument that should led to such a conclusion is not explicitly stated, so that we can only attempt to deduce it from his words. It seems that the main problem is the gap emerging between physics and mathematics concerning their respective aims and development criteria, particularly considering the importance of aesthetical criteria in mathematics. For ease of discussion, I will sum up Wigner's argument as follows:

(A) Mathematicians *invent* their mathematical theories and concepts in order to satisfy, among others, their own *aesthetical* criteria (let such criteria be $A, B, C, \ldots$, where some of them are aesthetical criteria).

(B) Phisicists elaborate their physical theories in order to satisfy their own criteria $a, b, c, \ldots$, employing for their aims also mathematical concepts.

(C) Criteria $A, B, C, \ldots$ are different from criteria $a, b, c, \ldots$.

(D) It is unreasonable that something elaborated on the basis of $A, B, C, \ldots$ can satisfy *in such an effective and precise manner* the critieria $a, b, c, \ldots$.

(E) Conclusion: the employment of mathematical concepts in phisics is unreasonable.[2]

[2]Steiner (1998) sums up Wigner's argument in a slightly different way. Actually he distinguishes between two different arguments that can be possibly traced out in the article. The first is summarized in the following way (see Steiner 1998, p. 45):

(1) Concepts $C_1, C_2, \ldots, C_n$ are unreasonably effective.
(2) Concepts $C_1, C_2, \ldots, C_n$ are mathematical.

So, we can focus now on the single premises (A)-(D), and try to understand whether they should be accepted or not and whether the conclusion actually follows from the premises or not. Wigner does not give us many convincing arguments to accpet his premises, and his analysis is surely unsatisfactory. However, to throw it away without considering his words worthy of a deeper analysis would be undoubtedly hasty.

2.2 Beauty and mathematical concepts

The first point of Wigner's account concerns, as we saw, the fact that mathematical concepts are invented by mathematicians according, among others, to aesthetical criteria. The claim is indeed double: he is saying firstly that mathematical concepts are *invented*, and secondly that what guides mathematicians in this invention is beauty. These two claims are actually strictly interwoven, and both these claims deserve our attention.

First of all, in which sense does Wigner say that mathematical concepts are 'invented'? His use of the notion is obviously shaped on the opposition invention/discovery: we do not discover mathematical concepts as we do, for example, with a new particle in physics; we do not chance upon them, we do not find them ready-made in nature. A mathematical concepts is rather something that we have to *establish*. The anthitesis 'invention/discovery' is sometimes

(3) Hence, mathematical concepts are unreasonably effective.

This argument is fallacious, since it only says that *some* mathematical concepts are unreasonable and no proof is given that their unreasonableness is due to the fact that they are *mathematical* concepts. A different argument treacable in Wigner's article is summed up by Steiner as follows (see Steiner 1998, p. 46):

(1) Mathematical concepts arise from the aesthetic impulse in humans.
(2) It is unreasonable to expect that what arises from the aesthetic impulse in humans should be significantly effective in physics.
(3) Nevertheless, a *significant* number of these concepts are *significantly* effective in physics.
(4) Hence, mathematical concepts are unreasonably effective in physics.

There is no substantial difference between this argument and the version I have just presented. Nevertheless, I prefer to mantain my version, since it permits (better than Steiner's one) to distinguish *two* possible reasons for which applicability could be unreasonable: 1) that mathematical concepts arise from aesthetical impulse in humans, and 2) that mathematical concepts are *invented*.

used to remark the distinction between realist and anti-realist philosophies of mathematics: whereas an anti-realist (a formalist, or an intuitionist, for example) would say that mathematical concepts are invented, a realist (typically a platonist *à la* Gödel) would rather say that they are *discovered*. This is not altogether right (I will later say some words about this), but this could led one to think that Wigner's use of the word "invention" is a clue of his adherence to an anti-realistic view of mathematics. Since mathematical concepts are just an invention of human intellect, there are no objects falling under such concepts, and mathematical terms do not refer to anything in the world.

Actually, if we interpret Wigner's claim in this stronger, ontological sense, one might consider such a point as playing a role in drawing the conclusion (E). Namely, what is unreasonable in premise (D) is not only the fact that some of the criteria $x_1, \ldots, x_n$ are aesthetical, but also the fact that they do not bind us to nothing existing in reality. This consideration makes room for a possible objection: one might reply to Wigner by saying that the problem of applicability is a problem for anti-realists only. A realist philosopher would not have such a problem, since, according to her, mathematical concepts are not invented but have a precise (although abstract) reference in reality: she is talking of something that *exists* (or, expressing a different form of realism, that expresses truth) and presumably has a role in shaping physical reality.

Wigner does not take an explicit stand on the realism/anti-realism debate, hence it is not possible to say for certain whether he is an anti-realist or not.[3] His terminology in this paper *sounds* anti-realist, and this seems to lay himself open to the previous criticism — given that the criticism is right, of course.[4] However, it is also possible that Wigner, in using this terminology, had no onto-

[3] I am not acquainted with other works in which he takes an explicit stand.

[4] Actually, in chapter 4 I will try to show that it is not: the problem of applicability of mathematics is not ancillary to the ontological problem in philosophy of mathematics; there is a philosophical problem concerning applicability that cannot be dispensed by an ontological choice and that therefore pertains both realist and anti-realist philosophers.

logical concern in his mind. Probably he is just underlining an epistemological difference between mathematics and physics, nothing that could label him as a realist or an anti-realist. When he says that «whereas it is unquestionably true that the concepts of elementary mathematics and particularly elementary geometry were formulated to describe entities which are directly suggested by the actual world, the same does not seem to be true of the more advanced concepts, in particular the concepts which play such an important role in physics» (p. 2), he is just saying that advanced mathematical concepts are not suggested by the *actual world*. If the meaning of "invention" is nothing more than this, then there is nothing here that could label him as an-antirealist, for many platonists too would agree with him on this point: mathematical truths are not suggested by the actual world, rather by the *ideal* world. Wigner's words are not sufficiently clear to determine whether this hypothesis is right or not, but if it were right then the previous criticism would have no grip.

Anyway, the fact remains that all he says about this invention of mathematical concepts is just that such an invention is made on the basis of aesthetical criteria — and here we come to the second claim. As well as about the previous one, no much is said by Wigner about this claim, so all we can do is to guess some hypotheses.

There is a widely shared opinion among mathematicians about the tight connection between mathematics and beauty.[5] Hence, Wigner's claim comes with no surprise. According to him, as we saw, mathematical concepts arise from the aesthetic impulse in humans. In order to better understand this claim, we would like to know what he means by "aesthetical criteria", but he is quite reticent about that. The only clarification he gives is when he esplicitly denies that simplicity is one of these aesthetical criteria. One could note that only a small number of mathematical concepts are used by physicists in formulating

[5] See, for example, (Rota 1977, Hardy [1940] 1992).

their laws of nature and that sometimes physicists do not *choose* these mathematical concepts, but they develop them independently and then recongnize them as having been conceived before by mathematicians.

> It is not true, however, as is so often stated, that this had to happen because mathematics uses the simplest possible concepts and these were bound to occur in any formalism. As we saw before, the concepts of mathematics are not chosen for their conceptual simplicity (even sequences of pairs of numbers are far from being the simplest concepts) but for their amenability to clever manipulations and to striking, brilliant arguments. (Wigner 1960, p. 7).

So, the only information we can gain about these aesthetical criteria is that *simplicity* is not among them, but «amenability to clever manipulations and to striking, brilliant arguments» is.

However, this argument misses the target: even if simplicity is not among the aesthetical criteria adopted by mathematicians, it could be that the resulting mathematical theories are indeed the simplest tool for pursuing a certain aim. For example, it could be that a mathematical structure is the simplest way to represent a certain physical system — even if that structure was not developed by mathematicians for its simplicity but for its aesthetical properties. The crucial point is that the fact that a theory has been selected on the basis of aesthetical criteria does not exclude that that theory could have some *other interesting properties*, and that those properties could make the theory desirable by the physicists for their own purposes. We could also admit that matrix theory were developed for aesthetical purposes (whatever they might be), but the resulting theory is not only pretty: it has also other interesting properties that prompted physicists to use that for representing and manipulating — as an instance — the rotation of a body in the space.

Moreover, at a first glimpse it seems quite hard to say that *all* the mathematics we know has been developed on aesthetical criteria. For there are branches of mathematics that were historically developed under the push of physical questions. That's the case of the analysis, developed by Newton and Leibniz during XVII century mainly to give an answer to concrete applied problems. We can also think that even in this case mathematicians selected the prettier theory among a number of possible theories, but the fact remains that all the possible theories, among which they selected the prettier one, had to be apt, as a necessary requisite, to solve the physical problems for which they were devised. So, if we conceive aesthetical criteria as only acting a *selection* within mathematics (what Wigner seems actually to do), there is no reason to think that there could not be another more foundamental criterion acting on the formulation of mathematical concepts such that it can explain why mathematics is so effective in physics.

However it is also possible to push Wigner's considerations a bit further and to claim that these aesthetical criteria has not (only) a selective, but also a *constitutive* role in forging mathematical concepts. Namely, a concept can be said "mathematical" only to the extent it satisfies these aesthetical criteria. That's what Steiner (1998) seems to uphold.[6] By providing several quotations from renowned mathematicians as von Neumann and Hardy, he arrives even to conclude that «That the aesthetic factor in mathematics is *constitutive* has actually become a truism in the mathematical community» (p. 65).[7] As Steiner describes it, it seems that a necessary condition on being a properly mathematical concept is that it satisfies our sense of beauty. «Where mathematicians used to look to utility in science (after all, many of them were also physicists), mathematicians *today* have adopted internal criteria to decide whether to study a structure as mathematical» (p. 7) — and beauty is among them.

As a consequence of this, Steiner can give reason for why a theory of chess is not a mathematical theory: «The standard philosophies of mathematics — logicism, formalism, or intuitionism — have no answer, since the distinction

[6]See chapter 3 for a detailed analysis of this important work.
[7]Italics mine.

between mathematics and chess is a predilection of mathematicians, rather than a logical distinction» (p. 63).

Thus, according to Steiner, «the mathematical sense reduces to the aesthetic» (p. 66); and to say that

> is to deprive the aesthetic sense of the only argument for its objectivity — namely, that the aesthetic sense is based on the objectivity of mathematical form, as the Pythagoreans in fact argued. If the Pythagorean position on aesthetics *today* begs the question — if, as I hold, the term "mathematical form" (given the multitude of "mathematical forms" today) is empty without introducing the human aesthetic sense — then there is no escape from the conclusion that the human aesthetic sense is nothing but species-specific preference. Classifications like beautiful/ugly are then anthropocentric; so, finally, are the mathematical classifications.

On this basis, Steiner can offer a stronger version of Wigner's puzzle:

> (1) Mathematical concepts arise from aesthetic impulse in humans.
> (2) It is unreasonable to expect that what arises from the aesthetic impuls in humans should be significantly effective in physics.
> (3) Nevertheless, a *significant* number of these concepts are *significantly* effective in physics.[8]
> (4) Hence, mathematical concepts are unreasonably effective in physics. (p. 46)

This version is not very different from the version offered here at page 33 and actually Steiner considers it as the *real* Wigner's argument. However, as we have just seen, Steiner seems to attach to these aesthetical criteria not only a selective role, but also a *constitutive* role. Steiner's premise (2) is then stronger than Wigner's analogue premise (D). For if mathematical concepts are anthropocentric, it seems very hard to explain how they can be so significantly effective in a non-anthropocentric discipline like physics. That's why I consider Steiner's version as a *stronger* version of Wigner's argument.

[8] A general objection that has been often made against Wigner is that he unduly stresses the *positive* cases of mathematical application. His argument seems to insist that mathematical concepts (in general) are unreasonably effective in science. However, this is not true: there are a lot mathematical concepts that have no application in science at all. So, his conclusion (*all* mathematical concepts are unreasonably effective in science) cannot be drawn from a short list of interesting cases. Steiner, in his version of Wigner's argument, tryes to rectify this point, by specifying in premise (2) that «a *significant* number of these concepts are *significantly* effective in physics». Not all mathematical concepts, but only a *significant* number of them, are effectively used in science, so that only *these* concepts would be unreasonably effective.

Since the anthropocentric character of mathematics is due, according to Steiner, to the constitutive role of these aesthetical criteria, we should understand whether they actually have such a constitutive role or not. The question is quite complex. Pincock (2012, p. 182) properly points out that it is surely true that, if a concept is studied by mathematicians, this is a good (and probably sufficient) reason to believe that such a concept is mathematical; nevertheless, it is quite weird to say that the *only* reason for which that concept is mathematical is because it is studied by mathematicians. We should distinguish between two different questions: on the one side there is the question (I) "What makes this concept mathematical or nonmathematical?"; on the other side there is the question (II) "What makes this concept a good or a bad mathematical concept?". Now, according to Pincock, there is no doubt that aesthetical criteria play an important role in answering the latter, but the arguments in favour of it cannot be used at the same time to argue that aesthetical criteria has a role in answering the former too. Steiner supports his argument by means of quotations from mathematicians — as we have seen — but these quotations seem rather to answer the latter question than the former. For example, Steiner quotes von Neumann's (1956) words: «I think that it is correct to say that his [*of the mathematician*] criteria of selection, and also those of success are mainly aesthetical» (p. 2062). Is von Neumann answering question (I) or question (II)? Question (II), I would say — so that Steiner's arguments in answering question (I) seem to answer at the most question (II).

> A mathematician discussing complex analysis and the Riemann hypothesis — *Pincock says* — once insisted to his class that compared to the Riemann hypothesis, the conjecture known as Fermat's last theorem was pointless and not worth pursuing. By this he did not mean to relegate number theory and Fermat's last theorem outside of mathematics, but was instead expressing his conviction that complex analysis is a better or more important area of mathematics than number theory. These judgments about the relative importance of this or that area of mathematics are central to mathematical practice as they provide the mathematician with the motive to pursue his or her specialized area of research. [...].

> Similarly, if a mathematician dismisses chess as "not mathematics" because it is "not beautiful", then what she is really saying is that it is not good or important from a mathematical perspective. (p. 182)

Mathematicians sometimes confuse question (I) with question (II), but the philosopher should nonetheless keep the two well distinguished. Also Hardy ([1940] 1992) says that there is no permanent place in the world for ugly mathematics» (p. 85), but then he says also that

> A chess problem is genuine mathematics, but it is in some way 'trivial' mathematics. However ingenious and intricate, however original and surprising the moves, there is something essential lacking. Chess problems are unimportant. The best mathematics is serious as well as beautiful. (p. 88)

Here it seems that Hardy is saying that, yes, a chess problem is 'mathematics' since it partakes of the same kind of beauty as mathematics, but such theorems deserve not to be pursued because they are not mathematically important, they are not 'serious' — so that the *real* discrimen is not beauty, but *importance*. It is true that then Hardy adds: «the beauty of a mathematical theorem *depends* a great deal on its seriousness» (p. 90); but this lead into a vicious circle that seems rather to prove a certain extent of confusion by him. Steiner is honest in reporting this vicious circle, but he prefers to conclude that «This almost circular reasoning brings him right back to my position» (p. 65, n. 41). Of course, Hardy's reasoning must be revised in some point — but how do we decide where exactly he is wrong? Steiner did not offer us compelling reasons to embrace his thesis, even if this is not enough to prove he is wrong. However, this is enough to show that a different, viable alternative is possible.

Coming back to Wigner, we can conclude that there seems not to be anything particularly misterious in premise (A) and (C): mathematical concepts can also be invented and these invented concept can also be selected on the basis of aesthetical considerations, but this does not mean that their application is miracolous, for they can still have a relevant characteristic that makes them a usefull and effective tool for physicists. Such an applicability, then, is not miracolous in any sense — if we understand "miracolous" as "having no possible explanation". Rather, it is still in search of an explanation, and this explanation can also be difficult to be find. And even if we modify Wigner's argument as Steiner does, we are absolutely not forced to admit that the applicability of mathematics to science is something miracolous, a gift «which we neither understand nor deserve».

2.3 Mathematical and physical concepts

The fundamental point in Wigner's argument is that something elaborated for certain aims cannot be so effectively usefull in satisfying different aims (premise D). Now, I've already objected that this argument does not work; for something elaborated for certain aims can however have some properties that in the end justify its employment for satisfying different aims, so that this is not a good reason to say that such application is misterious or unreasonable. It might also be unreasonable in the end, for my objection does not prove that it cannot be; but Wigner's argument actually does not prove that it is. However, what I want to stress in this section is that to set the question in terms of a comparation between *criteria*, or aims, is not the proper way to set the problem.

Actually, Wigner's argument is based on the fact that mathematical criteria $A, B, C, \ldots$ are different from physical criteria $a, b, c, \ldots$. But what are these physical criteria and in which sense are they different from mathematical ones? This seems to be an important point on which Wigner does not sufficiently hinder. A hammer is very different from a nail and they have been made having different criteria in mind, but it is just this difference that makes the hammer the proper tool to be 'applied' to the nail. This is probably an unsophisticated example, but I think it is not completely stupid. There is a difference between the criteria adopted by the physicists and the criteria adopted by the mathematicians, of course; but this difference is not necessarily a sign that *any* application of the latter to the first has to be unreasonable.

This is the first critic to Wigner, and it suffices to neutralize his conclusion about the "unreasonableness" of the applicability of mathematics to science. However, it is not the only. In this section I am going to underline three points on which Wigner does not hinder very much — which makes his analysis philosophically unsatisfactory — and which we will have to take into carefully consideration in our next inquiries.

claim

First, in which sense the physicist 'applies' mathematical concepts? Wigner distinguishes a simple use of mathematics as a calculation tool from the use of mathematical concepts to *forge* physical concepts and laws; but the point is more complex than this — and it has a very important influence on *what exactly we find problematic in the applicability of mathematics.* At the beginning of his article, Wigner refers a story about two old classmates that meet after a long time:

> One of them became a statistician and was working on population trends. He showed a reprint to his former classmate. The reprint started, as usual, with the Gaussian distribution and the statistician explained to his former classmate the meaning of the symbols for the actual population, for the average population, and so on. His classmate was a bit incredulous and was not quite sure whether the statistician was pulling his leg. "How can you know that?" was his query. "And what is this symbol here?" "Oh," said the statistician, "this is pi." "What is that?" "The ratio of the circumference of the circle to its diameter." "Well, now you are pushing your joke too far," said the classmate, "surely the population has nothing to do with the circumference of the circle." (p. 1).

The story is just a joke, and it is used by Wigner only to introduce his wonder about the "unexpected connections" in which mathematical concepts seem to turn up. But what exactly is the problem with this "unexpected" or "unreasonable" connections? In the story just told, it seems that the wonder of the second man is just the result of his ingenuity and completely lack of knowledge about the topics in question. Probably, a good course in statistic would have dispelled his astonishment. As Shapiro (1983) points out, referring a similar example,

> The problem can occur on several levels. It may begin when one wonders how it is possible for a particular mathematical fact to serve as an explanation of a non-mathematical event. [...]. In this case, a reply might consist of a detailed description of the relevant scientific theory which associates a certain class of functions with a class of physical phenomena. A question on a different level could then be raised as to what a class of mathematical objects (such as functions, infinite sets of ordered pairs) can have to do with physical phenomena. Here, the inquiry concerns the relevance of the given mathematical-scientific theory. A possible reply to the second question would be to point out that similar uses of mathematics have an important role in scientific methodology. When this — the entire enterprise — is questioned, one might note the vast success of the methodology in predictingand controlling the world.
>
> [...]. Such a statement, however, should not satisfy the epistemologist/philosopher of science, whose job is to account for scientific explanation and, in particular, to show how and why the mathematical methodology succeeds in predicting and controlling the non mathematical world. (pp. 525-526)

So, there are different senses of applicability and different senses according to which we can say that *there is a problem* in accounting for the applicability of mathematics, and Wigner didn't get into this point deeply enough. Mark Steiner dealt with these points in his (1998) and I will discuss his contribute in chapter 3. Anyway, for the moment it is enough to notice that *there is not a univocal sense of applicability*, and that a clear analysis of this point is required to set the problem in the proper way. For if we do not have clearly in mind *in which sense* we apply mathematical concepts and *in which sense* such applications are problematic, then how could we ever find a satisfying explanation of how and why we can apply such concepts?

Second, the general framework offered by Wigner in his article risks to suggest a loose comprehension of the applicability of mathematics. For such a framework runs the risk of representing the physicist simply as an *ultimate consumer* of mathematical concepts: it happens that the physicists, in doing their work, need of a mathematical concept or structure; and they just borrow it from the mathematicians, which already — on their own — developed it. But this is not right: there is a lot of work that the physicsits have to do in order to make their concepts available to a mathematical formulation and/or treatment.

Second, Wigner's image of the relations between physicists and mathematicians runs the risks of being trivialized in the following way: mathematicians devise their mathematical concepts for their own purposes and following their own methods; then, physicists select some of these concepts and simply "employ" them in their theories. It seems as if mathematics did all the work. Sometimes, as Wigner said, physicists elaborate mathematical concepts and then discover that those concepts were already devised by mathematicians; but also in these cases, the mathematical concepts were already available. So, it seems that it is just *by chance* that the physiscist needs to use just *that* mathematical concept and that such concept has been previously developed by mathematicians. However, the question is more complex than this. Let me make a very trivial example. When Kepler "applied" the conic theory (developed eighteen centuries before by Apollonius of Perga) to the planets' movement, he had previously to *idealize* the planets themselves, making them nothing more than *mathematical points in a bidimensional space*; namely, he had to abstract from their corporeal nature. This has been possible only because Kepler wanted to study the *motions* of the planets and, under the given physical conditions, it was sufficient for his aims to study only the motions of the *center of gravity* of any single planets, without taking the spatial distributions of their masses into account. Moreover, the extensions of the planets are sufficiently small compared to their distances to permit not to take them into consideration.

These 'abstractive moves' permit the elimination of the *non-relevant* characters in the description of the physical system and their accomplishment is preliminary required in order to apply the proper mathematical structure to the physical system under scrutiny. The physicist is well-aware of this and actually her work does not consist only in "employing" or "applying" such concepts, but also in making their application as easier and much convenient as possible. This is a very important point, on which we will have to pay the necessary attention.

Thirdly, this framework seems to presuppose a *clear* distinction between mathematical and physical concepts: there are two things clearly distinguished (or, at least, clearly distinguishable each other) and what we have to explain is the possibility and effectiveness of their *conjunction*. But this clear distinction is not clear at all. Let's take, as an example, the case of an electron: is it a physical concept? Yes, it would seem; but when we describe its properties we use concepts that rather seem to be mathematical, having no clear physical interpretation. For we say that an electron has spin $\frac{1}{2}$, but what is the physical interpretation of this mathematical value? We usually represent an electron as a small ball twisting on itself, and the spin as its internal angular momentum; nevertheless such a representation cannot be true, since if it were *literaly* accepted, the rotational speed of the electron would be higher than the speed of light, and this would contradict the special relativity.[9] Rather, it seems that physical conepts (and laws) are *constitutively* entangled with mathematical concepts.

The first of this three points, hence, suggests us our next move: first of all we have to specify the nature of the problem at hand. What do we mean by "applicability", and what is — exactly — the problem that such applicability arises? In particular: is there any specific *philosophical* problem here to be faced?

[9]On this interesting topic and on its implication to the debate on realism see Morrison (2007).

Chapter 3

Applicabilities of mathematics

One of the problems we stressed in Wigner's analysis was his vagueness on what exactly the problem of applicability consists in. Thus, this is the first issue we have to tackle. If it is true — as I suggested in chapter 2 — that there are many senses of applicability and that to each of them a different problem corresponds, then the first thing we have to do is to clarify these different senses.

To this aim, in this chapter I will discuss a work by Mark Steiner published in (1998) and titled *The Applicability of Mathematics as a Philosophical Problem*, whose importance in the debate around the applicability of mathematics can be hardly underestimated. Philosophers, according to him, have not forgotten the problem, but they regrettably limit themselves to citing the truism that mathematics is applicable and to maintaining polemically «that only his or her favorite philosophy can account for the applicability of mathematics» (p. 15).[1] Moreover, Steiner notices that

> philosophers and physicists often talk past one another on mathematical applicability. Philosophers concentrate upon the applicability of arithmetic; physicists (or physically-minded mathematicians), upon the "miraculous" appropriateness of matrix algebra or Hilbert spaces for quantum

[1]In chapter 3 I will make some considerations about this point, with a particular focus on ontology.

> mechanics. The physicists see no difficulty in the applicability of *arithmetic* to the world, and may accuse philosophers who focus upon arithmetic of mathematical ignorance. Philosophers return the compliment. (p. 15)

Actually, this consideration finds a confirmation in the analyses led by us in chapters 1 and 2. But the point is that, according to Steiner, «Neither charge is just: philosophers and physicists are speaking of "applications" and "applicability" of mathematics in different ways. There is simply no such thing as "the" problem of mathematical applicability» (p. 15). So, his declared aims consist in offering a

> comprehensive philosophical analysis of the application of mathematics, an analysis of:
>
> > What it is to *apply* mathematics;
> > What it is for mathematics to be *applicable*;
> > What philosophical problems the applicability of mathematics raises;
> > What solutions are possible. (p. 15)

In the first three chapters of his book, Steiner distinguishes four different senses of "applicability" of mathematics — four different senses in which we can "apply" mathematics to science (his concern is primarily on the application of mathematics to physics) and in which a "problem" can be raised concerning the applicability.

I will first introduce these four senses of applicability as Steiner describes them, and only then I will critically examine them.

3.1 The many senses of applicability

3.1.1 The semantic applicability

The first sense of applicability identified by Steiner is what he calls the "semantic" applicability of mathematics. It is connected to the *deductive role* of mathematics, both in everyday life and in scientific contexts, and it pertains the possibility of using mathematical theorems as premises in deductions. Steiner considers the following simple argument:

(1) $7 + 5 = 12$.

(2) There are seven apples on the table.

(3) There are five pears on the table.

(4) No apple is a pear.

(5) Apples and pears are the only fruits on the table.

(6) Hence, there are exactly twelve fruits on the table.

This is, according to Steiner, a typical example of semantic applicability. Why *semantic*? Because we are employing here the numeral terms "7", "5", "12", "seven", "five", "twelve" in two different semantic usages: in premises (1) they are used to *name* a mathematical object, while in premises (2), (3) and (6) they are used to *predicate* something about the apples, the pears, or both. Since in these statements both a mathematical and a non-mathematical vocabulary is used, these statements are considered by Steiner as an example of what he calls "mixed context". The problem is that, if we are not able to find a constant interpretation that permits us to employ the numeral terms in both the mentioned usages, the argument (1)-(6) losses its validity. The problem is not due to a "metaphysical gap" between numbers and material objects. For, if it were so, then it would not arise in contexts where this "gap" is not present. Yet, as Steiner notices, it arises also when we count the roots of an equation by adding the number of the real roots to the number of the imaginary roots.

Fortunately — at least according to Steiner — the problem has been already addressed and solved by Frege, in a manner that does not depend on his logicist thesis. According to Frege, numerals are always singular terms (they work as nouns, and therefore stand for objects: the numbers), and all mixed contexts in arithmetic can be reduced to the form:

The number of Fs is m,

which, adopting Parsons's (1964) notation, is formally translated by Steiner as:

$$NxFx = m. \tag{7}$$

Observing (7), we can see that the numerical attribution has indeed the character of a predication. Yet, this numerical attribution is not *to objects*, rather to a *concept*. The numeral "seven" in (2), for example, is not predicated of the pears, as it seems at first sight, but of the *number* of the pears — so that numerical predication is at least a second order predication. Moreover, (7) shows us that the numeral m is not the predicate, but just a part of it (the entire predicate is "is m", or "equals m").

Thus, all this considered, we need another premise to be added to (1)-(6) in order to have a valid argument:

$$\begin{aligned} &\forall F \forall G(NxFx = m \wedge NxGx = n \wedge \neg\exists x(Fx \wedge Gx) \\ &\quad \rightarrow Nx(Fx \vee Gx) = m + n). \end{aligned} \tag{8}$$

Frege considered (8) as a theorem of pure logic (since he considered natural numbers as logical objects); but, in order to clarify the applicability problem at hand, we need not to decide whether it is or not . All that concerns us is that such a theorem demonstrates a link between addition of natural numbers and disjoint set union — and that's what it does. If we understand addition in (8) as defined as the iteration of the succession relation for natural numbers, then (8) is a highly informative theorem, since it links two different mathematical ideas;

if instead we define it as simple cardinal addition, then (8) is quite trivial. But the essential does not change: concepts F and G in (8) can be now instantiated by our particular concepts "pears on the table" and "apples on the table" and the conclusion (6) can be deductively drawn.

Thus, according to Steiner, Frege solved the semantic applicability problem by showing that: (a) numerals refer to objects (the numbers); (b) numerals can be predicate of concepts and not of objects; (c) there is a logical connection between addition among natural numbers and disjoint set union.

3.1.2 The metaphysical applicability

Another class of problems concerning the applicability of mathematics is what Steiner calls the "metaphysical" problems of applicability.

> These problems, we are told, stem from a gap between mathematics and the world, a gap that threatens to make mathematics irrelevant. [...]. One [of these problems] is the very existence of mathematical "objects" and mathematical "truths", which some philosophers simply cannot accept. One such theorist is Hartry Field. His view of Frege's project amounts to the following: Frege's — valid — interpretation of arithmetic demands the existence of objects (numbers, sets) that (in Field's view) do not exist. Hence, both the theorems of pure mathematics and the "mixed" propositions of mathematical physics turn out to be *false* statements. (p. 19)

The problem is then: how is it possible that sistematically *false* premises can systematically lead to *true* conclusions? Steiner seems to present this problem as a problem for nominalists. But actually there are many versions of nominalism and what is problematic here is not so much the rejection of abstract objects as the claim that mathematical statements are literally false. Hence, the problem Steiner is raising here for nominalists is rather a problem for those particular nominalists that are also *fictionalists* (like Field). Anyway, what does it happen if we reject the typical nominalist adversion for abstract entities and we platonistically suppose that mathematical objects *exist*? Are the metaphysical problems of applicability completely ruled out if we assume this? No, they are not — Steiner says. For the metaphysical gap between mathematical (abstract) objects and physical (concrete) objects «blocks any nontrivial relation between mathematical and physical objects, contradicting physics which presupposes such relations» (p. 20). Indeed, physics seems to presuppose some kind of relation between mathematical and physical 'world', a relation that permits to the first to be in some sense 'effective' on the latter. But if the two worlds are platonistically divided by a clean cut, then what relation is still possible?

> Underlying these complaints — *Steiner adds* — is an argument like this:
>
> (1) On the platonist view, physical laws and theories must express relations between mathematical and nonmathematical objects.
> (2) Every relation in physics is a causal (or spatiotemporal) relation.
> (3) Mathematical objects do not participate in causal (or spatiotemporal) relations.
>
> Therefore,
>
> (4) On the platonist view, all physical laws and theories are false. (p. 21)

If the fictionalist concludes that mathematical statements are false, the platonist seems to be led to conclude, not very differently, that all physical laws are false.

Frege, being a Platonist, is surely exposed to this metaphysical problem; however, as Dummett (quoted by Steiner) has noticed, he has a quite easy way out from the difficulty:

> [Frege's] combination of logicism with platonism, had it worked, would have afforded so brilliant a solution of the problems of the philosophy of mathematics. [...]. Frege's idea was that [mathematical] objects should always be defined as extensions of concepts directly related to the application of the mathematical theory concerned: concepts to do with cardinality in the case of the natural numbers. [...]. In this way, application could be understood as being no more problematic than it would be according to non-platonist logicism: it would not consist in pure instantiations of formulas of higher-order logic, but would involve deductive operations so close to that as to dispel all mystery as to how application was possible. A mathematical theory, on this view, does indeed relate to a system of abstract objects in the sense in which we speak of pure sets [...]: they are objects characterized in such a way as to have a direct connection with non-logical concepts related to any one of the particular domains of reality, the physical universe among them. They could not otherwise have the applications they do. (Dummett 1991, p. 303)

As I already noticed in chapter 1,[2] Frege's solution is based on the satisfaction of two distinct conditions: (A) the analyticity of arithmetic, and (B) the reduction of arithmetical concepts to (logical) laws of concepts. The first guarantees that arithmetic is conservative; the second that the laws of arithmetic talk about concepts and not about objects. Thus, for Frege, applying arithmetical laws is no more than applying laws on concepts that can be variously instantiated:

> Frege argued that the laws of arithmetic are second-order laws governing all concepts whatsoever. Not only did he argue this point, he constructed a deductive system of arithmetic in which this second-order character is evident. In Frege's system, numerals appear in second-order predicates applying to ordinary concepts. In this sense, Frege "predicates" natural numbers of concepts. The concepts themselves may be true of physical objects. In short, mathematical entities relate, not directly to the physical world, but to concepts; and (some) concepts, obviously, apply to physical objects. The mystery thus vanishes without a trace. (p. 22)

Yet, what for Dummett is just a virtue of Frege's philosophy, for Steiner is a gain that all platonists can benefit: his solution can be 'exported' because it is independent of logicism:

> For example, suppose we regard set theory, rather than second-order logic, as the foundation of all mathematics, because all classical mathematics can be modeled in it. Frege's insight adapts readily to this new context: numbers characterize sets, not physical objects; while sets can contain, of course, physical bodies. Set theory is applicable, in the present sense (one of many senses, I remind you) simply because physical objects can be members of sets. This is a thoroughly nonmystical idea, always supposing we accept the existence of sets in the first place. (p. 22)

Frege's solution of the metaphysical problem is not conditioned at all by the inconsistency of his logical system. Steiner refers to Boolos's (1987) work, where it is shown that the program of Frege's *Grundlagen*, including all theorems there sketched, goes through in a consistent second-order theory (which Boolos calls

[2] See p. 11.

"FA") having only one "nonlogical" axiom:

$$\forall F \exists ! x \forall G (G \eta x \leftrightarrow F eq G).$$[3]

Namely, the only mathematical objects Frege needs for arithmetic are classes of equinumerous concepts. One might reply that FA is not logic, but this is absolutely irrelevant for the problems at hand: «FA captures the benefits of Frege's approach to arithmetic, logic or no» (Steiner 1998, p. 23). Moreover, his solution is not limited to the only arithmetic: since mathematics can be modeled in set theory (ZFC), we need only a way to apply set theory to physics. But this can be easily achieved by adding special functions from physical to mathematical objects — and functions themselves can be seen as sets. «As a result, modern — Fregean — logic shows that the only relation between a physical and a mathematical object we need recognize is that of set membership. And I take that this relation poses no problem — over and beyond any problems connected with the actual existence of sets themselves» (p. 23)

Thus — summing up —

> In the *Grundlagen*, Frege showed how to interpret both pure and mixed arithmetic statements so that we can use pure mathematics to deduce "applied" conclusions. This solves the semantic problem. He did not specify the underlying logic, but all of his proofs can be codified in Boolos's FA word for word. (That FA is not "logic" is irrelevant to the semantic problem of applicability.)
>
> And, in solving the semantic problem, Frege did not need to postulate any metaphysically suspect relations (such as causal relations) between mathematical and nonmathematical objects. Mathematical objects are related only to other mathematical objects and to concepts. That physical objects may fall under concepts and be members of sets is a problem only for those who do not believe in the existence of concepts or sets. Perhaps without even intending to, Frege disposed of the metaphysical "problem" of applicability, and rendered superfluous most recent discussion of "the" problem of applicability. (p. 23)

[3]The sign η espresses the relation holding between a concept G and the extension of a higher-level concept under which G falls. The sign "eq" express the equinumerosity of the concepts. In words, this axiom says that, given any concept F, there exists one and only one class x which is the class of all the concepts equinumerous with F.

3.1.3 The (particular) descriptive applicability

Frege addressed and solved — according to Steiner — the two previous problems of applicability: the semantic and the metaphysical. However, they are not all the problems of applicability that can be raised. Another sense of applicability is what Steiner calls "descriptive" applicability. It raises a corresponding problem that is different from the previous two. Actually, unlike the previous two, it does not concern mathematical concepts in general, but has to do with *specific* mathematical concepts. Steiner defines it as «the appropriateness of (specific) mathematical *concepts* in describing and *lawfully predicting* physical phenomena» (p. 24).

A first example of the relevance of this problem for a specific mathematical concepts is given by Steiner for the case of arithmetic:

> [...] what makes arithmetic so *useful* in daily life? Why can we use it to predict whether I will have carfare after I buy the newspaper? Can we say — in *non*mathematical terms — what the *world* must be like in order that valid arithmetic deductions should be effective in predicting observations? (p. 24)

The general idea behind these remarks can be unfolded as follows: when can we use a certain mathematical concept (or a certain mathematical structure, or a certain mathematical theory) to *describe* a certain 'piece' of the world? What are the conditions that such a 'piece' of world must satisfy in order to be describable by that particular mathematical concept? Steiner seems to be concerned here with the *representative* role of mathematics and he's wondering when such a representative role can be effectively granted — so that we can make *right predictions* from the mathematical concept at issue. The condition, according to which the account should be in *non-mathematical terms*, is precised by Steiner by saying that this project must not be confused with Field's (1980) one: he is not interested in *translating* any physical theory into a nominalistic language, but *explaining*, in nominalistic language, the conditions under which a mathematical concept will be applicable in description (see Steiner 1998, p. 24, footnote 2).

> These — *Steiner continues, explaining the difference between this problem and Frege's one* — were not Frege's questions, and could not have been: he attended to the applicability of mathematics in general, not to nature specifically. His concern was not with the usefulness of mathematical reasoning, but its validity — to which the state of the world is immaterial.
>
> Frege treats the *semantical* applicability of mathematical *theorems*; I will attend to the *descriptive* applicability — the appropriateness of (specific) mathematical *concepts* in describing *and lawfully predicting* physical phenomena. Whereas, for Frege, applying meant "deducing by means of", for me it will be "describing by means of". (p. 25)

The descriptions of which Steiner is talking about are — as he explains in footnote — *lawlike* or *projectible* descriptions in the sense of Goodman (1983): descriptions which could appear in natural laws and thus be used in predicting events.

Since this descriptive problem concerns specific mathematical concepts (and not mathematical concepts in general), it will actually consist in as many single problems as the mathematical concepts employed in science and everyday life are. So, obviously, an exhaustive exposition is impossible. Steiner mentions four examples: addition, multiplication, linearity and fibre boundle theory. Let's briefly consider each of them, since their discussion will be later useful.

First of all, addition. «Addition — Steiner says — is useful because of a *physical* regularity: gathering preserves the existence, the identity, and (what we call) the major properties, of assembled bodies» (p. 27). A typical example of "application of addition" (in the *descriptive* sense we are here dealing with) is *weighing*:[4]

> If one body balances 5 unit weights, and another balances 4, then both together will usually balance $5+4=9$ unit weights. The natural numbers indirectly describe, by laws of nature, not only the sets of unit weights placed on the scale, but the objects they balance. Addition of numbers becomes a metaphor for "adding" another object to the scale. Arithmetic

[4]In *nonrelativistic* contexts, of course — since according to Einstein's theory of general relativity weight is not additive.

> is not empirical, but it predicts experience indirectly by the law: if m and n are the numbers of unit weights that balance two bodies separately, then $m + n$ units balance both. Equivalently: if one object weights m units, and another weights n units, then the (mereological) sum of both "weighs $m + n$ units." This more usual expression looks like a tautology, but is as empirical as the former: the expression '$m + n$' is embedded in a nomological description of a phenomenon (weight). This description induces an isomorphism between the additive structure of the natural numbers and that of the magnitude, weight. (p. 28)

Similarly can be said for the multiplication:

> A familiar and genuine application of multiplication is tiling with unit squares. Suppose we have a rectangular floor and we inquire how many tiles cover it. The elementary answer is that if the floor lenght is m units and the width is n units, the number needed is usually $m \cdot n$. As in weighing, we have an isomorphism. The numbers m and n come to measure, not just the size of a set (of units), but the lenght of lines. Multiplication comes to portray decomposing the rectangle into squares by parallel lines; conversely, moving from one-dimensional to two-dimensional Euclidean "intervals". (p. 29)

What Steiner is pointing out, is that addition is a useful application of addition (of the additive structure of reals) because it is possible to set an isomorphism between this additive structure on reals and a property of phisical bodies. Formally, we can consider a set B of physical bodies, and define a structure $\langle B, \trianglelefteq, \oplus \rangle$ on it, such that

1. $x \trianglelefteq y$ means that, for any x, y in B, body x weighs the same as, or less than, body y;
2. $u \oplus z = w$ means that body w is the mereological sum of u and z.

Now, we can set a mapping ω from B to the set of reals, such that any physical body corresponds just to one real number and the following conditions are satisfied for any x, y, z in B:

i. $x \trianglelefteq y$ iff $\omega(x) \leq \omega(y)$;

ii. If $z = x \oplus y$, then $\omega(z) = \omega(x) + \omega(y)$;

iii. For an arbitrarily chosen body e in B, $\omega(e) = 1$.

Such a mapping from B to $\mathbb{R}$ is precisely what assures us that we can use real numbers to make *descriptive* predictions on the behaviour of weighing a collection of bodies.

The next example given by Steiner is a bit more complex but also a bit more confused. Steiner considers *linearity* and wonders «why does it pervade physical laws?» (p. 30).

> Because — *he answers* — the sum of two solutions of a (homogeneous) linear equation is again a solution. This property corresponds to the Principle of Superposition, exploited by Galileo: joint causes operate each as though the others were not present. (p. 30)

Thus, linearity (and nonlinearity) has a clear physical correlate: *superposition* (and its absence). When the effects of two causes in a physical system are independent of each other, then there is superposition and linearity applies. But this is not the only role for linearity in physics, since it is often useful also in contexts where the nonlinearity can be approximated by the linear.

> For example, we approximate a curve, over short distances, by its tangent, an idea which finds full flower in the famous Taylor series expansion for functions. Approximations like this are valid if the curve is smooth, or at least has smooth pieces, *certainly a physical property.* Hence we have an explanation for the second role of linearity in science: the smoothness of many natural processes. (pp. 30-31; italics mine)

Eventually, Steiner considers a last and more abstract example: the theory of fiber boundle. As he explains, «the remarkable applicability of fiber boundle theory to physics rests on the translatability of the concepts of fiber boundle theory into the concepts of gauge field theory» (p. 32). At this proposal, he gives a table in which he lists, on the left side, gauge field theory's concepts, and on the right side the corresponding fibre boubdle theory's concepts. We do not need to get deeper into this table (and actually even Steiner does not do it). What is important to notice is that the validity of the explanation is, also in this case, based on «matching mathematical to physical concepts» (p. 32). In this sense, the terms listed on the left side of the table (referring to the gauge field theory) are interpreted as terms for *physical* objects or physical properties, whereas the terms listed on the right side (referring to boundle fiber theory) are interpreted as terms for *mathematical* objects or properties.

Yet, despite these positive examples, this strategy seems not to be available for *any* mathematical concept: in many cases, we are left without any reasonable account for their descriptive applicability. Steiner cites, at this proposal, a number of mathematical concepts «whose descriptive applicability *now* seems mysterious» (p. 36). His first example concerns the applicability of complex analysis in physics; in particular, that of an analytic function, of which he considers three distinct cases of application: to fluid dynamics, to relativistic field theory, and to thermodynamics. In the first case, the applicability follows from the theorem according to which a two-dimensional ideal fluid obeys the Cauchy-Riemann equation. In the second case the applicability follows from theorems that link functions defined on a light cone to analyticity. In the third case, instead, the applicability if based on the possibility of treating the critical temperature of a ferromagnet as an analytic function of the number of dimensions of the magnet.

> Now, — *Steiner comments* — even if we were to regard the first two applications (hydrodynamics and relativistic field theory) as sufficiently explained by the theorems quoted, note that there is no one physical property which explains all three applications, or types of application. So the situation does not resemble the case of additivity, where one property explains just about every application of "+" in physics. (p. 39)

Moreover, it seems that the third application (to thermodynamic):

> is totally mysterious, from the point of view of explaining the descriptive applicability of analyticity. The assumption that the critical temperature of a magnet is an analytic function of its dimension is, in fact, physically meaningless. Not only will we have to condone, in physics, magnets of dimension 3.5 (we have gotten used to such things by reading about fractals), but we will have to swallow magnets of dimension $2 + 3i$! Here the analytic function is used as a calculational tool or formal trick: we cannot calculate the problem for three dimensions, so we calculate it for a four-dimensional magnet, then expand the function as a power series in

> the complex plane around the number 4, and plug in the value 3. Nobody knows (today) why this works. (p. 38)

Another interesting case of mathematical concept whose descriptive applicability seems to be misterious is Hilbert space. Its descriptive applicability to quantum mechanics seems to follow from what Steiner calls the "maximality principle": «If a Hilbert space H represents a quantum system Q, then each basis, or set of "axes", of H corresponds to a physical property of Q; and each physical property of Q corresponds to a basis, or at least a subset of basis, of H» (p. 39). As he precises, some physicists would accept only the latter clause: there are some bases corresponding to '*non*-physical' properties, that must be weeded out in a second moment by 'superselection rules'. However, what is particularly relevant in this "maximality principle" is the fact that

> the magnitude of *position* corresponds to a complete basis. Thus, position information about a system *at a given time* determines information concerning every other magnitude of the system *at that time*. This information is obtained simply by changing the basis of the Hilbert space, and recalculating the coordinates of the unit vector relative to the new basis. (p. 39)

Let consider a single particle. It is represented by a single vector. Now, once we choose the position basis, we have an infinite number of coordinates for the vector, since the particle could be in an infinite number of places, and by squaring these coordinates, we have the probability distribution of that particle. If now we desire information about some other property of the particle (for example, about its momentum) we have simply to change the basis to get a new set of coordinates.

This strategy can be applied to extrapolate from the formalism an astounding amount of informations, and all this simply follows from the assumption of a mathematical formalism (complex Hilbert space) and the maximality principle. Steiner's conclusion is that

> The role of Hilbert spaces in quantum mechanics, then, is more profound

> than the descriptive role of a single concept. An entire *formalism* – the Hilbert space formalism — is matched with nature. Information about nature is being "read off" the details of the formalism. (Imaging reading off details about elementary particles from the rules of chess — castling, en passant — à la Lewis Carrol in *Through the Looking Glass.*) No physicist today understands why this is possible, though there are those who are making valiant efforts. Thus, the descriptive applicability of the Hilbert space formalism, which follows from the maximality principle, remains a mystery. (p. 44)

In conclusion, summing up Steiner's idea to account for (particular) descriptive applicability, we can say that it consists in matching the mathematical concept at issue with a physical corresponding property, whose presence or absence makes the mathematical concepts properly applicable or inapplicable. It is important to notice that Steiner imposes a condition for this matching: that the physical property must be *the same for all the contexts* in which the mathematical concept seems to be relevantly applicable. For, descriptive applicability of complex analysis in physics seems miracolous just because it cannot be connected to *one* general property always present in all the different cases of application. Sometimes it is possible to do that, sometimes it is not — and in these latter cases the descriptive applicability of the mathematical concept at issue remains a mystery.

3.1.4 The heuristic applicability: naturalism *vs.* anthropocentrism

Even when descriptive applicability can be accounted in the way we have just seen, there is another problem raising, connected to the fact that generally mathematicians, and not physicists, developed the mathematical concepts and that only later such mathematical concepts revealed themselves as apprpopriate and effective in physics. «It concerns the applicability of mathematics as such, not of this or that concepts. It is a [*sic*] therefore an epistemological question, about the relation between Mind and Cosmos» (p. 45). It is nothing else but the question raised by Eugene Wigner in his (1960), which I have already discussed in the previous chapter.

Steiner criticizes Wigner beacuse his flawed presentation does not distinguish between two different arguments that can be find in his article. The first (argument A) is summed up by Steiner as follows:

(1) Concepts $C_1, C_2, \ldots, C_n$ are unreasonably effective.

(2) Concepts $C_1, C_2, \ldots, C_n$ are mathematical.

(3) Hence, mathematical concepts are unreasonably effective.

This is not a valid argument, evidently, since from it we can only deduce that *some* — not all — mathematical concepts are unreasonably effective and, by the way, no argument is given to show that the unreasonableness of these concepts is due to their being mathematical. The second argument (argument B), on the other hand, is almost the same argument we encountered in the previous chapter. Here is how Steiner formulates it.[5]

(1) Mathematical concepts arise from the aesthetic impulse in humans.

(2) It is unreasonable to expect that what arises from the aestheticl impulse in humans should be significantly effective in physics.

(3) Nevertheless, a *significant* number of these concepts are *significantly* effective in physics.

(4) Hence, mathematical concepts are unreasonably effective.

Also argument B is exposed to a risk, for «what is so significant about the number of mathematical concepts that have proved effective in physics? What about all the failed attempts to apply mathematics to nature? Are not, in fact, most such attempts doomed to failure?» (p. 46). If Wigner replies that even a single success in applying a mathematical concept is significant, he is thrown

[5]As I precised in footnote 2 at p. 32, my formulation slightly differ from Steiner's one, but they are essentially the same.

back to the fallacy of argument A, for he unduly extends the 'significativeness' (and hence the 'unreasonableness') of a single mathematical concept to the whole mathematics. On the other hand, if he limits his analysis to the only *significantly effective* mathematical concepts, one might easily retort that the notion of "significantly effective" is very vague and it is not sufficiently clear when effectiveness would be "significant".

Instead of turning down these challenges by finding a way out, Steiner proceeds to develop a different version of Wigner's argument, shaped to talk — like Wigner's one — about mathematical concepts in general, but rather centered on the peculiar role of mathematics in scientific *discovery*. This claim presupposes that Wigner's problem was not about discovery, and this cannot be proved (since Wigner is quite confused in his presentation, as we have already seen). However, Steiner's argument is surely original, having the merit of laying out an interesting philosophical argument. Part of this argument has been exposed in the previous chapter, and here I shall repeat myself to be clear.

Steiner distinguishes two roles for mathematics in discovering new laws: the first is deductive, the other is nondeductive. The first role consists, generally, in deducing new laws from old laws, and it can be accounted, according to Steiner, by Frege's treatment of the deductive role of mathematics we dealt with in sections 3.1.1 and 3.1.2. For example, the differential equation

$$\frac{d^2\theta}{dt^2} + \frac{g}{l} sin\theta = 0$$

representing the motion of a simple pendulum can be derived from the Newton's laws of gravity by means of logical deductions. More interesting is, instead, the second, *nondeductive* role. The source of Steiner's argument lays in a consideration regarding physics from the end of the nineteenth century:

> by the end of the nineteenth century, physicists began to suspect that

> the alien laws of the atom could not be deduced from those governing macroscopic bodies. Nor, of course, could they be determined by direct observation. Atomic physics seemed reduced to blind guessing, with an uncertain future. (p. 48)

So, how did physicists manage to discover successful theories concerning objects so far from our direct observation? How could this "blind guessing" be guided towards the right direction? Steiner's answer is: by analogy. «Having no choice, physicists attempted to frame theories "similar" to the ones they were supposed to replace» (p. 53). But the notion of similarity is somewhat vague if the "ground" of the analogy is not set forth. For any objects are similar in some respects and dissimilar in others. Moreover, such analogies could not be *physical*, since the atoms displayed no analogy with the clasical laws known until then: «reasoning by *physical* analogy had already been discredited in atomic theory. The whole trouble was that the laws (if any) of the atom (if any) were proving *not* to be analogous to those of bodies. The answer can only be, that (for lack of anything better) scientists began relying on nonphysical analogies» (p. 54). Steiner identifies two distinct kinds of *non-physical* analogy, that played and still *play* — according to him — an important role in discovering new laws of physics — to the extent that, he says, «without them, contemporary physics would not exist» (p. 54). He calls them 'Pythagorean' and 'formalist' analogies (or taxonomies). Here are the respective definitions, as stated by Steiner (see p. 54):

- **Pythagorean analogy** — A Pythagorean analogy (or taxonomy) at time t is a mathematical analogy between physical laws (or other descriptions) not paraphrasable at t into non-mathematical language.
- **Formalist analogy** — A formalist analogy (or taxonomy) is an analogy based on the syntax or even the orthography of the *language* or *notation* of physical theories, rather than what (if anything) it expresses.

Now, Steiner's argument can be summed up as follows: Pythagorean and formalist analogies play a crucial role in the fundamental physical discoveries of the last century, but they are both *anthropocentric* and hence deeply *anti-naturalistic*. Steiner introduces here two new terms, "anthropocentrism" and "naturalism" that must be carefully defined and considered. The term "naturalism" has known a wide variety of definitions over the history of philosophy, but in analytic field it refers usually to Quine: naturalism is the thesis according to which natural sciences are the last tribunal for truth and hence philosophy should be part of (or continuous with) natural sciences. However, Steiner does not adopt this definition, and actually does not even offer a positive definition of the terms. He just says that it must be intended in opposition to anthropocentrism, where anthropocentrism is defined as «the teaching that human race is in some way privileged, central to the scheme of things» (p. 55).[6] For example, a naturalist will say that the universe is indifferent to the goals and values of humanity and that believing the contrary is just a proof of hubris. Steiner's idea is that contemporary scientists (particularly physicists) has progressively — and, probably, even unconsciously — adopted an anthropocentric point of view in applying mathematics.

Steiner distinguishes three kinds of anthropocentrism:

- *overt* anthropocentrism,
- *covert* anthropocentrism, and
- *"play it safe"* anthropocentrism.

The first kind takes the form of *theories* which state, either explicitly or implicitly, that human race plays a privileged role in nature. So, for example, creationism is an *explicit* form of *overt* anthropocentrism, and geocentrism is an *implicit* form of *overt* anthropocentrism (since it implicitly assumes that human race occupies a privileged place in the universe). *Covert* anthropocentrism, instead, is not predicated of a theory, but rather of a *behavior* which

[6] Actually, as Steiner precises, he has «no desire to lay claim to the *word* naturalism» (p. 55): «If there were a decent word in English for "anti-anthropocentrism," I would gladly drop the term "naturalism"» (p. 55, footnote 19).

presupposes some anthropocentric doctrine. It means that «behavior by an agent A is covertly anthropocentric if it is irrational if A has no anthropocentric beliefs» (p. 56). A typical example of *covert* anthropocentrism consists in classifying phenomena by reference to human peculiarities. So, as an instance, Pythagoreanism is *covertly* anthropocentric for it classifies numbers as "male" and "female", and Aristotelian physics is likewise since it classified physical bodies into "heavenly" and "terrestrial". Finally, the third kind of anthropocentrism is defined by Steiner as the position «which pretends to avoid statements concerning the status of the human race in the Great Chain of Being. "Play it safe" anthropocentrism advocates, modestly, that science cannot confirm any hypothesis about the unobservable» (p. 58). So, for example, Van Fraassen's ideal that all we should pretend by our scientific hypothesis is that they are empirically adequate (and not *true*) is a form of *"play it safe"* anthropocentrism, since it implies that human science can only reflect human limitations.[7] According to Steiner, this last version of anthropocentrism is no different than the other two:

> A philosopher who objects to projecting anthropocentric hypotheses should also object to *limiting* or *weakening* hypotheses by restricting them to anthropocentric categories (such as empirical adequacy). The content of the prediction may now be weaker; but the prediction itself (the speech act) makes a *covert* anthropocentric statement. (pp. 58-59)

Let me make just few considerations about Steiner's kinds of anthropocentrism. Actually, it seems to me that there is only *one* anthropocentrism, consisting in the idea that a theory is anthropocentric if it claims that human race plays a privileged role in nature. What is different is the way in which I could state that claim: I could do it either explicitly or implicitly; or I could imply such a claim (either explicitly or implicitly) in my behavior. However, I do not agree with Steiner when he says that *"play it safe"* version of anthropocentrism is not different than the others. Actually I think *it is* different. He argues that,

[7] On Van Fraassen's notion of "empirical adequacy" see van Fraassen (1980) and the more recent van Fraassen (2008).

for example, «the same theories that tell us that we cannot see X- rays tell us also that what we do see (light) is a random sample of radiation; i.e. that there is nothing special about light, and that there is nothing wrong per se with projecting the properties of light on unobservable radiation. Deliberately to limit our projections concerning radiation to light, then, is to imply, covertly, theses about the centrality of the human race» (p. 59). I contest this point: if we understand the notion of empirical adequacy in terms of consistency with all our possible human observations (as Van Fraassen does), this last term has surely an anthropocentric character — and Van Fraassen himself admits it. But this does not imply any anthropocentric hypothesis, for we are not — as Steiner says — «*limiting* or *weakening* hypotheses by restricting them to anthropocentric categories» (p. 58); rather, we are anthropocentrically limiting our *possibilities to confirm* those hypotheses. To say that our knowledge of the word is limited by anthropocentric categories does not imply, in any sense, that in our hypotheses human race plays a central role, but only that these hypotheses can be tested only by those anthropocentric categories.

Anyway — coming back to Steiner's analysis — both *overt* and *covert* anthropocentrism have been strongly opposed in modern science, but nevertheless, Steiner argues, «*recent* physics — from about 1850 — has retreated from naturalism. Truly great discoveries in contemporary physics were made possible only by abandoning — often convertly and even unconsciously — the naturalistic point of view» (pp. 59-60). This conclusion is actually drawn from the following two premises:

I. Both the Pythagorean and formalist systems are anthropocentric; nevertheless,

II. Both Pythagorean and formalist analogies played a crucial role in the fundamental physical discoveries of this century.

Let us leave aside premise II for a while, and let us focus on premise I. The question is: why are these analogies *anthropocentric*? Let us consider Pythagorean analogies first. What is *anti*-naturalistic in them? This question, according to Steiner, hangs upon another: what is mathematics and what is the criterion for a concept to be "mathemtatical"? For example, why is chess a game and Hilbert space mathematics? We already discussed Steiner's view on this argument (see section 2.2), but let me briefly repeat it. Steiner argues that there are two criteria which determines whether a concept is mathematical or not: beauty and convenience. The first establishes that a concept is mathematical if it satisfies our aesthetical sense of beauty. The latter that a concept is mathematical if it compensates (or increases) our computational limits. As I underlined in section 2.2, following Pincock (2012), Steiner is confusing two different questions, and that in attempting to answer the first ("What makes this concept mathematical or non-mathematical?") he can at most answer to the second ("What makes this concept a good or a bad mathematical concept?"). Anyway, following Steiner's argument, beauty and convenience are highly anthropocentric categories, and hence the mathematics that results from the employment of these two categories cannot be non-anthropocentric: «[...] the human aesthetic sense is nothing but species-specific preference. Classifications like beautiful/ugly are then anthropocentric; so, finally, are the mathematical classifications» (p. 66). If the analogies we rely on in discovering new physical laws are Pythagorean — namely, if they are not paraphrasable into a non-mathematical language at the time we employ them — then, those analogies are anthropocentric.

Coming to formalist analogies, the anthropocentric character is even more blatant, since (at least) from John Locke onwards, we accepted the idea that the syntax (or other formal properties) of a language need not correspond to a scheme outside language. For example,

> the symmetries of our notation need not to reflect the symmetries the

> notation describes; does a sentence have an enhanced claim to be true because it is a palindrome? To think this is arrant anthropocentrism. [. . .].
>
> Expecting the form of our notation to mirror those of (even) the atomic world is like expecting the rules of chess to reflect those of the solar system. I shall argue, though, that some of the greatest discoveries of our century were made by studying the symmetries of notation. Expecting this to be any use is like expecting magic to work. (pp. 71-72)

Justifications for premises II are offered by Steiner in chapters 4, 5 and 6 of his book, where he presents a large amount of concrete cases, taken from contemporary physics, in which Pythagorean and formalist analogies seem to play a crucial role. I am not going to discuss all these cases in details, but some words are needed to better understand in which sense Pythagorean and formalist analogies are — according to Steiner — so crucial in contemporary physical development.

A typical Pythagorean strategy is the following: «Equation E has been derived under assumptions A. The equation has solutions for which A are no longer valid; but *just because they are solutions of E*, one looks for them in nature» (p. 76). As an instance, Steiner mentions Maxwell's discovery of electromagnetic radiation. Since the (experimentally well confirmed) laws of Faraday, Coulomb and Ampère turn out to contradict the conservation of electrical charge when put in differential form, Maxwell tried to "save" this conservation law by introducing in Ampère's law a new hypothetical current, which he called "displacement current". Although no empirical evidence pushed to doubt Ampère's law and there was very little experimental evidence for the reality of this hypothetical current, Maxwell followed this line and considered the magnetic field as given by the sum of the "real" current and the "displacement current". Then, *by ignoring that for Ampère's law magnetism is caused by an electric current*, Maxwell asserted that the previous equivalence was valid also for a zero "real" current. «This — *Steiner comments* — made electromagnetic radiation a mathematical possibility. The belief that it was also physically real required a Pythagorean analogy — one that paid off» (p. 77).

Another Pythagorean strategy frequently adopted in contemporary physics is the following one:

> One looks for solutions in nature even where there is reason to doubt their very possibility. There is no *a priori* reason to believe that every solution of an equation has a physical interpretation. There is nothing logically wrong, therefore, with discarding certain solutions of an equation, and it is often done (for example, unbounded solutions of Schroedinger's equation). Nevertheless, the Pythagorean scientist goes by the working hypothesis that a mathematical possibility will be realized by nature. (p.82)

As an example of this second strategy, Steiner mentions Dirac's discovery of anti-matter: Dirac's equation gave solutions describing electron very well. But it also allowed particles of negative energies as solutions. There were no reason to accept their very possibility, but Dirac went on and accepted these negative energies as real.

A third kind of strategy is so summed up by Steiner:

> Suppose we have succesfully classified a family of "objects" by a mathematical structure S. Then we project that this structure, *or some related mathematical structure* T, should classify other families of objects, even if, given present knowledge, (a) S is not reducible to a physical property, and (b) the relation between S and T is not reducible to a physical relation. We have doubly Pythagorean analogies. (p. 84)

One of the examples offered by Steiner is isospin: no *physical* analogy has been discovered, up to now, between spin and isospin; but isospin is nevertheless considered as a conserved quantity *mathematically* analogous to spin.

Unitll now, Steiner presented only strategies for discovering solutions and symmetries of the laws of nature. Another Pythagorean strategy, in which what is discovered is instead the equation itself, is at work when «One formulates equations by analogy to the mathematical form of other equations, even if little or no physical motivation exists for the analogy» (p. 94). An example is given by the procedure adopted by Heisenberg in deriving matrix mechanics. Starting with the classical Hamiltonian equations of mechanics, Heisenberg replaced all the variables in the equations by matrices, and all the operations by corresponding matrix operations.

> This procedure — *Steiner comments* — is an example of "quantization" — i.e., of transforming a false classical equation for an atomic system into (what is hoped to be) a true quantum equation for that system. There is no rationale for this procedure, [...], except a Pythagorean analogy. To put the matter another way, it is impossible to imagine a physicist *discovering* the matrix equation by direct physical reasoning, skipping entirely the classical step. This is because the matrix equation, though one can extract measurable "numbers" from it, and therefore confirm the equation, does not "say" anything about the physical system which can be expressed — even qualitatively — without the matrices. Matrices as such have no independent physical meaning. (pp. 96-97)

Eventually, two further Pythagorean strategies, very akin, are at work when «A refuted law is used to test new laws — the "old" law is stipulated to be a special or limiting case of any "new" law» (p. 105), and when «A refuted law — false by definition — is nevertheless used to derive new laws» (p. 106). What is Pythagorean in these strategies is that both

> presuppose that mathematical structures are more robust than the laws that instantiate them. Even if a law is refuted, its mathematical form (symmetry) must play a role, one projects, in future developments. Such a projection is Pythagorean, because we cannot characterize physically what we mean by "mathematics". (p. 106)

Regarding formalist analogies, Steiner does not make a list of different strategies, but only offers several examples, all taken from Quantum Mechanics. In this context, «formalist analogies often take the form of pseudodeductions: instead of preserving truth, formalist reasoning establishes meaning» (p. 116). The most interesting example offered by Steiner is probably the so called process of "quantization". This strategy consists in assuming that the system obeys the classical laws (which is false), and then in transforming this classical assumption into a (hopefully) true quantum description of the system, by means of syntactic transformations. According to Steiner,

> the discoveries made this way relied on symbolic manipulations that border on the magical. I say "magical" because the object of study of physics

> became more and more the formalism of physics itself, as though the symbols were reality — and the confusion of symbols with reality is what characterizes much of what we call magic. (p. 136)

All these examples clearly shows, according to Steiner, that both Pythagorean and formalist analogies play a crucial role in contemporary physics. Thus, finally, if we admit that Steiner has offered us convincingly reasons to believe both in premises I and II, we must conclude with him that an anthropocentric policy was a necessary factor in discovering today's fundamental physics, and that «This makes the universe look, intellectually, "user friendly" (in that our categories of beauty and convenience are found in the "real essences" of things) to our species, or other species like ours, if any» (p. 8).

Three clarifications are in order to better understand this conclusion. The first is that Steiner is not saying that this anthropocentric policy is *the only* necessary factor in discovering today's fundamental physics. It is just *one* of the many. The second is that the anthropocentrism Steiner is professing has to do only with the *discovery of*, not with the *content of*, present-day theories. The content of these theories may even be considered non-anthropocentric; what is anthropocentric is the manner by which we achieve them.[8] Finally, we can say that this conclusion challenges naturalism, but *only to the extent that* "naturalism" rejects any anthropocenric point of view.

3.2 Some remarks on Steiner's analysis

Is Steiner's analysis satisfying? In this section I will try to show that it is not, even if we must admit that many elements of interest are contained in it. Generally speaking, Steiner's conclusion is not very far from Wigner's one: for Wigner, the applicability of mathematics is in general a mystery that we *cannot*

[8] It follows from this that the particular version of anthropocentrism Steiner is talking about must be the *covert* one. For overt anthropocentrism (given Steiner's definition) is something we can predicate of *theories*, not of *discoveries* of theories.

solve. For Steiner there is no *single* problem of applicability; we can solve (and solved, actually) some of them, but some others are still mysterious. Particularly compelling are the anthropocentric (or anti-naturalistic) assumptions that we do in order to profitably employ mathematical reasoning in discovering new physical laws. Thus, in general, we must admit that applicability still remains, for the most part, mysterious.

3.2.1 Some remarks on the validity of Frege's solutions

As we have seen, Frege's philosophy is directly invoked to solve two different problems, the semantical and the metaphysical. So, the first question one could ask is: Is really Frege's philosophy of mathematics the solution of (almost) all of our problems? In the first case, the problem asks to find a constant interpretation for both "pure" and "mixed" contexts, and Frege's solution consists in saying that all occurrences of numerals in mixed contexts can be translated in a uniform interpretation of the form "The number of the Gs is n". Now, this solution is obviously suited for arithmetic, and it is not clear whether the semantic problem is a problem only for arithmetic or for other contexts as well. Steiner does not take an explicit stand on this point, but he apparently thinks the semantic problem is involved also in different areas of mathematics. Thus, if really the semantic problem can be raised also in non-arithmetical contexts, we have no clue about how to solve the problem in these contexts.[9]

A second problem concerning Frege's solution to the semantic problem is raised by Pincock (2012). Frege's solution implies two necessary assumptions: (1) it seems to presuppose platonism concerning mathematical entities; (2) it presupposes that the substantival interpretation of all mathematical terms is actually the correct logical form of all mathematical sentences. Even if we

[9] I tried to figure out some new, non-trivial example to illustrate, concretely, the occurence of semantic problems *of this kind* in non-arithmetic contexts, but I could not find any good instance. So, the doubt remains: is it just a problem for arithmetic, or has it a wider scope?

accept (1), it remains pointless if we do not accept (2). But neither Frege nor Steiner provide reasons to accept (2). Even more puzzling, Steiner claims that «One could [...] also solve the "semantic" problem of the applicability of mathematics with a theory according to which all numerals are really predicates» (Steiner 1998, p. 17n), and to this end he mentions the work of Hellman (1989). Now, Hellman's project consists in offering a modal-structuralist interpretation of mathematics according to which all mathematical claims are about possible structures. Purely numerical claims like "7+5=12" come out as general claims about the features of any number structure. Thus, "7+5=12" should be interpreted as "In any number structure S, adding$_S$ the 7_S to the 5_S *necessarily* results in the 12_S"; and our mixed statements like "There are seven apples on the table" should be interpreted (uniformly) as "The apple on the table instantiate the initial segment '7_S' of the natural number structure S" — in order to have a valid deduction. Now, as Pincock (2012) points out,

> As with the Fregean approach that Steiner defends, it is not clear if a modal-structuralist is entitled to their modal assumptions or the assumption that natural language sentences have logical forms that match their interpretation. Hellman, for one, is not committed to this strong assumption as he is not trying to solve the semantic problem as Steiner presents it. But given Steiner's apparent admission that this sort of adjectival strategy could succeed, the burden of the proof is then on Steiner to explain how his substantival solution is superior. [...]. Until he does this, I insist that the semantic problem remains open. (p. 171)

Coming to Steiner's fregean solution to the second, metaphysical problem, a further difficulty arises if we analyze more closely the relationship between the logicist thesis and Frege's solution to the metaphysical problem. The validity of Frege's solution crucially depends on the interpretation of mathematical concepts as *second order* concepts (i.e., they are concepts *of concepts*, and not of objects). The metaphysical application of a mathematical concept is no more a problem since we are simply talking of concepts that can be variously instantiated — not least by physical objects. But, in Frege's view, mathematical concepts are second order concepts just *because* they are *logical concepts*. In Dummett's (1991) words,

> Frege tacitly took the application of a theorem of arithmetic to consist in the instantiation, by specific concepts and relations, of a highly general truth of logic, involving quantification of second order or yet higher order: if the specific concepts and relations were mathematical ones, we should have an application within mathematics; if they were empirical ones, we should have an external application. (p. 256)

As we saw in chapter 1, Frege's explanation of the (metaphysical) applicability of arithmetic relies on two different components: (A) the analyticity of the arithmetical truths and (B) the reduction of arithmetic to logical terms, namely, to (logical) laws of concepts. (A) guarantees that the application of arithmetic is conservative, i.e. that it does not lead to true premises to false conclusions; and (B) enables us to apply arithmetic everywhere the proper concepts appear. (B) can be further analyzed as consisting of: (B.1) the logicist reduction, namely the fact that arithmetic is reduced to logic; and (B.2) the fact that this reduction is made such in a way that the arithmetical concepts result to be concepts of concepts (and hence applicable only to concepts and not to objects). Now, only (B.2) has an explanatory value regarding Frege's account of the applicability of mathematics; so, we can try to revive Frege's solution even in absence of (B.1). However, if we want to do this, we must come to terms with the fact that, in Frege's philosophy, (B.2) is actually a consequence of (B.1): arithmetical concepts are second order concepts just *because* they are *logical* concepts. The same must be said of (A): mathematics is analytic just *because* it consists of nothing but logical truths. It follows, if I am right, that if we want to save Frege's account without committing ourselves to logicism, we must offer indepent reasons why arithmetic is *conservative* and why mathematical concepts are *second order concepts* — which Steiner omits doing. Steiner holds that these solutions are independent of Frege's logicism, since we can frame all the *Grundlagen* in Boolos' FA, be it logic or not. That's true, but this only says that the *Grundlagen*'s system is consistent since we can express it in a second order logic (substituting Frege's Basic Law V with the so-called Hume's Principle), not that the philosophical justification for accepting it is no more needed.

Moreover, Steiner's extension of this strategy to the rest of mathematics is based on the modelization of classical mathematics in ZFC, and this move underlines how the only relevant relation between sets and their application turns out to be the setting up of a function of some kind. As Pincock (2012) points out,

> If this set-theoretic solution is deemed adequate, then it shows how low the bar is set to solve the metaphysical problem. Steiner raises the issue by alluding to a gap between mathematics and the physical world. A bridge across this gap need only show that mathematics is related in some way to the physical world. But as Steiner's own set-theoretic response indicates, there is no requirement that the bridge do anything else. It need not illuminate what [...] mathematics contributes to the success of science. To know that every physical object is a member of a variety of sets, including ordered pairs that are members of sets identified with functions of a certain sort, is totally trivial once we have adopted ZFC with these objects as individuals. (pp. 173-174)

Pincock's idea is rather that a clear comprehension of the applicability of mathematics requires a deep analysis of *what* and *how* mathematics contributes to science. Steiner's answer fills the gap between mathematical entities and physical entities, but it leaves open the problem of why and how this "filling the gap" can (and actually *is*) fruitful for science. For example, «this solution to Steiner's metaphysical problem does not address Field's worry about extrinsic explanation» (p. 174). Steiner might answer to this objection by saying that we should not confuse different problems, but try to keep them well separated. Thus, the trivialization of the metaphysical problem is just a (positive) consequence of his analysis. In other words, we must keep separated the *possibility* of filling the gap (by means of special functions from physical to mathematical objects — functions that can be themselves considered as ordered pairs, so that the only relatioship between physical and mathematical objects we need recognize is set membership) from the actual manner in which we can perform this filling, and just say that the mere *possibility* of filling the gap is enough to solve the metaphysical problem — it does not matter how we perform it. However, given this trivialization of the problem, there's no surprise that it can be solved also in different ways. For example, Pincock suggests Wilholt's (2004) solution, according to which some mathematical entities can be considered as properties or relations, and these properties or relations can be predicated of physical objects. Another example is given by the sort of relations which structuralist views appeal to.[10] Thus, «As with the semantic problem, these other solutions at least put the burden on Steiner to explain why his Frege-inspired solution is the best or most promising» (Pincock 2012, p. 174).[11]

There is another problem that can be raised concerning Steiner's solution to the metaphysical problem and which is strictly related to its "trivialization". The setting up of a function (or of a relation of membership) between mathematical and physical objects, seems to presuppose that we have *from the beginning* two kinds of clearly distincted objects (mathematical and physical) and that we can couple them together without any problem. However, as I've already noticed at the end of chapter 2, many physical concepts and objects seems rather to be the result of a highly abstract process of matematization, and sometimes it is even uncertain whether a concept (or an object) should be considered as a mathematical or physical one. But if physical objects are spelled out from mathematization, that means we can couple them with mathematical objects only *a posteriori*, namely, only *after* the process of mathematization has produced its fruits. Steiner is right in saying that we must keep distinguished the different problems of applicability that surround mathematics, but I think

[10]My proposal, articulated in chapter 5, is actually based on this sort of relations. Another similar examples of these structural relations can be found in Shapiro (1983), Bueno & Colyvan (2011), Pincock (2012) and others.

[11]In chapter 5 I will actually try to spell out some "metaphysical" reasons why we should prefer a structuralist account on others competitors.

that the various solutions should show kind of coherence among them. This does not necessarily invalidate Steiner's solution to metaphysical problem, but seems rather to suggest that a surplus of argumentation is needed in order to show that this solution is, in some sense, compatible with the solution to the other problems presented and with the roles of mathematics in science.

Thus, once again, the conclusion seems to be that, in formulating the problem (problem*s*) of applicability, we should avoid to understand this "application" in a too much litteral sense: physics and mathematics are not to be intended as two pieces of a puzzle, of which we must understand the way in which they can be connected. They are much more intertwined than this picture suggests, and if we are going to properly understand the applicability of mathematics to science, we cannot underestimate this point.

3.2.2 Description and representation

As we saw, *descriptive* applicability problems are only partially solved by Steiner. Depending on the particular mathematical concept involved, in some cases we are able to offer a non-mathematical description of what the world must be like in order that a specific mathematical concept can be used to effectively predict observations; whereas in some cases we are left without a clue about how this aim could be achieved.

There are two points that Steiner seems to presuppose in accounting for this descriptive problem. The first, as I've already noted, is that Steiner presupposes that, when a mathematical concepts is employed in two different physical contexts, the physical property underlying the two applications must be the same. The second is that Steiner seems to consider his descriptive account as an account for the representative role of mathematics. But if this is right, then — as is well noticed by Pincock (2012) — Steiner is considering the descriptive role of mathematics as the only form of representation that mathematics can offer — and this is surely reductive.

Let's start from the first point. Why should we limit our account by assuming that a unique physical property must explain all the applications of a mathematical concept? There is no particular reason, I think. We can admit that, for example, the descriptive efficacy of analytic functions in fluid dynamics has not to be, necessarily, based on the same properties as it is for the descriptive applicability of analytic functions in thermodynamics. Steiner would reply by saying that the situation for analytic functions does not resemble the case of additivity, where on the contrary we have a unique physical property which is able to explain the occurrence of addition in every context. But this implies that we are assuming the additive case as the standard measurement unit to evaluate all the other cases — and this is an unwarranted assumption.

However, even if we admit that different applications of the same mathematical concept do not need to be accounted by the same physical property, there are still mathematical concepts which we are still left without a clue about how to (descriptively) account for — for example, the application of analytic functions in fluid dynamics and thermodynamics. I do not have a solution for these problems, but I just want to notice that the difficulty of finding a physical property matching a mathematical concept *can* also be seen as a clue that the application at issue is actually *not descriptive*. This is a point that Steiner seems not to really take into consideration. The descriptive applicability seems to account for the possibility of using mathematics as a representative tool. But a representation is a very complex thing, and an element of a representation can play several, different roles — and description is just *one* of them. Pincock (2012), for example, distinguishes between *intrinsic* and *extrinsic* mathematics employed in representations: the first is the mathematics that *figures* in the content of a representation; the latter is the additional mathematics that, even not figuring in the representation, still contributes to the efficacy of the representation. So, for example, we can mathematically describe the dynamic of a system by means of some set of equations. These equations (along with the mathematics required to present these equations) will be the *intrinsic* mathematics appearing in the representation. But we could employ this representation to allow the derivation of further representations, and in doing this we could need some extra mathematics. In this case, this 'extra' would be what Pincock calls the *extrinsic* mathematics. Beyond this, Pincock specifies four dimensions along which mathematics can contribute to a scientific representation. These dimensions run along four basic dichotomies: (1) causal/acausal content, (2) concrete fixed/abstract varying content, (3) small-large representation (issues of scale), and (4) constitutive/derivative content. Thus, for a given mathematical representation, we can ask whether the mathematics involved is intrinsic or extrinsic, and then we can ask whether and how the mathematics contributes to (1), (2), (3) or (4) (it is also possible that the contribution combines different features of two or more of these dimensions). Pincock conclusion is that

> mathematics makes an *epistemic* contribution to the success of our scientific representations. Epistemic contributions include aiding in the confirmation of the accuracy of a given representation through prediction and experimentation. But they extend further into considerations of calibrating the content of a given representation to the evidence available, making an otherwise irresolvable problem tractable and offering crucial insights into the nature of physical systems. So even when mathematics is not playing the metaphysical role of isolating fundamentally mathematical structures inherent to the physical world, it can still be making an essential contribution to the success of science. For part of this success is the fact that we take our evidence to confirm the accuracy of our best scientific representations. And it is here that the mathematical character of these representations makes its decisive mark. (Pincock 2012, p. 8)

It is often very difficult to decide exactly when a mathematical contribution has to be dubbed as 'intrinsic' or 'extrinsic', as well as to decide which dimension the mathematics is playing a role in. However, we do not need to solve this problem here. For our aims it is enough to show that mathematics can also be *extrinsic*. If we accept this, it is clear that extrinsic mathematics cannot be accounted in the descriptive way proposed by Steiner. The point is that not all mathematics works descriptively: mathematics can play a great variety of roles in its contribution to representation; so, no surprise if Steiner's solution cannot work for all mathematical concepts applied in sciences. What surprises, rather, is the fact that Steiner didn't pay much attention to this variety of roles.

3.2.3 Deflating the criticisms to naturalism

The last considerations pertain Steiner's criticisms on naturalism and his idea of the anthropocentric character of mathematics. As we saw, it is strictly related to the heuristic role of mathematics in science: mathematical analogies and reasonings (both Pythagorean and formalist) permit to make important advancements in science, just in spite of this anthropocentric character — which contrasts with naturalism.

Some considerations are in order. First, is mathematics really anthropocentric? Second, do these analogies and reasonings really represent a problem for naturalism? And finally, is this way of setting out the problem satisfactorily?

Concerning the first question, we can go back to section 2.2, where I have already taken into consideration the reason why Steiner is led to thought of mathematics as 'anthropocentric'. As I said there, his argument is not conclusive. For he holds that the anthropocentric character of mathematics comes from the *constitutive* role that aesthetical criteria play in it, but I have shown there that Steiner's arguments can at best argue for a *selective* role of aesthetical criteria in mathematics. This does not permit us to reject Steiner's thesis, but his argument is not enough in order to hold it up. Hence, in absence of a stronger argument, it seems to me reasonable to comply only with the more modest thesis according to which aesthetical criteria in mathematics cannot have a role but *selective*.[12]

[12]Bangu (2006) comes to a similar conclusion by considering some historical reactions to

If we are not reasoned to think that mathematics is species-specific, we cannot conclude that mathematics is anthropocentric. The fact that the employment of mathematical reasoning can led to new discoveries in physics is then not necessarily a problem for naturalism. However, even if mathematics is not anthropocentric, that a physical discovery has been done only relying on mathematical analogies still constitutes a problem for the naturalist philosopher, since she seems to be in difficulty when we ask her to justify why scientists are led to believe something only relying on these mathematical analogies. The problem is quite complex. First of all, one might raise some doubts about Steiner's reconstruction of several cases he presents as examples of anti-naturalistic discoveries. Are we really sure, for example, that Heisenberg's discovery of isospin was made only by relying on a mathematical analogy? According to Steiner's reconstruction,

> In 1932, Heisenberg conjectured boldly that the proton and the neutron — ignoring their opposite charge — are two states of the same particle, "spinning" in opposite directions in a fictitious three-dimensional Euclidean "space". The space had to be fictitious, since (unlike the situation with the "up-down" electronic states) one cannot turn a neutron into a proton by standing on one's head. Heisenberg reasoned that the nucleus of the atom is invariant under $SU(2)$ transformations, those which describe the spin properties of the electron; and that there had to be, therefore, a new conserved quantity, *mathematically* analogous to spin. (pp. 86-87)

And then, commenting on this, Steiner notices:

> [...] Heisenberg's theory was not *just* that the neutron and the proton are the same particle. That hypothesis would require only a weaker symmetry: that one could "swap" neutrons and protons discontinuously (the permutation group), in any physical process not involving the charge. Heisenberg's theory is that the neutron is obtained from a proton by a continuous *abstract* "rotation of 180 degrees," and also that to return a neutron or a proton to its initial isospin state, one must "rotate" the particle a full 720 degrees in the *fictitious* isospin space. It seems clear that the mathematics is doing all the work in this analogy, and that Heisenberg's analogy was highly Pythagorean. (p. 87)

what Maddy (1997) calls "definabilism" (the view that all mathematical objects must be definable in an explicit and uniform way). Being definabilism anti-anthropocentric, Steiner's thesis turns out to be undermined.

However, a different reconstruction is possible and is offered by French (2000).[13] According to him, in the isospin case

> the effectiveness of mathematics surely does not seem quite so unreasonable, as group theory is brought to bear via a series of approximations and idealisations, the most important being the ontological move. In effect physics is manipulated in order to allow it to enter into a relationship with the appropriate mathematics, where what is appropriate depends on the underlying analogy. At the most basic level, what motivates this manipulation and therefore underpins the effectiveness of mathematics in this case are the empirical results concerning intra-nuclear forces and the near equivalence of masses. (p. 114)

Following French, we can reject Steiner's claim according to which «mathematics is doing all the work here» (p. 87). Nevertheless, even if we accept French's reconstruction, we have ruled out isospin case from the 'black list' of anthropocentric cases, but all the other cases are still there. These considerations do not exclude that *there actually are* Pythagorean discoveries in contemporary physics.

One might say that the role of Pythagorean and formalist analogies in physics is only heuristic. By recovering the outdated distinction between the context of discovery and the context of justification, one might say that even if it is true that the analogies are, in some sense, "anthropocentric", they are nonetheless part of the context of discovery. Their acceptance does not depend, in no sense, on the way they have been discovered. The discovery could have even been irrational or based on purely psicological facts, but its acceptance requests a justification — and if a *non-anthropocentric* justification can be given, then the problem does not raise. However, this argument cannot reply to Steiner, since the problem is exactly that there seems not to be other justifications of these discoveries different from the mathematical analogies that led to them. As Steiner correctly notices while discussing of isospin case, «even today, nobody

[13] French does not mention Steiner. His considerations are aimed to oppose Wigner's claim about the unreasonable effectiveness of mathematics. Wigner and Steiner have different arguments, but, as we saw, both agree on the point that it is (at least up to now) unreasonable how and why mathematics is so effective in science. To the extent that French's arguments are aimed to reject Wigner on this point, they are equally good to reject Steiner too.

knows why electron spin and isospin have the same symmetry — and even if someone were to explain the coincidence, the explanation was not available to Heisenberg in 1932» (p. 87). Hence, either we offer a different reconstruction of why Heisenberg was *justified* in postulating the existence of isospin (and we saw that, for this case, French's reconstruction is a valid alternative), but then we should do the same for all the other cases presented by Steiner); or we must admit that the distinction between context of discovery and context of justification is of no help in replying to Steiner on this point.

A more general critics to Steiner can be moved if we consider more closely what he means by Pythagorean analogy. Remember that «a Pythagorean analogy (or taxonomy) at time t is a mathematical analogy between physical laws (or other descriptions) not paraphrasable at t into nonmathematical language» (p. 54). This definition is clearly linked to the notion of descriptive applicability, since for Steiner to account for the descriptive applicability of a mathematical concept amounts exactly to finding a physical property associated to the mathematical concept that permits to paraphrase it into non-mathematical terms.[14] Thus, we can say that a mathematical analogy is Pythagorean exactly when the mathematical concepts involved cannot be *descriptively* accounted.[15] However, we already considered the limits of Steiner's analysis of descriptive applicability. Particularly, we pointed out that mathematics can play *non*-descriptive, but nonetheless *representative*, roles in science. Now, following Pincock (2012), this criticism can be exploited also to say that if a mathematical concept has no *descriptive* account of its effectiveness, this does not necessarily imply that it is used in a Pythagorean way. «[T]his reductive test is unreasonable — he says — and [. . .] it ignores other kinds of well-understood contributions from mathematics to the success of a scientific representation» (Pincock 2012,

[14]See section 3.1.3.

[15]In the case of the analogy, we should account not only for the single concepts employed, but also for the possibility of exporting the concept from a context to another.

p. 185).

This criticism is however limited to Pythagorean analogies and does not touch, in any sense, the cases in which *formalist* analogies are involved. Steiner's criticism to naturalism can then be motivated only by appealing to this latter kind of analogies. It is difficult to decide whether effectively mathematics is playing here a role that can be accounted only by appealing to an alleged anthropocentric feature. However, I think that we should keep well distinguished two claims in Steiner's analysis: one the one side (I) the claim that mathematics is anthropocentric; on the other side (II) the claim that some roles played by mathematics in contemporary physics raise a real problem for naturalism. It seems to me that the arguments offered by Steiner in support of (I) can be rejected (as we did in section 2.2). Nevertheless, he gave us good arguments in support of (II), since effectively we have no account for some relevant (and not isolated) cases of scientific discovery in which mathematics seems to play a relevant role. That said, we can at best conclude that this is a challenge to naturalism, but there is no argument here in support of anthropocentrism.

A completely different criticism is moved by Peter Simons, who objects that «by concentrating only on successful discoveries Steiner has biased the story» (Simons 2001). Steiner is well aware of this kind of criticism and he tries to avoid it by moving from theories to taxonomies (see Steiner 1998, p. 73). Theories are tools aimed to describe the world and in this attempt they may go frequently wrong; taxonomies are rather *frameworks* in which such theories are formulated. Steiner's point is that *taxonomies*, not theories, are anthropocentric; namely, such theories were developed in a (methodological) framework that is, in the end, non-naturalistic — despite what scientists say. Simons criticizes this distinction, since according to him Steiner made it too simplicistic. But even if we admit that Steiner is right on this point, the problem is that the examples he offers can be differently interpreted:

> To the extent that physical phenomena admit of description in relatively straightforward mathematical terms — and we should not forget that non-linear, chaotic and uncomputable physical situations abound — *some* of the vast array of pure mathematical concepts and results piled up without regard to application by thousands of mathematicians were in the end going to be the right ones. The tortuous developments Steiner entertainingly describes bear witness to physicists' often mathematically gauche and ill-fitting hunches and trials, and incidentally show that Pythagorean or formalist analogies by no means always lead in the right direction. (Simons 2001, p. 183)

In other words, the success of physicists in making surprising discoveries can be ascribed not to an alleged "anthropocentric, user-friendly universe", but rather to «their desperate attempt to discover what looked like the undiscoverable» (Steiner 1998, p. 73): «Robbed of convincing intuitive pictures, physicists grasped at anything they understood which might help in some way to give an account of new and puzzling phenomena» (Simons 2001, p. 183).[16]

3.3 Conclusions

Steiner's work has undoubtedly several merits. First of all, it cut out the long neglect reserved by philosophy to the problem of the applicability of mathematics, and he did more than other philosophers to show that there *is* a problem for philosophy here to be faced.[17] Another of his undisputed merits consists in having underlined that there is not *one* single problem, but rather a bunch of different problems orbiting around the applicability of mathematics. This is probably his main contribution to the discussion of the problem of applicability. Even if his solutions to all these problems may be unsatisfying — as I showed in section 3.2 — he traced a "geography" of the argument that future developments shall take into consideration, and in which we can now move more wittingly.

[16]Let alone the fact, underlined by Simons and previously considered by us, that «while professing to give us the history of discoveries by physicists Steiner gives us a history largely sanitized of the physical motivations and correctives that drove the physicists themselves» (Simons 2001, p. 183).

[17]Remember that the final attempt of Steiner's work was — as he himself says — «to persuade my advisor and mentor, Professor Paul Benacerraf, that there really *is* a philosophical problem about the applicability of mathematics in natural science» (Steiner 1998, p. vii).

Nevertheless, it must be noted that his "geography" of the land is far from being complete and definitive. Among the four different applicability problems he distinguishes (semantical, metaphysical, descriptive and heuristic), the last two seem to be the most problematic. Particularly, his analysis of representative roles of mathematics appears to be quite poor, and even misleading. He actually talks only of a "descriptive" role of mathematics, but we saw that mathematical representations can play a certain number of *non*-descriptive roles. Moreover, he distinguishes two contributions mathematics can give to the discovery of new laws or new entities in physics: deductive and non-deductive. However, non-deductive contributions can be linked, in some sense, to the representative role of mathematics, since it often happens that the mathematics involved in a scientific representation helps in discovering new laws or entities and fosters new developments in physical inquiry. In Steiner's account, however, the two roles — descriptive, or representative, and heuristic — seems to have no connection between them. This is a point which we will have to take care of in the next sections; for an account of mathematical applicability cannot be considered satisfying if it does not account for this particular aspect of mathematical application.

Chapter 4

Applicability and ontological issues

Up to now I have intentionally avoided to take into consideration ontological questions. This choice has been dictated by the general idea that the philosophy of mathematics should first of all looks at mathematics as it is concretely practiced, and only then draw the opportune conclusions about any philosophical question about mathematics. Hence, the problem of the applicability of mathematics, pertaining a concrete fact of *practiced* mathematics, should be analysed and discussed *before* and *independently of* any other philosophical question one might issue. In other words, I think we should look at the applicability problems without any ontological preconception.

However, in chapter 2 we took into consideration a possible reply to Wigner's thesis about the 'mystery' of applicability: if we interpret his claim that mathematical concepts are 'invented' as an anti-realistic declaration, one might suppose that the source of all these problems is just anti-realism. On this perspective, the applicability of mathematics would be a problem only for an anti-realist philosopher: for a realist philosopher all the anti-realist's problems would vanish like fog beneath the sun's rays. Analogously, also Steiner's claim that mathematics is anthropocentric can be undermined at its root by simply holding a *strong* realistic position. The anthropocentric character of mathematics strictly depends, on Steiner's view, on the fact that mathematical concepts are invented and constituted by means of aesthetical criteria. A radical platonist (*à la* Gödel, for example) can simply reply that mathematics is not invented at all, that it is just the study of an existing realm of objects that are discovered by means of intuition, and that any Pythagorean and formalist analogies just show that this realm is in some way deeply intertwined with the physical reality.[1] Some philosophers actually depict realism as the *panacea* of all the applicability problems;[2] and all the contemporary versions of indispensability arguments seem to bring grist to realist's mill, since their arguments implicitly show that realism is the only ontological choice compatible with the applicability of mathematics. If the applicability of mathematics implies a realistic stance on mathematical objects, then an anti-realistic stance should bring to apparently insurmountable difficulties in accounting for the applicability of mathematics.

The problem is serious and deserve a closer attention, that's why I have preferred to discuss it extensively here in a separate chapter. The aim of this chapter is to show that the problems of applicability are not a consequence of any ontological option. They face any philosopher of mathematics, independently of her specific ontological preference. It is probably true that they are more intricated and complex for an anti-realist, but they face realists as well.

In general, the ontological discussions that pay more attention to the applicability problems are centered around the so called "indispensability argument". Both those who do and who don't endorse this argument base their considera-

[1] Actually, the situation is much more puzzling than this. In various passages of his book, Steiner seems to sympathize with mathematical realism (see also (Steiner 1975)), but it is not clear to me in which sense this sympahty could be made compatible with his analysis of mathematics as "species-specific".

[2] See for example Davies (1992, pp. 140-60) and Penrose (1990, pp. 556-7).

tions upon the role of mathematics in empirical science. In the next section I will first consider the reasons of the former and then the reasons of the latter. My general criticism to this kind of arguments is that they are interested in applicability only in a very general sense. The only question they focus on is the generic question whether mathematics is really indispensable to science. In this chapter I will try to show that both supporters and non-supporters of the indispensability argument cannot escape the problems of applicability, and in some cases their arguments even ask fo a more refined account of mathematical applicability.

4.1 Indispensability and applicability

The indispensability argument is usually considered the most important and compelling argument for mathematical realism. Its first formulation is jointly credited to Quine (1961) and Putnam (1979*a*). For this reason the argument is also usually called the "Quine-Putnam argument". However, many commentators have pointed out that Quine and Putnam formulate the argument in two distinct ways and that they aim to prove different claims.[3] Quine's argument can be summarized in the following way:

(1_Q) we ought to have ontological commitment to all and only those entities that are indispensable$_Q$ to our best scientific theories;

(2_Q) mathematical entities are indispensable$_Q$ to our best scientific theories;

(3_Q) hence, we ought to have ontological commitment to (some) mathematical entities.

Putnam's argument, instead, can be summarized as follows:

(1_P) we ought to believe in the truth of any claim that plays an indispensable$_P$

[3] For a more detailed discussion about the differences between Quine's argument and Putnam's argument, see Liggins (2008).

role in our best scientific theories;

(2_P) mathematical claims play and indispensable$_P$ role in our best scientific theories;

(3_P) hence, we ought to believe in the truth of (some) mathematical claims.

The main difference between the two arguments is that Quine argues in favour of a *metaphysical* realism, while Putnam rather argues in favour of a *semantic* realism; but it is not the only difference.[4] As we will see, another important difference is that they bestow different meanings to the word "indispensable" — what is grafically expressed by the subscripts appended to the word "indispensable" in the different arguments.

A more detailed analysis of these two arguments is in order. Colyvan (2001*a*) probably offers the most pervasive and complete analysis of these kind of arguments. Colyvan thinks, in agreement with Quine, that the indispensability argument is actually an argument for Platonism, and hence for metaphysical realism. Hence, his argument is very close to Quine's argument. However, as Pincock (2012, chapt. 9) points out, Colyvan's notion of indispensability is different both from Quine's one and from Putnam's one. So, Colyvan's argument must be kept distinct from the other two.

In the following sections I will try to make some considerations about each of these arguments, and I remind that my main concern is not in understanding whether these argument can or cannot conclude for mathematical realism, but rather whether they say something about the problems concerning the applicabilty of mathematics or not.

[4]By "metaphysical" realism I mean the doctrine according to which mathematical *entities* do *exist* in some sense; by "semantic" realism I mean the doctrine according to which mathematical *statements* are *true* in some sense.

4.1.1 Quine's and Colyvan's argument

That Quine's argument cannot dispense the mathematical realist from the applicability problems has been already noted by many. Colyvan (2001*b*) himself points out that, whether the argument is right or not, it does not say anything about *why* mathematics is indispensable, since it assumes the indispensability as a brute fact — and for answering why mathematics is indispensable we have necessarily to deal with the applicability problems.[5] However, in this section I will try to go further and I will try to argue not only that Quinean realist has still to clear why mathematics is indispensable, but also that the argument *itself* force her to account for the applicability problems.

There are two ways to defend Quine's indispensability argument. The first consists in taking the bull by the horns and trying to found the argument *in itself*, autonomously. The second consists in considering the indispensability argument as a particular case of a more wider class of arguments, which can be grouped in by the fact that they all share the same logical form. In this second way we can, in some sense, 'transfer' epistemic confirmation from one argument to another.

Remarks on the notion of indispensability

Let us start with the first strategy. It usually implies that one focuses on the two premises and on what can support them. Premise (1_Q) is usually considered as founded on two important Quinean thesis: Conformational Holism and Naturalism. Conformational Holism is the thesis according to which we cannot confirm (or disconfirm) a single belief, but only a (more or less) wide collection of them. When we make a prediction, such a prediction is based on a large body of beliefs. So, if our best scientific theories lead to correct predictions, the credit for this success must be assigned to *all* the beliefs that allowed for

[5] An analogous objection has been move by Kitcher (1984, pp. 104-105).

those predictions. The second thesis, Naturalism, is described by Quine as «the recognition that it is within science itself, and not in some prior philosophy, that reality is to be identified and described» (Quine 1981*a*, p. 21). Science is therefore the main source for understanding what is there and what is not. The idea is that Naturalism tells us that we must commit ourselves in the entities required by our best scientific theories, and Confirmational Holism prohibits interpreting part of these theories non-realistically. Both these doctrines has been variously called into question, and there is actually a wide debate about the effectiveness of these two thesis in supporting (1_Q).[6] However, it is not my intention to focus on these debates. I will rather focus on the meaning of "indispensable$_Q$".

Quine's argument is evidently centered around the notion of "indispensable", but it is not very easy to understand what he meant by this concept. The first step consists in clarifying that "indispensability" cannot be intended as a generic "non-eliminability". If we understand "dispensability" as a mere "eliminability", for Craig's theorem we would have the unpleasant conclusion that *every* entity ξ is dispensable. The theorem can be formulated in the following way:

Theorem (Craig's theorem)**.** *Let T be a recursively enumerable theory,*[7] *and consider any division of the predicate letters of T into two disjoint sets $V_A = T_1, T_2, T_3, \ldots$ and $V_B = O_1, O_2, O_3, \ldots$. Let T_B consist of those theorems of T which contain only predicate letters from V_B. Then T_B is a recursively axiomatizable theory.*

If we take the two disjoint sets V_A and V_B as, respectively, the set of theoretical terms and the set of observation terms, we have that T_B has only observation

[6] See, for example, Panza & Sereni (forthcoming).

[7] We are assuming that a theory T is an infinite set of well-formed formulas which is closed under the usual rules of deduction. One way to present T is to specify a set of axioms S and to define T as the set of sentences in S together with all sentences that can be derived from sentences in S. A theory is *recursively axiomatizable* if it has at least one set S of axioms that is recursive, and it is *recursively enumerable* if it is recursively axiomatizable and its axioms and theorems can be ordered in a sequence that can be effectively produced.

terms. Now, the predictions of T are to be found, presumably, among those theorems of T which are in the vocabulary of V_B. So, we have that T_B will make exactly the same predictions as T (in this sense we can consider the two theories *equivalent*) and will have no theoretical term. So, Craig's theorem implies that we can *eliminate* any theoretical term.[8] Therefore, if "indispensable" simply means "non-eliminable", it turns out that *any* mathematical entity is actually *eliminable* and the second premise of the indispensability argument is untenable. Indispensability must then be intended in a different sense.

According to Quine, an entity ξ appearing in a theory T can be said to be indispensable only when its elimination from T produces a new theory T' that can even be equivalent to T but is notwithstanding less preferable than T. So, the notion of indispensable$_Q$ involves the notion of "preference of a theory over another". Now, in order to clarify the concept of theory preference (and, consequently, the notion of indispensability$_Q$), we must take into consideration what Quine called the process of "regimentation". When we want to systematically present our beliefs, we aim to do it in a way that is as most coherent and simple as possible. According to Quine, this goal is reached when we regiment our beliefs in the language of the first-order logic. This process of regimentation is particularly important because we can clarify our ontological committment only *after* this process of regimentation has been done. If Quine is right in claiming that the proper language in which we should regiment our scientific beliefs is the language of the first-order logic, then it is quite easy to see which entities we are ontologically committed to: we have simply to check which sentences of the form "$\exists x \dots x \dots$" are implied by our beliefs. It is at this level that we can prefer a theory over another. That said, we can better understand what Quine means by "indispensable$_Q$": an entity (or a class of entities) ξ is indispensable precisely in the sense that, *once the regimentation has been done*, it is not pos-

[8]For a more detailed analysis of Craig's theorem, see (Putnam 1965) and (Field 1980, p. 8).

sible to eliminate sentences which quantify over such an entity (or over such a class of entities). Mathematical entities seems to be 'indispensable' precisely in this sense: «certain things we want to say in science may compel us to admit into the range of values of the variables of quantification not only physical objects but also classes and relations of them; also numbers, functions, and other objects of pure mathematics» (Quine 1957, p. 16). But the regimentation, as Quine puts the matter in different places, is not very different from the general process of belief choice and belief adjustment, and hence is not very different from the general process of doing science:

> Our acceptance of an ontology is, I think, similar in principle to our acceptance of a scientific theory, say a system of physics: we adopt, at least insofar as we are reasonable, the simplest conceptual scheme into which the disoriented fragments of raw experience can be fitted and arranged. [...]. To whatever extent the adoption of any system of scientific theory may be said to be a matter of language, the same — but no more — may be said of the adoption of an ontology. (Quine 1961, p. 16)

And again:

> We can draw explicit ontological lines when desired. We can regiment our notation [...]. Various turns of phrase in ordinary language that seem to invoke novel sorts of objects may disappear under such regimentation. At other points new ontic commitments may emerge. There is room for choice, and one chooses with a view to simplicity in one's overall system of the world. (Quine 1981*a*, p. 9-10)

Regimentation seems thus to be guided by a general criterion of simplicity and transparency. To decide whether an entity is indispensable or not consists, actually, in deciding whether the theory in which the entity appears is preferable over the other in relation to its simplicity. However, as we saw in the previous chapter on Steiner's work, in some cases it is just the mathematical apparatus that makes a theory preferable over another, often just in virtue of a greater simplicity granted to the theory by the mathematics itself. Mathematical concepts often make the theory easier in relation to its effectiveness in making predictions and new discoveries. In this sense, one might say that the mathematized theory is simpler than a non-mathematized theory. If this is so, the situation turns out to be quite odd: mathematics is indispensable because a scientific theory *mathematically* formulated is to be preferred over a scientific theory non-mathematically formulated; it is to be preferred because it is simpler; and it is simpler because it is mathematically formulated.[9] This is not necessarily a problem, provided that we are able to explain *why*, *how* and *in which sense* mathematics contributes to this preference. But this means that the quinean realist should directly face the applicability problems if she wants to hold the validity of her argument. Otherwise, one might object that the notion of indispensability is, in some sense, vicously circular, since it should grant an indipendent and autonomous foundation for the claim that mathematics is indispensable, but mathematics turns out to play a role in shaping the notion of indispensability itself. So, it seems that, if we want to defend the indispensability argument in its *autonomous* validity, we must face the problems of applicability.

An anaologous remark can be moved against Colyvan's indispensability argument. Colyvan proposes an argument that, in his intentions, is aimed to support metaphysical realism, as well as Quine's argument. However, his notion of "indispensability" is a bit different from Quine's one. Colyvan (1999) defines "indispensability" in the following way: an entity is *dispensable* to a theory T if there exists a second theory T' with the following properties: (i) T' has exactly the same observational consequences as T, but in T' the entity in question is neither mentioned nor predicted; and (ii) T' is preferable to T. If an entity is not dispensable in this sense, then it is indispensable. The main difference between "indispensable$_Q$" and "indispensable$_C$" is that Colyvan's no-

[9]By the way, as Sober (1993) notices, there is no *non-mathematical* competing physical theory. From this follows that, if we accept the idea that scientific hypotheses are confirmed relative to competing hypotheses, then we cannot accept what the indispensability argument seems to claim, i.e. that mathematical theories are confirmed along with our best empirical hypotheses.

tion is not centered around the quinean process of regimentation. As we have just seen, Quine's regimentation prescribes that the ontological commitment is to be pulled out from the simplest and most transparent regimentation of our theories, and this is actually not very different from the process of theory choice — at least regarding the simplicity criterion. Colyvan, in a certain sense, goes much further than Quine on this road, and actually identifies the criteria according to which we can determine whether an entity is indispensable or not to the criteria according to which we can determine whether T' is preferable over T or not. As he says,

> whether an entity is indispensable or not is really a question of theory choice and so is guided by the usual canons of theory choice. These may include: simplicity, unificatory power, boldness, formal elegance and so on. It seems, then, that an entity can be indispensable even though empirically equivalent theories exist that do not quantify over the entity in question. (Colyvan 2001*b*, p. 270)

I think this point is primarily aimed to ruled out any attempt, standardly nominalistic or *à la* Field, to offer a non-mathematical reconstruction of contemporary physics: even if such a reconstruction is *theorically* possible, the result will be hardly preferable to our standard formulation and hence the indispensability of mathematics is save. However, in this way Colyvan exposes himself to the same remark we pointed out for Quine: in some cases it is just the mathematical apparatus that makes a theory preferable to another. In this sense, mathematics can be one of the criteria according to which we judge whether a theory is preferable over another or not. Once again, as well as for Quine, the mathematical realist should first avoid the charge of being viciously circular in his definition of "indispensable", and in order to do that she seems to be forced to offer some account of mathematical applicability.

So, it seems that, if we want to defend Quine's or Colyvan's indispensability argument in its *autonomous* validity, we must face the problems of applicability, since, given their definitions of "indispensable", the way in which the applicability is accounted for seems to play a role in the definition of indispensability itself.

Transferring epistemic confirmation

The second way to defend to indispensability argument for metaphysical realism in mathematics is by considering it as a particular case belonging to a wider class of arguments having the same form. Those, who hold that a realistic stance on mathematical entities would escape the realist from dealing with the applicability problems, usually appeal to this strategy in order to support their claim; therefore it is important to focus on this strategy in order to understand whether they are right in claiming this or not.

According to Colyvan (2001*a*), the general form of these arguments can be presented in the following way (G):[10]

> If apparent reference to some entity (or class of entities) ξ is indispensable to our best scientific theories, then we ought to believe in the existence of ξ. (p. 7)

As an example of application of this kind of arguments in a context different from mathematical ontology, Colyvan mentions the scientists' belief in the existence of dark matter:

> Most astronomers are convinced of the existence of so called "dark matter" to explain (among other things) certain facts about the rotation curves of spiral galaxies. [...] this is an indispensability argument. Anyone unconvinced of the existence of dark matter is not unconvinced of the cogency of the general form of the argument being used; it's just that they are inclined to think that there are better explanations of the facts in question. (p. 8)

In this case, the existence of dark matter is *explicatively* indispensable to our best scientific theories, and in this sense it can be considered as a particular case of inference to the best explanation. In Quine's version of the indispensability argument, however, mathematical entities seems to play no explanatory

[10]Since this strategy to defend the indispensability argument is not centered on the meaning of "indispensable", I will not distinguish any more Quine's from Colyvan's argument.

role — or at least, no explanatory role is assumed in order to draw the conclusion. All that matters is that mathematical entities are *referentially* indispensable; namely, there seems to be no way to formulate our best scientific theories without quantifying over mathematical entities (according to the meaning of "indispensable$_Q$" we have just seen). But this is the general meaning of "indispensable" as it is assumed by the formulation of the argument. So, it seems that we can variously specify the meaning of indispensable, but in Quine's argument it is assumed in its original and *unspecificated* meaning. We can find different specifications of the adjective "indispensable", for example in terms of 'causal effectiveness', [11] but I cannot think of another argument (employed in science) that, as well as Quine's argument, does not need any specfication of the adjective "indispensable" in order to draw the conclusion.

Thus, it seems that indispensability arguments in empirical science always go along with a specification of the adjective "indispensable" in terms of 'explanatory powers' or 'causal effectiveness', and that the general form of the argument never appears in empirical science. This bring me to think that, even if we can admit that Quine's argument can be seen as an application of (G) as well as the dark matter case, the two applications cannot be considered on the same level: in the latter case we have a (more or less) clear application of the notion of indispensability (indispensability in terms of explication); in the first case we don't. The furrow we have just cut between the two cases prevents us from transferring epistemic value from one case to another. What makes the latter argument plausible cannot be simply transferred on the former case, since part of this plausibility is played by the fact that the adjective "indispensable" is employed, in the dark matter case, in a specified sense. More precisely, the indispensability argument employed in the dark matter case (and in all the other

[11]"Explicatively" and "causally" are the only two specifications of "indispensable" that I am able to find. It must be noted that they could even be considered as a unique specification, since explanation and cause are the two faces of a same medal.

examples we can gather from empirical sciences) assignes to the dark matter a *role*, and it also assumes that this role is *indispensable*. Why is the theory X (which apparently refers to ξ) more explicative than the theory Y (which does not apparently refer to ξ)? Because ξ exists. Or, put in different words, what is it that makes X more explicative than Y? Because X apparently refers to ξ, and then ξ is explicatively indispensable to X *in order for it to be more explicative than* Y. But if X is more explicative than Y, we have more reasons to believe X is true rather than Y, and hence to believe that ξ does actually exist. The role of ξ in X is to increase the explanatory power of X, and it seems to be an *indispensable* role: if we remove X's reference to ξ, the explanatory power of X decreases. So, in order to preserve the superiority of X over Y, we must accept the existence of ξ. It is important to notice *from where* the indispensability arguments in empirical science gains its epistemic power. The theory X is better than its competitors *because of* its reference to ξ. It's the fact that X is more reasoned to be believed true that justifies us in believing that ξ does actually exist.

However, one might still claim that a possible reason to transfer epistemic confirmation from one argument to another is based on the fact that both the indispensability arguments (Quine's one and that concerning the dark matter) share the appeal to what is usually called "no miracle argument". The "no miracle argument" is usely appealed by scientific realist in order to justify their position: we must accept the existence of the entities postulated by our best scientific theories, otherwise we must admit that the fact that the nature works as it works is just a miracle. For example, if we don't believe in electrons (I mean, if we accept the electron theory without believing in the existence of electrons), how can we possibly explain the behaviour of a galvanometer? There seems to be no way other than accepting it is a miracle. The analogy between Quine's argument and the argument in favour of the existence of electron is underlined and criticized also by Colyvan (2001*b*):

> It's no miracle, claim scientific realists, that electron theory is remarkably effective in describing all sorts of physical phenomena such as lightning, electromagnetism, the generation of X-rays in Roentgen tubes and so on. Why is it no miracle? Beacuse electrons exist and are at least partially causally responsible for the phenomena in question. Furthemore, it's no surprise that electron theory is able to play an active role in novel discoveries such as superconductors. Again this is explained by the existence of electrons and their causal powers. (pp.270-1)

If we reject the scientific realistic claim according to which electrons do exist, we should admit that the extraordinary effectiveness of this theory in making predictions and describing a large class of phenomena is just a miracle. In the case of scientific realism, there is indeed a pressure on anti-realists, since they seem not to be able to explain the efficacy of the electron theory in describing reality.[12] But can we raise the same problem for the case of mathematical entities? According to Colyvan (2001*b*) himself, the argument can hardly be exported in the field of mathematical realism, since

> There is an important disanalogy [...] between the case of electrons and the case of sets. Electrons have causal powers — they can bring about changes in the world. Mathematical entities such as sets are usually taken to be causally idle — they are platonic in the sense that they do not exist in space-time nor do they have causal powers. So how is that the positing of such platonic entities reduces mystery? (Colyvan 2001*b*, p. 271)

The fact that the existence of electrons is able to remove the aura of mystery around the theory does not come from the bare existence of the electron, but from the fact that they exist *in a certain way*, namely they are causally active. The same cannot be immediately said about mathematical entities, since a platonist usually conceives them as abstract entities with no causal power.

These considerations suggest that it is not possible to transfer epistemic confirmation from the indispensability arguments as they are employed in empirical sciences to Quine's argument, for the very reason that the notion of indispensability refers, in the first case, to a well specified role played by the entities

[12] As an example of such an argument for scientific realism, see (Smart 1963).

at issue; in the latter, to no specific role. However, one might still insist that the two arguments are substantially the same (and that hence we can transfer epistemic confirmation from one argument to the other) by arguing that mathematical entities *do* really *have* a specified and non-generic indispensable role in science, either explanatory or causal. We have therefore two possible directions of development, both of which have been actually followed in recent times. Bigelow (1988) and Maddy (1990), for example, have argued for a causally effectiveness of mathematical entities. However, in her next works Maddy seems to have abandoned such a view (see Maddy 1997, **?**). This option is quite marginal in the contemporary debate, and actually the fact that the quinean realist is compelled to say that mathematical entities are causally active is rather seen as a *difficulty* for Platonism (see for example Cheyne & Pigden 1996). On other side, great part of recent debates around the indispensability argument tries to show that mathematical entities actually play an indispensable *explanatory* role in empirical sciences.[13] In both cases, the supporters of the indispensability argument try to boost the indispensability argument by showing that the indispensable role mathematical entities play in science can be specified in one sense or in another.

Explanatory indispensability argument

Of these two strategies, the first seems to be the most difficult. Unfortunately, at least for now, there is no convincing argument that can satisfactorily clarify in which sense mathematical entities can be said to be causally active. One might say that we can employ the mathematical entity (or class of entities) ξ in describing the physical phenomenon P *because* ξ is *part of the causal chain* that determines the nature of P. It is not completely clear to me whether this claim could be of any help in clarifying the applicability of mathematics or

[13]See for example (Colyvan 2002, Lyon & Colyvan 2008, Colyvan 2010, Baker 2005, Baker 2009).

not; but surely this claim cannot be considered as a satisfying account of the applicability of mathematics until we offer a satifying account of what means for a mathematical entity to be part of the causal chain of a certain phenomenon.

The second strategy, instead, has been widely discussed in recent times. It turns out in an explanatory version of the indispensability argument that can be summed up as follows:

(1_{EXP}) we ought to have ontological commitment to any entity that plays an indispensable explanatory role in our best scientific theories;

(2_{EXP}) mathematical entities play an indispensable explanatory role in our best scientific theories;

(3_{EXP}) hence, we ought to have ontological commitment to mathematical entities.

As for the previous indispensability arguments, this argument needs some clarifications. First of all, one should precise what she means by "explanatorily indispensable". Secondarily, she should define what she means by "mathematical explanation" in science. Finally, she should offer a convincing example of indispensable mathematical explanation in science. The first two points are usually answered toghether. A scientific explanation is said to be "mathematical" if it makes use of a mathematical claim. But in order to understand that the explanation is really mathematical (i.e. , that the mathematics in it is *non-eliminable*) we should put it on probation by means of a replacement test: if we eliminate the mathematical claim form the explanation and the explanation is no longer an explanation, then the explanation can be said to be "mathematical". However, this 'replacement test' clarifies what we mean by mathematical explanation in science, but it does not exhaust the meaning of "explanatorily indispensable", since we can have different, competing explanations, one of which may make no use of mathematical claims. Hence, we must submit the explanation to another test, let us call it the "comparison test": consider all the possible explanations of a certain phenomenon; if the mathematical one is the best one (i.e. if it has the greatest explanatory power), then the mathematics employed is *explanatorily indispensable*. Given this definitions, we can now look if there really are, in empirical sciences, indispensable mathematical explanations.

Are there indispensable mathematical explanations in science? The answer to this question is the subject of a wide debate among philosophers. On the one side, there are philosophers like Colyvan, Baker, Batterman and Pincock who think that there really are examples of such explanations. Three examples have become paradigmatic: the explanation of the periodic life cycle of some species of cicada, the explanation of the hexagonal form of the bee's honeycomb and the explanation of why it is not possible to cross all the bridges of Königsberg exactly once in a circuit that returns to the starting point. On the other side, philosophers like Melia, Daly and Langford have tried to show that these explanations are not real examples of mathematical explanations.[14] I am not going to get into the details of this debate (I remember that my main concern in this chapter is to understand whether there is an ontological option that can be of any help in solving or clarifing the applicability problem). However, let me make some considerations.

First of all, as Pincock (2012, pp. 206-207) points out, it must be noted that we explained the notion of "indispensable mathematical explanation" by means of two tests that require to eliminate or substitute the mathematical *claims* appearing in the scientific explanation. However, premise (2_{EXP}) refers to mathematical *entities*. Hence, we need an argument that shows the link between these two tests and the relevance of mathematical *entities* in explanation. In absence of this link, it would be probably better to reformulate the whole argument as an argument for *semantic*, rather than *metaphysical*, realism. I will deal with these kind of indispensability arguments in the next section, so I will

[14]See for example (Melia 2000, Melia 2002, Daly & Langford 2009).

leave open the question for now.

The second consideration is that even if we admit that there are indispensable mathematical explanations in science, this is of no help for the analysis of the applicability problems. For two reasons: first, the explanatory role of mathematics is not the only role for mathematics in science, and hence we have still to clarify the effectiveness of mathematics in all these other roles; second, the indispensability argument *does not* offer an account for *why* mathematics is helpful in explaining physical phenomena. The eliminability test and the comparison test permit us to say whether a mathematical claim plays an indispensable role in explaining a certain phenomenon or not, but they are not aimed to account for the conditions that a mathematical claim has to satisfy in order to have an explanatory power.

Hence, also the explanatory indispensability argument seems not to be of any help in claryifing the applicability problems. One can, of course, opt for a realistic stance about mathematical entities, but there seems to be no sense in which this stance can help in accounting for the effectiveness of mathematics in empirical sciences.

4.1.2 Putnam's argument for semantic realism

About the truthness of mathematical claims

Although it is often confused with Quine's argument, Putnam's one is different from it. The main and the most evident difference is that Putnam argues not for a metaphysical realism, but for a *semantic* realism about mathematical statements: his conclusion is that the confirmational holism commits us to believe that mathematical statements employed in science are true, along with other true scientific statements that appear in our best scientific theories. The second important difference consists in the different meaning they bestow to the notion of "indispensability". As we saw, Quine's notion of "indispensable$_Q$" is centered around the practice of regimentation. Putnam's indispensability, instead, is rather aimed to grasp the real meaning of scientists' claims. According to him, «one of our important purposes in doing physics is to try to state 'true or nearly true' (the phrase is Newton's) laws, and not merely to build bridges or predict experiences» (Putnam 1979*a*, p. 338). Therefore, one of the main tasks of philosophy consists in acknowledging this purpose and to respect this character of physical inquiry. Thus, Putnam's argument assumes that scientific claims have a more or less clear meaning that philosophical reflection should respect and should not demand to reformulate (as in Quine's regimentation). It follows that the notion of "indispensable$_P$" refers to the scientific claims in their original formulation: something is indispensable$_P$ if it is required by the scientists in their very formulations.

This feature of Putnam's indispensability seems to fit better with an argument for *metaphysical* realism: if there is no need of regimentations or reformulations (that, depending on their fulfilment, could eliminate the reference to abstract entities), and since it seems that scientific claims directly refer to mathematical entities, the argument could easily argue for the existence of mathematical entities rather than for semantic realism. However, Putnam is cautious and prefers to opt for a weaker (*semantic*) conclusion. The reason for this is that Putnam is worried by what he calls "equivalent constructions" in mathematics, something that seems to be very intrinsic in the mathematical formulation: «the chief characteristic of mathematical propositions is the very wide variety of equivalent formulations that they possess»(Putnam 1979*b*, p. 47). Among these possible formulations, some of them do not need abstract objects (for example, modal formulations *à la* Hellman (1989)), and hence it may be not necessary to include abstract entities in our ontology.

That said, let us go back to our main question in this chapter: how can semantic realism be of any help in solving the applicability problems? A first possible answer is that if mathematical propositions are true, then we can apply them just because they are true. The applicability of mathematical claims is therefore justified by their being true. However, this answer cannot be considered satisfying: after all, the proposition "My right incisor is chipped" is true, but I can hardly believe that such a proposition, even if true, can be of any employment in science. In other words, there are billions of true propositions that have no application in science. Why mathematical propositions are different?

A slighlty more articulated answer is that mathematical claims can be successfully employed in science because they are true *and* pertain to the nature of the world. However, this answer too can be hardly considered satisfying, since it does not avoid the difficulites of the previous answer: in which sense, exactly, mathematical claims 'pertain to' the natural world? In the absence of an answer on this point, semantic realism cannot be of any help in accounting for the effectiveness of mathematics.

Explanatory indispensability argument for semantic realism

As we saw at the end of section 4.1.1, it is also possible to formulate an *explanatory* indispensability argument for semantic realism:

(1_{exp}) We ought to believe in the truth of any claim that plays an indispensable explanatory role in our best scientific theories;

(2_{exp}) mathematical claims play an indispensable explanatory role in our best scientific theories;

(3_{exp}) hence, we ought to believe in the truth of mathematical claims.

The argument is similar to the analogous argument for metaphysical realism, but premise (2_{exp}) refers to mathematical claims instead of mathematical entities. In this sense, the two tests we previously discussed about the explicative value of a claim can directly support premise (2_{exp}), while for premise (2_{EXP}) we needed another argument to show that if a mathematical claim plays an indispensable explanatory role in our best scientific theory then the entities entailed by that claim are indispensable as well.

Apparently, this argument seems to be able to remedy to the difficulty of the previous argument. There I noted that semantic realism cannot be of any help in explaining the effectiveness of mathematics in science because it does not explain in which sense mathematical (true) claims would pertain to natural world. This argument seems to suggest that mathematical claims pertain to the scientific description of natural world precisely in the sense that they are explanatorily relevant to this description. So, one might say, on the basis of this argument, that mathematics is effective in science *because* mathematical claims are true *and* they *explanatorily* pertain to scientific subjects. However, it must be noted that the addition of the adverb "explanatorily" does not add very much to the sense of "to pertain to". This is just a way to beg the question: if we are interested in understanding why and how mathematics can be so effective in science, then we are interested in understanding in which sense mathematical claims can be, among other things, explanatorily relevant for the scientific discourse. Thus, once again, it seems that a stance on mathematical realism, be it semantic or metaphysical, is of no help in clarifying the applicability problems.

But there is something more. Let us consider the following, typical example of mathematical explanation: why do some species of cicada have a prime life cycle? The answer involves a biological claim and a mathematical theorem:

1. It is evolutionary advantageous to have a life cycle which minimizes intersections with other periods (for example of predators) [*biological claim*].
2. Prime periods minimizes intersections better than non-prime periods [*mathematical theorem*].
3. Hence, organisms with periodic life cycles are likely to evolve periods that

are prime.

Since this explanation seems to be presently the best explanation of the phenomenon at our disposal, the mathematical theorem in (2) can be reasonably said to have an indispensable explanatory role. Thus, by applying the explanatory indispensability argument, we can conclude that the mathematical claim in the second premise is true. Confirmational holism compels us to believe in it as well as we believe in the existence of, say, atoms. However, as Pincock (2012, chapter 10.2) points out, we could reformulate this explanation by substituting the mathematical theorem (2) by another, *weaker* mathematical theorem. For example, we could substitute (2) with the following theorem (2′): «Prime periods *of less than 100 years* minimize intersections». The theorem is equally true (actually, it is implied by 2), and even if it is weaker than (2) it can support the explanation as well. Thus, what is really indispensable, (2) or (2′)? In a certain sense, (2′) is even preferable over (2), since (2′) is easier to be proved. So, why should we prefer (2) to (2′)? One might say that we should prefer (2) over (2′) because (2′) lead to explanations having a less explanatory power, and hence (2) can lead to a more unified scientific theory. However, this is a general prescription that needs a wider and more general argument. After all, there is nothing in the previous cicada example that leads us to prefer (2) or (2′). As Pincock (2012) points out, «the weaker mathematical claims are able to cover all the actual instances of the phenomena at issue [...]. I do not believe that the ability to explain nonactual instances of these phenomena should heighten the explanatory power of these explanations» (p. 213).

Thus, confirmational holism seems to allow us to conclude that if a mathematical claim is explanatorily indispensable then it is true, but it seems we cannot indentify any mathematical claim that is really indispensable in this sense. The problem is even more difficult if we look at the question from an epistemic point of view, as Pincock (2012) suggests. Suppose I am a biologist and I don't know whether the mathematical claim in (2) is a theorem or not. Suppose also that, by empirically comparing the periods of intersections for the primes and non-primes from 1 to 100 I find that prime periods minimize intersections, and thus I come to believe premise (2′). But how can I come to know that (2) is a theorem of mathematics? Pincock's thesis is that there is nothing in application that could bring me to believe (2).

> My claim [...] is that the ways mathematics helps with scientific explanation are not sensitive enough to sort out the various options that agents must choose between when they need to decide which stronger mathematical claims to believe. The differences between these options are so fine grained that it would be unreasonable to base such a choice on their application in explanation. (p. 214)

Pincock objects that we cannot consider the application as a source of belief for mathematical claims. Actually, it seems rather the opposite: we apply mathematical claims only *after* (and because) we believe in them.

Thus, Pincock suggests that this argument is actually affected by some kind of vicious circularity: we try to conclude that mathematical claims are true by arguing from their application, but application already assumes that they are believed to be true.[15] One might object by saying that the circularity reported by Pincock is really a problem only if we assume that the indispensability argument should also tell us *which* mathematical propositions we should believe in. However — the objection might go on — this is not the case: the indispensabilty argument just gives us a reason to believe that, in general, mathematical claims are true propositions. Given that there really are indispensable mathematical claims (it is not important to know exactly which), the conclusion can be considered to properly follow from the premises. Anyway, be this objection right or not, Pincock's criticism seems to show an apparent conflict between natu-

[15] For another kind of circularity related to the example of the cicada's prime life cycle, see Bangu (2012, chapter 8.2). According to him, the explanationist strategy works only if we assume that the explanandum is true. But if the explanandum contains an irreducible mathematical component (as it seems to be in this case), we must admit that its truth makers (mathematical entities, among them) do exist. It follows that the realist is here assuming realism before arguing for it.

ralism and confirmational holism: on the one side, confirmational holism leads us to think that mathematical claims receive the same kind of justification that scientific claims receive; but on the other side, this conflicts with naturalism, since the practice of working mathematicians does not seem to suggest, in any sense, that mathematical claims require any application to be considered true: confirmational holism, at least when applied to mathematics, seems to conflict with mathematical practice — and hence with naturalism.

Another criticism is the following.[16] In the analysis of water waves dispersion, scientists starts with a suitable form of Navier-Stokes equations from fluid mechanics, and then they consider the limit where the ratio of the depth of the ocean to the wavelenght goes to infinity. In different words, they assume that such a ratio is infinite.[17] Analogously, in formulating the Navier-Stokes equations, scientists implicitly assume that matter is continuous. Both these assumptions are clearly false: the first assumption concerning the deep of the water is patently false; the second one is false if we assume that our best scientific theories about the ultimate composition of matter are true. Notwithstanding, scientists make such assumptions in order to explain the behaviour of water waves and of fluids, respectively; and these assumptions seem to have an *indispensable* explanatory role, even if they are *false* claims and nobody will hold them for true.[18] These assumptions are not mathematical, strictly speaking. However it seems that now we have two different classes of claims: (1) true claims having an indispensable explanatory role in our best scientific theories, and (2) *false* claims (idealisations, for the most part) that notwithstanding play an indispensable explanatory role in our best scientific theories. The problem is: how can we say that mathematical claims fall within the first of the two

[16]The argument is analogous to the ones presented in (Maddy 1992, Maddy 1995), and it is actually inspired by these. However I hold there is a substantial difference in the way the argument is aimed. Therefore, I prefer to present it as mine, so to assume all the responability for it.

[17]See Batterman (2010) and Pincock (2011) for more details.

[18]See Batterman (2010).

classes? It seems that we need a specific argument to show that, but how can such an argument be? To prove that a mathematical claim p falls into (1) we should prove that it is true (or, at least, that it is not false), but if we have an argument to prove that p is true we don't need any indispensability argument to prove what we have already proved in a different way!

However, this does not only stigmatize the risk of a vicious circularity in the argument; it also shows — once again — a conflict between naturalism and confirmational holism: scientists do not hesitate to assume patently false claims in order to get the job done, but confirmational holism does not permit us to account for this peculiarity of the scientific practice, thus pushing us *away* from naturalism. This second criticism also shows an important feature of the indispensability argument that is more interesting for us. After all, it is not clear to which extent confirmational holism is needed to support premise (1_{exp}).[19] If we can do without it, these criticisms do not really undermine the indispensability argument. However, if we give up confirmational holism, then we can avoid the previous difficulty by means of an analysis of the roles that a mathematical claim can play within a scientific theory. In this way we can distinguish between mathematical claims which play an explanatory role from those which play a causal/acausal, representative or heuristic role. But this exactly amounts to dealing with the applicability problems, with particular reference to the representative, explicative and heuristic roles of mathematics in science. In other words, the indispensability of mathematics cannot be uncritically assumed to justify ontological realism, but it is important (and indispensable — let me say) to deal with the concrete problems posed by the application of mathematics to remove all the difficulties previously shown.

[19]This point is currently matter of discussion. See for example Panza & Sereni (forthcoming).

4.1.3 Conclusions

In the previous sections, I considered various versions of the indispensability argument, both for metaphysical realism and for semantical realism, and I moved several criticisms against them. The criticisms are not conclusive, and it is probably possible to pass over them. However, the conclusions of any section were always the same:

1. even assuming that the indispensability argument at issue is sound, nothing is said about the applicability problems which we are concerned with. Moreover,
2. a preliminary, closer and deeper analysis of the applicability of mathematics seems to be indispensable in order to refine the argument and avoid some tricky difficulties.

Therefore, my general conclusion is that not only a realistic stance on mathematical ontology would not exempt us from the applicability problems; but also that a coherent argument for mathematical realism seems to require a preliminary and deep inquiry into the tecnicalities of the mathematical applicability.

However, the realist philosopher might still claim that she is in a better position than the anti-realist to account for the applicability of mathematics. After all, the previous criticisms to the indispensability argument were not intended to support anti-realism against realism. Even if they effectively undermine the realist's expectations, they were intended not to criticize the realism in itself, but only the indispensability argument as a good argument for mathematical realism. Even if realism cannot offer us a solution to the applicability problems, it can still turn out to be a better choice than anti-realism when we are going to deal with this kind of problems. Thus, in the next section I will consider the anti-realist's arguments and I will check whether we should give some credit to the realist's claim or not.

4.2 Field's anti-realism

4.2.1 Science without numbers

The most interesting nominalist counter-proposal to the Quine-Putnam argument for mathematical realism has probably been advanced by Field (1980). He proposed an articulated reply to the indispensability argument that aims

1. to belie the indispensability argument,
2. to defend the nominalism's reasons, and
3. to explain, at the same time, why mathematics is so effective in science.

In order to justify our interest in Field's proposal, it must be said that many nominalistic systems have been proposed, but what is interesting in Field's one is that it is probably the only which explicitly focuses on the necessity of accounting for the applicability of mathematics in science. He aims to get this goal by means of a 'non-standard' nominalistic strategy: while 'standard' nominalistic strategies aims to eliminate the reference to abstract mathematical entities by offering a nominalistic 'translation' of mathematical statements, he accepts mathematical statements at their face value and develops a strategy that straightly focuses on applicability.[20] As he points out,

> what I do here gives an attractive account of how mathematics is applied to the physical world. This is I think in sharp contrast to many other nominalistic doctrines, e.g. doctrines which reinterpret mathematical statements as statements about linguistic entities or about mental constructions. Such nominalistic doctrines do nothing toward illuminating the way in which mathematics is applied to the physical world. (p. 6)

Field's proposal is actually based on three fundamental components:

- a fictionalist attitude toward mathematical entities,
- principle of conservativity, and
- nominalization.

[20] An example of this 'standard' nominalistic approach can be found in (Chihara 1973).

The first component, *fictionalism*, is the philosophical thesis according to which mathematical statements are false or 'untrue', as well as the proposition "Sherlock Holmes has lodgings at 221b Baker Street, London" is said to be false or 'untrue': the latter can be (unproperly) regarded as true only within the literary fiction of Arthur Conan Doyle's tales, while the formers can be (unproperly) regarded as true only within the fiction of mathematical practice. From this follows that, if no part of mathematics is true, then no mathematical entities have to be posited to account for mathematical truth. To commit ourselves to the existence of, say, the number π would make no more sense than to commit ourselves to the existence of Sherlock Holmes.[21]

However, fictionalism by itself is not enough:

> If one *just* advocates finctionalism about a portion of mathematics, without showing how that part of mathematics is dispensable in applications, then one is engaging in intellectual doublethink: one is merely taking back in one's philosophical moments what one asserts in doing science, without proposing an alternative formulation of science that accords with one's philosophy. This (Quinean) objection to fictionalism about mathematics can only be undercut by showing that there is an alternative formulation of science that does not require the use of any part of mathematics that refers to or quantifies over abstract entities. (Field 1980, p. 2)

In other words, since the indispensability argument is the *only* serious argument for the existence of mathematical entities, Field needs now to undermine this argument. While the previous objections to the indispensability argument that we dealt with in the previous section were all aimed to undermine the first premise, Field take a different route, and tries to undermine the *second* premise, by showing that

1. mathematical entities are useful, even if not in the same sense in which we say that theoretical entities are useful; and
2. mathematical entities are notwithstanding *dispensable*.

[21] A further consequence is that the problem of accounting for the knowledge of mathematical truths vanishes. Benacerraf's (1973) dilemma is thus radically sweeped away: since there is no mathematical truth, we don't need a uniform semantic for them anymore, and since there is no mathematical entity the knowability of mathematical statements is not a problem anymore.

The first goal is reached by means of what he calls the principle of indispensability, which can be stated as follows:

Principle (C). *Let $N = \{A_i\}_{i\in\mathbb{N}}$ any body of nominalistically statable assertions, and S any mathematical theory. Now, for any A_i let A_i^* be the assertion that results by restricting each quantifier of A_i with the formula '$\neg M(x_j)$' for the appropriate variable x_j, where $M(x_j)$ intuitively means "x_j is a mathematical entity".*[22] *Finally, let N^* be the set of all the A_i^*. Then A^* is not a consequence of $N^* + S + \exists x \neg M(x)$ unless A is a consequence of N.*

Intuitively, and a bit improperly, this means that for any mathematical theory S and any body of nominalistic assertions N, $N + S$ is a conservative extension of N. Assuming this principle, it follows that a nominalist is free to use any mathematical existence-assertion to deduce nominalistically-stated assertions from nominalistically-stated premises, and «he can do this not because he thinks those intervening premises are true, but because he knows that they preserve truth among nominalistically-stated claims» (p. 14). Of course, this can be done only *once we have a nominalistic axiom system for the particular sciences*, therefore the next step will obviously consists in offering such a nominalistic axiom system.

However, why should we believe in this principle of conservativeness? The principle of conservativeness follows from the following

Principle (C′). *Let A, A^*, N and N^* be as in the prevoius principle. Then A^* is not a consequence of $N^* + S$ unless it is a consequence of N^* alone.*

In turn, Principle (C′) is equivalent (assuming that the underlying logic is compact) to the following

[22] Thus, if $A_i = \forall x_j P(x_j)$ then $A_i^* = \forall x_j(\neg M(x_j) \rightarrow P(x_j))$, and if $A_i = \exists x_j P(x_j)$ then $A_i^* = \exists x_j(\neg M(x_j) \wedge P(x_j))$. The introduction of A^* is required by the fact that, being N a nominalistic theory, it may imply somethig that rules out the existence of abstract entities, and hence $N + S$ might be inconsistent.

Principle (C$''$)**.** *Let A, A^* and S be as in the two following principles. Then A^* is not a consequence of S unless it is logically true.*

Now, according to Field, that mathematical theories satisfy principle (C$''$) is perfectly obvious, for otherwise it would have been absolutely impossible to regard mathematical theories as '*a priori* true' or 'true in all possible worlds': «though these characterizations of mathematics may be contested, it is hard to see how any knowledgable person could regard our mathematical theories in these ways if those theories implied results about concrete entities alone that were not logically true» (p. 12).[23]

The principle of conservativeness shows that mathematics can be really useful in science, since it can be employed to make deductions that turn out to be considerably shorter and easier than their non-mathematical equivalents. Moreover, assuming fictionalism, we are not commited to the existence of any mathematical entities. But the principle of conservativeness also shows that this utility is very different from the utility of theoretical entities. The utility of theoretical entities consists in the fact that, by adopting a theory T quantifying over some theoretical entities, we can draw new and explanatorily fruitful consequences. On the contrary, in the mathematical case we don't arrive at any genuinely new conclusion, since all the conclusions we can draw were already derivable from the premises, without recourse to mathematical entities (even if in a long-winded fashion).

Having shown that mathematics can be proved to be useful without assuming the truthness of mathematical claims, Field must now prove that mathematical entities are notwithstanding *dispensable*. Up to now he has merely argued that *if* we had a nominalized science (namely, a nominalistic axiom system for any branch of science), we are legitimated to introduce mathematics as an auxiliary

[23] Field offers also a proof for all these derivations, but it is not necessary to report it here. The place — which I refer the interested reader to — is (Field 1980, p. 12; pp. 16-19; and pp. 108-110, note 9).

device in drawing inferences. However, he still has to argue that such a nominalized science is possible and can effectively offered. The task of offering a *complete* nominalized axiom system for the whole science is obviously an enormous task, and Field restricts himself to the more reasonable task of offering a *partial* nominalization of a particular, *representative* («fairly typical», he says) branch of physics, the Newtonian theory of gravitation.

We don't need to report here all the details of Field's proposal; a general sketch of the strategical line he adopted is enough for our aims. He follows the strategy adopted by Hilbert in his *Grundlagen der Geometrie* (1899). In this work, Hilbert distinguishes between two different approaches: metric and synthetic. The key idea of the synthetic approach proposed by Hilbert consists in replacing any discourse about metric (concerning distances and locations, which require a quantification over real numbers) with the two following predicates:

- **betweenness**: y Bet xz, meaning "y is a point on the line-segment whose enpoints are x and z"; and
- **segment-congruence**: xy Cong zw, meaning "the distance from point x to point y is the same size as the distance from point z to point w".

Now, since Hilbert's theory does not quantify over real numbers, the notion of distance cannot be defined within the theory. However, by means of the following *representation theorem*, we have a *meta*theoretic proof which, by associating claims about distances with what we can say in the theory, permits us to show that the standard Euclidean theorems about lenghts can be restated as theorems about function d.

Theorem (Representation Theorem)**.** *Given any model of the Hilbert axiom system for space, there would be at least one function d mapping pair of points onto the set of the non-negative real numbers satisfying the following 'homomorphism conditions':*

(a) $\forall x, y, z, w(xy\ Cong\ zw \leftrightarrow d(xy) = d(zw))$*;*

(b) $\forall x, y, z(y\ Bet\ xz \leftrightarrow d(xy) + d(yz) = d(yz))$*.*

In virtue of this theorem, numerical claims can be considered as abstract counterparts of purely geometrical claims.

Along with this representation theorem, Hilbert is also able to prove a *uniqueness* theorem:

Theorem (Uniqueness Theorem)**.** *Let* d_1 *and* d_2 *be two functions mapping pairs of points into non-negative reals, and let both of them satisfy the two homomorphism conditions (a) and (b) of the Representation Theorem. Then* d_1 *and* d_2 *differ only by a positive multiplicative constant; and conversely, if* d_1 *and* d_2 *differ only by a positive multiplicative constant, then* d_1 *satisfies (a) and (b) if and only if* d_2 *does.*

The importance of this theorem consists in its *explicative* value. Geometric laws, when folumalted in terms of distance, are invariant under the multiplication of all the distances by a positive constant, but are not invariant under any other transformation of scale. Now, this fact receives, in virtue of the Uniqueness Theorem, a satisfying *explanation* — and what makes this explanation 'satisfying' is the fact that the *explanantes* are *intrinsic* facts about physical space, namely facts concerning physical space that do not refers to numbers. The Uniqueness Theorem plays a very important role in Field's system, as we will see in a while.

Now, Filed's key idea to offer a nominalistc axiom system for physics follows the Hilbert's *synthetic* strategy just exposed. In short, Field tries to extend Hilbert's synthetic theory for space to a synthetic joint axiom system containing axioms for space-time, gravitational potential and mass-density, and then to prove a general *representation theorem* and a general *uniqueness theorem* for this new axiom system. Thus, we can employ mathematical (abstract) entities to formulate abstract counterparts of concrete statements. «Then, in proving a conclusion in N^* from premises in N^*, we can at any convenient point 'ascend' from concrete statements to their abstract counterparts, proceed at the abstract level for a while, and then finally 'descend' back to the concrete» (p. 24).

I can omit to report here the specific way in which Field thinks to reach this purpose. What is interesting is rather that the general uniqueness theorem offers, as in the previous case, a reason to *prefer* Field's nominalized axiom system to other non-nominalized theories: the reason is that in this way we can offer *intrinsic* explanations of physical facts, without appealing to *extrinsic* entities like mathematical objects. As Field says,

> I believe that such 'synthetic' approaches to physical theory are advantageous not merely because they are nominalistic, but also because they are in some ways more illuminating than metric approaches: they explain what is going on without appealing to extraneous, causally irrelevant entities. [...]. If, as at first blush appears to be the case, we need to invoke some real numbers like 6.67×10^{-11} (the gravitational constant in $\mathrm{m}^3/\mathrm{kg}^{-1}/\mathrm{s}^{-2}$) in our explanation of why the moon follows the path that it does, it isn't because we think that real numbers plays a role as a *cause* of the moon's moving that way; it plays a very different role in the explanation than electrons play in the explanation of the workings of electric devices. The role it plays is as an entity *extrinsic to the process to be explained*, an entity related to the process to be explained only by a function (a rather arbitrarily chosen function at that). Surely it would be illuminating if we could show that a purely intrinsic explanation of the process was possible, an explanation that did not invoke functions to extrinsic and causally irrelevant entities. (p. 43)

The possibility of giving intrinsic explanations is considered by Field a clear advantage of his synthetic approach, and he is persuaded that this advantage makes his nominalistic theory more attractive both than other nominalistic 'standard' systems and other platonic systems.[24]

Now, Field has completed the circle: fictionalism suggests that we don't need to regard mathematical statements as true; the principle of conservativity shows that mathematics can be useful merely as a truth-preserving tool; and the

[24]Remember that a key point for defining the notion of indispensability was the *attractiveness* of the new theory. If we don't impose on the new theory such a condition of attractiveness, we fall into the difficulties raised by Craig's theorem. See above, p. 94.

synthetic, nominalistic axiom system he offered for Newton's theory of gravity shows that, thanks to the principle of conservativity, the consequences which we arrive at by means of mathematical tools were deducible from the nominalistic axiom system alone, without any reference to mathematical entities. Field thinks thus of having belied the indispensability argument, of having offered a coherent nominalistic solution, and of having offered a satisfying account for mathematical applicability. Therefore, we should now consider whether he is right in holding this, especially for what concerns his last claim about applicability.

4.2.2 Nominalism and applicability

Various aspects of Field's project have been criticized. The critics can be collected within two different categories: criticisms concerning the real possibility of extending his project to the whole science (and not only to a small, even if representative, part of it), and criticisms that blame the project of not being really satisfying. As an example from the first category, I may mention Malament (1982), who objects to Field that his project can be hardly extended to some branch of physics like, for example, quantum mechanics. Balaguer (1998) tried to offer a nominalization of quantum mechanics on the fictionalist line traced by Field, but his solution is not compatible with all the interpretation of quantum mechanics and some doubts remain about whether it is really nominalistic or not.[25] As an example from the second category, on the other hand, I may mention Resnik (1985), who objects to Field that his nominalization of the Newton's theory of gravitation actually presents some features that would be hardly acceptable by a 'standard' nominalist philosopher.

However, it is not my concern to focus here on this kind of objections, since I am mainly interested in understanding whether Field can legitimately claim

[25]See Bueno (2003) for more on this point.

of having offered a *satisfying* account of mathematical applicability.

In Field's account, the role of mathematics in physics is reduced to the computational simplification. Mathematics *can* be used because it satisfies the principle of conservativeness, and it *is* actually used in physics because it permits simpler calculations and easier conceptual manipulations. However, as we saw in the previous chapter, the problems raised by the application of mathematics are more and more complicated than this alone. Mathematics is surely useful as a calculational tool, but this is not its only merit. Apart from the fact that, to be meticulous, Field does not even tell us *why* mathematics permits easier calculations, we are left with a number of problems concerning applicability which Field does not address at all. For example, we discussed in the previous chapter the role of mathematics in *discovering* new laws and even new physical entities. The examples that we considered were all taken by the quantum mechanics. This suggests that, maybe, the 'piece' of physics nominalized by Field was not so 'fairly representative' as he said — or, at least, it is not representative at all if our main concern is about heuristic problems of applicability.

Field claimed that his nominalistic account avoids the troubles related to Craig's theorem because epistemic considerations (i.e. the possibility of offering *intrinsic* explanations) make his account preferable to the other platonsitic accounts.[26] However, this 'preferability' is questioned if we consider the roles played by mathematics in science. Mathematics is surely an excellent tool for simplificating calculations, but this is not the only role played by it. If we admit that mathematics *do* really play these other roles, it seems to me that the idea according to which we should prefer Field's account because of its epistemic su-

[26]At this proposal, it has been variously noted, however, that the advantge of intrinsic explanations seems to hide some kind of circularity. As Colyvan (2001*a*) points out, «if one thinks, as the indispensabilist does, that numbers are real and intimately (although not causally) involved with the way the world works, it is not clear that explanations of features of space that do not involve numbers are any more intrinsic than electron-free explanations of lightning. The point is that if you think nominalism is correct, then nominalist explanations will seem intrinsic while Platonist ones will not. The Platonist need not concede this» (p. 88).

periority turns out to be untenable. The nominalist *à la* Field might either try to argue that mathematics actually *does not* play these predictive and heuristic roles, or she might try to show that even these roles can be nominalistically accounted, but — as far as I know — none of these strategies has been satisfactorily developed up to now. Anyway, it seems to me that, whatever strategy she might follow, the moral is that she cannot argue in one sense or in the other without dealing with the problems of applicability, in a deeper and wider way than they have done up to now.

4.3 Conclusions

In the present chapter I took into consideration the main arguments for ontological realism in mathematics. As we have seen, the applicability of mathematics seems to play a certain determining role in supporting these arguments. However, this applicability remains unanalysed and it is assumed only in its generic and undisputed sense. Therefore, even if based on the applicability of mathematics, these arguments do not say anything about the problems concerning it, and do not help in any sense to clarify the concrete roles played by mathematics in science.

Not only. On closer analysis, it turns out that this generic and unanalysed assumption leaves the realist open to some possible criticisms, and for this reason she has rather to deal with these applicability problems. So, not only a realistic stance on ontological matter does not help in clarifying the applicability problems, but — if my remarks are right — a preliminary examination of these problems seems to be indispensable.

For the anti-realist the question seems not to be very different. Even if she adopts a non-standard nominalistic strategy (*à la* Field), this very strategy seems to push her to a closer examination of the applicability problems, otherwise she risks to expose her position to serious criticisms and objections.

To sum up, from the applicability point of view there seems not to be any real advantage in adopting a realist over an anti-realist view — or viceversa. Wherever our ontological preferences may go, we are inevitably pushed back to the applicability problems. It seems that a discussion about the ontology of mathematical objects cannot avoid to take seriously the applicability problems we discussed in chapter 3. Thus, these considerations shows that the problem of the applicability of mathematics deserves to be considered on the same level as the other typical problems usually discussed in philosophy of mathematics, and even — as Field properly points out — as «the really fundamental one» (Field 1980, p. vii). For these reasons, I deem desirable a different approach — an approach that, instead of starting from ontology for moving on towards applicability, starts *from* applicability intended as an autonomous and valuable problem. The hope is that also the other open questions in philosophy of mathematics will benefit from such an analysis.[27]

[27]Examples of this kind of approach — in which ontological conclusions stem out from applicability considerations — can be found in Pincock (2012) and Bangu (2012). Bangu, particularly, argues for an indispensable *heuristic* role of mathematics in science, and then he proposes a new indispensability argument based on such a new mathematical role. I will discuss in chapter 6 the cases which he brings forward to argue in favour of this heuristic role of mathematics and I will show that these cases should be rather accounted in a different way, so that no indispensable heuristic role (as he intends it) need to be invoked for mathematics.

Part III

An Account for Mathematical Representativeness

Chapter 5

Structures and applicability

5.1 Aims and purposes of the present chapter

In chapter 3 I discussed Steiner's attempt to define a multiplicity of philosophical problems concerning the applicability of mathematics. As we saw, he distinguishes between four different problems:

1. a *semantic* problem,
2. a *metaphysical* problem,
3. a *descriptive* problem, and
4. a *heuristic* problem.

Among the criticisms I moved against Steiner's work, I pointed out that the third problem — the *descriptive* one — had been poorly accounted for. Particularly, I noticed that the descriptive effectiveness of mathematics would be better characterized as a *representative* effectiveness, and that 'description' is but one of the representative roles mathematics can play. Appealing to this multiplicity of representative roles, I also suggested that Steiner's inexplicable cases of descriptive applicability could rather be accounted as *representative-but-not-descriptive* cases of mathematical effectiveness, and that hence their effectiveness could have been justified in a representative-but-not-descriptive manner. However, I did not offer such a justification in chapter 3, hence my suggestion remained in the abstract.

To consider the descriptive role of mathematics in a more general representative background amounts to admitting that mathematical import in scientific representations can follow different lines. A representation is not necessarily bounded to reproducing the causal (physical) elements appearing in the phenomenon, but can vary along different components — as we saw when I mentioned Pincock's (2012) work in section 3.2.2. Thus, mathematical relevance does not consist solely in reproducing a physical nexus among physical components of reality, but it can also have an epistemic import. For example, when we modelize the behavior of a fluid substance by means of the Navier-Stokes equations, the model does not aim to reproduce the causal nexus laying in the phenomenon, since the model is continuous while the phenomenon is supposed to be discrete (at least according to our best theories concerning the ultimate composition of matter). However, this *acausal* feature of the model has an epistemic import, since it is easier to work with a differential-equation model than with a corresponding discrete difference-equation model. In this case, the model offers a *falsified* image of the phenomenon at issue, but nonetheless (and even thanks to this falsification) it can offer a better tool (epistemically speaking) to understand the behavior of the fluid substances. In other words, to confine the effectiveness of mathematics to its descriptive role would amount to admit that mathematics can contribute in scientific explanation only by means of isolating causal chains. However, as we saw in chapter 4, the role played by mathematics in scientific explanation is often divided from purely causal considerations (remember the example of the cicada's periodic life cycle). Therefore, we expect that a philosophical explanation of mathematical effectiveness will have to account for this general feature of mathematical representation.

Another criticism I moved against Steiner's reconstruction pertained the fact that he did not connect the representative problems with the heuristic ones. I think this is a key point. Heuristic and representative roles of mathematics cannot be considered separately, since often a good mathematical representation is the indispensable prodrome of important discoveries. To put the matter in different words, there is no possibility of analogy if no representation has been setted out; the representation is the indispensable ground upon which the analogical creativity can be exercised.

Let me offer an example, so as to highlight the real nature of the problem at issue. As we have already seen, one of the example made by Steiner to illustrate what he calls "Pythagorean analogies" is Dirac's discovery of the positron (or 'anti-electron', as he initially called it). At the very base of his discovery there is the attempt to extent the Klein-Gordon equation (a relativistic version of the Schrödinger equation) to the electron. By introducing a higher dimension 4×4 matrices, he found a new equation that both describes the behaviour of the electron and manages to incorporate special relativity:

$$\left[\gamma^{\mu}\left(i\frac{\partial}{\partial x^{\mu}} + eA_{\mu}(x)\right) + m\right]\psi(x) = 0$$

where $A_{\mu}(x)$ are the electromagnetic potentials (specifying the electric and magnetic fields acting on the electron at every point x of spacetime, and γ^{μ} with $\mu = 0, 1, 2, 3$ are the so-called "Dirac matrices":

$$\gamma^0 = \begin{bmatrix} 1 & 0 & 0 & 0 \\ 0 & 1 & 0 & 0 \\ 0 & 0 & -1 & 0 \\ 0 & 0 & 0 & -1 \end{bmatrix}, \gamma^1 = \begin{bmatrix} 0 & 0 & 0 & -1 \\ 0 & 0 & -1 & 0 \\ 0 & 1 & 0 & 0 \\ 1 & 0 & 0 & 0 \end{bmatrix},$$

$$\gamma^2 = \begin{bmatrix} 0 & 0 & 0 & i \\ 0 & 0 & -i & 0 \\ 0 & -i & 0 & 0 \\ i & 0 & 0 & 0 \end{bmatrix}, \gamma^3 = \begin{bmatrix} 0 & 0 & -1 & 0 \\ 0 & 1 & 0 & 1 \\ 1 & 0 & 0 & 0 \\ 0 & -1 & 0 & 0 \end{bmatrix}.$$

The equation turns out to have four possible solutions, two 'positive-energy' solutions and two 'negative-energy' solutions. However in 1928, when he derived the above equation, no situation was known in which these 'negative-energy' solutions played any role. Instead of discarding these 'strange' solutions, Dirac conjectured that these solutions actually describe a new elementary particle — the positron.

As Steiner (1998) and Bangu (2012) notice, there is something strange in this line of though. The shift to a 4×4 dimension matrices is not so obvious as it may appear, and the conclusion that there *must* exist something *real* corresponding to the two 'negative-energy' solutions is quite odd.[1] However, I think it is quite clear that the ground on which the new discovery can sprout up is the new representation of the behaviour of electrons made possible by the new mathematical equation described by Dirac. The discovery is deep-rooted in the representation offered by the new equation and there is clearly a cooperation between the heuristic and representative roles of mathematics operating here. Thus, a better comprehension of the 'oddities' latent in the discovery of the positron cannot avoid to take into consideration the representative ground from which it is arosen.

I am not saying that heuristic problems are nothing but representative problems in disguise. I am just saying that a good representation is the indispensable ground for any heuristic role of mathematics, and that a closer analysis of the relations between representation and heuristics may help in clarifying the (illu-

[1] Bangu, particularly, insists on this existential deduction to endorse that there is at work here a non-naturalistic reasoning. See (Bangu 2008, Bangu 2012) on this point. I will deal with Bangu's reconstruction in the next chapter.

sory?) miraculous effectiveness of mathematics in some discoveries. This point has already been noted, for example by French (2000). However, I want to precise that my concern here is not that of proving that *any* case of heuristic applicability of mathematics can be reduced to a representative application, but rather that some cases of heuristic applicability may be better clarified by a deeper account of mathematical representative role.[2]

In this chapter I am going to discuss the possibility to offer an account for representative problems relative to the applicability of mathematics. In doing this, I will just focus my attention on this intertwine between representative and heuristic roles of mathematics. I will take into consideration one of the most recently discussed accounts for representative roles of mathematics, the so-called 'structural account' (also called 'mapping account'). According to it, the representative effectiveness of mathematics can be satisfactorily accounted for if we say that, when a mathematical structure satisfies certain conditions, it can be considered as a good representation of a physical system. These conditions should grant a certain correspondence between the mathematical structure and the physical system under analysis, and once this correspondence has been made explicit, it should permit us to interpret the mathematical formalism of the theory *over* the physical elements of which the theory speaks.

Such a structural account has many advantages. First of all, it fits well with the contemporary tendency to look at scientific theories as models, so emphasizing the semantical perspective. A structural account of the applied mathematics is surely in compliance with this approach. Moreover, such an account underlines what is often considered a "familiar enough point" (Baker's (2003) expression), namely that «it is solely the structural features of mathematical theories that are relevant to their use in science» (Baker 2003, 54). Indeed, as

[2]In this chapter I am going to discuss the question from a pure theoretical point of view. In the next chapter I will offer some examples of Steiner's cases of heuristic applicability that may benefit from this approach.

also Suppes (1967*b*) noted,

> We cannot literally take a number in our hands and apply it to a physical object. What we can do is to show that the structure of a set of phenomena under certain empirical operations is the same as the structure of some set of numbers under arithmetical operations and relations. The definition of isomorphism of models in the given context makes the intuitive idea of *same structure* precise. The great significance of finding such an isomorphism of models is that we may then use all our familiar knowledge of computational methods, as applied to the arithmetical model, to infer facts about the isomorphic empirical model. (p. 59)

5.2 The structural account

A very naïve formulation of the structural account is the following: mathematics is so useful and effective as a representative tool in science because (A) mathematics studies structures and (B) these structures can be found in nature and their description is hence part of the scientific picture of reality. This is the way in which Steiner presents, in the introduction of his book, the structural account, referring to it as «a distressingly common "explanation" for the effectiveness of mathematics in physics». His judgement on it is negative yet: «I believe that the currency of this explanation stems from a confusion among the various senses of "applicability", which I also want to clarify in this book» (p. 6). The mention is very cursory and it is not clear why he thinks that this account is to be rejected. I guess that the confusion of which he talks is about metaphysical and descriptive problems. However, it must be noted that we are presenting here the structural account only as an account for *representative* roles of mathematics, and not also as an account for metaphysical problems. However, if this is the aim for which the structural account is usually presented, there are also cases in which there seems to be a metaphysical intention in proposing it. Steiner's formulation (and his consequent attack) does not sound so remote if we consider the following, likewise naïve, formulation by Shapiro (1997):

> the contents of the nonmathematical universe exhibit underlying math-

> ematical structures in their interrelations and interactions. According to classical mechanics, for example, a mathematical structure much like the inverse-square variation of real numbers is exemplified in the mutual attraction of physical objects. In general, physical laws expressed in mathematical terms can be construed as proposals that a certain mathematically defined structure is exemplified in a particular area of physical reality. (p. 248)[3]

The problem of such a formulation is that it leaves many questions unresolved. First of all, it does not clarify in which sense mathematical structures can be "exemplified" in the nonmathematical universe. The structures that we found in nature are the very same structures that we study in mathematics? Or should we rather think that we have two different structures among which a structure-preserving relation exists? But then, how can we define the 'physical' structures that exemplify mathematical structures? And again, if it is clear that the structural account is based on the idea that there is some kind of 'structure-preserving' relation between the mathematical formulation of a theory and what the theory speaks about, it is absolutely not clear how this relation can be spelled out in an explicative or explicit way.[4]

Moreover, Steiner's presentation of the structural account may led one to think that there must be some implicative relation among the structural account itself, mathematical structuralism and structural realism.[5] Premise (A) seems to allude to mathematical structuralism — the idea according to which what matters to a mathematical theory is not the nature of the objects of which it talks, but rather the internal relations among its objects. Steiner mentions the structural account in a passage where he is discussing the impossibility of finding a criterion for a structure to be mathematical, and it is just this impossibility, according to Steiner, to remove from consideration the structural account. So, it seems that, according to Steiner, mathematical structuralism is *implied* by

[3] See also Shapiro (1983).

[4] In the next sections I will deal with these problems. As it will become clear later, these difficulties do not concern only the naïve presentation of the structural account.

[5] For an introduction to mathematical structuralism, see (Resnik 1981, Resnik 1982, Resnik 1997, Shapiro 1997). As to structural realism, see (Worrall 1989, Ladyman 1998).

the structural account and that one cannot mantain the latter without holding the first. The structural account can be obviously worked out in harmony with mathematical structuralism, but there is nothing that forces a supporter of the structural account to hold mathematical structuralism. After all, premise (A) says only that mathematics studies structures. It does not say that mathematics is *nothing but* the study of structures, or that this is the best way to characterize the mathematical practice. Rather, the structural account only needs that we agree on the simple fact that mathematics *can* be presented as the study of structures — and this is quite undisputed. Of course, one may discuss about the fact that this is, or is not, the best way to present mathematics; or that mathematics can, or cannot, be reduced to the study of structures; or that this is, or is not, the most productive way to study mathematics. However, it is a fact that any branch of mathematics can be described as the study of a particular (class of) structure(-s) — and this is all that the structural account requires. In other words, we are not forced to buy mathematical structuralism together with the structural account.

However, Steiner's rejection of the structural account is apparently based just on this misunderstanding — that one could not mantain the structural account without accepting mathematical structuralism. Since this belief is actually untenable, as we have just shown, Steiner's consideration appears now to be quite hasty. We can accept the structural account even if we do not agree on any kind of structural reductionism in mathematics.

An analogous remark can be made about the premise (B). Such a premise may be seen as an implicit allusion to scientific realism, and one may think that the supporter of the structural account is forced to be a structural realist as well. However, as well as for mathematical structuralism, this is not exact. The problem is here a little more complex, depending on what we mean by "structural realism". However, premise (B) only says that structures are *part* of our scientific image — it does not say that they are *all* what we found in nature. Once again, therefore, to buy the structural account does not force us to buy scientific realism along with it.[6]

Up to now we considered two quite naïve formulations of the structural account, Steiner's (1998) and Shapiro's (1997) ones. Further and more sophisticated formulations can be found, for example, in French (2000), Pincock (2004), van Fraassen (2008), Bueno & Colyvan (2011) and others. These accounts differ under many aspects. Actually, the structural account can be stated in various ways, depending on the author's main interests. For example, one may focus, as Pincock (2004) does, on the conditions that a statement of applied mathematics (a 'mixed' statement) must satisfy in order to be considered as true: «According to the mapping account of applications, the truth of a statement of applied mathematics (or 'applied statement') depends on the existence of a mapping of a certain kind from a physical situation to a mathematical domain» (p. 69). Thus, if such a mapping exists, we are entitled to say that our 'applied statements' are true. However, my focus in this chapter will be on the *epistemic* conditions that enable us to consider a mathematical structure as a good representation of a given physical system (the target). In other words, how do we come to know that a certain mathematical structure can be used to represent a physical system in such a way that we can employ that mathematical structure to gain (possibly new) knowledge about the physical domain at issue?

However, before facing these epistemic problems, it is important to understand what this 'structural similarity' consists in. The mathematical representation has obviously to preserve the structure of the physical domain, but this is not something that can be easily done when we do not completely know the target, or when our knowledge of it is still tentative — and here is where heuristic

[6] This is, by the way, in line with what we said in the previous chapter: ontological and metaphysical discussions over mathematical entities lies *outside* the structural account, and rather the structural account can in case be considered as a starting point for ontological debates.

role of mathematics comes into play. At least two problems afflict the structural account about this point:

1. which kind of relation is the structure-preserving relation that should grant the representativeness? And
2. How can we define a relation between a mathematical structure and a physical system, if on the one side we have an abstract entity and on the other side a concrete one?

This two points are usually neglected in the discussions of the structural account I have previously mentioned. For example, this is how Pincock (2004) exemplifies the kind of structure-preserving relations he has in mind:

> Counting, for example, involves isomorphisms from the objects counted to an initial segment of the natural numbers. More sophisticated applications will involve other kinds of mappings, such as homomorphisms that respect certain features of the physical situation, e.g., the mass of physical objects. (p. 69)

The example is surely appropriate but is also very simple and do not really help in clarifying the matter. Moreover, Pincock mantains a certain vagueness about the *minimal* structure-preserving relation that should grant the representativeness. This is a point on which I think is important to insist. Usually, supporters of the structural account just exemplifies some kind of structure-preserving relations: homomorphism, isomorphism or, by appealing to the 'partial' structure program proposed by da Costa and others,[7] 'partial' homo- or iso-morphism, and so on; and then they say that this or that relation obtains according to the specific case at issue. So, it turns out that in some cases the proper structure-relation seems to be the homomorphism, in other cases it seems to be the isomorphism, in other cases again it seems to be something else. Now, it seems to me that once we admit that the condition of representativeness is properly captured by some kind of structure- preserving relation, we should at least be

[7]See for example Bueno (1997).

able to specify the *minimal* relation according to which the representation is valid. I mean, we should specify the minimal *necessary* structure-preserving relation that permits us to say that the mathematical structure *can* represent the physical system at issue. This relation may even not be sufficient, but at least it should fix a *minimal threshold under which the representation cannot take place*. I think that the neglect of this point leaves the supporter of the structural account open to possible criticisms, since it leaves the whole problem of these structure-preserving relation into an unhelpful, hazy vagueness.

In section 5.2.1 I will deal with this problem and I will try to specify a minimal structure-preserving relation that could grant to a mathematical structure an access to representativity. However, before doing this, it is previously necessary to define in a proper way what I mean by 'structural relation' and 'structure-preserving relation'. It will turn out that this definition is not so easy as one might think, and several difficulties nestle within such a definition. Some of this difficulties are rarely discussed in the literature, despite of their importance. In the next sections I will try to overcome these problems by integrating the structural account with some considerations that — I hope — will clarify in which sense we can legitimately employ the notions of 'structure' and 'structure-preserving relation' to account for the representative (and, in some sense, also heuristic) effectiveness of mathematics.

5.2.1 Minimal condition

The first problem to solve is to understand how to define this 'structural relation' in a rigorous manner. As a first attempt, let say that M is a mathematical structure and S a structured physical domain, or a 'physical structure'. I take a mathematical structure to be defined, as usually, as an ordered quadruple

$$M = \langle dom(M), \{R_i\}_{i\in I}, \{f_j\}_{j\in J}, \{c_k\}_{k\in K}\rangle,$$

where $dom(M)$ is a non-empty set of objects (this is our 'universe'; it must be noted that the nature of these objects is absolutely irrelevant); $\{R_i\}_{i\in I}$ is a non-empty set of n_i-ary relations defined on $dom(M)$; $\{f_j\}_{j\in J}$ is a set of functions defined on $dom(M)$; and $\{c_k\}_{k\in K}$ is a set of special elements belonging to $dom(M)$ (including, for example, the unity element). I, J, K are three disjoint sets of indices. I will leave open, for the moment, the question concerning what a 'physical' structure is.[8] For the moment, we can just take S as a specular ordered quadruple, with a domain composed by physical objects with various relations defined over them. We will say that S is homomorphic to M *iff* there exists a $\phi\colon S \to M$ such that:

1. for any function f_A in S there exists a correspondent f_B in M such that, for any $x_1, \ldots, x_n \in dom(S)$,

$$\phi(f_A(x_1, \ldots, x_n)) = f_B(\phi(x_1), \ldots, \phi(x_n));$$

2. for any relation R_a in S there exists a correspondent R_b in M such that, for any $(x_1, \ldots, x_m) \in dom(S)$, $(x_1, \ldots, x_m) \in R_a$ *iff* $(\phi(x_1), \ldots, \phi(x_m)) \in R_b$;
3. for any constant c_α in S there exists a c_β in M such that $\phi(c_\alpha) = c_\beta$.

With this definition in mind, we can now say that when ϕ is injective we have a *monomorphism*, when ϕ is surjective we have an *epimorphism*, and when ϕ is both injective and surjective we have an *isomorphism*.

All these structural relations guarantee some kind of structure preservation. The problem now is to understand whether there is a 'minimal' structural relation that an applied mathematical structure has to satisfy in order to guarantee its representative effectiveness. At a first glance it seems that isomorphism is the best situation to work with, since it grants a perfect correspondence be-

[8] This problem will be discussed in section 5.3.

tween the two domains. Of course it is, but it is also a very tight condition to be imposed as a minimal request. It is not difficult to find a case of mathematical representation which does not meet this condition and nevertheless is effective in representing a physical domain. A typical example is offered by the Navier-Stokes equations. Here we use a set of equations to succesfully model the behaviour of a fluid substance. These equations implicitly assume that the substance at issue is *continuous*, although our best theories say that the ultimate composition of matter is *not* continous. So, assuming that our best theories are true, it does not seem to be possible to set up an isomorphism between the mathematical structure and the physical one, since the former is 'richer' than the latter. Nonetheless, these equations are effectively employed in representing the behaviour of the phenomenon at issue.

On the other side, the loosest condition we can impose is the homomorphic one. However, in this way we run the risk that the condition we impose does not suffice to secure the representation with a content. Namely, fixed a physical domain, there will always exist a homomorphism from it to a mathematical structure that has no representative content. Let us take, as an example, the trivial homomorphism that collapses all the elements in $dom(S)$ to one element in $dom(M)$ (the unity element, if it exists) and all the relations in R_S to the identity relation. This is a homomorphism, but we all agree that this would not be a good mathematical representation. It seems that a minimal condition should be found between these two extreme cases, isomorphism and (trivial) homomorphism; and that the way to articulate such a minimal condition is by taking into consideration the notion of 'content' of a representation.

In order to clarify the notion of 'content', let us take the example of a city map. What we desire is that the map reproduces the relevant aspects of the geographical area at issue, where by "relevant aspects" I mean those peculiarities (streets, distances, corners, eventually altitudes, and so on...) that we are interested in knowing and that make the map *effective* for the aims it has been created for. These relevant aspects will be the *content* of our representation. Also in this case we have to avoid the case of the trivial homomorphism; namely, we want that every relevant aspect of the represented area be *distinctly* represented by a mark (a line, a dot, or whatever else) on the map. One might think that this amounts to demanding that the map be such that there is a *monomorphic* relation from the land to the map.[9] But the monomorphism is such only when we have fixed what we mean by "relevant aspect". If we are interested only in distances, we can reasonably avoid any indication about the altitude; but if we want to know also how much exertion we will have to bear in going from point A to point B, we will need in addition an indication of the altitudes and of the differences in elevation.

Similarly, the same considerations can be made for the case of a mathematical representation. Also in this situation we want that the mathematical representing structure be able to grasp univocally and distinctively every single *relevant* element of the physical domain we are going to represent. We want that the mathematical structure be able to grasp all the relevant elements and all the relevant facts and relations in the physical system, *without any loss of information* — and it seems that the only way to grant this is to impose that the homomorphism is injective, i.e. that it is a monomorphism. Thus, it seems that the minimal condition we are searching for is the monomorphism condition — but such a monomorphic relation can be defined only when we have clear in mind what the relevant aspects of the physical domain are.

Bueno & Colyvan (2011) consider the same problem, although under a different perspective. As they say, «It would seem that the mapping employed will depend on the richness of the two structures in question» (p. 348), S and M.

[9] Injective homomorphisms are aslo usually called "embeddings", but in this chapter I will continue talking of monomorphisms instead, since in the literature on the argument "embedding" is often used as a generic term (see for example Pincock 2004).

If S is richer than M (i.e., there are more objects or more structural relations between them in the physical domain than can be represented in the mathematical structure), then ϕ can be a simple homomorphism (neither injective, nor surjective) or an epimorphism. But if M is richer than S, then we must consider a third possibility, monomorphism. But we also want, in order for mathematics to be useful, that ϕ be invertible, so that we can «move freely into and out of the mathematics, just as we can move freely between our street directory and the city» (pp. 348-9) — and this implies that ϕ must be a monomorphism, with the further consequence that it is apparently not possible for the physical structure to be richer than the mathematical one. However, Bueno & Colyvan (2011) bring forward examples of mathematical representations in which this is exaclty what seems to be the case; namely, cases in which the physical structure seems to be *richer* than the mathematical. For this reason, they prefer not to conclude in favour of a relation over another as a minimal condition for representativeness. It simply depends on the situation which relation is the best.

Now, I want to resist this conclusion. Actually, I think that the cases of richer physical structure they present are the consequence of their overlooking the fact that, when we represent a physical domain, we are interested only in some *relevant* aspects of that domain. If we keep this point into consideration, cases in which S is structurally richer than M are simply ruled out. For if we admit that M is a good representation of S and nevertheless S is richer than M, this means that there are objects or relations in S that simply are irrelevant for the aims which we made the representation for: if the mathematical representation is not able to grasp them and remains nonetheless effective, then these elements cannot be considered as relevant. The toy example offered by Bueno and Colyvan in their article can be easily trated this way. «Consider, for example, modeling time with a 12-hour clock — essentially arithmetic mod 12. As it stands such a clock does not allow one to make sense of 2 am on different days, for instance» (p. 349).

We can easily see that such a time representation is made with a very narrow scope, and the possibility of distinguishing 2:00 am of 23rd May from 2:00 am 24th May does not fall within this scope. In other words, once we identify the aims of the representation, this aim will define a class of relevant elements associated with that aim — and days are not within the class of relevant elements defined by the scope of misuring hours and minutes.

Our conclusion is then that we *can* identify a minimal condition for a representative mathematical structure, and this minimal condition is *monomorphism*. However, we can do that only relating the minimal structure-preserving relation with the content of the representation, which is in turn a very high pragmatic notion. But problems do not end here.

5.2.2 Some remarks on the structural account

My conclusion in the previous section has been that we *can* identify a minimal condition for a representative mathematical structure, and this minimal condition is *monomorphism*. However, such a minimal condition can only be articulated around the notion of 'content' of a representation, and this notion immediately leads us to the highly pragmatic (and highly problematic) notion of 'relevant aspects' of a physical domain. The representation is made with a certain aim in view, and it is this aim that determines which is the relevant content of the representation.

This is a very delicate point. For example, if I want to give a mathematical representation of the kinematics of a system of bodies I will ignore the colours of the bodies, since they are irrelevant to my representation. So, I will consider a physical structure S that has no colors among its objects (or among its relations), but I will only consider the relevant relations between masses, bodies, positions on the one side, and the structure of real numbers on the other side.[10]

[10]In order to avoid a possible objection (that has been sometimes excepted to me), I want

The problem is: how can I select the relevant aspects of the piece of nature that I am trying to represent? It seems that this task should *precede* the application of mathematics, but it often happens that it is just mathematics that helps us in selecting the relevant aspects of a physical system. We have already seen an example of this when I presented Dirac's discovery of the positron.[11] The point I want to stress is that *the world does not come equipped with a structure*; rather such a structure is often determined just by our theories and by the mathematical structures we employ in the representation. The realist might say that, after all, we can 'carve nature at joints', but this is not very helpful, because we don't know from the beginning which is the proper way to 'carve nature at joints'. This is a very complicated issue which is not possible to discuss extensively here, but I will shortly try to say something more. However, this remark seems to suggest that there is no hope to articulate an account of representative effectiveness of mathematics in *purely* structural terms, since the pragmatic component seems to be irreducible.[12]

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to precise the following point. In the example at issue, to each body will correspond a mass, and to each mass will correspond a real number. Now, two bodies may have the same mass, and hence may correspond to the same real number. Therefore, one might notice that in this case the physical domain (the bodies of the system and the relations among them) is (non-trivially) *homomorphic* to the mathematical structure (the additive structure of real numbers), but is *not* injective, since two bodies can correspond to the same mass-number — and hence the monomorphism rule seems to be infringed. If it were so, it would turn out that the monomorphism cannot be considered as a minimal condition for representativeness. However, in this case the structure-preserving relation should be *not* from the physical system to the additive structure of real numbers, but rather from the physical system to a *vector space*, whose number of dimensions depends on the number of bodies composing the system (in the Euclidean space, for each body we will have three dimensions for its position, three for its velocity, and one for its mass), on which a relation of correspondence between these vectors is fixed. In this way the monomorphism is preserved. I recall I am dealing only with the *representative* effectiveness of mathematics and that the monomorphism condition I am trying to defend as the proper *minimal* request refers just to this particular effectiveness of mathematics. Measuring is surely an important way in which we 'apply' mathematics, but it is different from representation — even if representation, of course, subsumes measuring practices. Even if measuring can be accounted for in structural terms as much as mathematical representation can, it will satisfy different conditions — which I will not deal with in this work.

[11] Another example, the discovery of the omega minus particle, will be discussed at lenght in the next chapter. Other examples are offered by Batterman (2002), who stresses the importance of 'asympthotic reasoning' in removing explanatory non-relevant aspects. The problem is here interwoven with mathematical explanatory power in science.

[12] Bueno & Colyvan (2011) come to the same conclusion. They also say that Pincock's (2004) aim was precisely to account for the applicabilty of mathematics in *purely* structural terms, but I must disagree with them on this point, since in no place does Pincock make such a claim.

That an account of the representative effectiveness of mathematics cannot be achieved in purely structural terms, is also shown by the fact that (whatever be the minimal condition to be prescribed) the mathematical structure *can* be richer than the targeted physical domain, and that is what usually happens. So, as Bueno & Colyvan (2011) rightly point out, we may be confronted with situations in which our mathematical structure predicts *more* than one possible solution and not all these solutions have an empirical counterpart. Let us take the very simple case of a quadratic equation used to predict where a projectile will land. Such an equation will have two solutions and it may happen that these two solutions will not coincide. If, for example, we want to predict the landing of a projectile launched from the cliff of a mountain, one of the two solutions will be negative and will have no physical interpretation. In other cases, instead, mathematical solutions previously considered as physically meaningless are suddendly and unexpectedly reassessed in virtue of a new interpretation of them. Once again, Dirac's postulation of the existence of positrons we saw in section 5.1 perfectly exemplifies the point at issue. Now, the problem is: how can we distinguish, within a mathematical structure, its representing from its non-representing parts? The problem is quite simple for the case of a projectile, but what about more complex and intricated cases? According to Bueno & Colyvan (2011),

> such crucial information required to solve this physical problem *is not part of the mapping between mathematical structure and physical structure.* In short, the mapping account of mathematical applications is incomplete.
> [. . .]. Moreover, the incompleteness of the mapping account is seen clearly as a result of problems relating to the specifications of the mappings in question. (pp. 349-50)

The structural account is not able, *by itself*, to tell us which of the possible choices is the right one (or, are the right ones) and which is not. What is more, it is even unable to *justify* why, in the projectile example, the positive solution is the right one while the negative one is not. From a *purely* structural perspective, both of them are indistinguishable as far as their rightness is concerned. The lesson to be learned is: our account cannot avoid taking *non*-structural components into consideration.

Secondarily, this way to set out the issue presupposes that we already have a structured physical domain. That is just what permits us to say that the mathematical description of that domain is effective *because* the mathematical structure employed is a 'good copy', or a 'good representation', of the physical structure underlying the physical domain. But what is this 'physical' strucure, and in which sense the physical domain is 'already structured'? Moreover, that the physical domain has to be in some sense pre-structured seems to suggest that we already know at least what elements compose this domain. But in many contexts this is not the case, and nevertheless mathematics helps us to deepen our knowledge of the elements of the domain. For example, in particle physics mathematical models play a very valuable role in discovering new particles.[13] How is that possible? I will try to say something more about this aspect in section 5.3.

Thirdly, the considerations presented until now justify us in saying that, whatever the minimal structural condition be, this condition is just *necessary*, but not *sufficient* in order to account for the representative effectiveness of mathematics. In other words, *if* M is effective in representing S, *then* we can say that there is a monomorphism from S to M — but the converse is not true. In order to prove the converse, we should prove that any monomorphism from S to M makes of M an *effective* representation of S. But we have already noticed that the monomorphism must go from the *relevant* elements and relations in S to the elements and relations in M. So, in order to prove the converse, we should already know what the relevant aspects of a physical domain are.

[13] An interesting case in this regard is the discovery of the omega minus particle, made by Gell-Mann and Ne'eman in 1963. I will extensively discuss this interesting case in the next chapter.

Finally, the structural account, as we have seen, says that a mathematical representation is effective only if there exists a preserving structure relation (whatever it be: monomorphism, homomorphism, or anything else) from the physical domain to the mathematical structure. But how do we come to say that such a structure-preserving relation really subsists? To say that, we have to know in advance how the physical domain is structured — we have at least to know what elements compose it and how they interact. However, as I have already noted, mathematical representations are usually employed to *discover* new entities or new relations in the physical domain. In these cases, there is a difficulty about how we can come to know that a particular monomorphism really subsists.[14]

To sum up, we saw that a mathematical structure M seems to be succesfully applicable only if there exists a monomorphism from the (relevant) physical structure S to M. This seems to be a necessary condition, since, if such a monomorphism does not exist, some elements of S (objects or relations) will be not distinguished by the representing mathematical structure M, and this would be a loss of information. Yet, this cannot be taken to be a sufficient condition, for the reason I have just pointed out. Moreover, the structural account suffers from different problems that must be solved in some way. In the next sections I will try to give my modest contribution to make the structural account more satisfying, but it seems clear since now that it will be impossible to exclude from the account some non-structural, pragmatic components.

[14]It must be noted that it is not sufficient that *there is* a monomomorphism: we must be able to spell it out in order to make the representation effective — i.e., in order to make the representation a useful tool to make verifiable predictions and hypotheses.

5.3 Physical and mathematical structures

5.3.1 The Problem of the Coordination revised

In the previous section, I have pointed out that the physical domain has to be already, in some sense, 'structured' in order to define a structure-preserving relation from it to a mathematical structure. Namely, on the physical side we should already have something like a 'physical' structure. But what does this expression mean? What is a physical structure? One might say that a physical structure is just a mathematical structure embedded in nature, so that the only difference between the two is that the first is abstract and the second is a model for it. Well, but how do we know that the latter is a model for the former? What we have not considered, up to now, is the fact that the physical structure is something that we do not know, something hidden in the phenomena of nature. The mathematical structure is often just a 'tool' by means of which we manage to grasp this hidden physical structure. But, given that the monomorphic relation is the only way for us to be sure that the mathematical structure can be effectively and successfully used to know the physical structure, how can we set up such a monomorphic relation if one of the two terms of the relation is unknown? And how can we know that such a relation *really subsists*?

Actually, there are two problems here that we should keep distinct: the former concerns *how* we can set up a relation between physical and mathematical structures; the latter concerns how we can understand that such a relation *is actually* a monomorphism. Let us start with the first problem. The point is: how can we fix a relation (whatever it is: isomorphism, homomorphism or monomorphism) between a mathematical structure and a physical structure *that we do not know*? Or, to use a different terminology, how can we compare a structure to an alleged model of it if we do not know the proper interpretation that links the former to the latter — and we do not even know the structure?[15]

The problem just sketched can be seen as a variation of the well-known problem of the coordination raised by Reichenbach (1965).[16] «The mathematical object of knowledge — he says — is uniquely determined by the axioms and definitions of mathematics» (p. 34). On the contrary,

> The physical object cannot be determined by axioms and definitions. It is a thing of the real world, not an object of the logical world of mathematics. Offhand it looks as if the method of representing physical events by mathematical equations is the same as that of mathematics. Physics has developed the method of defining one magnitude in terms of others by relating them to more and more general magnitudes and by ultimately arriving at "axioms", that is, the fundamental equations of physics. Yet what is obtained in this fashion is just a system of mathematical relations. What is lacking in such system is a statement regarding the significance of physics, the assertion that the system of equations is true for reality. (p. 36)

If, in a certain sense, mathematical truths are granted by the internal *coherence* of a mathematical structure, the same cannot be said for the physical relations. In this case we need something like a 'coordination': «physical things are coördinated to equations. Not only the totality of real things is coördinated to the total system of equations, but *individual* things are coördinated to *individual* equations» (p. 37). Of course, one might say that what we are looking for is just a function (a mapping, an intepretation) between mathematical objects (and operations, and relations) and physical objects (and operations, and relations) — so, what's the problem? Well, in the specific case of the physical coordination, the matter is much more complex than this:

> [...], if two sets of points are given, we establish a correspondence between them by coordinating to every point of one set a point of the other set. For this purpose, the elements of each set must be defined; that is, for each element there must exist another definition in addition to that which determines the coordination to the other set. Such definitions are lacking

[15] This point is strictly linked to the fact that a mathematical representation can be (and often *is*) useful also in fostering new discoveries. Bueno & Colyvan (2011) do not pay any attenation to this point, and it seems to me that an account of the representative effectiveness of mathematics which fails in accounting for it should be considered unsatisfying.

[16] On this parallelism with Reichenbach's problem of coordination, see also van Fraassen (2006, 2008).

> on one side of the coordination dealing with the cognition of reality. Although the equations, that is, the conceptual side of the coordination, are uniquely defined, the "real" is not. (p. 37)

In short, baldly stated, the problem is that if the target of the representation is not a mathematical object, then we do not have a well-defined range for the function.

The situation is re-stated and analysed by van Fraassen (2006) as follows:

> a function that relates A [*the phenomenon*] and B [*the mathematical structure*] must have a set as its domain. If A is, for example, a thunderstorm or a cloud chamber — a physical process, event, or object — then A is not a set. *Fine*, the realist answers, but A has parts and the function's domain is the set of these parts. Moreover, there are specific relations between these parts, and these relations have as their extensions sets of sequences in that domain. The function provides a proper matching provided that the images of these relations are relevant relations in the model. (p. 540)

So, we have A (the phenomenon at issue), B (the mathematical representation of it), and something that we could call $S(A) = \langle SA_1, SA_2 \rangle$, where SA_1 is the set of parts of A, and SA_2 is the family of sets that are extensions of relations on these parts. In this account, $S(A)$ is actually a mathematical structure and the Reichenbach problem in comparing $S(A)$ with B is ruled out, for both are abstract (mathematical) structures. But in this way we are just pushing the problem one step back; now the problem is: how can we compare A with $S(A)$? But that is not all. We have several ways to divide up A. Which of these is the right one? Realist might answer that the right one is that that 'carves nature at joints', but this sounds more like beating about the bush, since we do not know what is the proper way to 'carve nature at joints'.

5.3.2 Phenomena, data models and theory models

So, let us take one step back, and try to find a way out. There are some points that should be noted and that may permit us to deflate (at least part of) the problem. The problem at hand is clearly a problem of representation: how can we represent a concrete physical system by means of an abstract structure such as a mathematical one? Now, as Van Fraassen properly points out,

> The question of how a specific mathematical object can be used to represent some specific phenomena makes sense only in a context in which some description of the latter is at hand. Reichenbach, it seems to me, mistakenly pursues the 'profound' 'fundational' question of how such use is possible *outside any such context* — as if theories are received by babies or primitives before the acquisition of language.
> [...].
> He is explicitly addressing a situation in which there is no description at hand for what is to be represented. (van Fraassen 2006, pp. 541-2)

But we actually *have* a description at hand for physical entities, and this is just that offered by our own language and by our presupposed scientific theories. All this, evidently, involves a large amount of pragmatic elements, but it is quite obvious that mathematics is not an instrument that we apply in an aseptic context. Therefore, all things considered, the question of how we can represent a physical phenomenon by means of abstract objects such as mathematical ones is not really problematic. At least, it is no more problematic than the problem of denotation in philosophy of language. In other words, there is nothing problematic in using mathematical objects in order to *denote* parts of a physical phenomenon, where these parts are those delivered by our language and theories.

However, there are still two problems to solve: (1) since there are several ways to divide up a phenomenon into parts, which of these ways is the right one, and how can we recognize it? (2) How can we understand that a certain mathematical structure has a monomorphic relation to the target phenomenon (so that we can say that such a mathematical structure is a suited and effective representation of it)?

Question (1) and (2) are particularly tangled up. As van Fraassen (2008) interestingly points out,

> [...] the assertion or denial of isomorphism depends on a certain selection

> on our part. In the case of two mathematical objects we can make the selection in a straightforward way, since they are already 'given' in a format which lends itself to us. Given a particular Hilbert space and a family of operators on it singled out by some equations, the relevant questions can obviously be formulated: for example, does this family contain an element I such that for all its members X, $IX = XI = X$? But how do I fomulate questions of this sort for a part of nature, without using a selective description of it that already rests on a 'mathematization'? (p. 366, note 7)

Now, Van Fraassen analyzes the issue as follows. In concrete settings, the structure-preserving relation intervenes not between the phenomenon and the theory model, but between the *data model* and the theory model. When we collect the data, what we have is already a mathematical structure, and the mathematical structure of the theory model tries to embed the mathematical structure of the data model. However, according to Van Fraassen, this does not push the problem one step back. Why? Because the «construction of a data model is precisely the selective relevant description (by the user of the theory) required for the possibility of representation of the phenomenon» (van Fraassen 2006, p. 544). The point that Van Fraassen emphasizes is that

> *There is nothing in an abstract structure itself that can determine that it is the* ***relevant*** *data model, to be matched by the theory.* That is why our talk of data models 'between' the theoretical model and the phenomena does not simply push Reichenbach's question one step back, to be faced all over again in the same way. [...].
>
> That is, the phenomenon, what it is like, taken by itself, does not determine which structures are data models for it. That depends on our selective attention to the phenomenon, and our decisions in attending to certain aspects, to represent them in certain ways and to a certain extent. (pp. 544-5)[17]

In representing a phenomenon, there is an ineliminable indexical component: the representation is a representation of something (as thus and so) *by somebody*. The data model is the phenomenon as represented by someone, and when we want to check a claim of adequacy, we will compare the theory model with the data model. But to say that the theory is adequate to the phenomena *as*

[17]Italics and bolds are in the text.

represented by us (namely, our data model) is the same, *for us*, as to say that the theory is adequate to the phenomenon *tout court*. This last point is, on Van Fraassen's point of view, a *pragmatical tautology*, and «Appreciating that this equivalence for us is a pragmatical tautology removes the basis for the challenge» (p. 545) — that is, removes the necessity to push Reichenbach's question one step back.

5.4 In Search of a Way Out

5.4.1 A proposal of integration for the structural account

Van Fraassen's solution, by appealing to the notion of 'pragmatical tautology', sounds quite tricky, and not many would follow him on this road. Nevertheless, there are two points in Van Fraassen's analysis that I want to retain and emphasize: (A) there is nothing in the phenomenon that determines which structures are data models for it — namely, which mathematical structures are able to capture its relevant aspects; (B) when we want to check a claim of adequacy for a theory, we compare the theory model *with the data model* (and not with the phenomenon itself). So, the effectiveness of mathematics is not simply a matter of *conditions* that we have pre-emptively to satisfy for its application. What we need is a system that permits us to check the adequacy of a mathematical representation in the double sense of: (I) checking the adequacy of the model theory as properly embedding (namely, monomorphically — as we saw) the data model, and (II) checking the adequacy of the data model in representing the relevant aspects of the phenomenon at issue. Moreover, all these considerations show that the structural account — as presented so far — is not enough in order to account for the applicability of mathematics. It can constitute, at best, a good starting point, but it needs an integration, and such an integration has to consider the pragmatical elements that intervene in the realization of a mathematical model for a physical phenomenon.

In this section I am going to propose an integration of the structural account that, I think, can solve part of these problems, and can also answer question (2), which I left open ("How can we understand that a certain mathematical structure has a monomorphic relation to the target phenomenon, so that we can say that such a mathematical structure is a suited and effective representation of it?"). My idea aims to fuse together the structural account with the DDI model account proposed by Hughes (1997), in a fashion that resembles the 'Inferential Account' proposed by Bueno & Colyvan (2011), yet different from it on some important points. The DDI model is a general account of modeling in physics. According to it, modeling is conceived as consisting of three main components: denotation, demonstration and interpretation (from which the name "DDI"). As Hughes clarifies, he is

> not arguing that denotation, demonstration and interpretation constitute a set of speech acts individually necessary and jointly sufficient for an act of theoretical representation to take place. [He is] making the more modest suggestion that, if we examine a theoretical model with these three activities in mind, we shall achieve some insight into the kind of representation that it provides. Furthermore, we shall rarely be led to assert things that are false. (p. 329)

In short, I can sum up the DDI account as follows. A model *denotes* a physical phenomenon; it is a symbol for it and stands for it. In this manner, denotation plays the fundamental representative role, in accordance with Goodman's (1968) dictum that «denotation is the core of representation and is independent of resemblance» (p. 5). Such a representation has also an internal dynamics, which permits us to make *demonstration* within it and make novel predictions. But the predictions that we draw in the model remain predictions *about* the model if we do not intervene with a process of *interpretation* that permits us to move back from the model to the phenomenon at issue.

Now, my idea consists in integrating this account by means of our previous considerations about the structural account and the data models, *plus* some extra considerations about the way in which we can recognize the existence of a monomorphism between a mathematical structure and the structure of a data model. The following figure gives a visual representation of my proposal.

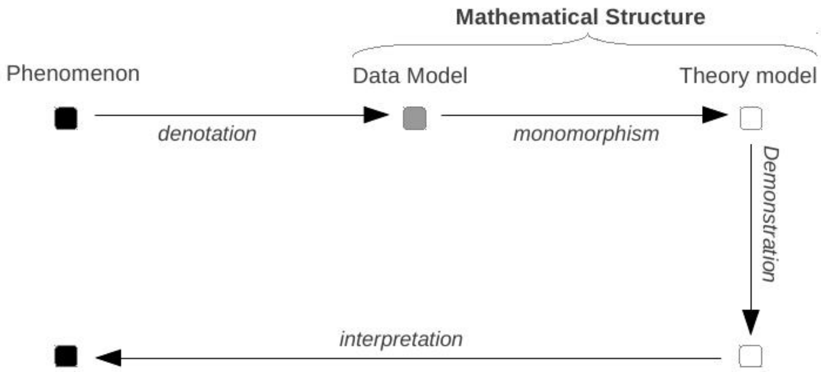

Figure 5.1: Integration of the structural account

The DDI account is preserved, as it is evident by the presence of the denotation-demonstration-interpretation triad. Yet, whereas in the DDI scheme denotation and interpretation occurred between the phenomenon and the model, here the 'model' is composed by two parts: the data model and the theory model. According to Van Fraassen's considerations, the phenomenon is represented by the data model (and only derivatively, or secondarily, by the theory model), and this representative role is centered on the notion of denotation. Still, we have to add an important remark: such a denotation is only 'partial'; namely, the denotation does not give a complete coordination between elements of the phenomenon and terms of the data model. The only elements for which we give a denotation are those that appear in the data model. So, for example, if we are trying to describe the interrelations among pressure, volume and temperature in a termic system, we will register the value for each of these values in different situations, and we will associate a number for any measurement. So, in our data model, we will denote pressure, volume and temperature by means of positive numbers. But this denotation is only 'partial': nothing is said about — for example — the denotation of negative numbers. We do not know if the structure that we are delineating in this way will be defined only on positive numbers or also on negative numbers. It will be a matter of interpretation to understand whether and how we will have to interpret negative numbers appearing in the mathematical structure as values for temperature, or pressure, or volume.

The data model, obviously, is not a complete mathematical structure. It is rather a *clue*, or a trace, of mathematical structure. Our task consists in finding the proper mathematical structure according to which the consequents in the physical phenomenon — as represented by the data model — are always a consequence in our mathematical structure, which corresponds to the monomorphic condition.[18] Now, the problem is that, given this condition, we cannot take for granted any consequence that we can demostrate in the mathematical structure. For the monomorphic condition gurantees that, if something is part of the hidden structure of the data model, then such a part will be surely deducible within our mathematical structure; but it does not guarantee that *every* consequence deducible from our mathematical structure is a consequence in the data model (and, hence, also in the phenomenon represented by it). So, how can we do? The answer is that every consequence that we can demonstrate in the mathematical structure must be empirically checked by means of an interpretation that permits us to go from the mathematical side to the empirical one. But this leads to two possibilities. Let us suppose that γ is a mathematical term deduced from the mathematical structure M, and suppose that we *do not know* how to interpret such a term. The two possibilities are either to hazard an interpretative guess at γ and then check its correctness; or, if we have no idea concerning

[18] On this regard, I think that the famous quotation from Hertz ([1894] 1956) — «the necessary consequents of the images in thought are always the images of the necessary consequents of the things pictured» (p. 2) — should be revised in the following way: *the necessary consequents of the things pictured should always be the necessary consequents of the images*. The original claim corresponds to the isomorphic condition, but we saw that such a condition is too restrictive.

how such a term could be interpreted, to say that γ is just a mathematical sign having no interpretation, depending on the fact that the structure-preserving relation is not an isomorphism but a monomorphism.

Let me make a toy example to better explain this point. Let us take the following puzzle: five men find themselves shipwrecked on an island, with nothing edible in sight but coconuts, plenty of these, and a monkey. They agree to split the coconuts into five equal integer lots, any remainder going to the monkey. Man 1 suddenly feels hungry in the middle of the night, and decides to take his share of coconuts at that very moment. He finds the remainder to be one after division by five, so he gives this remaining coconut to the monkey and takes his fifth of the rest, lumping the coconuts that remain back together. A while later, also Man 2 wakes up hungry, and does exactly the same thing: takes a fifth of the coconuts, gives the monkey the remainder, which is again one, and leaves the rest behind. So do men 3, 4, and 5, too. In the morning they all get up, and no one mentions anything about his coconut-affair on the previous night. So they share out the remaining lot in five equal parts finding, once again, a remainder of one left for the monkey. Find the initial number of coconuts.[19] There are in fact an infinite number of solutions to this problem, but obviously we are asked to find the smallest number of coconuts that satisfies the condition. The answer is 15,621 (the reader may check by himself the correctness of this solution). However, a story says that it was Paul Dirac to note that such a puzzle may have also another solution: -4 coconuts! This answer is right: each time a man arrives at the heap of coconuts, he finds -4 coconuts; since $-4 \div 5 = -1$ with a reminder of $+1$,[20] he takes away the remainder from the heap and gives it to the monkey (*i.e.*, he gives $+1$ to the monkey); what remains

[19] Reported in (Barrow 1988, p. 254).

[20] Typically, quotient and remainder functions are defined only for natural numbers, hence such an expression makes no sense until we define these functions for negative numbers too. Alternatively, one can check the validity of this negative solution by substituting -4 in the diophantine equation, assuming that the equation is defined also on negative numbers.

in the heap is -5 coconuts; his one-fifth share is -1, which he takes, leaving -4 coconuts behind for the next man; and so on, till the final division.[21] The point is that when we set up the equation for the solution of the problem, we have a linear diofantine equation. Now, in a problem like this we are obviously led to search positive solutions, since we are interpreting numbers as set of coconuts, and intuitively we do not handle sets of coconuts having negative cardinality. We *know* that there could be negative solutions, but we consider this only as a mathematical fact *without interpretation against reality*. However, if we can find a possible interpretation for such negative numbers, there is nothing to stop us to check this interpretation in reality and to examine whether it could be fruitful. In the case of coconut puzzle, our experience of the macroscopic world suggests us that such a negative interpretation will not be very fruitful; but in more abstract physical contexts (like quantic world, for example) this fact could be less obvious, and by giving credit to this interpretation we could be led to new interesting discoveries.

I think there is nothing 'miraculous' here, as Wigner (1960) alleged. It is just a matter of interpretation: some interpretations could be entirely unproductive, some other interpretations could be very fruitful; but whether it is the first or the second case that occurs, it is not (only) a matter of which mathematical structure we adopt, but also of whether the interpretation that we stick to the mathematical structure passes the empirical test or not.[22]

The way in which we set up our interpretation is quite complex and involves different aspects. The basis is obviously given by the initial denotation on

[21] The story also says that, by thinking about this problem, Paul Dirac came to the idea of the anti-matter. Such a story is quoted also by Barrows, but I was not able to check its reliability.

[22] As it has been often noted, Wigner's analysis seems to have been led astray by an overstimation of successful over unsuccesful cases of mathematical application in physics (see for example Azzouni 2000, Pincock 2012). A closer attention to unsuccesful cases would have probably pointed out that the apparent 'miracle' of applied mathematics is often just the result of a long chains of failure in finding the proper interpretation for the suitable mathematical structure.

which we have built up our data model. As far as the terms, for which we have given a denotation, are concerned, the interpretation is simply the inverse of the denotation. But then we have to extend this initial, partial denotation. In general, empirical verification is the main judge for interpretation; so, there are no tight prescriptions to be rigorously followed.[23] However, we usually extend the interpretation in conformity with a general principle of 'coherence'. It is hard to precisely clarify what this principle is; perhaps an example can be more helpful. Let us suppose we denote the velocity of a body along a certain direction by means of a vector v_1. If, very trivially, this vector is then transformed into a different vector v_2 in virtue of a certain relation describing the kinematics of that body in the system at issue, we will coherently interpret this new vector as the velocity of that body at a later instant of time. We will not interpret it as, for example, the mass, or the acceleration of the body, since this interpretation would be incoherent with the original one. A less trivial and more interesting example of the 'principle of coherence' is offered by the already mentioned case of the discovery of the omega minus particle — which I will extensively deal with in the next chapter. Here we have that each particle belonging to a particular class (the spin-$\frac{3}{2}$ barions), along with its properties, is represented by means of a position in the $S - I3$ (strangeness-isospin) plane (according to the $SU(3)$ formalism). Given that the nine already known (in 1962) spin-$\frac{3}{2}$ barions determined that this class of particles would have formed a decuplet scheme in such a plane, a tenth position was still vacant. So, since all the other "positions" in the scheme represented a certain particle of the class, a general principle of coherence led Gell-Mann and Ne'emann to think that this position represented a particle too — a new particle still to be discovered and

[23]In some cases, the theory model can refer to the past. In these cases it would be impossible to *empirically* test the 'predictions' of such a theory, because the initial conditions cannot be recreated at the present time. The evaluation of this question is quite complex and cannot be examined in the present work. I would say that in these cases, all we can do is to rely on an inference to the best explanation, or hope that some other experiment could offer an *indirect* confirmation of the theory.

whose existence was experimentally certified one year later.

Moreover, the extension of the original interpretation will have to be made so as to mantain a certain coherence also with our experience. In the previous coconuts case, for example, experience suggests us that it is meaningless to extend the interpretation for negative numbers, because in our empirical experience we never meet with a set of -4 objects. However, in some cases, what we need for advancement is just the breaking of such an apparent coherence — and the more we go far from our immediate experience, the less we will have difficulty to 'force' this coherence in order to explore new possibilities. This is particularly evident in Quantum Mechanics.

It is important to note that the non-existence of coherent interpretations for some statements about a mathematical structure does not invalidate the whole structure *as a suitable representation*. We can abandon a mathematical structure for several reasons. We can do it because the mathematical structure is too complicated and we are not able to handle it; or, we can abandon it because it is not complicated enough and we are not able to draw interesting predictions from it; or, we can abandon it because, at a certain point, we realize that a relevant fact or a relevant property of the phenomenon at issue is not (properly) represented by it. However, the fact that a mathematical structure, in a sense, represents more than what it is asked to represent is not a real problem; potentially, it is rather a source of *richness* and *novelty* which we should keep into consideration. If our mathematical structure makes a prediction which is not verified, we can try to shuffle off the guilt of such a failure upon the interpretation. If we can do it in a coherent way, we can continue working with that mathematical structure; but if we cannot do it, then we have to revise our mathematical structure and find a more suitable one.

5.4.2 How do we detect the monomorphism?

There is still a question I left open: the question concerning how we understand that a mathematical structure is monomorphic to a data model. Indeed, if we *do not* know the structure that the data model partially represents, how can we say that such a structure is actually monomorphic to the mathematical structure under scrutiny? We know the mathematical structure, but we do not know the structure partially revealed by the data model (which, in our intentions, should represent the phenomenon). In some cases we know in advance that the mathematical structure we are going to adopt is richer than the phenomenon we aim to represent, and we test the mathematical structure assuming that the data model is at least monomorphic to it. This is the case, for example, of the Navier-Stokes equations, where we know in advance that the continuous mathematics we are going to apply is richer than the ultimate discrete dynamic of the phenomenon at issue. In some cases, yet, we have no idea about it. So, what can we do?

In these cases, it seems to me, there is no alternative but to *suppose* that the mathematical structure we want to adopt is actually *isomorphic* to the data model structure. If this hypothesis were true, then it would be true that — to repeat Hertz's words — «the necessary consequents of the images in thought are always the images of the necessary consequents of the things pictured». In other words, if this hypothesis were true, we could be sure that any consequence in the mathematical structure has some correspondence in the data model structure, and hence that it represents a fact in the phenomenon. So, if we run into a difficulty (for example, when my mathematical structure tells me that a possible solution for the coconut puzzle is -4), we can shuffle off the guilt of such a failure upon the isomorphism hypothesis: the solution suggested by the mathematical structure does not take place in the real world because our mathematical structure is too rich, and the data model is only monomorphic (not isomorphic) to it. We do not have to abandon the mathematical structure, since we can simply abandon the hypothesis of isomorphism. We have thus a proof that the mathematical structure is not isomorphic to the data model, but in spite of this we can still rely on the hypothesis that the data model is monomorphic to our mathematical structure — and hence, on the hypothesis that the mathematical structure can still be representatively effective. Of course, not all the difficulties can be settled in this way. In the coconut example, we can proceed in this way because the mathematical structure gives us also a *valid* solution (i.e. 15,621). But if the only solution given by our mathematical structure were not valid, then we could not shuffle off the guilt upon the hypothesis of isomorphism, but directly upon the mathematical structure itself. In that case the mathematical structure would not be suitable, and we would have to find a more suitable one.

The following diagram, showing the possible working flow of a scientist engaged in representing a physical domain by means of a mathematical structure, should help in clarifying the previous considerations. At the beginning of the process, the scientist has a mathematical structure M that she thinks is a good candidate for representation, but she does not know whether it *perfectly* (namely, isomorphically) represents the data model or not. So, she initially supposes that the data model is *iso*morphic to the mathematical structure.[24] This could turn out to be a good hypothesis and then she can go on working with such an assumption. But such a hypothesis may bring to a failure. In that case, depending on the kind of failure at issue, she can shift to a different assumption, i.e. that the data model is rather *mono*morphic to the mathematical structure; otherwise she can shift to a different interpretation; or she can shift to a completely different mathematical structure to work with. Also the monomorphism

[24] I am not saying that this hypothesis is *always* necessary. In some cases she already knows that M does not perfectly represent the phenomenon, and then the isomorphism hypothesis need not to be assumed, of course.

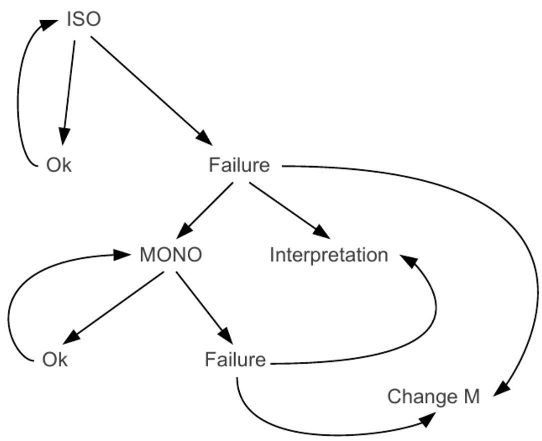

Figure 5.2: Working flow of a scientist engaged in representing a physical domain by means of a mathematical structure.

hypothesis can turn out either satisfying or not. Again, just like before, depending on the kind of failure, she can attempt to shift to a different interpretation or to a different mathematical structure. In different words, she makes a strong hypothesis (isomorphism) and if this hypothesis turns out to be too strong, she tries to come to a compromise, by weakening the hypothesis, by modifying the interpretation, or by shifting to a different mathematical structure.

5.5 Conclusions

In this chapter, I presented the structural account for the representative effectiveness of mathematics and I took into consideration some difficulties related to it. Part of these difficulties can be settled, as we saw; but some criticalities remain. This shows that the structural account needs to be integrated in order to account for representative applicability, especially where mathematical representations seem to play an active role in making new discoveries and to foster new advancements in scientific research. I tried to provide the required integration, by considering the problems and the deficiencies emerged by means of the DDI model proposed by Hughes's (1997) and van Fraassen's considerations.

It seems to me that the resulting account is in a better condition to make the applicability of mathematics less misterious and miraculous than it is often presented. In this integrated account, a major role is played by a certain number of pragmatical and contextual elements. Such a role is not always schematizable in a rigorous manner, but I think this is not an imperfection. Rather, I think that all this bestows upon the account an amount of dynamism and plasticity, which is good for understanding, for example, the evolution of different theories and theory changes.

Chapter 6

Some concrete examples

6.1 Introduction

In the previous chapter I presented a proposal of integration for the structural account of mathematical representative effectiveness. I highlighted some limits of the structural account as it is usually presented and I offered some arguments to show how to improve it and make it more effective. However, I considered the question only from a purely theoretical point of view. In the present chapter, I am going to introduce some concrete examples by means of which I will show the advantages that follow by adopting such an approach.

Since one of the main aims of an account for the effectiveness of mathematics consists — as I underlined in the previous chapter — in accounting *both* for representative *and* heuristic role of mathematics in scientific discoveries, I will focus my attention on cases that are usually presented as interesting examples of the mathematical heuristic potential in physics. The two main examples which I will discuss are:

1. the prediction of the existence of the omega minus particle by Gell-Mann and Ne'eman in 1962; and

2. the prediction of the existence of the positron by Paul Dirac in 1930.

Both these examples are mentioned by Steiner (1998) as examples of formal analogies in science. In his reconstruction, the heuristic potential of these existential predictions rested on the anthropocentric character of mathematics and on the 'human-friendly' feature of the universe. The reconstruction I will offer in the following pages will show that such an effectiveness can be better explained and accounted by means of the account previously illustrated.

6.2 The prediction of the omega minus particle

What is interesting in the discovery of the Ω^- particle is the fact that the prediction of this new physical entity seemed to be motivated *only* by the mathematics employed (the theory of irreducible group representations) and by purely mathematical considerations. Apparently, no empirical fact or consideration justified the belief that such a particle should be included in the number of the particles already known at that time.

The case of the prediction of the omega minus particle is only touched on by Steiner (1998). He just mentions it as an example of analogy reasoning in physics, but he does not enter into the details of the discovery. A more detailed analysis is offered by Bangu, first in his (2008) article and then in his (2012) book. Here, he offers a detailed historical and theoretical reconstruction of this discovery, and, in the spirit of Steiner's work, makes a case for naturalistic philosophers. In a clearer way than Steiner's, he shows that the prediction of the omega minus seems to rely on a methodological principle — what he calls "reification principle"[1] — which is not justifiable by naturalistic criteria.

In this section I will take into question Bangu's reconstruction and I will

[1]In (Bangu 2012) the principle is called "identity principle", but the shift of terminology does not change the substance of his argument. Since no argument is offered to motivate the latter terminology over the first, I will refer to this principle by means of the first terminology, since I think is less unambiguous.

show that, by relying on the account I have previously presented, a different reconstruction can be offered, without any commitment to such a reification principle.

6.2.1 Brief historical outline

The introduction in quantum mechanics of the mathematical framework of irreducible group representations dates from around 1930, and was mostly due to the works of Weyl and Wigner.[2] Indeed it was initially not well accepted by the physicists' community, because of the difficulties in learning and manipulating such a complex mathematical formalism,[3] but after the early successes, physics began to systematically use internal symmetries in the classification of elementary particles. It was just this mathematical framework that gave Gell-Mann and Ne'eman the proper mathematical tools for accomplishing their prediction.

In general terms, following Wigner (1939), an elementary particle is identified with a physical system whose possible states transform into each other according to some *representation* of the appropriate symmetry group. For example, the proton-neutron symmetry was originally captured in terms of the group structure called SU(2). Subsequently it became evident that the strong interactions are governed by a bigger symmetry: when the conservation of strangeness was recognized to be a characteristic of strong interactions and a new quantum number was added to that of isospin, the new symmetry governing these interactions turned out to be SU(3).

[2]For more details, see (Weyl 1931, Wigner 1926, Wigner 1927, Wigner 1939, Wigner 1959).

[3]Althought this was the principal reason, it was not the only. For example, Max Born said to Ehrenfest in September 29, 1930: «I find group theory a very beautiful mathematical tool but its application in atomic physics always seemed to me to be inappropriate (like shooting sparrows with artillery)» (von Meyenn 1983, p. 341). His objection was not because group theory was not easy to use, but because «in reality it is not in accord with the way things are» (Gavroglu 1995, p. 56). As Bonolis points out: «He probably meant that from the fact that certain mathematical tools (such as group-theoretic techniques) are useful for solving a given problem (for example to establish convenient representation theorems in Quantum Mechanics), it does not necessarily follow that one has to believe that the structures generated by these techniques truly describe the world» (Bonolis 2004, pp. 78-79).

Given the symmetry group governing a physical system, the superposed states of the system transform into each other according to the irreducible representation of the symmetry group. These physical transformations are expressed mathematically as operators on the state space corresponding to the observables and these operators are conceived as invariants of the symmetry group in question: their eigenvalues supply the labels for identifying and classifying the irreducible representations of the appropriate symmetry groups.

The discovery of the omega minus particle was based on a very simple strategy, just a particular case of the general suggestion given by Dirac:

> The most powerful method of advance that can be suggested at present is to employ all the resources of pure mathematics in attempts to perfect and generalize the mathematical formalism that forms the existing basis of theoretical physics, and *after* each success in this direction, to try to interpret the new mathematical features in terms of physical entities. (Dirac 1931)[4]

The complete history is obviously full of technicalities and is not easy to be followed through, but we do not need to get into these technicalities in order to understand the idea behind the discovery of the omega minus particle. Actually, as Bangu glosses,

> the main idea is rather strightforward: given the classification scheme for the already known spin-$\frac{3}{2}$ baryons, the unoccupied, apparently superfluous entry in the scheme was taken as a guide to the existence of a new particle. It was exactly this surplus that suggested the existence of a new physical reality (in the form of new particles, to fill in gaps in multiplets). (Bangu 2008, p. 243).

6.2.2 Bangu's reconstruction

Bangu's reconstruction of the Gell-Mann and Ne'eman predictive reasoning (hereafter, GMNPR) is based on the detailed account given by Ne'eman. Here is the whole passage to which Bangu referred:

[4] Quoted by Bonolis (2004, p. 50).

In 1961 four baryons of spin $\frac{3}{2}$ were known. These were the four resonances Δ^-, Δ^0, Δ^+, Δ^{++} which had been discovered by Fermi in 1952. It was not clear that they could not be fitted into an octet, and the eightfold way predicted that they were part of a decuplet or of a family of 27 particles. A decuplet would form a triangle in the $S-I3$ [strangeness-isospin] plane, while the 27 particles would be arranged in a large hexagon. (According to the formalism of SU(3), supermultiplets of 1, 8, 10 and 27 particles were allowed.) In the same year (1961) the three resonances $\Sigma(1385)$ were discovered, with strangeness -1 and probable spin $\frac{3}{2}$, which could fit well either into the decuplet or the 27-member family.

At a conference of particle physics held at CERN, Geneva, in 1962, two new resonances were reported, with strangeness -2, and the electric charge -1 and 0 (today known as the $\Xi(1530)$). They fitted well into the third course of both schemes (and could thus be predicted to have spin $\frac{3}{2}$). On the other hand, Gerson and Shoulamit Goldhaber reported a 'failure': in collisions of K^+ or K^0 with protons and neutrons, one did not find resonances. Such resonances would indeed be expected if the family had 27 members. The creators of the eightfold way, who attended the conference, felt that this failure clearly pointed out that the solution lay in the decuplet. They saw the pyramid [in fig. 6.1] being completed before their very eyes. Only the apex was missing, and with the aid of the model they had conceived, it was possible to describe exactly what the properties of the missing particle should be! Before the conclusion of the conference Gell-Mann went up to the blackboard and spelled out the anticipated characteristics of the missing particle, which he called 'omega minus' (because of its negative charge and because omega is the last letter of the Greek alphabet). He also advised the experimentalists to look for that particle in their accelerators. Yuval Ne'eman had spoken in a similar vein to the Goldhabers the previous evening and had presented them in a written form with an explanation of the theory and the prediction. (Ne'eman & Kirsh 1996, pp. 202-203)

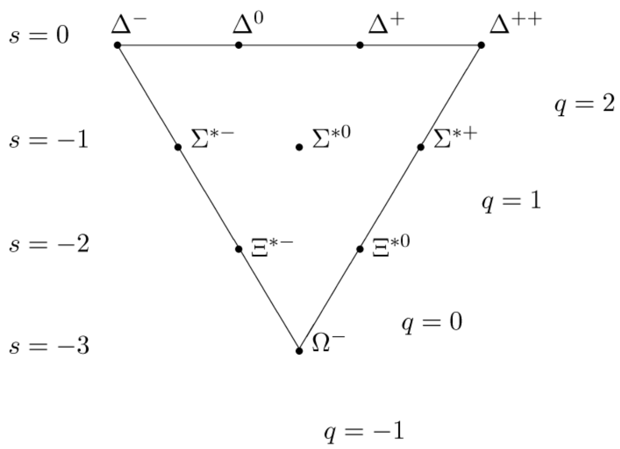

Figure 6.1: spin-$\frac{3}{2}$ baryon decuplet.
Credits: http://math.ucr.edu/home/baez/diary/march_2007.html.

Actually, when few years later, in 1964, experimentalist physicists looked for the omega minus particle in their accelerators, they found out exactly what Gell-Mann and Ne'eman predicted: the particle exists and it has exactly the predicted characteristics (see fig. 6.2).[5]

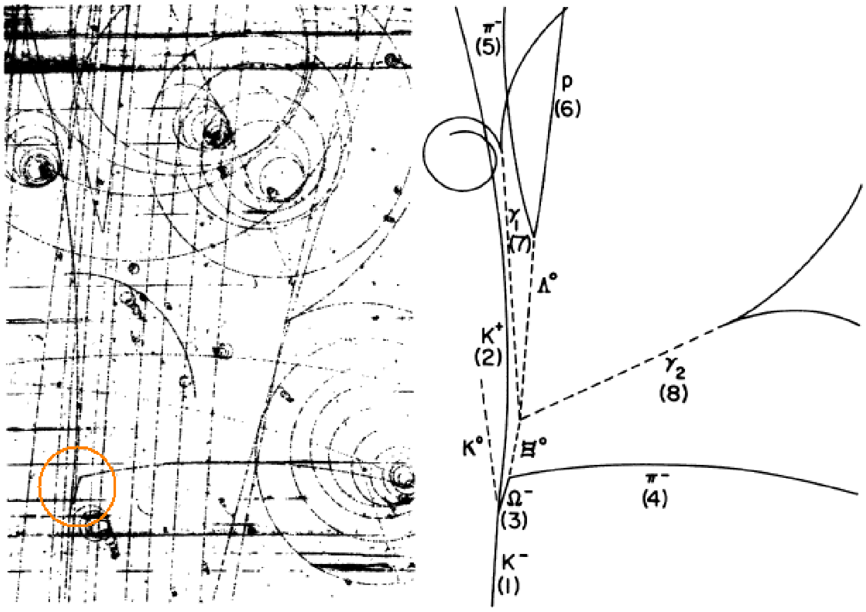

Figure 6.2: Photograph (left side) and line diagram (right side) of the decay of an omega minus particle in a bubble chamber. The short track of the omega minus particle is highlighted by the circle in the low left corner.
Credits: Barnes et al. (1964) (Brookhaven National Laboratories).

Now, one might ask: why did the experimentalist physicists trusted Gell-Mann and Ne'eman's prediction? On which ground they carried on the detection of the new particle, and why did they think that the (supposed) new entity would have had exactly the same characteristics as guessed by Gell-Mann and Ne'eman?

Here are the logical steps that, according to Bangu, underpin the previous historical report:[6]

[5] Actually, the story is not so simple. They could prove the existence of the omega minus particle, along with its characteristics, *except* for its spin. Although this hyperon was discovered more than 40 years ago, a conclusive measurement of its spin has only recently been obtained by Aubert et al. (2006).

[6] See (Bangu 2008, pp. 243-248).

(P1) – *Each of the upper nine positions in the symmetry scheme has a physical interpretation.*

(H) – *Spin-$\frac{3}{2}$ baryons fit the symmetry scheme.*

(P2) – *The apex is formally/mathematically similar to the other nine positions. (It is similar in so far as it is, like them, an element of the scheme).*

(P3) – *The physical existence of a baryon having the predicted characteristics is not forbidden (can occur in nature).*

(RP) – *If Γ and Γ' are elements of the mathematical formalism describing a physical context, and Γ' is formally similar to Γ, then, if Γ has a physical referent, Γ' has a physical referent as well.*

(C) – *The apex position has a physical interpretation. (That is, the coordinates of this position describe a 10th spin-$\frac{3}{2}$ baryon.)*

Obviously, the truth of C entails the confirmation of H. «This line of reasoning — Bangu glosses — is supposed to answer the question asked by the experimentalist physicist ready to perform the detections, namely 'What are the grounds to believe that (a) there is an entity (b) having the predicted physical characteristics?'». (Bangu 2008, p. 245)

GMNPR and the DN model

The need to introduce RP is not *prima facie* evident and in order to justify its employment Bangu compares the omega minus prediction with other two cases of 'standard' prediction (standard in respect to the DN model)[7]: the Urbain Leverrier and John Adam's prediction of the planet Neptune and Wolfgang Pauli's prediction of the neutrino. In both these cases, the existential predictions are accounted for within this model with no particular difficulties: when accurate measurements reveal that there is an anomaly in a physical system (which we

[7]According to the DN model (see, for example, Hempel 1965), to explain a scientific fact is to show how that fact can be derived from (i) a set of laws of nature and (ii) some initial conditions — which is the same as to predict it by applying the laws to the appropriate circumstances.

previously supposed to be properly described by a set of laws), then we have two possibilities to explain away this anomaly: either (A) we try to correct the laws, keeping the initial conditions unchanged; or (B) we keep the laws unchanged and we hypothesize that the initial conditions are different from what we have previously supposed to be the case. It appears clear that (A) is a very costly option to follow. So, if following (B) we are lead to suppose that something there should exist that was not previously accounted for, we make a prediction and then we test it empirically.

In the two standard cases the new entity is postulated for *explanatory* reasons: the neutrino and the planet Neptune were postulated in order to explain away a physical anomaly. Moreover, the physical characteristics of the postulated entity are inferred from an analysis of the physical *interactions* between the hypothetical entity and the rest of the system. For example, the characteristics of Neptune (mass, position, velocity, etc) were inferred from the analysis of how the new entity should be in order to explain (away) the anomaly. Thus, the criteria on which Bangu bases his comparison are the following two: (a) *explanation* and (b) *interaction*.

(a) *Explanation*: In order to say that the omega minus particle prediction was made for explanatory reasons, Bangu has to show what the anomalous phenomenon to be explained away is. His answer is:

> The expectation that another spin-$\frac{3}{2}$ baryon exists was fostered by the perceived regularity mentioned in the P1 premise: the fact that nine other similar positions (out of 10!) had a physical referent was regarded, naturally, as a (striking) coincidence, a regularity in need of explanation. And, as usual, the best explanation for it could be that, in fact, the law-like generalization H: 'Spin-$\frac{3}{2}$ baryons fit the symmetry scheme' is a true law of nature. The idea then is to attempt to account for/explain the 'nine-out-of-ten' regularity by saying that it is a nomological property of spin-$\frac{3}{2}$ baryons in general to fit the symmetry; the next step would be to somehow use this property to infer that another spin-$\frac{3}{2}$ baryon exists. The basic insight is thus quite simple: by assuming the law-like generalization H, it would be an anomaly *not* to have a referent for the apex position. (Bangu 2008, p. 247)

The anomaly to be explained away is, in this case, the presence of an empty place in the SU(3) scheme. So far, there is no difference between the case under examination and the two standard cases of existential prediction.

(b) *Interaction*: It is here that RP comes in. The properties of Ω^- are obtained on the basis of the analogy between the elements of the formalism (by means of P1 and, mainly, P2). But to conclude C from P1 and P2 — Bangu argues — we need RP. More precisely, RP is indispensable in "spelling out" the properties of Ω^- from the formalism: «[...] Gell-Mann did just that, indicating that the new baryon should have the characteristics arrived at via Γ', the unoccupied spot in the scheme. Thus, this principle plays the crucial role of giving precise indication about the characteristics of the new particle». (Bangu 2008, p. 248)

The crucial role played by RP is just what makes the omega minus different from the other typical DN predictions. The point at issue is that its physical characteristics «[are] *not* based on the calculation of the parameters of its interactions with other elements of the system. These characteristics are postulated *directly* from the formalism, by relying on RP». (Bangu 2008, p. 249)

Bangu's conclusions

Bangu's article continues by making some remarks about what we have said above. First of all, RP cannot be assumed as a generalized methodological principle. Although the omega minus particle case seems to suggest that a novel, non-standard, pluralistic stance of methodology should be taken (and in spite of Mark Steiner's advocating of a sort of "Pythagorization" of the concept of prediction),[8] Bangu claims that this proposal may not avoid a number of problems — above all, the fact that there is no naturalistic explanation of why RP could work. For there are situations in physics in which RP does not work,

[8] See (Steiner 1998, p. 162).

so that we are not justified in taking RP as a valid methodological principle.

Secondly, he takes into consideration, and rejects, three possible attempts that a standard empiricist/naturalist methodologist could make to solve the problem raised in connection to the DN model.

1. The first consists in saying that RP «served in fact a heuristic role – to conjure, or invent a hypothesis, not to prove it. Unlike the physicist's justification method, his discovery method is not even supposed to make the object of critical inquiry» (Bangu 2008, p. 251). In this way, the methodological unreliability of RP does not have any effect on the scientific respectability of the GMNPR. Bangu's reply is that the case at issue does not fall under the scheme presupposed by the traditional distinction between the "context of discovery" and the "context of justification". In particular, «A heuristics typically helps us discover the *equation* of X, not X itself — getting to X itself is possible only via a very problematic reification step» (Bangu 2008, p. 252). Moreover, since the prediction/discovery of the new baryon played a justificatory role (for it established that the SU(3) scheme is the correct symmetry) and since this prediction relied on using RP, it follows that RP played rather a justifiatory role.[9]

2. The second attempt to solve the problem consists in emphisizing the role of P3 in the GMNPR, to the detriment of RP — namely, to emphasize the "physical reasons" underlying the prediction to the detriment of the formal ones. Bangu replies by noticing that P3 has in fact *no* role in the GMNPR.

   > [...] a carefull look at the GMNPR scheme — *Bangu writes* — reveals that this premise was only a consistency check upon the existential conclusion (C), and not part of its derivation — the derivation consisting of course of P1, P2, H and RP. [...], (P3) plays only a secondary role: it is the final check upon the credibility of the predictive

[9] See (Bangu 2008, p. 253).

> existential conclusion already advanced. More precisely, (P3) has *no* role in drawning the existential conclusion per se; it only became relevant in a weak, secondary, or passive sense, in accepting it *after* the inferential existential step was taken. (Bangu 2008, p. 254)

3. The third attempt consists in observing that the crucial reificatory step (namely, the indication of the new particle's characteristics) was taken by interpreting the mathematical formalism: it is not the formalism itself that predicts, but our interpretation of it. However, Bangu noticed that the notion of interpretation at work in the omega minus case is a non-standard and more problematic notion of interpretation:

> Gell-Mann did interpret the formalism, but not in the standard sense [...]. Indeed, as it should be clear by now, his interpretation proceeded (consciously) along pythagorean lines and was brought to a specific conclusion by using the (RP). And, again, it is the physicists' employment of this principle that stirred up the empiricist methodologist's concerns. (Bangu 2008, p. 255)

Therefore, we must conclude that the DN model is not able to account for the omega minus prediction.

Having certified that neither the standard DN model accounts for the omega minus prediction, nor could a "Pythagorean" model be used as a substitute for it, Bangu suggests that a third, "weaker" approach could be taken into consideration: so called "methodological opportunism". Since the problem dealt with until now concerns more the scientist-qua-methodologist than the working scientist, Bangu — following a suggestion by Einstein[10] — claims that the working scientist should not be inhibited by epistemological considerations:

> This kind of puzzle, the scientist-qua-methodological opportunist stresses, should not be allowed to *interfere* with the practice of science, as no attempt to offer a principled, systematic account (either standard or non-standard) of those episodes should be followed too far or taken too seriously. (Bangu 2008, p. 256)

[10]See (Einstein 1949, p. 684): «[The scientist] accepts gratefully the epistemological conceptual analysis; but external conditions, which are set for him by the facts of experience, do not permit him to let himself be too much restricted in the construction of his conceptual world by the adherence to an epistemological system. He therefore must appear to the systematic epistemologist as a type of unscrupulous opportunist».

In his (2012) book, however, Bangu seems to be no more interested in this methodological considerations. Rather, he accepts the shocking strangeness of the reification principle (or "identity principle", as he now prefers to call it) and levers on it to propose a new version of the indispensability argument.

6.2.3 Avoiding reification: a different reconstruction of the GMNPR

The difficulties raised by Bangu have their roots in the employment of RP in order to obtain the conclusion C. In this section I advance another reading of the GMNPR argument by appealing to the considerations I spelt out in chapter 5. Such a new reading has the merit of totally dispensing with the recourse to RP, and consequently with the difficulties connected to it.

Where is the anomaly?

Before of presenting my reconstruction, I want to take into consideration an aspect on which Bangu seems to be confused. He seems to be persuaded that in the history of the omega minus prediction, as reported by Ne'eman and Kirsh, there is an "anomaly" to be explained away. In this regard, he says, there would be no difference between the omega minus case and the other standard cases of existential prediction. However, when he tries to pindown this anomaly, he says that it consists in the fact that *if* no resonance (having the predicted characteristics) were observed, *then* there would be an empty place in the symmetry scheme: «by assuming the law-like generalization H, it *would* be an anomaly not to have a referent for the apex position». (Bangu 2008, p. 247)[11]

Actually, he is saying that there isn't any anomaly yet: there *would* be only if no resonance were observed (or if a resonance without the predicted characteristics were observed). Bangu evokes a ghost anomaly as if it were

[11] Italics mine.

real, but in effect we do not have to do here with an *actual* anomaly. In the standard cases, the anomaly is revealed by the measurements (it is *actual* in this precise sense) and its presence looms over the theory like the sword of Damocles: until the anomaly is removed, the ability of the theory to account for the phenomena at issue is seriously jeopardized. On the contrary, in this case there is no empirical observation that jeopardizes our law-like generalization H. Thus, we could say, the prediction of the omega minus particle seems to be made in order to *prevent* an anomaly, rather than to *explain it away.* Later on, in his (2012) book, Bangu seems to realize that the alleged anomaly is just a 'ghost' anomaly; so, he precises the point by saying that the omega minus prediction has been made in order «to deal with (eliminate, prevent) an anomaly» (Bangu 2012, p. 102).

However, there is another aspect we should consider. The anomaly that we should prevent may assume two different forms: either (A) we could find no particle at all; or (B) we could find a particle not having the predicted characteristics. These two cases must be carefully distinguished. In case (B) we would have a real anomaly, since the measurements cannot be accounted for by our theory. In case (A), instead, the anomaly seems to consist simply in the fact that the symmetry scheme could turn out to have an empty place. But if this were the case, would it be really an anomaly?

The presence of an empty place in the symmetry scheme (i.e., of a place in the formalism having no physical referent) can be regarded as an anomaly *only if* we presuppose the validity of RP. The situation appears to be very odd: the anomaly exists *only if the principle that should explain away the anomaly is valid.* Namely, the anomaly can be identified as such *only in the light of RP.*

Moreover, if (A) were the case, would we be compelled to drop out our mathematical structure as no longer reliable? Namely, suppose that experimentalist physicists had not found any new particle corresponding to the characteristics pointed out. Should we drop out the SU(3) symmetry scheme? We wouldn't, of course. It could still be regarded as a valuable tool for *representing* the class of spin-$\frac{3}{2}$ baryons, for it continues to account for what happens in physical reality.

The fact that the formalism seems to commit ourselves to the existence of an entity that does not exist cannot be regarded as a wrong conclusion. There are many cases in which some formalism commits us to entities that we do not regard as actually existing, but still we continue to use those formalisms without worrying about these "fictional" entities. We have already seen cases like this. Remember, for example, the case of the applicability of analytic funtions to termodynamic discussed in 3.1.3: we know we can treat the critical temperature of a ferromagnet as an analytic function of the number of its dimensions. But, following the formalism, we should admit magnets of dimension 3.5, or even $2 + 3i$. This is patently meaningless. Analytic function seems rather to be a calculational tool. Since we cannot calculate the problem for the 3-dimensional magnet, we calculate it for a 4-dimensional magnet, then we expand the function as a power series in a complex plane around the number 4, and finally we plug in the value 3.

In my account, this can be perfectly treated. If (A) were the case, then we can simply admit that the mathematical structure does not isomorphically fit the phenomenon we are trying to represent. This is perfectly sound, and we do not drop out the formalism, since the phenomenon countinues being *mono*morphic to the mathematical structure and hence the latter continues being *representatively* effective.

Despite these considerations, Bangu thinks of (A), as well as of (B), as an anomaly. But if I am right in claiming that there is no anomaly here to be explained away, then we should admit that the standard cases of existential prediction and the omega minus case are different in so far as the latter *does not* serve an explanatory purpose. If there is no anomaly to be explained away, all that Bangu says about the relations between the case of the omega minus prediction and the standard DN model is cut out at the root: the case of the omega minus prediction cannot be accounted for in the same way as we can accout for the prediction of Neptune.

One may ask: If the omega minus prediction was not made in order to explain away an anomaly, why did physicists actually make that prediction? My answer is: they simply suggested a hypothesis, as it often happens in science, and the motivation for suggesting such a hypothesis was not *entirely* based on mathematical formalism.

A different reconstruction of the GMNPR: outline

Let me first present my proposal for a different reconstruction of the GMNPR, and then I will offer some clarifications about it. My reconstruction is articulated as follows.[12]

> (P1*) – *The nine spin-$\frac{3}{2}$ baryons are suitably represented by the symmetry scheme.*

From this premise, by generalization, we can obtain the following hypothesis:

> (H*) – *The symmetry scheme* suitably represents *the* whole *class of spin-$\frac{3}{2}$ baryons (be it completed by the nine baryons already known or not).*

Now, we notice the following mathematical observation:

> (MO) – *The symmetry scheme reveals another possible element (another state vector); namely, the symmetry scheme says that another* ideal *(mathematical) object exists, belonging to the set of objects described by the scheme.*[13]

[12] The asterisks mark those statements which are reformulations of Bangu's statements named by the same abbreviation. The Hs are hypotheses.

[13] "MO" stands for "Mathematical Observation".

Then, we 'force' hypothesis H^* and we formulate the following, stronger hypothesis:

> (H_A) – *The symmetry scheme is (not only suitable, but also) not "redundant", where by "redundant" I mean that the mathematical structure is not only monomorphic, but also* isomorphic *to the physical system.*

Hypothesis H_A is stronger than H^* in the very sense that H_A implies H^*, but the converse does not hold. If hypothesis H_A is assumed to be true, then from H_A and MO follows that:

> (H_B) – *The 'empty place' in the mathematical structure* must *have a physical interpretation, and the most coherent interpretation for it is that it represents a tenth spin-$\frac{3}{2}$ baryon. Also its characteristics are interpreted coherently with the characteristics of the other nine spin-$\frac{3}{2}$ baryons.*

Then, we consider the following premise:,

> ($P3^*$) – *The existence of a tenth baryon (having the predicted characteristics) is possible (*prima facie*). Namely, there is no* a priori *reason to exclude that H_B is true.*

Finally, from H_B together with the premise $P3^*$, the following conclusion follows, which still has a hypothetical character:

> (H_C) – *There exists a tenth baryon having the predicted characteristics.*

The argument starts from a reasonable hypothesis (H^*) and finishes with another hypothesis (H_C). Along its development, however, the argument makes use of other intermediate hypothesis (H_A and H_B). Thus, the reasonableness of H_C depends over the reasonableness of H_B and H_C. In particular, we have to show that none of these assumptions conceals RP among its wrinkles. Let us review the argument and linger on every single step, in order to clarify it and make some remarks.

A different reconstruction of the GMNPR: remarks

Premise P1* is a reformulation of Bangu's P1 "Each of the upper nine positions in the symmetry scheme has a physical interpretation". The difference is substantial. Bangu's formulation is in terms of "physical interpretations" of the symmetry scheme; my formulation, instead, is in terms of "representative effectiveness" of the symmetry scheme for the nine baryons already detected. However, the two formulations are equivalent: if each of the upper nine positions in the symmetry scheme has a physical interpretation (and we are satisfied by this interpretation), then we can consider the symmetry scheme as a good representation *at least* for those nine baryons; conversely, if the symmetry scheme is a good representation for at least those nine baryons, then we must have an interpretation from elements in the formalism to the nine baryons.

Premise P1* reproduces more closely the physicists' perspective: when they deal with a mathematical structure, they do not look at it as an interpreted structure, but rather as a representation, whose representative effectiveness they are interested in evaluating. However, this may be seen just as a 'stylistic' preference and it is not clear why one should prefer P1* over P1. The real advantage of adopting P1* instead of P1 consists rather in the fact that P1*, unlike P1, permits the generalization H*. This point is very important. Also P1 can be generalized, but the resulting generalization would be very different from H*. It would appear as follows:

> (H**) – *Each position in the symmetry scheme has a physical interpretation.*

H** is *much* stronger than H*, to the extent that it is very close (if not equivalent) to RP. Actually, H** already suggests the existence of a tenth $\frac{3}{2}$-spin baryon, and such a generalization would be completely unmotivated. Hypothesis H*, on the contrary, is perfectly reasonable. It is just an induction over what observed up to now: the symmetry scheme adopted has alredy revealed itself effective in representing the nine known baryons, so why do not we guess that it is effective in representing the whole class of $\frac{3}{2}$-spin baryons too? This hypothesis does not suggest, in any sense, that there must be hidden baryons still to be discovered, neither it suggests that we should look for such hidden baryons. After all, the nine baryons we already know might be the *whole* class of $\frac{3}{2}$-spin baryons. If nothing has to be added to this class, then H^* is justified *a fortiori*. If, on the contrary, something else has to be added to this class, then we will see whether this hypothesis will turn out to be right or not. But up to now there is nothing that prevents us from making such a hypothesis. Actually, hypothesis H^* *had already been made* by physicists when they first proposed such a symmetry scheme to classify this particular kind of particles: they proposed it *just because* they thought that it could be a good candidate to represent the class of $\frac{3}{2}$-spin baryons.

Remember that, when I say that the symmetry scheme represents the whole class of the $\frac{3}{2}$-spin baryons, I am not saying that it is *isomorphic* to it. In the previous chapter we saw that a mathematical structure M can be said to represent a physical system S if and only if S is *monomorphic* to M, in the sense that we previously precised. M might even be *isomorphic* to S, but this is not necessary. When M is not isomorphic to S, we can say that M is 'redundant', meaning that the domain of M is richer than S; namely, there are elements in M that are not to be interpreted over S. Thus, in making hypothesis H^*, we are just guessing that the class of $\frac{3}{2}$-spin baryons is monomorphic to the symmetry scheme, leaving undecided whether it is also isomorphic or not. As I precised in section 6.2.3, hypothesis H^* would turn out to be false only if a new $\frac{3}{2}$-spin baryon were discovered *having not* the characteristics predicted by the mathematical structure (case B in section 6.2.3). If no new $\frac{3}{2}$-spin baryon were discovered, then there would be no need to drop out the mathematical structure adopted, but we would only admit that the mathematical formalism is, in this case, suitable in representing the class of $\frac{3}{2}$-spin baryons *but redundant*.

However, we notice that, if we assume that a mathematical structure M represents a physical system S, then only two alternatives are possible: generally speaking, either the mathematical structure M is redundant; or it is not, in which case it is isomorphic to S. In section 5.4.2, I have also poined out that if we do not know whether a mathematical structure is redundant or not, we have only one possibility: to force the original hypothesis in a stronger one and try to verify whether M is isomorphic to S or not. In our case, we already have good reasons to believe H^*, namely that our symmetry scheme is a good representation for the whole class of $\frac{3}{2}$-spin baryons. But this does not imply neither that the mathematical structure at issue is isomorphic, nor that it is monomorphic-but-not-isomorphic. This case is similar to the case discussed in section 5.4.2, and then we can do what I suggested in that chapter;[14] namely, to pass from hypothesis H^* to hypothesis H_A, which is stronger than the former.

Now, one might object: Why are we compelled to such a stronger hypothesis? Is not hiding here the so much vituperate Reification Principle? Have we not let it surreptitiously insinuate itself into H_A?[15] The suspicion is legitimate and I cannot underestimate it. However, there are at least three good reasons why H_A is not RP in disguise:

1. The shift from H^* to H_A is methodologically justified, while the employment of RP is not; for, as Bangu admits, RP is not always reliable.

[14] In section 5.4.2 I was talking about how we can detect that a certain mathematical structure is monomorphically injected from a physical system. Here, instead, I am discussing a case which is a bit different: in the present case, we already have a structure which we are justified to believe that *at least* embeds the physical system at issue; however, we do not know whether it *only* embeds the physical system or it is also isomorphic to it. Nonetheless, the cases are similar to the extent that the monomorphism hypothesis is a hypothesis which we *cannot* work with *unless we have already spelt out the complete interpretation from the mathematical terms to the physical ones*. But this can be done only if we perfectly know the composition of the physical system at issue — and this is not our case.

[15] This is actually the objection that Sorin Bangu moved, during a private conversation, against my account. I believe that the following considerations resolve his legitimate doubts.

2. Hypothesis H_A is just a hypothesis; it is not a principle. As such, H_A can be falsified and hence replaced by a better hypothesis, while RP cannot.
3. Hypothesis H_A does not concern the reality, but rather the *relation* between the reality and the mathematical structure that we are using to represent it.

Let me start from the first consideration. Why are we justified (*methodologically* justified) in passing from H^* to H_A? As I have precised in the previous chapter, we can work with the monomorphism hypothesis only if we know the extent of this monomorphic relation; i.e., if we know what we can, and what we cannot, infer from the mathematical structure to the physical system. If we do not know the limits of our monomorphism, then we have no other alternative than verifying the limits of the *isomorphism* hypothesis. Such a stronger hypothesis has the merit of being the *only* hypothesis permitting us to make inference *from* the mathematical structure *to* the physical system (unless we have already defined the monomorphism). These inferences can then be confronted with reality by means of experiments, and, consequently, be confirmed or rejected.

Actually, there is no way to define the limits of a monomorphism except forcing the isomorphism hypothesis.[16] Let consider, for example, the following case. Suppose we have two *mathematical* structures, A and B, and suppose that A is monomorphic-but-not-isomorphic to B. How can we proceed? We should prove either

1. that do no exist a function $\phi\colon A \to B$ such that any element $b \in dom(B)$ belongs to the image of ϕ; or, similarly, that for any function $\phi\colon A \to B$

[16] I am not considering here — since they are trivial — cases in which we have already reasons to believe that a certain mathematical element or relation does not belong to the image of the structure-preserving relation. In the coconut toy example discussed in chapter 5, for example, we already know that an interpretation of the negative numbers (however it may be) is not relevant for the problem at issue. In similar cases, we already know the limits of the monomorphic relation.

there exists an element $b \in dom(B)$ such that b do not belong to the image of ϕ. Or, alternatively,

2. that B has a property/function/relation that is surely not satisfied by A.

In order to prove one of the two point, one could proceed by assuming, *per absurdum*, that there exists an isomorphism, and then show that such an assumption led to a contradiction. But different ways to prove 1 or 2, which do not need to assume the existence of an isomorphism, may be viable. However, we are not facing the problem of confronting two *mathematical* structures. We are trying to confront a mathematical structure and a 'physical structure' — and we have seen in the previous chapter that this situation is much more complicated. The complication is due, among other things, by the fact that here we have also to *interpret* mathematical terms and relations, and moreover by the fact that we don't know exactly the 'physical structure'. For these reason, we cannot follow strategy (1), since we do not have a complete list of the elements of A (our 'physical structure'). This means that we know that all the *known* elements of A are associated to elements in B (the mathematical structure), but we do not know whether the known elements in A are *all* the elements in A. There could be other elements whose existence we still ignore. Therefore, we do not know whether, for any elements b in B, b belongs to the image of A or not, and hence we do not know whether A and B are isomorphic or not. The same can be said, *mutatis mutandis*, for the strategy (2). Thus, the only way to verify whether effectively A and B are isomorphic is to proceed by 'empirical way': we try effectively to associate every element and relation in B to an element and relation in A (by means of an oportune interpretation), and then we check the effective existence of such an element or relation in A by means of experiments or empirical proof. If we do not find any empirical confirmation, then we can either modify the interpretation, or admit that the isomorphism does not hold.

These considerations shows that the shift from H* to H$_A$ is methodologically legitimate: since we cannot work with the monomorphism hypothesis unless we know the limits of the monomorphism, in some cases we are legitimate to force this hypothesis by turning it into an isomorphism hypothesis, and then we can try to delimitate the scope of ths isomorphism (if it can be delimitate) 'from outside'.

The second remark concerns the *status* of H$_A$ and RP. H$_A$ is a hypothesis, RP is a principle (at least, this is how it is presented by Bangu). The difference is substantial. After all, a hypothesis is just a hypothesis, and there is nothing harmful in making a hypothesis. At least, there is nothing harmful if it can be falsified — and we saw that there is at least one sense in which our hypothesis H$_A$ is falsifiable. Thus, H$_A$ is not different from H*, and no more methodologically problematic than the latter. Moreover, since H$_A$ is just a hypothesis, it has no methodological value; namely, it does not impose on us a prescription, or a rule to follow in some cases. It is simply a guess. RP, on the contrary, is a methodological principle, and as such it prescibe us how to behave given some situations. In particular, RP prescribes us to find a physical referent for Γ' every time that Γ and Γ' are formally similar and Γ has a physical referent. A part from the fact that this rule cannot be always followed, if we employ such a rule in an argument, then we have the problem of justifying the legitimacy of such a rule — and we saw that all the problems with Bangu's reconstruction are centered around the justification of RP. Instead, the employment of H$_A$ among our premises do not raise any difficulty; at least — as already said — no more than H* does.

Finally, the third remark concerns the scope of H$_A$ and RP. RP actually says something *on* reality: it says that if the formalism is such and such, then *reality* is such and such. On the contrary, H$_A$ is not an hypothesis *on* reality; it is rather an hypothesis concerning the relationship between the mathematical formalism and reality described by it. If the mathematical formalism stands in a certain relation with the physical phenomenon, then we are justified to make inferences *from* the mathematical formalisms *to* reality. But these inferences ultimately depend on the assumption that the relation between the mathematical formalism and reality is such and such. If our inferences are disconfirmed by experience, then we can simply conclude that our hypothesis on this relation was false. Actually, RP completely bypasses this step: it does not call into question the relation between the formalism and the reality; it simply forces us to find a referent for those elements in the formalism that are still without a reference. But this cannot be done *in general*. It can be done *only if* the formalism (the mathematical structure) stands in an isomorphic relation with the physical phenomenon, and we are justified to find a reference for these elements only if we are justified to believe that such an isomorphic relation obtains between the mathematical structure and the physical phenomenon.

Also notice that RP is a generic principle, and applies to *any* mathematical structure we may want to employ (better: which *should* apply to any mathematical structure, since we already know that such a principle is not always valid). On the contrary, H_A does not refer to mathematical structures *in general*, but is related to a very specific mathematical structure (in this case, the decuplet scheme resulting from the SU(3) group). Thus, if we want to apply the same kind of argument for making an analogous existential prediction from a different mathematical structure (see, for example, my next example), we will have to formulate a different isomorphism hypothesis, since the mathematical structure and the physical system at issue will be different from those which H_A refers to. Such a new isomorphism hypothesis will have to be justified, and its justification (if such a justification is possible) will be obviously independent from the justification we offered for accepting H_A. Therefore, we could say that, if H_A is true, then $\mathrm{RP}|_{M_A}$ is true, where "$\mathrm{RP}|_{M_A}$" denotes RP *restricted to that* *particular mathematical structure* M_A to which H_A refers. In different words, H_A implies the truthness only of *a particular instantiation* of RP. The converse does not hold: if RP is true in general, then we should be justified to say that *any* mathematical structure, employed in representing a certain physical phenomenon, is isomorphic to that physical phenomenon. But this is obviously false; remember, for example, the coconut puzzle in the previous chapter: in that case we are not justified to think that there is a physical reference also for negative numbers. Hence, it follows that RP is false as well. Therefore, an hypothesis like H_A cannot be formulate *in general*, i.e. for any mathematical structure we are dealing with; but it can be formulate for those mathematical structures only, which (1) are effective in representing a certain physical phenomenon, and (2) can be isomorphic to the physical phenomenon they are aiming to represent (i.e., for which an equivalent of our premise P3* holds).

Now, if we assume that H_A is true, the mathematical observation MO permits us to infer H_B. Actually, if we assume that the symmetry scheme is isomorphic to the class of $\frac{3}{2}$-spin baryons (H_A), then we have to find a referent also for the empty place in the symmetry scheme. In searching for such a physical referent for this empty place, we have to satisfy what in the previous chapter I called the 'general principle of coherence'. Namely, the physical referent for the empty place must be coherent with the interpretation of other portions of the formalism. In this case, there is no doubt that the only coherent interpretation is that the empty place represents a tenth $\frac{3}{2}$-spin baryon having the characteristics determined by the formalism.

From here to H_C it's a short step, and P3* just works as a *laissez-passez* for this final step. However, if we compare our conclusion H_C with Bangu's conclusion C, we notice a substantial difference. Actually, C talks about the existence of a physical *interpretation* for the apex position, while H_C talks about the *existence* (*tout court*) of a tenth baryon. C is much similar to H_B

than to H_C. Moreover, according to Bangu's reconstruction, we need P3 to conclude C, while in our reconstruction P3* is needed not to conclude H_B (the equivalent of C), but rather to conclude H_C. This raises two questions: the first concerns the reason why we need a further step; the second concerns the role of P3/P3*. The first question is easily answered: Gell-Mann and Ne'eman argued for the existence of a new baryon, not for the possibility of an interpretation of the apex position, and experimental physicists looked for the existence of a tenth baryon, not for the existence of an interpretation for the apex position. Regarding the second question, consider that P3/P3* claims that nothing in nature seems to impede the existence of a tenth baryon having the predicted characteristic. An interpretation is just an interpretation, and we are free to adopt any interpretation whatsoever. But the fruitfulness of an interpretation is valued on the basis of its fruits; namely, on the basis of the image of the world resulting from such an interpretation. The only reason we can have to refute a certain interpretation, is that such an interpretation compels us to the existence of something that, according to our previous knowledge of the world, cannot exist.[17] P3/P3* is needed just to show that the existential consequences of such an interpretation does not collide with anything in our previous knowledge of the world, and hence that we can reasonably commit to the consequences implied by such an interpretation. But the interpretation, *per se*, does not need any legitimation.

We come hence to the conclusion of the GMNPR, H_C, and such a conclusion can then be verified by experimental physicists in their laboratories. Observe that H_C is as hypothetical as H_A: H_A implies H_B and H_B implies H_C. An eventual confirmation of H_C is a confirmation not only of H_C, but also of H_B and H_A. The existence of a tenth baryon with the predicted characteristics

[17] Or, in more general terms, that such an interpretation compels us to accept something (the existence of a new entity, the validity of a certain relation, and so on . . .) that, according to our previous knowledge of the world, we cannot accept.

is a confirmation of the hypothesis that permitted such a prediction; i.e., the hypothesis according to which the physical system at issue is not only monomorphic, but *iso*morphic to the mathematical structure we adopted to represent it. If no tenth baryon had have been discovered, then we should admit that H_C, H_B and H_A were bad hypothesis. If it were the case, we could just discard them (actually, it would means that the mathematical structure adopted is not isomorphic to the physical system represented), but hypothesis H^* *would remain untouched*, since H_C does not follow from H^*, but from H_A only. Namely, that the mathematical structure adopted is not isomorphic to the physical system does not mean that the mathematical structure does not represent it at all.

In Bangu's reconstruction, this distinction misses — and actually, if C were disconfirmed, such a disconfirmation would throw a sinister light on H too, since C depends on H, as well as on RP. Remember what I said about the presence of an anomaly to be explained away: the anomaly exists only if we assume RP. Actually, if C is disconfirmed, there can be only two culprits: H or RP. But if we accept RP, then the only culprit remaining is H. Thus, as already reported in section 6.2.3, RP itself creates the anomaly that aims to avoid! The problem — now is clear — is that RP does not permit to distinguish between two different features a mathematical structure can satisfies: its representative effectiveness and its being isomorphic to its representative target. The former feature can be satisfied even if the latter is not.

A further clarification is needed in order to conclude the exposition of my alternative reconstruction of GMNPR. According to Bangu's account, the main role played by RP just consists in «giving precise indications about the characteristcs of the new particle» (Bangu 2008, p. 248). The difference between the DN standard model and the case of the omega minus prediction lies, according to him, in the fact that the prediction (the calculation) of the specific characteristics of the particle had not been made by a quantitative analysis of its interaction, but rather «they were postulated *directly* from the formalism, by relying on RP» (Bangu 2008, p. 249). Actually, Bangu distinguishes two different questions:

(a) What are the grounds to believe that there is a new entity?

(b) What are the grounds to believe that the new entity should have the predicted physical characteristics?

Question (a) can be answered by saying that the existence of a new baryon is postulated in order to explain away an anomaly (according to the DN model), but question (b) cannot be answered without assuming RP. So, all methodological worries weigh on question (b).

I have already shown that, according to my account, the omega minus prediction is not made in order to explain away an anomaly. Therefore, we cannot hope that the DN model could account for this prediction *in the same way* as it accounts for Neptune prediction. However, I have to take question (b) into consideration in order to show that RP does not play any role even in the prediction of characteristics. I do not agree with Bangu when he says that the predicted characteristics were postulated *directly* from the formalism, since they are postulated from the formalism *plus* an hypothesis on the relation between this formalism and the physical system at issue; and I don't agree when he adds that they were predicted by means of RP. However, one might reply that actually RP hides itself in my assumption H_B, which answers (b).

In order to show that RP does not play any role in answering (b), I don't need to say much more than I have just said. I only want to show that in my account the two questions (a) and (b) collapse into each other, so that my previous considerations are enough to answer both. In order to achieve this purpose, I will take into consideration the role played by the premise MO.

So far I have talked about the prediction of a new entity. But if we carefully examine the formalism at work, we see that SU(3) does not deal with "entities"; it simply deals with *properties*, whose possible combinations must obey some invariance laws. Every position in the decuplet corresponds to a state vector. These state vectors represent nothing but a distribution of properties corresponding to each particle, and are linked to each other by some equations given by the neutron-proton symmetry. Given the nine-out-of-ten state vectors of the decuplet, Gell-Mann had simply to calculate the tenth state vector. It provides a distribution of properties for the (supposed existing) corresponding particle. MO simply consists in a mathematical observation about "clusters" of properties (or state vectors): in order to complete the order of the symmetry group, another element (another state vector) must be added to the system, and the formal system itself *forces* this element to have certain particular properties. Namely, the symmetry scheme permits just *one* other possible state vector. Now, since we previously interpreted the state vectors as particles (the nine baryons), we coherently extend this interpretation to the tenth state vector and we make the prediction in H_B. Does RP play any role in this interpretation of state vectors as particles (i.e. as distinct objects)? My answer is: no, it doesn't, because the interpretation simply meets a general coherence principle and is not imposed by the formalism, but rather by the previous (satisfying) interpretation of the other state vectors.

One might reply by observing that, if we make explicit what "extending the interpretation" means, we find something very similar to RP. For it is possible to state it in the following way: "If Γ and Γ' are elements of the mathematical formalism describing a physical context, and Γ' is formally similar to Γ, then, if Γ has an interpretation I, Γ' has an interpretation I' which is of the same kind as I". However, this similarity with RP does not imply that by extending the interpretation for the state vectors we are also implicitly using RP. RP talks about "physical referent"; on the contrary, the above principle talks about "interpretation of the same kind". No reification is at work in the latter. For the notion of interpretation does not imply the existence of a physical referent for the element of the mathematical formalism that is being interpreted. Therefore, the extension of the interpretation cannot be confused with the employment of RP.

Thus, in answering (a), we are answering (b) too. While in the Neptune case we can distinguish two moments (the prediction of a new entity in order to explain away an anomaly and the prediction of that entity's characteristics in order that it may play its explanatory role), in the omega minus case we cannot distinguish these moments, since what we predict is that a new cluster of properties (a new state vector, according to the formalism) could exist, and this (according to our previous interpretation of the formalism) amounts to saying that a new entity *with the predicted characteristics* could exist. In other words, in the Neptune case, first we predict the existence of a new planet, and then we calculate (by interaction) its characteristics; in the omega minus case, instead, first we predict a new combination of properties (by mathematical considerations), and then (by ontological considerations) we associate them with an entity which embodies them. What permits us to predict the new combination of properties is just the formalism here at work (the finite group theory, the vector space, etc.) *plus* some empirical facts (the discovery of those resonances that force the SU(3) symmetry scheme into a decuplet) and a hypothesis on the relation between the mathematical structure and the physical system represented by it, and not the usage of a controversial methodological principle such as RP.

6.3 Dirac's prediction of the positron

Another case discussed by Steiner to illustrate the employment of formal analogies in contemporary physics, is Dirac's prediction of the positron. I have already presented it in section 5.1,[18] thus I do not need to linger on it as I did with the omega minus prediction. According to Bangu, also this case would be accountable only by assuming the validity of the highly controversial principle of reification (RP).

Here is how Bangu reconstructs the reasoning that led Dirac to the prediction of the positron:[19]

> (P_1') – *Each of the positive-energy solutions has a physical referent.*
>
> (P_2') – *The negative-energy solutions are mathematically similar to the positive-energy solutions. (They are similar since they are solutions for the same equation.)*
>
> (RP) – *If Γ and Γ' are elements of the mathematical formalism describing a physical context, and Γ' is formally similar to Γ, then, if Γ has a physical referent, Γ' has a physical referent as well.*
>
> (P_3') – *The physical existence of a particle having the predicted characteristics is physically possible (it is compatible with the laws of physics).*
>
> Therefore:
>
> (C') – *The negative-energy solutions have a physical referent too. Call it 'anti-electron'; it has the same mass m as the electron, and charge $+e$.*

This reconstruction clearly mirrors the reconstruction of the reasoning that led to the discovery of the omega minus particle. We can therefore extend to this case our previous considerations, and reformulate the reasoning in the following way, which excludes the employment of RP:

[18] See pp. 131-132.

[19] See Bangu (2012, pp. 95-109)

(P_1') – *The positive-energy solutions are suitably represented by the mathematical structure identified by Dirac by means of his new relativistic equation for the electron.*

(H'^*) – *His relativistic equation suitably represents the physical phenomenon at issue; namely, the relativistic behavior of electron.*

(H_A') – *His relativistic equation represents the physical phenomenon at issue not only* suitably, *but also* non-redundantly*; namely, the mathematical structure identified by his equation is* isomorphic *to the physical phenomenon.*

(MO') – *Dirac's relativistic equation for electron has two further negative solutions; namely, the mathematical structure identified by Dirac's equation admits the existence of two further ideal (mathematical) objects.*

(H_B') – *These further solutions (these further mathematical objects) must have a physical interpretation, and such an interpretation must be coherent with the global interpretation of the mathematical structure adopted. As Dirac suggested, the most coherent interpretation is that these negative-solutions corresponds to a new particle having the same mass m as the electron but charge $+e$.*

(P_3') – *The physical existence of a particle having the predicted characteristics is physically possible (it is compatible with the laws of physics). Namely, there is no* a priori *reason to exclude that H_B is true.* Therefore:

(H_C') – *There exist a new particle represented by the negative-energy solutions. Call it 'anti-electron'; it has the same mass m as the electron, and charge $+e$.*

Also in this case, we have expunged the reference to RP, and the conclusion is obtained by means of the hypotheses H'^*, H_A', and H_B'. The conclusion H_C', depending on these hypothesis, has an hypothetical character as well. The same arguments I previously advanced to show that RP is not concealed in anyone of these hypothesis can be reproduced here to the same extent. Hypothesis $H^{'*}$ is just a generalization from $P_1^{'}$. $H_A^{'}$ is justified by the fact that we do not know whether the physical system at issue is isomorphic or only monomorphic to the mathematical structure adopted; thus, we adopt the only hypothesis that permits us to make verifiable hypothesis on the structure of the physical system; namely, $H_A^{'}$. Finally, $H_B^{'}$ is just a consequence of assuming $H_A^{'}$ together with $MO^{'}$.

However, some little differences between this prediction and the prediction of the omega minus particle are worth of remark. First of all, the prediction of the anti-electron shows, better than the previous case, that such a prediction has not been advanced in order to *eliminate* or *prevent* any anomaly. Simply, there is no anomaly, neither to eliminate nor to prevent. Actually, which would be the anomaly? Suppose that no positron have been discovered, and that Dirac's prediction have turned out in a flop — or, if you prefer, that Dirac never realized that such a prediction could have been made. Dirac's equation would still be a good representation for the relativistic behavior of electron, and I do not see any difficulty in admitting that the two negative-energy solutions are just an odd consequence of the mathematical structure adopted. It would be an anomaly only if we pre-emptively assume RP — which would be a *petitio principi.* In this case, the comparison with the coconut puzzle is as enlightening as possible. In the coconut case, we saw that a negative solution is mathematically possible, although no interpretation can be offered for such a solution. Similarly, we notice that in Dirac's case a negative-energy solution is mathematically possible, but in this case a fruitful interpretation has been found, and its accuracy empirically confirmed. The only difference between the two cases is that in the former we cannot find any satisfying interpretation for the odd solution, while in the latter we can. Thus, in the former case we come to the conclusion that the mathematical structure employed is — according to our terminology — 'redundant', i.e. non-isomorphic to the physical system represented; while in the latter case we come to the conclusion that the mathematical structure is — at least for the moment, and barring further adjustments — isomorphic to the physical system represented.

Secondarily, the interpretation of the negative-energy solutions as standing for a new entity seems to be less immediate than the interpretation of the apex position as standing for a new baryon. In a certain sense, such an interpretation seems to be much more 'open-minded' and 'unconventional', as if in solving the coconut puzzle we suggested that the -4 solution might make physical sense. As I noted when I was discussing the coconut puzzle, the more we go far from our immediate experience, the more we can hazard unconventional interpretation for the mathematical structures we are dealing with, since the constrictions we have to take into consideration are fewer. However, we must always remember that an interpretation, as such, is just an interpretation and its agreement with reality must always be checked. Remember the flow diagram represented at p. 164: if the interpretation fails to match with reality (i.e., if no omega minus baryon, or no positron were detected), we can (1) change the interpretation and check this new interpretation again; or we can (2) modify our hypothesis about the relation between the mathematical structure and the physical system and say that the former is not isomorphic to the lattere, although it is still effective in representig the latter; or we can (3) modify such an hypothesis in a more radical way, and say that the mathematical structure actually *does not* represent the physical system we are dealing with. Which of these options we have to follow depends on the kind of failure we run into: for example, if we detect a tenth baryon *having different characteristics* from those predicted by SU(3), then we must admit that SU(3) fails in representing the class of $\frac{3}{2}$-spin baryons; if, on the contrary, we do not detect any tenth baryon, we can insist that the mathematical structure determined by SU(3) (the decuplet scheme) is still a good representation for the class of $\frac{3}{2}$-spin baryons, although it is not isomorphic to it; or, again, if we have other interpretations available, we can still insist that such a mathematical structure is *isomorphic* to the class of $\frac{3}{2}$-spin baryons, in which case we have to check the validity of the new interpretations.

6.4 Final considerations

The two reconstructions I proposed in the present chapter have shown that we do not need to assume RP in order to justify the reasons of those who employed those mathematical structures to make the predictions at issue. We can do it in a very reasonable way if we assume the 'modified' structural account I presented in the previous chapter. These reconstructions have also shown that, at least in the two cases just discussed, we do not need to assume any 'Pythagorean', or extra-naturalistic assumption about mathematics in order to clarify its heuristic role. Simply, such an heuristic role can be justified by assuming — once again — my account for representative effectiveness of mathematics, a certain physicists' creativity in proposing verifiable interpretations for those terms in the mathematical structure for which an interpreatation has not already given, and — why not? — a certain amount of luck in proposing the right interpretation.[20] The key element in my account that permitted such alternative reconstructions is the role played by the interpretation. As we saw in the previous chapter, standard structural accounts of the applicability of mathematics are inclined to underestimate such a component — and even to completely overlook it. However, it becomes decisive not only in permitting a preciser account for the representative effectiveness of mathematics, but also in offering an account for the heuristic potentialities of a mathematical structure in terms of its

[20]Once again, we must remember that we usually tend to take into consideration *only those positive cases* in which a certain interpretation, or a certain prediction, turns out to be, in the end, right; but we must always remember that the history of science is full of unsuccessful attempts, and that unsuccesful cases are as instructive as positive cases are. On this regard, see Pincock (2012, ch. 7) and Azzouni (2000).

representative values. Such predictions were possible because we already had a good mathematical structure that represented the physical system at issue. On this representative basis, physicists could then make hypothesis on the degree of precision of these representations, and then — assuming that the precision is absolute (i.e. that there is an *isomorphic* relation between the mathematical structure and the physical syste) — make interpretative attempts in order to fix the exact relation between the representative mathematical structure and its target. Thus, what in Steiner's terminology seems to be a case of analogy between mathematics and physics, actually is just a case of good mathematical representation that, under the opportune interpretation, turns out to be very productive.

Let me conclude with a final observation. As I said, hypotheses like H_A or $\mathrm{H}^{'}_A$ concern the relation between a certain mathematical structure and the physical system represented by them. However, one might notice that it is quite strange that a physicist should formulate such hypotheses. After all, physicists do not make hypothesis on the relation between mathematical structures and the world, but make hypotheses on the world *by means of mathematical structure*. Actually, I hold these two formulations are equivalent. It is maybe true that physicists do not *explicitly* state such hypotheses, but when they make hypothesis on the world by means of mathematical considerations, they are *implicitly* claiming that they are justified in doing that *beacause of* a certain relation obtaining between the world and the mathematical structure. We might even say that physicists are led, in their researches, by the (maybe implicit and unjustified)[21] idea that for *any* physical system there is an isomorphic mathematical structure which is a *perfect* (non-redundant, in my terminology) representation of it. Thus, when physicists select a certain mathematical structure as a good candidate to represent a certain physical system, they implicitly hope that such

[21]For more on this point, see my conclusions, p. 205.

a mathematical structure is the *perfect* representation of that physical system — i.e., that the mathematical structure is isomorphic to the target. It is probably not an absurdity to consider such an idea as a leading idea in scientific research.

Conclusions

In the previous chapters, I considered several topics connected to the effectiveness of mathematics in physics and I drew various conclusions. We saw that the dismissal of the problem of mathematical applicability in contemporary philosophical analysis is not justified by an effective overcoming of it, but is simply due to contingent reasons. Moreover, by analyzing Wigner's (1960) and Steiner's (1998) works, we saw that *there is* a philosophical problem concerning the effectiveness of mathematics, and in fact there are *several* problems concerning such an effectiveness, mainly because of the fact that mathematics can play different roles in science (as we saw when I discussed Pincock's (2012) work). These problems are not isolated from the other philosophical problems concerning mathematics (such as, for example, ontological and epistemological problems), and they should be considered on a par with them, since a comprehensive account of the effectiveness of mathematics can help, in a consistent way, to clarify these other issues. In the last two chapters of this work I offered an account for the representative effectiveness of mathematics, with a particular attention to the heuristic imports that such a representative effectiveness can have (and actually, had) in some cases.

The account I proposed is grounded on a structural basis, but by analyzing other 'traditional' structural accounts, I came to the conclusion that these 'traditional' structural accounts need to be improved and clarified under different respects in order to be considered satisfying. First of all, I precised which is the *minimal* structural relation that has to obtain in order to grant representativity. I argued that such a minimal relation is the monomorphic relation from the 'physical structure' at issue to the candidate mathematical structure. This precisation, however, did not remove any vagueness, since the monomorphic relation can be fixed only after we determined the *relevant elements* of a 'physical structure'. But this, in turn, can be done only once we have fixed our interests in representing such a physical system. It must be noted that, in a certain sense, traditional structural accounts do not avoid vagueness, since when they invoke a structural relation between mathematical and 'physical' structures as a condition for applicability they leave uncertain which this relation has to be (they usually invoke homomorphism, but we saw that homomorphism is too generic). Secondary, I underlined that a structural relation cannot, by itself, grant any content at all if it does not go with an interpretation. It is mainly by emphasizing the role of interpretation in mathematical representation that I could connect representative and heuristic roles of mathematics in science. Finally, I stressed that to speak about 'physical' structures to be match with mathematical structures is an improper way of speaking. In fact, I used several times the expression "physical structure", but I put it everytime among inverted commas, in order to make it evident that it was just an abbreviate way of speaking. Actually, I precised that the monomorphic relation subsists among the mathematical structure and the data model, which is already mathematically structured (even if in an incomplete manner), and acts as an intermediatory between the mathematical structure and the phenomena of the physical world.

However, the remarks I made, and the conclusions I drew, in the previous pages, suggest some other general considerations concerning the relation between mathematics, world and physics, which I will present in the following sections. These considerations are not to be considered, strictly speaking, as definitive or certain; they are compatible with my previous considerations, but they need much more work in order to be satisfactorily argued. However, I will insert them here just as a sketch for future researches, and as an evidence of the fruitfulness of the analysis previously made.

Mathematics and the structure of the world

The account of the representative and heuristic effectiveness of mathematics I offered in chapter 5 permits some interesting considerations about the relation between mathematics and world.

When, in chapter 5, I pointed out that the *minimal* structural relation that must obtain between a mathematical structure and a physical system, in order for the former to be representatively effective, is the monomorphism from the physical system to the mathematical structure, I also precised that this role cannot be played by an isomorphism relation, since such a relation seems to be too stronger. In fact, when we look at some concrete exemples of mathematical representation in physics, we find that often the mathematical structures employed to represent some physical phenomena are richer than these latter. However, it is clear that if we can find a mathematical structure which is perfectly isomorphic to a physical system, we are in the best situation we could desire: we have found the 'right' structure and there is no improvement to seek here; everything we can deduce from the mathematical structure must have a correspondence in the physical system. Now, one might ask whether, for any physical system or phenomenon we want to understand and represent, there is a *perfectly fitting* (that is, isomorphic) mathematical structure.

It is not easy to find a conclusive and satisfying answer to this question. In a (2000) article, Mark Wilson distinguishes between two philosophical 'aptitudes' or positions, which he calls "mathematical optimism" and "mathematical opportunism". According to him, «Mathematical optimism [...] claims that for every physical occurrence there is a mathematical process that copies its structure isomorphically» (p. 297). Thus, if we characterize mathematics as the "general science of possible structures" (as mathematical structuralists do — but we saw that we do not need to be mathematical structuralists to accept this very simple claim), then

> Presumably this large bag of general "structures" is expected to contain an adequate copy of any "physically possible structure" that we could ever assign to the physical world, on the grounds that if a structure is coherent at all, it must have a suitable representative within mathematics. In other words, somewhere deep within mathematics' big bag must lie a mathematical assemblage that is structurally isomorphic to that of the physical world before us, even if it turns out that we will never be able to get our hands on that structure concretely. (pp. 296-297)

Maybe we cannot find the "right" copy for a certain "physical structure", but this is just because sometimes the "right" structures are simply too much complicated and we cannot handle their complexity. However, for mathematical optimists it holds as a general principle that a certain "physical structure" has always an isomorphic mathematical copy.

However, as Wilson notices,

> many of the originators of mathematical physics in the early modern period would never have accepted such a cheery presumption; they mantained that it is only when the processes of nature enjoy a special simplicity that mathematics can track its workings adequately. (p. 297)

This is what he calls "mathematical opportunism": «it is the job of the applied mathematics to look out for the *special circumstances* that allow mathematics to say something useful about physical behavior» (p. 297). In other words, mathematical opportunists are motivated by the belief that the success of applied mathematics is based on a certain number of constraints that physical worlds *must obey* in order to be described by mathematical structures. It is not simply a matter of unmanageable complexity: «the success of applied mathematics require some alien element that cannot be regarded as invariably present in the physical world» (p. 299).[22]

It is not easy to decide in favour of one of these two doctrines. If it is true, as Wilson remarks, that many of the originators of mathematical physics in the early modern period would have described themselves as mathematical *opportunist*, it is also true that sometimes physicists themselves seems to endorse an extreme mathematical optimism. For example, the swedish-american cosmologist Max Tegmark, in his (2007), endorse what he calls the *Mathematical Universe Hypothesis* (MUH): «Our external physical reality is a mathematical structure» (p. 102).[23] In other words, Tegmark argues that, if we assume that there exists an external physical reality completely independent of us humans (this is what he calls the *External Reality Hypothesis*, ERH), then the external reality described by a hypothetical Theory of Everything (TOE) would be a mathematical structure, since such a TOE would only consist of abstract terms and their relations. Such a claim can be surely considered as a very extreme form of mathematical optimism.

I do not think that many would follow Tegmark along this line of thought. Note that Tegmark's MUH does not talk about "representation", it talks about "reality": he is not saying that there is a mathematical structure that isomorphically represents everything in the TOE; he is saying that all what there is in the TOE is *just* a mathematical structure. In the end, Tegmark's MUH is just an *empirical* hypothesis which is far from being porvable — in any sense. In fact, in his (2007) article, Tegmark shows a diagram in which he represents how different scientific theories can be organized in a family tree. Within this family tree, every scientific discipline can be derived from General Relativity and/or Quantum Field Theory, and these two last theories are derived in turn

[22]According to Wilson (2000), «many of the skeptical remarks one finds in the anti-realist literature are better recast as considerations in favor of *mathematical opportunism*, although most of these arguments underestimates the resources available to an honest optimist» (p. 299).

[23]See also Tegmark (2014).

from an hypothetical TOE. A justification for MUH is given by the (empirical) observation that

> the ratio of equations to baggage decreases as we move down the tree, dropping near zero for highly applied fields as medicine and sociology. In contrast, theories near the top are highly mathematical, and physicists are struggling to articulate the concepts, if any, in terms of which we can understand them. (p. 103)

According to Tegmark, a description can be considered "complete" only if it is well-defined also according to non-human sentient entities (aliens, for example) that lack the common understanding of concepts that we humans have evolved (e.g., "particle", "observation", "force", and any other words appearing in a theory). These concepts are the "baggage" of which he talks in the previous quotation.[24]

Now, leaving aside the fact that such a complete derivation of scientific theories from an hypothetical TOE is highly problematic (and actually still far from being realized), many doubts can be cast on Tegmark's claim according to which the TOE should be a completely "baggage-free" theory.

However, coming back to our original question (is there, for any physical phenomenon, a suitable, *isomorphic*, mathematical representation of it?), it is clear that a solution to this problem is far from being achieved — *and achievable*. It is a matter of fact that applied mathematics, today, largely relies on *non-isomorphic* representations, but it is hard to take this as an evidence for mathematical opportunism, since such representations could be due simply to the fact that an eventual isomorphic representation would be humanly unmanageable because of its overwhelming complexity.

Probably, the most prudent attitude in this regard consists in adopting a 'modest' and 'moderate' optimism, with a touch of opportunist skepticism. It must be noted, however, that if one found an argument in favor of mathematical opportunism (and, consequently, against mathematical optimism), then a

[24]See (Tegmark 2007, pp. 102-104).

problem for structural realism would arise, particularly for its ontic version (*à la* Ladyman & Ross (2007)).[25] Indeed, if some physical phenomenon cannot be *isomorphically* grasped by a mathematical structure, then we should conclude that mathematics can have here only an instrumental value. But if it is so, then it seems to be hard to argue in favor of an ontic meaning for these mathematical structures.

Structural surplus, heuristics and theory development

I have already stressed, when I presented my account in chapter 5, the importance of the *surplus* structure. Actually, such a surplus is what often permits to generate new heuristic opportunities for increasing the knowledge and making new discoveries: the fact that a theory may have a mathematical surplus in its representation means that such a mathematical surplus — which at the moment rests uninterpreted within the representation itself — may be at any time 'activated' by means of a new interpretation. Such a notion of structural "surplus", therefore, can play a significant role not only in analyzing single discoveries or single progresses in science (as I did in chapter 6 by discussing the discovery of the omega minus particle and of the positron), but also in analyzing the evolution of physical theories themselves.

The importance of the structural surplus of mathematical representations has been examined by Michael Redhead in various works. For example, in (Redhead 1975) he is concerned with an anlysis of the role played by symmetries in inter-theory relations. However, in order to offer a suitable framework for his discussion, he outlines a brief sketch of the role of mathematics in theoretical physics. The account he offers presents some analogy with the account here presented in chapter 5, but some differences should be noted. He considers a

[25] *Epistemic* versions of structural realism (*à la* Worrall (1989)), on the contrary, would remain untouched.

theory T as embedded in a mathematical structure M', by this meaning that there exists an *isomorphism* between T and a substructure M of M', so that M' is a non-simple conservative extension of M (see fig. 6.3). «If we like —

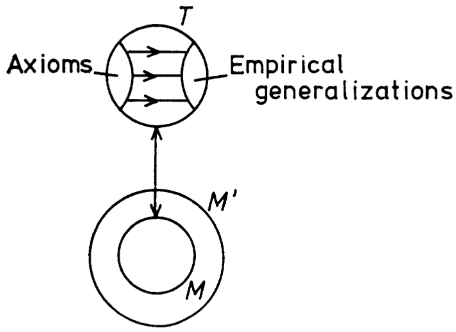

Figure 6.3: Redhead (1975, p. 87).

Redhead goes on — we can introduce an uninterpreted calculus C of which T and M are regarded as isomorphic models, or we can introduce a calculus C' for M' and introduce a new theory T' which is partially interpreted via the structure T» (p. 87; see fig. 6.4).

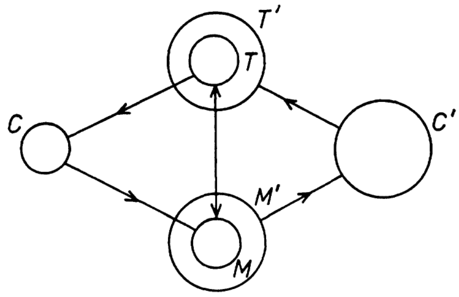

Figure 6.4: Redhead (1975, p. 88).

As Redhead comments,

> The relative complement of T in T' we refer to as the *surplus structure* in the mathematical representation of the theory T. Now we can reverse our line of argument and starting with a given T' ask what is the corresponding T? To a positivist T would involve only observational terms but to a realist theoretical terms may also appear in T. Even to a realist it is not clear in some cases whether the surplus structure should or should not be accorded ontological reference [...]. In other cases terms in T' which starts their

> life effectively as uninterpreted symbols may acquire a reference in reality as that particular branch of science develops. (p. 88)

It is clear that Redhead's concern in introducing such a calculus aims at the same target we aimed in chapter 5 and 6, when I discussed how and why a mathematical representation can have surprising heuristic implications. However, there are some differences between Redhead's analysis and mine. The first, immediate, difference is that Redhead compares mathematical structures with *theories*, while I compared mathematical structures with data models which, in the end, reflect physical reality. But this is not a big difference, since, presumably, these theories are in some sense compared to physical reality. Secondarily, Redhead claims that the theory T must be *isomorphic* to a substructure M of M'. Now, this can be seen just as a boundary case of my monomorphic relation. Indeed, if T is isomorphic to M, why do we deal with M'? And how can we fix an M' of which M is a substructure? After all, there can be an infinite number of structures of which M is a substructure. In my account, on the contrary, we just know that M' (which is fixed from the start) embeds T (that is, T is monomorphic to M'). Now, we can hope for reducing M' to a substructure M of it, which is isomorphic to T, but this is not always feasible. For example, if no tenth baryon had ever been discovered, how could Gell-Mann and Ne'eman reduce their mathematical structure M' to an M isomorphic to T (where T does not have a tenth baryon Ω^- among the objects of its domain)? It is not clear, not only "how", but even "that" this task could have been completed. In a certain sense, by thinking that a theory T is *always* isomorphic to a mathematical structure M, he is actually endorsing an unjustified form of mathematical optimism.

Methodological opportunism

The last section of Bangu's (2008) article is dedicated to some general considerations concerning the role and the importance of mathematics in physics and science in general. In these conclusions, he underlines that

> The problem presented here is [...] not a direct challenge for the working physicists, but for the physicist-qua-methodologist; that is, a challenge for her ability to propose a systematic philosophical/methodological framework able to accommodate this episode. Consequently, our problem is an instance of a broader kind of concern: what is the most appropriate way to construe the relation between philosophical/methodological standards and scientific practice. (Bangu 2008, p. 255)

According to Bangu, the best account for this relation between methodological standards and scientific practice is what he calls 'methodological opportunism', whose spirit is well captured by the following Einstein's (1949) words:

> The reciprocal relationship of epistemology and science is of noteworthy kind. They are dependent upon each other. Epistemology without contact with science becomes an empty scheme. Science without epistemology is – insofar as it is thinkable at all – primitive and muddled. However, no sooner has the epistemologist, who is seeking a clear system, fought his way through to such a system, than he is inclined to interpret the thought-content of science in the sense of his system and to reject whatever does not fit into his system. The scientist, however, cannot afford to carry his striving for epistemological systematic that far. He accepts gratefully the epistemological conceptual analysis; but the external conditions, which are set for him by the facts of experience, do not permit him to let himself be too much restricted in the construction of his conceptual world by the adherence to an epistemological system. He therefore must appear to the systematic epistemologist as a type of *unscrupulous opportunist*: he appears as realist insofar as he seeks to describe a world independent of the acts of perception; as idealist insofar as he looks upon the concepts and theories as the free inventions of the human spirit (not logically derivable from what is empirically given); as positivist insofar as he considers his concepts and theories justified only to the extent to which they furnish a logical representation of relations among sensory experiences. He may even appear as Platonist or Pythagorean insofar as he considers the viewpoint of logical simplicity as an indispensable and effective tool of his research. (pp. 683-684; italics mine)

By embracing this kind of opportunism, the scientists can reject the DN model, the RP principle, and the various reconstructions that epistemologists may propose in order to methodologically account for some particular scientific discoveries. They are free to ignore these epistemological difficulties and to adopt any stratagem they want in order to endorse scientific progress.

> This kind of puzzle, the scientist-qua-methodological opportunist stresses, should not be allowed to interfere with the practice of science, as no attempt to offer a principled, systematic account (either standard or non-standard) of those episodes should be followed too far or taken too seriously. As the Einstein advice goes, scientists should patiently listen to the epistemologists' arguments; in the same time, they should also learn to live without longing for the benefits of the coherence offered by the commitment to a particular 'epistemological system' of either empiricist (the DN scheme) or Pythagorean sort. (Bangu 2008, p. 256)

We must be carefull not to confuse this opportunism with the mathematical opportunism which I have previously discussed. There, the opportunism concerned the possibility of finding an isomorphic mathematical structure for any phenomenon in nature; here, instead, the opportunism concerns the epistemological rigor which scientists should comply with in their work. In different words, epistemologists try to understand the puzzles that scientific research submits to their attention, but scientists as such are not in duty bound to respect their prescriptions.

I perfectly agree with Bangu on his final considerations. The problems I dealt with in the two final chapters of this work are philosophical problems and there is no particular reason for working scientists to take them too seriously, or to worry for these puzzles too much. However, the account presented in these pages permits to shed light on some of these puzzles. Especially, we are now in a better position to understand the methodological steps that led to the discovery of the omega minus particle and of the positron, and to all the similar scientific discoveries. This reconstruction avoids controversial principles like RP and, at the same time, succeeds in recognizing the non-standard peculiarity of these important discoveries. Moreover, we have now a reliable account for the representative effectiveness of mathematics in science that also permits to account for some — at least up to now — puzzling cases of mathematical heuristic effectiveness. It seems to me that, even if the scientist-qua- scientist can certainly ignore our considerations, from the philosophical point of view we obtained an unquestionable remarkable profit.

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