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Abstract: Some of the great physicists' belief in the existence of a connection between the aesthetical features of a theory (such as beauty and simplicity) and its truth is still one of the most intriguing issues in the aesthetics of science. In this paper I explore the philosophical credibility of a version of this thesis, focusing on the connection between the mathematical beauty and simplicity of a theory and its truth. I discuss a heuristic interpretation of this thesis, attempting to clarify where the appeal of this Pythagorean view comes from and what are the arguments favoring its acceptance or rejection. Along the way, I sketch the historical context in which this heuristic interpretation gained credibility (the quantum crisis in physics in the 1920s), as well as the more general implications of this thesis for physicists' metaphysical outlook.



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Pythagorean Heuristic In Physics

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Some of the great physicists' belief in the existence of a connection between the aesthetical features of a theory (such as beauty and simplicity) and its truth is still one of the most intriguing issues in the aesthetics of science. In this paper I explore the philosophical credibility of a version of this thesis, focusing on the connection between the mathematical beauty and simplicity of a theory and its truth. I discuss a heuristic interpretation of this thesis, attempting to clarify where the appeal of this Pythagorean view comes from and what are the arguments favoring its acceptance or rejection. Along the way, I sketch the historical context in which this heuristic interpretation gained credibility (the quantum crisis in physics in the 1920s), as well as the more general implications of this thesis for physicists' metaphysical outlook.

1. Introduction

Within days of the derivation of the famous equation, E. Schroedinger wrote to his friend W. Wien on December 27th 1925.

At the moment I'm struggling with a new atomic theory. If only I knew more mathematics! I'm very optimistic about this thing and I expect that if I can only solve it, it will be *very* beautiful. I think I can specify a vibrating system that has as eigenfrequencies the hydrogen *term* frequencies—and in a relatively natural way, not through *ad hoc* assumption¹.

In a 1954 paper, P. A. M. Dirac remarked:

I'd like to thank Margie Morrison, Jim Brown, Alasdair Urquhart and an anonymous referee of this journal for comments and constructive criticism.

1. Excerpt from E. Schroedinger's letter to W. Wien, 27th Dec. 1925. Quoted in Moore (1989, p. 196)

With all the violent changes to which physical theory is subjected in modern times, there is just one rock which weathers every storm, to which one can always hold fast—the assumption that the fundamental laws of nature correspond to a beautiful mathematical theory. (Dirac 1954, p. 143)

Dirac's and Schroedinger's pronouncements are not singular; many other important physicists (Kepler, Minkowski, Einstein, Weyl, Heisenberg, etc.) held the view that there is (or must be) a certain strong correlation between the truth of a theory and the aesthetical features (such as beauty and simplicity²) displayed by its mathematical form. Yet this connection is questionable. On one hand, mathematical beauty and simplicity have subjective connotations. Aesthetic criteria to judge mathematical formalisms are notoriously loose. On the other hand, empirical adequacy and truth are traditionally taken to be objective notions. As expected, a number of authors pointed out these difficulties. Toward the end of his examination of Dirac's philosophy of science, H. Kragh concludes:

[Dirac's] principle of mathematical beauty, like related aesthetic principles, is problematical. The main problem is that beauty is essentially subjective and hence cannot serve as a commonly defined tool for guiding and evaluating science. It is, to say the least, difficult to justify aesthetic judgments by rational arguments. (. . .) No wonder that eminent physicists do not agree on which theories are beautiful and which are ugly. (Kragh 1990, pp. 287–8)

In similar vein, McAllister writes:

Contrary to Dirac, Einstein and others, I see little evidence that aesthetic properties correlated with high degrees of empirical adequacy in theories have yet been identified in any branch of science. (McAllister 1996, p. 102)³

Nevertheless, the fact that the truth-beauty-simplicity connection enjoys a great deal of support among the great physicists should be, I believe, a *prima facie* reason for concern. The wonder is then, why the belief in the truth-beauty connection, although apparently wide of the mark, is so popular among some (although not *all*) of the great scientific minds who ever lived? What is its source of appeal? What are the motivations behind

2. While 'beauty' and 'simplicity' do not strictly speaking belong to the same category, it is quite common among physicists (see the quotation from Heisenberg below) and philosophers of science (Osborne 1984, p. 295; Kuipers 2002, p. 292) to consider them as typical examples of aesthetical features of a theory.

3. For further discussions of McAllister (1996), see Kuipers (2002).

claiming such a connection? How are we to make sense of this connection after all?

My goal in this paper is to reevaluate the philosophical credibility of this connection. To this end, several preliminary clarifications are in order. First, there are two distinct ways to understand this connection: either it maintains that true theories are beautiful and / or simple, or it advances a stronger, more controversial view that mathematically beautiful and simple theories are (or must be) true in so far as they are beautiful or reflect, as Einstein put it, “the simplest conceivable mathematical ideas” (Einstein, 1954, p. 274). I’ll confine my investigations to the latter view, i.e., to the ‘beauty / simplicity—entails—truth’ version of the connection. I’ll call this relation the Connection thesis—hereafter, the C-thesis.

Second, I distinguish two interpretations of the C-thesis. According to the first interpretation, the C-thesis plays a role in what is called ‘the context of justification’ of a theory. In the second interpretation, the thesis should be understood as making a point in the ‘context of theory discovery’⁴. I argue that although the thesis is untenable in the first interpretation, its second (heuristic) interpretation can render it more credible. In this interpretation, I regard it as meant to offer a useful piece of advice as how to move physical research ahead in some difficult circumstances. (As we’ll see, what makes these circumstances difficult is that considerations from physical phenomenology prove unable to suggest new physical hypotheses). According to this interpretation, the thesis plays a methodological role, functioning, in a way to be explained, as a heuristic guide.

As it is the case with any piece of advice, the C-thesis reflects the commitments (presumably of ‘deep’, metaphysical nature) of those who advance it. I explore these commitments and I conclude that the C-thesis is rooted in a species of non-naturalist metaphysics. Since this is somehow surprising—in so far as the holders of this thesis are some first line natural scientists—further investigation into what sort of non-naturalism it promotes is required. I’ll call this metaphysics ‘Pythagoreanism’⁵. Along the way I pin down the most important characteristics of Pythagoreanism and I attempt a detailed analysis of the relation between this metaphysics and the heuristic interpretation of the C-thesis. I conclude that although a number of reasons (of pragmatic nature) can be put forward to explain scientists’ commitments to the C-thesis, the qualified success of this thesis as a heuristic guide can’t fully vindicate Pythagoreanism as a credible metaphysics.

4. I operate the divide between the context of justification and the context of discovery for expository reasons only. It should not be implied that I intend to take a stance in the debate over the existence or relevance of such a distinction.

5. Steiner (1998) re-introduced this label in recent literature.

2. Aesthetic criteria in theory choice

According to the first interpretation, the C-thesis states that scientists are guided by the aesthetical merits of the mathematical formalism (symmetry, simplicity, etc.) of a theory in the process of theory choice. More precisely, this guidance is instrumental in accepting theories as well as in rejecting them. There are, again, two ways to understand this view. First, the holder of the C-thesis may claim that the more beautiful a theory is, the more likely is to be true. The aesthetic criterion is thus used to decide over two theories having the same degree of empirical accuracy. Yet this would be a weak claim. A stronger claim is sometimes made, maintaining that the mathematical beauty of a theory should be given priority over its empirical adequacy *simpliciter*. That is, a mathematically beautiful theory should be preferred *regardless* of its empirical accuracy. Dirac endorsed both these claims, the weak one as well as the strong one. With regard to the weak claim, he wrote:

A theory with mathematical beauty is more likely to be correct than an ugly one that fits some experimental data. (Dirac 1970, p. 29)

Here is his pronouncement in favor of the stronger claim:

So, when a theoretical physicist has found such a [mathematically beautiful] theory, people put great confidence in it. If a discrepancy should turn up between the predictions of such a theory and an experimental result, one's first reaction would be to suspect experimental error, and only after exhaustive experimental checks would one accept the view that the theory needs modification, which would mean that one must look for a theory with a still more beautiful mathematical basis. (Dirac 1954, p. 143)

Here Dirac alludes to the General Theory of Relativity⁶. Yet the passage is not immediately transparent. Is it intended to have a normative meaning? I.e., does Dirac say that physicists *ought* to hold fast a beautiful theory no

6. In fact, Dirac seems to have in mind the widely-spread view that Einstein did not care too much about the empirical tests of his theory. However, as Hentschel (1992) points out, this carelessness is to some extent a myth. In his article, Hentschel marshals impressive historical evidence to support the claim that Einstein "regarded these experiments as crucial" (1992, p. 593); he also identifies a possible source of the myth, concluding that Einstein was suspicious about empirical disconfirmations of his theories "not because of his blind reliance on his own capabilities as a theoretician (. . .) but because [these disconfirmations] lacked (. . .) consistency with other well-established results (. . .)." (Hentschel 1992, p. 624)

matter what? Dirac understood the C-thesis in the strong form indeed; later on in his life, he remarked:

If the equations of physics are not mathematically beautiful that denotes an imperfection, and it means that the theory is at fault and needs improvement. There are occasions when mathematical beauty should take priority over agreement with experiment⁷.

Hermann Weyl is another supporter of the strong version: "My work always tried to unite the true with the beautiful. But when I had to choose one or the other, I usually choose the beautiful."⁸ Similarly, Heisenberg seems to endorse the strong version in a discussion with Einstein:

If nature leads us to mathematical forms of great simplicity and beauty—by forms I'm referring to coherent systems of hypotheses, axioms, etc.—(. . .) we cannot help thinking that they are 'true', that they reveal a genuine feature of nature. (. . .)

You [Einstein] may object that by speaking of simplicity and beauty I am introducing aesthetic criteria of truth, and I frankly admit that I am strongly attracted by the simplicity and beauty of the mathematical schemes with which nature presents us. (Heisenberg 1971, pp. 68–9)

Despite the undeniable fact that the C-thesis enjoys some of the greatest physicists' approval (at least at a declarative level), the history of physics does not record any clear cases in which the aesthetic criteria have played a special, decisive role in the acceptance of a certain theory⁹. With regard to the weak claim, one illustration of the relevance of the C-thesis can be found in the 16th century astronomy. Kuhn (1957, pp. 181, 172) argues that it was mainly aesthetic considerations (simplicity) that favored the Copernican over the Ptolemaic astronomical system. However, since both systems displayed roughly the same capacity to account for the astronomical observations¹⁰, beauty did not actually override empirical accuracy.

7. Quoted in Mehra (1972, p. 59). Discussed in Kragh (1990, p. 284)

8. Quoted in Chandrasekhar (1979).

9. Some authors claim that Einstein's GTR was accepted primarily on aesthetic grounds. Osborne (1984, p. 292) writes: "This is one of many cases where a theory has been adopted primarily on aesthetic grounds and later confirmed experimentally." Let alone the reference to "many cases" (which I think is an exaggeration), the passage is rather ambiguous. It is not clear whether the aesthetic grounds did play a *decisive* role, given that the author mentions the experimental confirmation right after.

10. However, the Copernican system predicted stellar parallax (the apparent shift in position of a nearby star against the background of more distant stars resulting from the motion of the observer on earth as earth revolves around the sun), while the Ptolemaic system, because of its postulation of a motionless earth, remained silent on this. However, stellar

Historical evidence supporting the strong claim—that despite systematic and consistent experimental refutations a beautiful theory was still accepted—is, it seems, simply nil.

So, cases of acceptance on aesthetic basis are highly problematic; in addition to this, situations of erroneous rejection on aesthetic grounds can be documented too. One of the most notorious and recent cases in this later category is Dirac's dismissal of Quantum Electrodynamics. Freeman Dyson recollects a conversation with Dirac in which the later said: "I might have thought that the new ideas were correct if they had not been so ugly"¹¹. Dirac was simply wrong: the theory in question turned out to be correct, even if 'ugly'.

All in all then, when the strong C-thesis is interpreted as claiming that the beauty-truth criterial correlations are relevant in theory choice, no convincing historical evidence can be put forward for it—on the contrary. Bohr's remark to Rosenfeld (1967, p. 117)—"I cannot understand what it means to call a theory beautiful if it is not true."—is, in this context, exemplary for some of the best scientists' attitude. When aesthetic criteria were employed to reject certain theories (i.e., in making 'ugliness-entails-falsehood' judgments) they proved, again, unreliable. Although the trivial point that aesthetics reasons can play *some* role in theory choice (in acceptance as well as in rejection) is correct, it seems that there is not much left to say about the connection between mathematical beauty and empirical accuracy beyond this trivial point.

In fact, as virtually all authors writing on this topic noted, the gist of the problem is the unreliability of aesthetic criteria themselves. Are there such criteria after all? Are they effective, in the sense of being instrumental in indicating the truth of a theory?¹² Why should we expect they are?¹³ For one thing, since mathematical beauty comes in various forms, differ-

parallax was difficult to observe because stars are much farther than people thought at that time. F. Bessel measured the first stellar parallax in 1838.

11. In a conversation with Dyson (1986, p. 103). Richard Feynman, one of the people targeted by Dirac's remarks about "ugliness", reacted promptly. A. Zee (1990, p. 310) recounts (though not fully agreeing with Feynman) the following: "Echoing a fairly widespread arrogance of the physicist, Feynman once said that had God not created mathematicians, physics would have been delayed by about a week (. . .) According to Feynman, physicists would have invented what they needed, and the rest, as far as he was concerned, should not have been bothered with in the first place."

12. One of the earliest rejections of the aesthetic criteria in theory choice is Laudan's (1984). He claims that values can have a place in a rational activity only if they are 'operationalized' (explicitly conceivable and realizable in practice); aesthetic values are thus ruled out. For an unconvincing criticism of Laudan's view, see Martin (1989).

13. McAllister's (1996), (1998) investigations of these issues show that we should be skeptical about the existence of the beauty-entails-truth correlation in the context of

ent scientists may have different preferences. So, how can one cope with the lack of consensus over the beauty of a certain equation of mathematical physics? We are supposed to analyze mathematical beauty and simplicity—but “beautiful” or “simple” for whom? Of course, one way out of this difficulty is immediate: if two scientists disagree on an aesthetic aspect, then one of them is misled by her aesthetic sense. Thus, an important point the defenders of aesthetic criteria make is a distinction between the very *existence* and the *applicability* of aesthetic criteria: if these criteria are difficult to apply it does not follow that they don’t exist. If a theory is chosen or rejected as a result of an aesthetic impulse and this decision proves wrong, then the impulse was not truly aesthetic. This hypothesis is consistent with Dirac’s urge that in cases of experimental failure the physicist must try to find an even more beautiful equation. Although one has to admit that this hypothesis *might* be correct, as any *ad hoc* hypothesis it is clearly suspect.

Yet, one should not conclude that Dirac or other physicists were somewhat naïve when holding such a view. Dirac, for one, fully realized that criteria for recognizing beauty are ‘subjective’:

It is quite clear that beauty does depend on one’s culture and upbringing for certain kinds of beauty, pictures, literature, poetry and so on . . .

(Quoted in Dyson 1986, p. 102)

So, isn’t it that what scientists take to have aesthetic value falls under the influence of subjective, social factors? Being exposed to certain aesthetic criteria during their training, scientists would tend to employ these socially induced criteria when judging their work. Dirac disagrees and postulates that the mathematical beauty of a theory is immune to these influences, proclaiming a certain specificity of mathematical beauty¹⁴:

Mathematical beauty is of a rather different kind. I should say perhaps it is of a completely different kind and transcends these

justification. Moreover, he clarifies, in his discussion of the notion of ‘aesthetic induction’, the unreliability of the converse truth-entails-beauty connection.

14. The justifications of Dirac’s claim are rather unclear. (In fact, Kragh (1990, p. 288) straightforwardly maintains that Dirac’s claim lacks any justification.) At the end of this section, drawing on the interpretation of Dirac’s aestheticism offered by the celebrated contemporary physicist Wilczek (Nobel Prize 2004), I’ll propose a quick explanation as to why Dirac thought that mathematical beauty of an equation could escape the influence of subjective or social factors. Although important, a detailed analysis of the role of the social factors in deciding what is beautiful is beyond the scope of this paper.

personal factors. It is the same in all countries and at all periods of time.

(Quoted in Dyson 1986, p. 102)

If we regard the view advanced here as a sort of empirical hypothesis (about the nature of mathematical beauty¹⁵), that is, as a factual claim about scientist's psychology, the main obstacle to its acceptance is the lack of factual support for it. What kind of evidence can one put forward to endorse such a hypothesis? Has anyone collected physicists' introspective reports? In any case, since one can easily discern how Dirac's point involves some rudiments of Cartesian innatism or even Platonism, it is amusing to realize that a physicist well known for spurning philosophy¹⁶ ultimately draws support for his lifetime methodological convictions from a (paradigmatic case of) philosophical idea.

I conclude that the C-thesis is untenable when understood as making a point in the context of justification. When it is interpreted in this way, it is internally coherent and daring, but is weakly supported by historical evidence. Its examination should be brought to a halt at this point, because the correlation between beauty and truth (or empirical adequacy) is very loose. This correlation is a contingent fact, and not the expression of a somewhat deeper, yet-to-be-discovered relation between truth and some aesthetical features of the mathematical formalism.

However, at this moment it is important to emphasize that this conclusion follows the discussion of the interpretation of the C-thesis in the context of theory justification. Though convincing, this rejection is far from being sufficient to decide the C-thesis' status, since it is not clear that this thesis has been intended to have this interpretation in the first place—that is, to have *only* this interpretation. And, in fact, as I'll argue below, the heuristic interpretation is as legitimate as the first interpretation. As *it* is well known, heuristics was a constant concern for some of the greatest physicists of 20th century. Einstein, for instance, is famous for his 1933 Herbert Spencer lecture in which he discussed heuristics at length. He believed that “the creative principles [in physics] reside in mathematics” (Einstein 1954, p. 274); that is to say, as he clarifies earlier on in the lecture, in constructing physical theories corresponding to the simplest mathematical equations¹⁷. As we've already seen, Dirac also emphasized, perhaps even more than Einstein or any other physicist, the role of aes-

15. De Regt (2002) discusses how and why this hypothesis is revamped by a number of recent authors in the aesthetics of science. See also McAllister (2002).

16. Kragh (1990, p. 260) documents Dirac's aversion to philosophy.

17. I quote the entire passage in section 3.

thetic criteria in theory discovery. He wrote, first about his own methodological credo, then about Einstein's and Schroedinger's:

We may try to make progress by following in Hamilton's footsteps, taking mathematical beauty as our guiding beacon, and setting up theories which are of interest, in the first place, only because of the beauty of their mathematics. (Dirac 1964, p. 59)

When Einstein was working on building up his theory of gravitation he was not trying to account for some results of observations. Far from it. His entire procedure was to search for a beautiful theory, a theory of a type that nature would choose. He was guided only by the requirement that this theory should have the beauty and elegance which one would expect to be provided by any fundamental description of nature. (Dirac 1980, p. 44)¹⁸

(. . .) Schroedinger and I both had a very strong appreciation of mathematical beauty and this dominated all our work. It was a sort of act of faith with us that any equations which describe fundamental laws of Nature must have great mathematical beauty in them. It was a very profitable religion to hold and can be considered as the basis of much of our success. (Dirac 1977, p. 156)¹⁹

The gradual emergence of the heuristic version of the C-thesis is perhaps best captured by the contemporary experimental physicist A. D. Krisch in a short fragment of his recollections of a discussion with Dirac:

I recall one scientific discussion with Dirac in Coral Gables on what is the proper technique for searching for truth in science. Dirac stated that ' . . . the elegance of the formulation was very important in choosing the direction for one's research.' I replied that the Scientific Method suggests that the only absolute criterion for truth is the reproductibility of experimental observations. (. . .) In thinking about this discussion later, I have come to believe that the reproductibility of experimental observations may, indeed, be our only absolute criterion of truth, but the creative elegance of rare people such as Dirac may sometimes point us to that truth (Krisch 1987, pp. 51–2).

Although the heuristic interpretation of the C-thesis (i.e., taking "elegance" as a guide when trying to guess the correct physical theory) is acknowledged in the philosophical literature, it has rarely been paid thor-

18. See also Dirac (1982).

19. Quoted in Longair (2003, p. 6)

ough attention.²⁰ In the following sections I shall approach some less discussed aspects of this interpretation, trying to fill in this interpretive gap.

Before I embark on such a task let me note that a definition of mathematical beauty or mathematical simplicity is beyond the scope of this paper. I'm primarily interested in discussing the efficacy of these concepts in suggesting empirically adequate theories, not in defining them precisely. However, any account of mathematical beauty in physics should take into consideration what physicists themselves judge to be beautiful about their equations. And this raises a serious problem, since very few of those immersed in the practice of high-level physics ever attempted to formulate explicitly their reasons to assign beauty to a certain mathematical formalism. The contemporary theorist Frank Wilczek is one of these physicists, and his account, while not fully articulated, can be offered as an interesting sample of an attempt at a definition. Wilczek recently outlined an analysis of the beauty of Dirac's equation, claiming that certain analogies can help us understand why this equation is beautiful:

One feature that can make a piece of music, a novel or a play beautiful is the accumulation of tension between important, well-developed themes, which is then resolved in a surprising and convincing way. One feature that can make a work of architecture or sculpture beautiful is symmetry—balance of proportions, intricacy towards a purpose. The Dirac equation possesses both these features to the highest degree.

Recall that Dirac was working to reconcile the quantum mechanics of electrons with special relativity. It is quite beautiful to see how the tension between conflicting demands of simplicity and relativity can be harmonized, and to find that there is essentially only one way to do it. That is one aspect of the mathematical beauty of the Dirac's equation. Another aspect, its symmetry and balance, is almost sensual. Space and time, energy and momentum, appear on an equal footing. (Wilczek 2002, p. 128)

According to Wilczek then, among the features that matter when it comes to assessing the aesthetical merits of an equation, one is essential: its ca-

20. One notable exception is Norton (2000), who discusses Einstein's commitment to (what I called) the C-thesis in detail. For more on Norton's views, see below. For disagreement with some of Norton's views, see Engler (2005) and Stachel (1995). Steiner (1998) discusses heuristic at length. Less recently, Mamchur (1987) and Polkinghorne (1990) tackled the relation between aesthetics and heuristics in science.

capacity to resolve (what previously counted as) a serious conceptual tension. A natural, 'simple' way to deal with the tension and harmonize the components is a mark of its aesthetic superiority.²¹

3. The heuristic interpretation. Mathematics and physicist's imagination

The heuristic interpretation of the C-thesis holds that the scientists successfully employ aesthetic criteria (i.e. are guided by mathematical beauty and / or simplicity) in the process of theory construction. *Prima facie*, this should be no special reason for concern. Many scientists believe that *any* methods can be legitimately used in theory discovery. Consequently, *some* role for aesthetic guidance in theory construction is traditionally allowed. However, the important question is whether there is anything more to say about the capacity of the aesthetic criteria to lead to the discovery of empirically successful theories. The question is not whether the heuristic embedded in the C-thesis is infallible—no heuristics is so. My wonder is rather different, whether a holder of the heuristic version of the C-thesis can make a stronger claim, namely that there is something *special* about the relation between aesthetic criteria employed in the very invention of a theory and empirical accuracy (or truth) of a theory invented in this way. So, is it the case that true theories can be discovered on a systematic basis by following the aesthetic guidance of beautiful and simple mathematics? If the hypothesis that an aesthetically based heuristic strategy is successful is true, then further inquiry into what might explain its workings will be required. If we can find good theories by picking up those that are mathematically beautiful and / or simple it might be that this is so because there is a relation of structural similarity between the physical world and certain mathematical formalisms; or, in Einstein's words, "Nature is the realization of the simplest conceivable mathematical ideas" (Einstein 1954, p. 274). In short, the question is: is there any relation between one's heuristic and one's metaphysics?

In this section, I address two aspects of this issue. First, I note that the historical record supporting the heuristic interpretation of the C-thesis is

21. While one might find Wilczek's analysis too reductive (aren't there cases of mathematically beautiful equations that don't satisfy his canon?), his view might help us explain (partially) why Dirac thought that social, or 'subjective' factors have not much to do with mathematical beauty (see above). If the mark of an equation's beauty is its capacity to resolve conceptual tensions in an elegant way, one can easily see that this characterization of beauty is at least in part invariant under social factors. On one hand, whether or not an equation can resolve some conceptual tensions is an objective fact, independent of social, subjective influences. On the other, whether or not this resolution is 'simple' or 'elegant' is not; what counts as an 'elegant' solution may be conditioned by one's (socially acquired) conception of beauty.

impressive indeed. Moreover, I show that the historical context plays an important role in evaluating the scientists' rationality when appealing to the C-thesis for heuristic purposes. Aside from general epistemological queries on the nature of mathematical beauty, it is sometimes the 'internal' development of physics, involving historical and personal accidents, that is relevant for the conclusions one wants to draw about the reasonableness of the C-thesis. When phenomenological considerations (experiments, measurements, modeling, etc.) and other traditional scientific tools (such as thought experiments, analogical reasoning etc.) prove unable to indicate new hypotheses to test, some physicists explicitly suggested that it may be certain mathematical features of the formalism (its beauty, or simplicity, naturalness etc.) that could help with theory construction. Now, if it happens that in certain circumstances beautiful mathematics *alone* guides the formation of hypotheses, then, since some of these guesses happen to be correct, *it looks like* beautiful mathematics is a guide to the truth indeed. I will attempt to assess the credibility of such a claim.

Secondly, I remark that the successes of this heuristics should be carefully weighted against the failures, which are telling too. An interesting argument emerges to the effect that it is the failures, and not the successes that should constitute the expected outcome of the use of mathematically inspired heuristic. In other words, the success of the mathematically guided heuristic is rather exceptional—"a gift", as Wigner (1960) would put it.²² I discuss these two aspects in turn. Let me begin with the successes of the C-thesis.

It is customary among physicists to view mathematics not just as a precise language (both in the descriptive and computational sense), but as an indispensable tool for theory discovery²³ as well. The celebrated contemporary physicist Freeman Dyson writes:

One factor that has remained constant through all the twists and turns of the history of physical science is the decisive importance of mathematical imagination. (. . .) 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. (Dyson 1964, p. 129)

Dyson, it is true, speaks here about mathematics *per se*, not about beautiful mathematics. But when he goes on and presents a number of cases in

22. Wigner (1960), however, did not discuss the guidance of mathematics in theory construction, but the appropriateness of mathematical concepts in the description of physical world.

23. Field (1980) argues against the first part of this claim and ignores the second.

which reliance on mathematics led to important discoveries, the most spectacular example he dwells on is Einstein's GTR. And this example, as we'll see below, involved not only mathematics per se, but certain aesthetic features (simplicity, or "beauty and elegance" as Dirac notes above) of the mathematical formalism. Einstein is thus another well-known case of great physicist who endorsed a heuristic version of the C-thesis. In the Herbert Spenser Lecture of 1933, he notes:

Our experience hitherto justifies us in believing that nature is the realization of the simplest conceivable ideas. I am convinced that we can discover by means of purely mathematical constructions the concepts and the laws connecting them with each other, which furnish the key to the understanding of natural phenomena. Experience may suggest the appropriate mathematical concepts but they most certainly cannot be deduced from it. Experience remains, of course, the sole criterion of physical utility of a mathematical construction. But the creative principle resides in mathematics. (Einstein 1954, p. 274)²⁴

Other examples of successful guidance offered by aesthetically pleasant mathematics in theory construction can be documented. Schroedinger is another case in point discussed by Dyson. The conviction that "the laws of nature must have mathematical beauty" led Dirac to some of his most important discoveries. In a passage strikingly similar to the above quotation from Einstein, Dirac makes the following remarks about the role of pure mathematical manipulation in discovering empirically accurate physical laws:

One may describe this situation by saying that the mathematician plays a game in which he himself invents the rules while the physicist plays a game in which the rules are provided by Nature, but as

24. Norton (1995) and (2000) discusses Einstein's case at length. He points out that Einstein's attitude toward the heuristic power of mathematics describes in fact a continuum with two extremes: the young Einstein, who was rather skeptical over this power (Norton 2000, p. 155) and the mature Einstein of 1933, who advocates the opposite view. Norton argues that when Einstein speaks about "our experience" (see quotation), "this experience is not just the communal experience of scientists" (Norton 2000, p. 137). According to Norton, Einstein alludes to one of his own most thrilling experiences, involving his greatest discovery, the GTR field equations. More specifically, Norton (1995, p. 62) reveals that Einstein's "experience" refers to his frantic November 1915 race for deriving the GTR field equations. Hilbert, who proceeded through pure formal methods, communicated the equations five days before Einstein (!), who, as Norton argues, was able to derive them (and get full credit, eventually) as a result of finally trusting the heuristic power of mathematics.

time goes on it becomes increasingly evident that the rules which the mathematician finds interesting are the same as those which Nature has chosen (Dirac 1939, p. 124).

In the passage quoted above, Dirac does not refer to mathematical beauty explicitly, but to mathematics *simpliciter*. However, when Dirac calls a theory “interesting”, he usually means that what stirs up this interest is its beauty²⁵. According to Dirac then, those physicists endowed with an uncorrupted sense of mathematical beauty and strong convictions in the mathematical harmony of the world have the best chance to succeed in creating new theories. Like Einstein, Dirac confesses his personal experience, since he himself illustrated such a mixture of skillful mathematician and lucid physicist. Similarly, in his 1972 article *Missed opportunities*, Freeman Dyson complained about the fact that mathematics could have prefigured many physical discoveries, were the communication between the mathematical and physical minds better. Referring to his own case, he pointed out that this mutual ignorance could manifest even when the mathematician and the physicist inhabit the same body.

At this point, one may wonder why did the physicists need to appeal to the non-physical, aesthetical guidance of mathematics to make discoveries in physics? What determined physicists to appeal to such peculiar a methodology in the first place? I argue that physicists’ commitment to this heuristic form of Pythagoreanism was rationally justified on pragmatic grounds. The circumstances in which they relied on such heuristics have to do with a more general problem: the limitations of the human mind’s capacities when faced with daunting physical problems, especially in dealing with the quantum domain. It is generally acknowledged among physicists and philosophers of science that part of the difficulty in understanding atomic and subatomic phenomena resides in their remoteness from everyday human experience. Few (if any) processes and entities one studies at the micro level resemble those in the macro world. The first limitations one faces are, naturally, of experimental nature; the most severe ones are, however, conceptual. Not only did the physicists have a rough time in understanding and explaining quantum phenomena; with the advent of quantum mechanics, the very notions of ‘scientific understanding’ or ‘scientific explanation’ became subject of debate among physicists. This was so in part because most of the traditional concepts (e.g., trajectory, position, particle, force, etc.) and criteria (such as criteria of identity) imported by physicists from the macro level turned out to be, in most cases,

25. See Dirac’s reference to “theories which are of interest, in the first place, only because of the beauty of their mathematics” in Dirac (1964, p. 59). I quoted the whole passage above.

ambiguous, inappropriate at the micro level. Despite these limitations, at the beginning of quantum theorizing some use has been made of the classical concepts and theories, and I allude here at Bohr's principle of complementarity (roughly stating, in one of its various formulations, that quantum descriptions should recover, in the limit, the classical descriptions.) However, from a certain point on, developing the quantum theory under the guidance of the complementarity principle became an unreliable practice. Heisenberg writes:

One may say that the concept of complementarity, introduced by Bohr (. . .) has encouraged physicists to use an ambiguous rather than unambiguous language, to use classical concepts in a somewhat vague manner . . . When this vague and unsystematic use of language leads into difficulties, the physicist has to withdraw into the mathematical scheme (Heisenberg 1958: 154–5)

Once one departs from the macro world one loses the grip on the traditional language and imagery of physics. So, one might even advance the alarming hypothesis that the human mind might not be equipped to confront these difficulties. Dirac outlined this crisis-view in the introduction of his 1931 paper:

There are at present fundamental problems in theoretical physics awaiting solution, e.g., the relativistic formulation of quantum mechanics and the nature of quantum nuclei (to be followed by more difficult ones such as the problem of life), the solution of which problems will presumably require a more drastic revision of our fundamental concepts than any that have gone before. Quite likely, these changes will be so great that it will be beyond the power of human intelligence to get the necessary new ideas by direct attempts to formulate the experimental data in mathematical terms.

What is the remedy then? Dirac suggests the following:

The theoretical worker in the future will therefore have to proceed in a more indirect way. 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, p. 60)²⁶

26. In accord with Dirac's methodological aestheticism, I interpret his urge to "perfect" the formalism as an urge to "look for a theory with a still more beautiful mathematical ba-

Heisenberg's and Dirac's Pythagorean prophecies were fulfilled. The "withdrawal" into mathematics recommended by them has taken place, yielding new insights and helping extend our theoretical knowledge. One such spectacular case (because of its wide implications) is the accomplishment of the electro-weak unification within the Standard Model of particle physics by Glashow, Weinberg and Salam in the 70's. The fascinating story of this unification illustrates the Heisenberg-Dirac methodological views. The exploration of this unification is carried out in great detail in M. Morrison's recent book (2000) and her conclusion is directly relevant for the issue regarding the special heuristic role of mathematics (gauge theory and various symmetries) in (unified) theory construction. According to her analysis, "[T]he unification achieved by the Glashow-Weinberg-Salam model emerges out of the formal constraints of gauge theory and the identification of particles with a particular symmetry group." (Morrison 2000, p. 126). That is, considerations from the physical phenomenology played by no means the major role in suggesting to the theoreticians the direction of research. Instead, the very possibility of integration of the two different mathematical structures into a single, harmonious symmetry ($U(1) \times SU(2)$) constituted the driving force. Thus, one can describe the search for (electro-weak) unification in quasi-aesthetic terms: if achieving this sort of harmony and elegance in describing nature is mathematically possible, then why not pursue it, despite the important differences found in the phenomenology of electromagnetic and weak interactions²⁷: "[I]n this case [electroweak unification] the mechanisms in-

sis" (see the passage quoted above). Worth noting, Einstein himself faced a similar crisis while working on his GTR. He suggested a similar, aesthetically-guided solution: by following mathematical simplicity one arrives at the construction of the correct theory. Here is a passage from his 1949 *Autobiographical Notes*: "I have learned something else from the theory of gravitation: no collection of empirical facts however comprehensive can ever lead to the setting up of such complicated equations [the equations of the unified field]. A theory can be tested by experience, but there is no way from experience to the construction of a theory. Equations of such complexity as are the equations of the gravitational field can be found only through the discovery of a logically simple mathematical condition that determines the equations completely or almost completely. Once one has obtained those sufficiently strong formal conditions, one requires only little knowledge of facts for the construction of the theory; in the case of the equations of gravitation it is the four-dimensionality and the symmetric tensor as expression for the structure of space that, together with the invariance with respect to the continuous transformation group, determine the equations all but completely." (Einstein 1979, p. 85)

27. I'm referring here of course to the difference in mass exhibited by the carriers of these two forces, the photon for electromagnetism and the three bosons (W^+ , W^- and Z^0) for the weak force. The renormalizability of the theory aside, the electro-weak unification had to overcome this crucial difficulty. And it did, due to an ingenious idea proposed by

volved in producing unity say more about the kinds of mathematical models and structures at the theoretician's disposal than about the ontological status of phenomena themselves" (Morrison 2000, p. 135). Morrison's discussion contains a lucid analysis of the experimental and theoretical difficulties the unification had to overcome; despite them, the unified theory of the weak and electromagnetic interactions "was achieved because of the power of the mathematics to generate a kind of particle dynamics that provided the form of various field interactions. As a result, the mathematical structure can, in some sense, be seen to dictate how the physics of the theory is fleshed out." (Morrison 2000, p. 114).

To sum up, then. The scenario put forward by the holder of the heuristic version of the C-thesis looks roughly like this. At the beginning of the quantum era physics faced a "grave crisis of ideas"²⁸, since the traditional tools employed in theory construction turned out not to be transferable to the micro world phenomena. Experimentation and manipulation in (sub)atomic physics were often at an impasse too, for obvious reasons. An interesting general suggestion was made as how to advance the research, Bohr's complementarity principle. But, as Heisenberg pointed out, this guidance soon became unreliable, and Dirac came up with his Pythagorean, "indirect way": develop the mathematical formalism in the direction indicated by your sense of beauty ("perfect" the formalism, as Dirac says), and then hope that it will guide your physical theory in the right direction. This happened indeed and, more often than not, the method worked well. Mathematical beauty served as a guide to physical truth indeed; moreover, the context was such that nothing else could help—i.e., no much help from the phenomenology of physics was available. So, the holder of the C-thesis wants to conclude, there must be something special about the heuristic connection between mathematical beauty and physical truth after all.

In terms of particular physicists involved in this story, Dirac and the mature Einstein are the paradigmatic illustrations of supporters of the heuristic version of the C-thesis. As is well known, Dirac's prediction of the positron is the immediate result of this heuristic. He thought that his 1928 four-dimensional matrix relativist equation for the electron was simply too beautiful to be discarded because of the negative-energy solutions. The physical interpretation of these solutions (as positive-energy anti-matter particles) predicted the positron²⁹ and thus anti-matter became

P. Higgs, who developed a mechanism able to explain how it is possible that the bosons become massive while the photon remains massless.

28. In Schroedinger (1980, p. 11).

29. The positron was detected experimentally by Anderson in 1932.

physical reality. As we saw, Einstein was another famous holder of the aesthetically based heuristics, although, as aestheticians of science rightly note, he seldom used the word 'beauty' when praising a certain formalism, preferring 'simplicity' instead. (Osborne 1984, p. 295)³⁰

Einstein's and Dirac's work are perhaps the most successful demonstration of the usefulness of holding the C-thesis. However, if one implies that such a conviction is successful on a regular basis, then one is most likely wrong. The historical cases in which this conviction proved useful are, as we have seen, impressive; yet the situations in which the belief in the heuristic version of the C-thesis hampered (or was felt as hampering) physicists' imagination are numerous too. I now pursue a cursory examination of some of these later cases. These quick historical reminders provide a more faithful representation of the historical record; for obvious reasons, people tend to forget the failures and record only successes.

So, the main claim to be defended in the remaining of this section is that either some of the work done under mathematics' aesthetic guidance proves worthless, or that this aesthetic guidance played, for some important figures in physics, no role whatsoever—fact explicitly acknowledged by these physicists themselves. Dyson (1964) coins the name "mathematical conservatism" to describe the dampening force mathematics may exercise on physicist's imagination. The first important illustration of how a mathematical-aesthetic conservative mind works is the ancient Greek astronomer Ptolemy and his conviction that only circles can be orbits of planets. A great deal of the sophisticated methods of the Ptolemaic epicycle astronomy becomes comprehensible if we take into account the tension generated by Ptolemy's desire to accommodate a wrong-headed aesthetic bias toward circular motion with the important task of accounting for planets' trajectories observed in the sky. If one circular motion couldn't do the job, it turned out that a clever combination of two circular motions could, hence the elaboration of the (empirically adequate but false) epicycle theory. Kepler's case is, again, revealing. As Dyson remarks, his great discoveries were achieved after a "tooth-and-nail" struggle against his own mathematical and aesthetical intuitions. The histories of science have it that the greatness of Kepler lies in his unrelenting, heroic search for the correct trajectories. According to the version of the story I credit here, this is only half of the truth—and not the most relevant half. It is certainly Kepler's great merit that he did not give up the search and finally found out the correct trajectories; but there is an even greater merit

30. Osborne also notes that Y. Elkana's statement "for Einstein, simplicity was equivalent to beauty" (Elkana 1982, p. 222) "is not false, although somewhat too forthright." (Osborne 1984, p. 299, endnote 15)

that this happened *despite* his own aesthetic convictions. So, from the point of view of the C-thesis, the Keplerian story is a complicated business involving at least two antagonistic components. On one hand, there is his (Pythagorean) conviction that a deep correlation can be found between (what he took to be beautiful) mathematics and the physical world. On the other hand, one must praise his wise decision to set aside, when writing down his theories, some of the things implied by the above Pythagorean conviction (namely, the supremacy of the circle in astronomical theory.)

More recently, one may remark that Bohr's great pioneering contributions made at the beginnings of quantum theory had nothing to do with trusting mathematical intuition. His work displayed lack of sympathy toward following the lead of mathematical beauty in constructing physical theories. (As already noted, mathematical beauty in particular meant not much, if anything, to Bohr.) Apparently, this unfeeling attitude, not to say aversion, can be explained by Bohr's notorious lack of mathematical skill, which is the subject of various anecdotes.³¹ "Physical understanding should precede mathematical formulation" said the mathematically clumsy Bohr³² but Feynman, who was a much more mathematically gifted physicist, often expressed similar thoughts.³³

A number of episodes involving Dirac himself are worth noting too. They show that even the master of mathematical beauty heuristic can't deny the limited practical utility of the heuristics inspired by the C-thesis. In an interview with Kuhn in 1963, Wigner spoke about Dirac's monomaniacal fascination with the Hamiltonian formalism since the beginning of his career in the 20s. In his Larmor Lecture in Dublin in 1963, Dirac reinforced this fascination, stating in a clear form the heuristic version of the C-thesis indeed. He said:

We may try to make progress by following in Hamilton's footsteps, taking mathematical beauty as our guiding beacon, and setting up theories which are of interest, in the first place, only because of the beauty of their mathematics. (Dirac, 1964, p. 59)

31. Beller (1999, p. 259) notes John Slater's reaction when he came to work with Bohr in Copenhagen: "I had supposed (. . .) that although Bohr's papers looked like hand-waving, they were just covering up all the mathematics and careful thought that had gone on underneath. The thing I convinced myself of after a month was that there was nothing underneath." This is consistent with Pauli's remark that Bohr's "interest in physics was not so much that of a mathematician as that of a craftsman and that of a philosopher" (Pauli 1964, 2: 1052).

32. Quoted in Blaedel (1988, p. 111).

33. See his diatribe against mathematics in his discussion with A. Zee in Mickens (1990, p. 310).

But, follows Wigner, Dirac's total commitment to this formalism resulted in a failure to establish the commutation relations for fermions in 1927. Another episode involves the famous monopole paper of 1931, which ends with the prophecy that given the beauty of the theory predicting the existence of monopoles, "one would be surprised if Nature had made no use of it." Even Dirac's caustic friend W. Pauli³⁴ considered the monopole theory beautiful; notwithstanding this opinion, he amended Dirac's insistence in pushing forward his speculative views by referring to him as 'Monopoleon.'³⁵ Needless to say, no monopole has been discovered experimentally so far and although certain gauge fields theories (such as SU(5)) predicted monopoles too, the characteristics of the 'new' monopole differs from Dirac's monopole.

The morale of these not-so-well-known stories is that more caution is required when we claim the success of the heuristic guide embedded in the C-thesis. What is so special, one may ask, about the aesthetic guidance offered by beautiful mathematics after all? Several wonderful successes are systematically mixed up with delusions, so one may begin to think that the usefulness of this guide is rather limited, a matter of good fortune. Moreover, several great physicists did well without the guidance of beautiful mathematics. Hence, it might be illusory to believe that the C-thesis supplies a general, relatively reliable method to unearth the secrets of the universe. This view is widely spread among professional physicists today. Steven Weinberg tells a story about a talk that Dirac gave to an audience composed mostly of students. In his talk, Dirac attempted to persuade them to ignore what the equations of physics mean, and to pay attention to the beauty of those equations instead. Weinberg remembers that the faculty attending the talk "groaned" at the prospect that the students will set out to follow Dirac's advice.³⁶

So, the skeptics may object that caution when claiming the "success" of the C-thesis is not enough, and that the flat rejection of the success claim is actually the correct stance to take. Thus, it is not just that the C-thesis has *occasionally* hampered physicists, since, as noted, the guidance of mathematical beauty is sometimes imprecise. It's worse, the skeptics argue. They plainly speak about the "pernicious influence of mathematics on science", as the contemporary mathematician Schwartz called it (Schwartz 1986). Failure in making physical discoveries following the heuristics proposed by the C-thesis is the rule, and not the exception. Schwartz points

34. "From a purely logical point of view the magnetic poles seem to be more satisfactory than the magnetic dipoles." Quoted in Kragh (1990, p. 215).

35. In a 1931 letter to H. Bethe, quoted in Kragh (1990, p. 219).

36. In McAllister (1996, p. 90)

out to the fundamental differences in the nature of the two disciplines and echoes Kuhn's distinction between 'classical sciences' (e.g., mathematized astronomy) and 'Baconian', experimental sciences. Kuhn writes:

This separation between the classical and Baconian sciences can be traced from the origin of the later. Bacon himself was distrustful, not only of mathematics, but of the entire quasi-deductive structure of classical science. Those critics who ridicule him for failing to recognize the best science of his day have missed the point. He did not reject Copernicanism because he preferred the Ptolemaic system. Rather, he rejected both because he thought that no system so complex, abstract and mathematical could contribute to either the understanding or the control of nature. His followers in the experimental tradition, though they accepted Copernican cosmology, seldom even attempted to acquire the mathematical skill and sophistication required to understand or pursue the classical sciences. (Kuhn 1977, p. 48)

Schwartz's points are similar to Kuhn's. According to Schwartz, mathematics, in order to be useful, must deal with well-defined situations. But real physical situations are, by their very nature, complex, intricate, messy. The precondition of usefulness of abstract mathematics in theory construction is that the physical situation be described by an essentially simple model, containing only a small number of parameters (or, in cases where the physical situation is complex, it must happen that only a few parameters actually matter). If this high degree of modeling is not achieved, i.e., if we cannot "relax" the axioms (1986, p. 21), all that mathematics succeeds to do, claims Schwartz, was to prove "that the fundamental objects of the scientist's calculations do not exist." (1986, p. 21). Schwartz does not speak here only against "beautiful mathematics"; his target is all kind of mathematics, including those domains traditionally considered 'ugly' or cumbersome mathematics—such as differential equations. Also, not only does he contrast the rigor characteristic to mathematics with the loose arguments so often employed by physicists; he also underscores the differences in what we broadly call 'styles of thinking'. These differences might prove crucial in employing mathematics as a guide to theory construction in physics. Schwartz concludes:

The literal-mindedness of mathematics thus makes it essential, if mathematics is to be appropriately used in science, that the assumptions upon which mathematics is to elaborate be correctly chosen from a larger point of view, invisible to mathematics itself. (. . .) Mathematics is able to deal successfully only with the sim-

plest of situations, more precisely, with a complex situation only to the extent that rare good fortune makes this complex situation hinge upon a few dominant simple factors. (. . .)

That form of wisdom which is the opposite of single-mindedness, the ability to keep many threads in hand, to draw for an argument from many disparate sources is quite foreign to mathematics. (Schwartz 1986, p. 21–2)

Dyson (1964) partially concurs with Schwartz's opinion. Dyson stresses the "two-edged" feature of mathematics, its capacity to be both good and bad for creative work in physics. As already pointed out, the reasons for this dualism lie, Dyson notes, "in the nature of mathematics itself." Dyson starts by partially agreeing with Mach, who once remarked that "The power of mathematics rests on its evasion of all unnecessary thought and on its wonderful saving of mental operations."³⁷ This evasion is a good thing, says Dyson, because it "gives freedom to the imagination." Yet, after this initial agreement, he points out the essential fact that "many situations in science demand for their understanding not the evasion of thought but thought." (Dyson 1964, p. 133).

4. Pythagoreanism: metaphysics and heuristics

The success of the heuristics embodied in the C-thesis is thus far from being established. The C-thesis is, I conclude, at best open to further exploration. But what about its underlying metaphysics? Traditionally, this metaphysics is identified as Pythagoreanism. In fact, two Pythagorean themes should be distinguished. First, there is an ontological theme: physical objects *are*, in so far as they are composed of elementary particles, mathematical objects.³⁸ A weaker but perhaps more easily defensible version of the Pythagorean idea emphasizes not objects' constitution, but the structure of their relations: the world instantiates a beautiful, harmonious pattern. Now, associated with this structuralist view, an epistemological theme can be discerned, centered on the idea that humans can find the reflection of this harmony in beautiful mathematics. I take the coupling of these last two tenets to constitute the hard core of what I call here Pythagoreanism, a traditional name for the Heisenberg—Dirac—Einstein meta-physical doctrine holding that the mathematical equations describing na-

37. In (Dyson 1964, p. 133).

38. See (Heisenberg 1998, p. 219): "The particles of modern physics are representations of symmetry groups and to that extent they resemble the symmetrical bodies of Plato's philosophy".

ture must possess aesthetical qualities (such as simplicity, symmetry, beauty, etc.)

Now, suppose that the exploration of the heuristic interpretation of the C-thesis carried out so far would have revealed an undisputable, systematic success of this thesis as a heuristic guide. Then, a natural question arises: can one infer the correctness of Pythagoreanism, the underlying metaphysics of this thesis, via an inference to the best explanation? For the (best) explanation of the success of the C-thesis in tracking accurate theories would be that the world was, in essence, Pythagorean. Hence, scientists' commitment to Pythagoreanism would have been vindicated. Yet, as we saw, the success of the Pythagorean heuristic guide embedded in the C-thesis is qualified. So, the C-thesis' limited success is by no means sufficient to vindicate Pythagoreanism as a credible metaphysical position. For this reason, a second question crops up: what is scientists' rationale for embracing this metaphysics then? By asking this, I intend to go beyond the obvious possibility that they could have adhered to Pythagoreanism regardless of its (limited) capacity to yield palpable consequences. They could hold it fast no matter what, regarding it like a sort of religion—and Dirac confessed this pretty clearly when, in one of the above quotations, he compared his adherence to Pythagoreanism with holding a religious view.

Let me address the second issue first. To begin with, it is important to highlight that the adherence to Pythagoreanism is not necessarily incompatible with the broader philosophical, 'Baconian' qualms advanced by Dyson and Schwartz. It is true that there are profound differences between mathematical and physical thinking and it is true that blindly trusting the path sketched by the mathematical formalism can easily lead one astray. Granted, the Pythagorean heuristics is not infallible. But this misses the point, the supporter of the C-thesis may retort; no heuristic strategy is so, otherwise there would be no difference between ordinary and revolutionary physicists. The important point can be perceived only by those in the working physicist's shoes. As we've seen above, in theoretical physics there is something definitely worse than going astray sometimes, namely standing still, not moving at all. Even Baconians ought to agree that when everything else is perceived as unhelpful in theory discovery (for the reasons sketched above), even following the lead of abstract mathematical formalism becomes an option. So, holding the heuristic interpretation of the C-thesis amounts to adopting the policy of going minimal, for the least evil. True, looking for mathematical beauty and mathematical simplicity does not always work, but when no other strategy looks credible, following the lead of mathematical beauty and simplicity in inventing physical hypotheses might help. So, it is not that physicists

picked up mathematical beauty and simplicity to lead their way (i.e., became Pythagoreans) while having plenty of other options available. They somewhat felt they *had to* make this move, since no other strategies looked more credible. One has to agree that, understood in this (minimalist) spirit, Einstein's and Dirac's Pythagoreanism no longer looks outrageous.

The answer to the first question concerns the way we should understand the connection between the mathematically driven heuristics and its success. This success, as noted, is often impressive. Yet, while we can elucidate the failures (the Baconian Dyson-Schwartz argument above), we still lack a similar explanation of successes. What does this (limited) success show? How can we account for it? Does this success show that Pythagoreanism is a credible metaphysics? The most serious problem for the Pythagoreans seems to come from the fact that the heuristic success of mathematics in theory construction *may be* illusory, in the following sense. If, for the reasons that Dirac, Heisenberg and Einstein outlined above, the aesthetic qualities of the mathematical formalism were perceived to be the main (or the only) driving force in generating physical hypotheses, then it is somehow expected that *some* of these guessed hypotheses are successful. If this happens, one may conclude that the mathematically-aesthetically driven heuristic is successful *simpliciter*. Yet this conclusion, drawn in this context, is rather hasty. This success might not have so much to do with the relation between mathematics and the world (i.e., *contra* Pythagoreans, no mathematical quality of nature is proven). If this success shows anything, then it shows something (trivial) about physicists' relation to mathematics: namely, that monomaniacal insistence in using this aesthetically inspired heuristic pays off.

So, is there anything special about the role mathematical beauty plays in successful theory construction? Contrary to what both sides (Pythagoreans and Baconians) may expect, it is surprisingly hard to tell. The historical record shows impressive successes but, when examined carefully, it reveals failures too. The Pythagoreans (Dirac, Heisenberg, Einstein, etc.) thought that mathematics should be privileged as the leading force of physical research when theory discovery was difficult. In one form or another, they all thought that, as Einstein put it, "the creative principles reside in mathematics"—more exactly, in picking up those physical theories possessing mathematical beauty or simplicity. They were right—not always right, but more often than not. Does this show anything about the relation between beautiful mathematics and physical truth? Once one highlights the failures of this strategy, one becomes tempted to ascribe the successes not to Nature's intrinsic qualities but to physicists' insistence in applying the same mathematically driven heuristic strategy time and again—insistence justified by the belief in the lack of a better alternative.

To this, the Pythagoreans may reply that it is insistence *in the right direction* that pays off. If one wants to get a hint at the structure of the subatomic particles by importing the structure exhibited by the rules of baseball, the result will certainly be disappointing. Essentially, not the same is the case when one follows the lead of certain symmetries in group theory, that is, some of “the rules the mathematician finds interesting”. So, the holder of the C-thesis claims, there *might be* something special about the role of beautiful mathematics in theory construction after all.³⁹

5. Concluding remarks

I began the analysis of the C-thesis by reviewing some of the arguments for the rejection of the ‘beauty-entails-truth’ correlation, arguments assuming that this correlation makes a point in the context of theory justification. After assessing some historical evidence, I found this thesis untenable. In the next sections I explored the less discussed view holding that the C-thesis may play a role in the context of theory discovery. I’ve argued that we should judge the rationality of scientists’ adherence to Pythagorean heuristics from inside, so to speak, taking into account the special circumstances in which this heuristics flourished. Quantum physicists’ endorsement of the C-thesis can be explained, I argued, on pragmatic grounds: they thought it was the only strategy that might help them in making scientific progress. Not only did this heuristics seem profitable, but it also seemed the only heuristic one could embrace as long as the difficulties involved in writing the quantum theory were, as Dirac feared, “so great that it will be beyond the power of human intelligence” to conquer them. It was the desire to overcome the heuristic crisis in quantum physics that supplied the reasons for the endorsement of the otherwise rather mystical Pythagorean doctrine. I found that the evidence gathered in favor of the success of this thesis (viewed as a heuristic guide) is impressive. However, the failures are telling too and I concluded that one should suspend her judgment on the status of the C-thesis.

One important aspect discussed throughout the paper was the relation between this thesis and its underlying metaphysics, identified as Pythag-

39. Pythagoreanism’s bright side (i.e., the success of this methodology) is discussed at length in literature by Mark Steiner, in his 1998 wonderful book *The Applicability of Mathematics as a Philosophical Problem* (Harvard Univ. Press.) Steiner begins with Pythagoreanism and attempts to make a case for a revival of a form of anti-naturalism, or, as he calls it ‘anthropocentrism’—the quasi-theistic view that the human mind holds a privileged, central place in the scheme of things. Steiner’s conclusion about anthropocentrism is challenging, given that the whole modern science is usually characterized as anti-anthropocentric. There is much to say with regard to this thoughtful argument, but I will not discuss it here.

oreanism. Two natural questions emerged. First, one may wonder whether the failures of Pythagorean heuristics speak against this metaphysics. I claimed that they do: if a particular metaphysics (Pythagoreanism) generates a certain methodological guide (the C-thesis) whose practical value is dubious, it follows that the metaphysics in question is *prima facie* dubious too, and should perhaps be abandoned. Nevertheless, no significant step back from Pythagoreanism was recorded—on the contrary⁴⁰. Obviously, scientists could endorse their favorite metaphysics regardless of its palpable consequences, as an article of faith, with no rational basis. If so, in light of the failures, their Pythagoreanism appears as an unsubstantiated tenet.

Secondly, but equally important, I asked whether the successes of the heuristics embedded into the C-thesis speak in favor of the Pythagorean metaphysics. I found that a definite answer was difficult to give. Apparently, we should answer in affirmative. Yet repeated, insistent application of the same aesthetically based methods of discovery may yield results over time, so this success is not conclusive. However, invoking insistence is not sufficient to dismiss Pythagoreanism, since it is insistence *in the right direction* that can pay off. So, the success of the heuristic based on beautiful mathematics is still mysterious: what does mathematical beauty have to do with ‘the right direction’ after all? or, as Dyson put it “Why should nature care about our feelings of beauty? Why should the electron prefer a beautiful equation to an ugly one?” (Dyson 1986, p. 103).

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40. One possibility is that the scientists have not even perceived the failures, though this is rather unlikely; for instance, it is known that Dirac agreed (later in his life) that monopoles might not exist.

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