Confirmation, or Pursuit-Worthiness? Lessons from J. J. Sakurai's 1960 Theory of the Strong Force for the Debate on Non-Empirical Physics

Pablo Ruiz de Olano

Abstract

Over the last few decades, our theories of fundamental physics have become increasingly detached from empirical data. Recently, Richard Dawid has argued that the progressive separation of theory from experiment is concomitant with a number of changes in the methodology of the discipline. More precisely, Dawid has argued that the new methods of fundamental physics amount to a form of non-empirical confirmation, and that physical theories may therefore be confirmed even in the absence of empirical data. In this paper, I critically engage with Dawid's views on non-empirical physics. My main target is the excessively central role that, in my view, the notion of non-empirical confirmation plays on Dawid's analysis. I will therefore argue that, while non-empirical methods may legitimately be employed in physics, those are not always deployed with the purpose of confirming scientific theories. Non-empirical arguments may also be used in order to ground pragmatic choices regarding what theories deserve to be further developed—and this is an aspect of the work that non-empirical methods perform that cannot be solely understood in terms of Dawid's notion of non-empirical confirmation. I support these claims by making use of a case-study from the early history of particle physics. The case-study concerns a theory of the strong force that J. J. Sakurai introduced in 1960. As we shall see, both the genesis of Sakurai's theory as well as the arguments that he used to defend it provide direct support for my own views on the role that non-empirical methods play in physics. Finally, I conclude the paper by introducing a notion that I believe is useful in making sense of the manner in which the pragmatic and the epistemic dimensions of non-empirical reasoning relate to each other, namely the notion of a cognitive attitude.

1 Introduction

Over the last few decades, our theories of fundamental physics have become increasingly detached from empirical data. The Standard Model of Particle Physics, for example, could be extensively tested soon after its formulation during the late 1960s and early 1970s. The first Grand Unified Theories that were developed immediately afterwards, however, made very little contact with experience already. And nowadays, String Theory offers very few prospects of empirical testability.

Recently, Richard Dawid has examined the deep methodological changes that underlie these developments. In his view, the progressive detachment of theory from experiment is concomitant with a transformation of the manner in which theories of fundamental physics are evaluated. As empirical testing became more and more difficult, Dawid claims, physicists developed new strategies to determine what theories ought to be trusted. These strategies rely on what Dawid calls non-empirical evidence, and they boil down to three arguments that can be used to argue that a theory is empirically adequate within a given domain—or in Dawid's own terminology, to argue that a given theory is *viable*. Dawid's central thesis is that the deployment of these three arguments in support of a theory amounts to a form of non-empirical confirmation, and that scientific theories can therefore be confirmed even in the absence of empirical data.

In this paper, I will critically engage with Dawid's account of the role that non-empirical methods play in physics. My criticism will be directed at the somewhat monolithic role that non-empirical confirmation plays in Dawid's analysis. Dawid, as we shall see, appears to assume that his concept of non-empirical confirmation suffices to capture all that is methodologically significant in the manner in which physicists operate in contexts in which empirical testing is hard or impossible to attain. My own position will amount to a denial of this last claim. Although I concede that Dawid's notion of non-empirical confirmation may capture something important about the manner in which physics proceeds in the absence of empirical data, I will deny that all methodologically important forms of non-empirical reasoning reduce to it.

More precisely, I will argue in favour of the following two claims. First, I will agree with Dawid that non-empirical methods can legitimately be employed in physics, and that these methods become important in situations in which empirical evidence can provide little or no guidance. Importantly, this includes the three arguments that Dawid sees as capable of achieving non-empirical confirmation. I will disagree with Dawid, however, in one crucial respect. My second claim will thus be that Dawid's three arguments, and non-empirical methods more generally, are not necessarily deployed with the purpose of confirming scientific theories. These arguments can also be used, in my view, in order to ground pragmatic choices regarding what theories are worth pursuing further. It is part of this second claim of mine that this constitutes an important aspect of the role that non-empirical methods play in physics, which cannot be solely understood in terms of Dawid's notion of non-empirical confirmation.

I will defend these views by making use of a case-study form the early history of high-energy physics. The case-study concerns a theory of the strong interaction that Jun Jon Sakurai introduced in 1960 and it provides direct support for the two main claims in the paper. This is as follows. As we shall see, Sakurai was unable to derive explicit quantitative predictions from his theory. Because of this, and in spite of the abundant empirical data available at the time, he was forced to rely on non-empirical evidence to defend his theory. The main argument that Sakurai used for this purpose, in fact, very closely resembled one of Dawid's three arguments, namely the Unexpected Explanatory Interconnections Argument, or UEA. This supports the first one of the two main claims above. Sakurai's goal in making use of the UEA, however, did not consist of non-empirically confirming his theory or of otherwise arguing that the theory was viable. As Sakurai himself admitted, there were serious theoretical problems that prevented his theory from being viable. His primary goal in making use of the UEA, therefore, consisted merely of showing that his theory was worth investigating further. This supports my second claim concerning the role that non-empirical methods can play in grounding pragmatic assessments of a theory's pursuit-worthiness.

Although it may look like these two claims do little to undermine Dawid's views, they do raise important difficulties for his overall framework. The reason is that, as we shall see, Dawid's account of non-empirical theory confirmation is partly based on a historical argument. According to Dawid, for instance, arguments such as the UEA would have been used over the history of physics in order to guide theory development. Their alleged role in leading to the successful completion of the Standard Model, in fact, counts in Dawid's eyes as a consideration in favor of his theory. However, a crucial component of this historical claim is that Dawid's three arguments would have guided theory development solely via the mechanism of non-empirical theory confirmation. By bringing to the surface historical uses of the three arguments that do not fit this picture, then, one may challenge the historical grounds on which Dawid's take on non-empirical confirmation rests. This exercise, furthermore, can give us important clues concerning what is missing in Dawid's framework and how to perfect it further.

The paper is divided into five different sections. Section 2 is devoted to Dawid's work on non-empirical physics. The section introduces the main problems that motivate Dawid's work and the manner in which he chooses to address them. It also highlights the main aspect of Dawid's position that will be subject to criticism in the paper, namely the very central role that non-empirical confirmation plays in it. Having done that, I go on to introduce the details of my case-study in Section 3. I then devote Section 4 to showing that the case-study does indeed support my own critique of Dawid's position. The section also contains a suggestion on how to make sense of the pragmatic dimensions of non-empirical reasoning, and a few thoughts on the importance of doing so for understanding current developments in fundamental physics. Finally, Section 5 summarizes the paper and states its main conclusions.

2 Dawid's Work on Non-Empirical Confirmation

Over the last few decades, our theories of fundamental physics have become increasingly detached from experience. As our case-study in Section 3 below will illustrate, experimental results could only offer partial guidance in the development of the Standard Model. Once the Standard Model was formulated during the late 1960s and early 1970s, however, it could immediately be subject to empirical testing. Early experimental tests were successful and it was soon clear that the Standard Model was likely to provide an empirically adequate description of subnuclear phenomena. But as physicists started pondering over what the next step in providing a fundamental description of physical reality would be, things changed. And thus we have that, since the 1970s, all theories purporting to describe physics beyond the Standard Model have made very little contact with experience, if at all. This includes the various Grand Unified Theories formulated from the mid-1970s onwards, Supersymmetry, Loop Quantum Gravity and, particularly, String Theory.[1]

[1]The development of fundamental physics during the last decades of the twentieth century has not been the subject of much scholarly work. As a result of that, the overall trend of detachment of theory from experiment has not received much attention either. A few works can be useful in this respect, however, and they deserved to be mentioned here. Dawid himself devotes some attention to this topic, for instance, as do the final chapters of Andrew Pickering's classic book on the history of high-energy physics (Pickering 1986; Dawid 2013, pp. 347–402; 1–9, 75–96). For the specific case of String Theory, see Dean Rickles' book, which provides an overview of the theory's history (Rickles 2014). Cushing's book on the S-matrix program and the edited volume by Cappelli *et al* offer interesting

Recently, Richard Dawid has urged historians and philosophers of science to engage with the deep philosophical questions that these developments raise (Dawid 2006, 2013, pp. 95–96, 1–4). As Dawid points out, it is remarkable that work in the area has continued at all, in spite of the growing disconnect between theory and experiment. Theoretical activity, in fact, seems to have accelerated as the gap between theory and experiment widened. Even more remarkably, physicists appear to have found ways of agreeing with each other on what the relative merits of the various theories that we mentioned above are. Since the 1980s, indeed, String Theory has gained more and more support among specialists, eventually becoming the hegemonic approach to the study of fundamental physics (Rickles 2014; Dawid 2013, pp. 9–18). And although the status of String Theory has been the subject of some controversy, its popularity raises the question of what are the mechanisms that have allowed physicists to build this consensus, or near consensus, in the absence of empirical testing.

A related question concerns the epistemic status of String Theory and of the rest of our current theories of fundamental physics. On the one hand, these theories make very little or no contact with experience. String Theory, in particular, offers very few prospects of empirical testability. By the standards of traditional accounts of theory evaluation, therefore, these theories count as little else than mere speculations. On the other hand, however, some of our theories of fundamental physics are the outcome of several decades' worth of scientific theorizing, often at the hands of the most talented theoretical physicists of their generation. Those with the expertise required to evaluate these theories, furthermore, tend to place a high degree of trust in their success. And

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perspectives on the early stages of String Theory's development (Cushing 1990; Cappelli et al. 2012).

although experts' trust could turn out to be misplaced, it seems wrong simply to disregard these carefully crafted theories as entirely baseless speculations (Dawid 2009, 2013, pp. 19–30).

Clearly, there is ample room for historians and philosophers of science to step in and try to shed light on these questions. Dawid's own attempt at doing so consists of arguing that, as fundamental physics became more and more detached from experience, physicists created new strategies to evaluate scientific theories. It is these strategies, according to him, that have allowed physicists to operate in the current data-deprived environment and find ways of agreeing with each other. These new methods would be implicit in scientific practice, however, and the logic behind them would not have yet been made explicit. The goal of much of Dawid's work can thus be seen as an attempt to examine these new strategies rigorously and clarify their conceptual foundations. In doing so, Dawid hopes to intervene in current debates on the status of fundamental physics and steer them into a more constructive direction (Dawid 2013, pp. 2–4, 30, 71–72).

More precisely, Dawid argues that the new strategies of theory evaluation that physicists developed after the formulation of the Standard Model amount to a form of non-empirical confirmation. Faced with the impossibility of testing their theories empirically, that is to say, physicists would have found ways of supporting them by resorting to what he calls non-empirical or, more recently, meta-empirical evidence. This is evidence that, while possibly observable in nature, does not fall within the domain of applicability of the theory under evaluation.[2] Typical examples, as we shall see, include

[2]Dawid's terminology has shifted over time, and the more recent designator of "meta-empirical evidence" is standard by now (Dawid 2021; McCoy 2021). This new term was introduced to refer to a specific kind of non-empirical evidence that pertains to the "meta-

observations about the research context in which a theory is developed, or about the properties of the mathematical formalism in which it is cast. A central aim of Dawid's work, then, consists of showing that the ways in which physicists make use of this kind of evidence to support their theories count as a genuine form of confirmation.

Dawid's first step in developing his account of non-empirical confirmation consists of redefining the goal of scientific confirmation as the generation of trust in the future predictions of a scientific theory. When scientists claim that a theory has been confirmed, then, they mean to say that they place a great deal of trust in that theory's future predictions. Importantly, this includes both predictions that concern well-known phenomena as well as those that concern new, yet-to-be-observed effects—which correspond to the theory's novel predictions. Dawid, in fact, introduces a new concept to make sense of this idea, namely the concept of *viability*. A theory is viable within a given domain if and only if it is able to account for all the phenomena within that domain,

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level" of activities that scientists' engage in as they test and develop the theory under evaluation. Dawid's contention is that this kind of evidence can support inferences about the space of theories that the theory belongs to, and that these inferences can be used to produce estimates of that theory's chances of being viable. The resulting process receives the name of "meta-empirical confirmation," which Dawid takes to constitute a particularly effective and methodologically relevant kind of non-empirical confirmation. All three of the arguments that I introduce below provide the basis for Dawid's methodology of meta-empirical confirmation, and all three of them rely on meta-empirical evidence. I stick to the earlier label of "non-empirical confirmation" throughout the paper, but all relevant forms of non-empirical confirmation discussed in it count as meta-empirical in this newer, more precise terminology.

regardless of whether those phenomena are known to obtain or not. We thus have that, according to Dawid, the goal of scientific confirmation consists of establishing whether scientific theories are viable or not, with some degree of confidence (Dawid 2013; Dawid et al. 2015; Dawid 2016, 2019, pp. 68-72). [3]

Dawid's redefinition of the goals of scientific confirmation is important because it opens the door to incorporating non-empirical evidence into his framework. His reasoning is as follows. In order to confirm a theory, one has to show that the theory is viable and that its future predictions are therefore likely to succeed. As we just saw, this includes both predictions about phenomena that are known to exist as well as the theory's novel predictions. Typically, predictions that concern known phenomena will have already

[3]It is important to highlight that the concept viability, as understood in Dawid's work, is to be understood in terms of a theory's match with empirical evidence, provided that the theory in question is left roughly as it is. This is consistent not only with Dawid's usage of the term throughout his work, including the explicit definitions that he offers, but also with his stated aim of providing a theory of confirmation (Dawid 2019, pp. 102, 106-107). Like all theories of scientific confirmation, Dawid's account of non-empirical confirmation attempts to measure the extent of a theory's agreement with empirical data. It is implicit in the very idea behind this practice that the theory under evaluation is to remain fixed in all relevant respects, or nearly so. One attempts to test the theory as it exists, that is to say, and not as it could potentially be. Relaxing this requirement could perhaps provide Dawid with a way of incorporating some of the criticism presented in this paper, but that would come at the expense of blurring the line that separates theory confirmation from theory development. This would also be hard to square, furthermore, with Dawid's goal of framing his theory in the language of Bayesian confirmation theory.

been subject to empirical testing. Whether or not these predictions will turn out to be successful in the future is, according to Dawid, a matter of mere induction. This is not, therefore, where the main philosophical problem behind the notion of confirmation lies. After all, physicists routinely assume that predictions that matched up with experience in the past will continue to do so, and that those that did not will continue to fail. The real difficulty in showing that a theory is viable consists, in Dawid's view, in determining whether that theory's novel predictions are likely to agree with experience or not.

Establishing that a theory's novel predictions are likely be successful does indeed seem challenging. If those predictions concern phenomena that may not even be known to exist, how could one possibly show that they are likely to succeed? In order to answer this question, Dawid introduces one further concept, namely the concept of scientific underdetermination. If we are unsure as to whether the novel predictions of a theory will match up with experience, Dawid says, that is because other theories might exist, which are underdetermined by the available empirical evidence. These theories would agree with the theory under evaluation when it comes to the available empirical data but they would disagree with it elsewhere; the possible existence of these alternative theories thus lowers the chances of our original theory being viable. In order to show that a scientific theory is viable within a given domain, then, one has to show that underdetermination in that domain is limited—and the more stringent limitations to scientific underdetermination are, the higher the theory's chances of being viable will be (Dawid 2013, 2016, 2019, pp. 44–49).

Dawid's next question concerns the kinds of considerations that can be used to establish that scientific underdetermination is limited in a given context. His answer is that, as one would have expected, empirical evidence constitutes a very effective way of doing so. The easiest way of finding out if a theory's novel predictions are successful or not, indeed, consists of actually comparing them with experience. The resulting observations will reduce the spectrum of theories that can possibly account for all the phenomena within the domain of interest and place new limitations on scientific underdetermination. This is, in Dawid's eyes, the very manner in which empirical confirmation operates. The same mechanism that makes empirical confirmation possible, however, allows for other kinds of considerations to play a role in scientific confirmation, and this includes non-empirical evidence. Since non-empirical evidence can conceivably be used to place limits on scientific underdetermination, in other words, it becomes possible to confirm scientific theories non-empirically (Dawid 2013, pp. 44–49).

This much establishes, in Dawid's view, that non-empirical confirmation is possible in principle. Dawid's claim however, is that non-empirical confirmation is actually used by physicists in order to evaluate their theories of fundamental physics and, in particular, in order to defend String Theory from its critics. More precisely, he argues that there are three specific arguments that physicists have tended to use for this purpose. Dawid's three arguments are as follows (Dawid 2013, pp. 30–38):

- The No Alternatives Argument (NAA): imagine that physicists managed to formulate a theory that successfully resolves some important theoretical problem. Imagine further that physicists spent considerable time and effort looking for different theories that also solve this problem, and fail to find one. The NAA states that physicists' failure to find these alternative theories lends support to the original theory.

- The Unexpected Explanatory Interconnections Argument (UEA): imagine once again that physicists developed a scientific theory largely in order to solve one central theoretical problem. And imagine that, afterwards, the theory turned out unexpectedly to provide solutions to other seemingly unrelated theoretical problems. The UEA tells us that this lends support to the theory in question.

- The Meta Inductive Argument (MIA): finally, imagine that a number of methods are employed within a research tradition, in order to produce scientific theories. And imagine further that these theories turned out to be, more often than not, successful. The MIA states that this provides us with some reason to believe that further theories within the same research tradition will be successful as well.

As is easy to see, all three of these arguments make use of non-empirical evidence in order to support a scientific theory. The NAA, for example, appeals to an observable fact that does nevertheless not belong to the domain of applicability of the theory under evaluation: it points out that physicists have failed to find alternatives to it, and it takes this fact to provide support for the theory in question. And the same is true of the UEA and the MIA, which make reference to other observable facts about the context in which the search for scientific theories takes place.

Note however that Dawid's contention is not merely that the three arguments make use of non-empirical evidence to provide some vague sort of support in favour of a scientific theory. His claim is that the arguments do so in a very specific way, which amounts to a form of non-empirical confirmation. The reason is that, according to Dawid, the three arguments provide reasons to believe that underdetermination is limited in the context of interest, and that the theory under evaluation is therefore likely to be viable. The NAA, for instance, can be read as suggesting that if physicists have not found alternatives to the theory of interest, that is because there are in fact very few or no such alternatives—and one can provide similar readings of both the UEA and the MIA. Showing that a scientific theory is likely to be viable by placing limits on scientific underdetermination is precisely what scientific confirmation consists of and, since the three arguments do so by making use of non-empirical evidence, we have that they are capable of non-empirically confirming (or disconfirming) scientific theories (Dawid 2013, pp. 50–58).

Finally, there is another aspect of the manner in which the three arguments operate that is important to highlight. According to Dawid, the deployment of a single argument will typically fail to increase significantly a theory's chances of being viable. The three arguments, however, mutually reinforce each other, strengthening their individual cases that underdetermination is limited in a given context. When all three of Dawid's arguments can legitimately be applied to a single theory, therefore, it is possible for the theory in question to be confirmed to a significant degree (Dawid 2013, 2019, pp. 50–56; 108, 116–119). This point is of considerable importance, as it allows Dawid to defend the idea that non-empirical confirmation can be of practical use in actual research contexts. It is not just that non-empirical confirmation can be shown to exist as an in principle possibility in the sense of it being capable of affecting a theory's chances of being viable, perhaps by some very small amount. Additionally, we have that the degree of confirmation that one can achieve by making use of the three arguments is, at least in some cases, high enough to deserve being taken into consideration. Dawid also believes that the three arguments apply to the case of String Theory, so this allows him to explain how is it that the newly developed methods of non-empirical confirmation led physicists to embrace String Theory and discard its competitors (Dawid 2013, pp. 30–39, 50–56).

This constitutes the basic outline of Dawid's position on the role that non-empirical methods play in physics. Although there are a few more elements to it, the above introduces the basic tools that he uses to address the philosophical questions raised by the growing disconnect between theory and experiment in fundamental physics.[4] In a nutshell, Dawid's position is that, as the gulf between theory and experiment grew after the 1970s, physicists developed a new set of methods that allowed them to confirm their theories non-empirically. The new methodology of non-empirical confirmation would be encapsulated in the three arguments above, and it would suffice to account for the sense in which at least some of our current theories of fundamental physics amount to more than mere speculations. It is this same methodology, furthermore, that would have

[4]A few additional aspects of Dawid's work that are worth mentioning but are not immediately relevant for our purposes are as follows. First, Dawid uses the language of Bayesian confirmation theory to formulate some of his views and make precise his claim that non-empirical confirmation amounts to a genuine form of scientific confirmation. Similarly, Dawid sometimes makes the claim that non-empirical confirmation operates as a kind of inference to the best explanation, which infers the likely viability of a theory from the non-empirical evidence presented in the three arguments. Towards the end of his book, he also engages with the contention that String Theory constitutes a final theory, and considers the extent to which it is sensible to hold a structural realist view towards it (Dawid 2013). In more recent times, he has widened the scope of his research on non-empirical confirmation and moved on to investigate the role that non-empirical evidence plays in the usual process of empirical confirmation. According to this more recent line of investigation, non-empirical evidence would have an important role to play also in situations in which empirical evidence is available (Dawid 2013, 2018, 2021, pp. 97–123).

allowed physicists to craft their current consensus around String Theory.

Dawid's intervention on current debates on the foundations of physics is nothing short of impressive. It combines elements of the history of physics, the philosophy of science, and of physics itself to address a topic of both philosophical and scientific relevance. In spite of the obvious merits of Dawid's approach, however, and regardless of how much one agrees with its overall tenor, there are aspects of his work that call for critical engagement. In this paper, I will be engaging with one particular aspect of Dawid's analysis, which concerns the very central role that the concept of non-empirical confirmation plays in his analysis. My position will be, in fact, that the importance that Dawid attributes to non-empirical confirmation is excessive.

What was said above should suffice to show that the concept of non-empirical confirmation is indeed central to Dawid's work on non-empirical physics. It is, after all, the main tool that he uses to address the philosophical questions posed by the rise of non-empirical physics. But in what sense is the notion of non-empirical confirmation *too central* to Dawid's analysis? One way of answering this question consists of saying that he expects too much out of it. Dawid's strategy, after all, consisted in tackling the puzzles of non-empirical physics by analyzing the ways in which the methods of fundamental physics have changed over time. The concept of scientific confirmation, however, was never meant to account for every methodologically important aspect of the manner in which science operates. It thus seems unreasonable to expect that a modified notion of confirmation should succeed, all by itself, at answering the entire range of questions raised by the non-empirical turn in physics.

At times, Dawid himself brings up questions that do not seem easy to answer solely in terms on his notion of non-empirical confirmation. Typically, these questions concern the topic of theory development, as opposed to that of theory evaluation. For instance, Dawid claims sometimes that String Theory is a "highly incomplete theory" and that this is one of the factors that get in the way of its being tested empirically—as the incomplete nature of String Theory complicates the derivation of empirical predictions (Dawid 2006, 2009, 2013, pp. 1–2). Some other times, he says that it is not only the current consensus around String Theory that the absence of empirical testing makes mysterious, but also the "directedness" that the field has exhibited after the mid-1980s (Dawid 2013, pp. 6–7). Interesting and pertinent as these questions are, it is not entirely clear how the development of a new notion of empirical confirmation might help answering them. The reason of, course, is that they seem to have more to do with the question of how theories are developed than with the question of how they ought to be evaluated.[5]

[5]It is of course possible that Dawid does not expect his notion of non-empirical confirmation to answer these questions all by itself. Certain passages in Dawid's writings, in fact, suggest that mathematics might provide an aid in guiding the development of our current theories of fundamental physics (Dawid 2013, 2019). In the case of String Theory, for instance, some have argued that while the theory may not be complete, it is still uniquely determined by a number of mathematical constraints. Accordingly, the process of bringing String Theory into its more or less complete, final form, could be guided by consistency requirements alone. This point of view, of course, is not without problems. First, this series of indications would have to be developed into a more detailed account of theory development that can convincingly address some of the challenges that we just presented. Secondly, and perhaps more worryingly, this attempted solution would seem to apply to the case of String Theory only. To the extent that non-empirical methods

My own critique will target this very aspect of Dawid's work. It will target, in other words, Dawid's tendency to address questions of theory development by making use of what is ultimately an account of theory confirmation. As I said before, I will do this by defending the two claims concerning the role that non-empirical methods play in science that I introduced in Section 1. Before I go on to show how my case-study supports these two claims, however, it will be useful to say a few words about the sense in which those two claims amount to a substantive criticism of Dawid. After all, it looks like he would have an easy time admitting that non-empirical methods can be legitimately be used in physics, and that this includes their role in deciding what scientific theories are worth developing further. So how is it exactly that these two claims are at odds with Dawid's own views? [6]

are meant apply more generally, and provide general directives to navigate contexts in which empirical data is absent, therefore, this solution does not seem to be completely satisfactory.

[6]It is worth pointing out that a other authors have engaged with Dawid's work in ways that resemble my own criticism in this paper. Chris Smeenk, for instance, has argued that a certain species of non-empirical method, namely eliminative methods, are mostly of pragmatic value in the context of contemporary cosmology (Smeenk 2019). Peter Achinstein has also written on the pragmatic import behind scientific speculations, although his stance is not as explicitly critical (Achinstein 2019). Finally, Frank Cabrera has argued that the debate on non-empirical physics is better understood by distinguishing between the context of justification and the context of pursuit to which he believes Dawid's arguments belong (Cabrera 2021). Although my own comments are meant in somewhat of a similar spirit, my strategy differs from that of all of these authors. As we shall see, I

A first thing to note is that there is a genuine, if perhaps subtle difference between my position and Dawid's. For my second claim does not merely state that non-empirical methods can be used to ground pragmatic decisions concerning a theory's pursuit-worthiness. Additionally, it states that their role in grounding these kinds of decisions cannot be understood solely in terms of Dawid's notion of non-empirical theory confirmation. It is of course possible that the mechanism of non-empirical confirmation might sometimes play a role—perhaps an important one—in allowing the three arguments appropriately to assess a theory's pursuit-worthiness. In my view, however, the three arguments' ability to guide theory development cannot be reduced to their ability to confirm theories non-empirically. There are additional ways in which non-empirical evidence can be legitimately mobilised to show that a theory is worth pursuing further.

By contrast, Dawid appears to hold the opposite view. When considering the various ways in which the three arguments might guide theory development, for instance, he says the following (Dawid 2018, p. 494):

> (...) it seems somewhat inconsistent to admit that arguments of non-empirical theory assessment play a substantial and legitimate role in a scientist's selection of the theory she wants to work on without conceding that this substantial role is rooted in an epistemologically significant analysis. After all, the eventual goal of a scientific theory is to be physically viable. Considerations which do not increase the subjective probability of theory's

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take a closer look at the finer details behind Dawid's framework, this being an important aspect of my argumentative strategy.

> viability thus offer only limited help for making strategic decisions.

And in a different article, Dawid elaborates on the same idea (Dawid 2019, p. 103):

> The strongest reasons for working on a theory in this light are those that have an epistemic foundation suggesting that the theory in question is likely to be viable. To the extent that epistemic arguments can be developed, it is of high importance for the physicist to take them into account. The physicist herself may well treat those arguments pragmatically as a way of understanding whether there are good reasons to work on a given theory. From an operative professional perspective, framing what is at issue in that way is perfectly adequate. In the end, however, a strong commitment to working on a particular theory hinges on the question whether there exists a good reason for having trust in the theory's viability.

Whatever role the three arguments might play in determining what theories are worth developing further, that is to say, it must ultimately boil down to their ability to assess these theories' chances of being viable.

While subtle, the difference between these two points of view is quite important. It has, in fact, far-reaching consequences for the overall debate on non-empirical physics. To understand why, remember that the purpose of Dawid's intervention in the debate consisted of systematizing the position adopted by defenders of String Theory. In doing that, he hoped to be able to show that their position is philosophically defensible, and to extract some methodological lessons that might be more widely applicable (Dawid 2013, pp. 2–4, 30, 71–72). If that was the purpose of Dawid's framework, however, then it seems legitimate to criticize it on those very same grounds. One may thus criticize the internal consistency of Dawid's account, and its ability to offer a plausible defense of String Theory.

In my view, Dawid's take on theory development leads to difficulties on both of those fronts. More precisely, I believe that Dawid's views on this subject compromise his ability to put together a framework that offers a persuasive defense of String Theory and is also internally consistent. The relevant kinds of complications manifest themselves in different ways. Sometimes, Dawid's views on theory development do a poor job at tackling some of the questions that come up in the debate on String Theory—simply because those questions concern theory development in the first place. As an example, consider Dawid's claim that physicists' use of non-empirical methods can account for the "directedness" exhibited by high-energy physics after the 1980s, which I mentioned earlier. While not completely impossible, it is hard to see how exactly an account of scientific confirmation might succeed at doing that.

Some other times, a seemingly innocuous statement can lead to unforeseen complications. In these kinds of situations, the need to preserve the consistency of Dawid's framework ends up causing trouble. Dawid's framework, after all, constitutes a fairly intricate system with many closely interrelated parts. The requirement to avoid self-contradiction can thus become quite stringent at times, and a seemingly trivial claim can lead to unexpected difficulties elsewhere in the formalism. And at times, these complications will concern the topic of theory development. A particularly vivid example concerns Dawid's statement that all three of his arguments apply to String Theory, which as we saw plays a crucial role in his system. While seemingly unproblematic, this claim entails a number statements regarding the manner in which scientific theories develop over time. And as I will soon show, these statements turn out to be historically inaccurate.

To see this, let us take a moment to consider what it would mean for each one of Dawid's arguments to apply to String Theory. To claim that the NAA applies to String Theory is to assert that there are no alternative theories that can plausibly claim to unify all four fundamental interactions of nature (Dawid 2013, 2019, pp. 31–33). The UEA simply asserts that String Theory turned out unexpectedly to solve many theoretical problems that it was not designed to address (Dawid 2013, 2019, pp. 33–35). The MIA, however, inadvertently introduces a historical claim into Dawid's otherwise purely systematic framework. To say that the MIA applies to String Theory is to say that the methods used in its construction are roughly the same methods that led to the formulation of the Standard Model. It is because the Standard Model turned out to be viable, according to Dawid, that we have some reason to believe that String Theory is a viable theory as well (Dawid 2013, 2019, pp. 35–37).

But what does this historical claim amount to, exactly? And what are the methods alluded in it? Part of what Dawid seems to have in mind is that the UEA and the NAA were themselves used in crafting the Standard Model, and later on in creating String Theory (Dawid 2013, pp. 55–56).[7] In deciding where to turn and how to proceed next,

[7]This condition is not stated as clearly in later writings, but it is mentioned explicitly in Dawid's book (Dawid 2013, pp. 35–36, 56). In any case, note that the condition seems necessary to sustain Dawid's claim that all three arguments reinforce each other, making significant non-empirical confirmation possible for String Theory. If the UEA and the NAA are not among the methods that led to the formulation of both the Standard Model and String Theory, then no obvious relation would obtain between the MIA and these two arguments (Dawid 2013, 2019, pp. 56; 115–117). Without that, the interconnections

therefore, physicists would have helped themselves of these two arguments. They would have done so first when looking for a theory of elementary interactions during the 50s and 60s, and then again when looking for a theory of everything after the mid-1970s. And in both cases, it is the ability of the NAA and the UEA to confirm theories non-empirically that would have proven useful in developing the relevant theories. It is the mechanism of non-empirical theory confirmation that led physicists' to the empirically successful Standard Model. And since this same mechanism guided the development of String Theory, the confirmed viability of the Standard Model makes the as-of-yet empirically untested viability of String Theory likely.

We thus see that Dawid's views on theory development have consequences beyond the immediately obvious. We started with an important but seemingly ahistorical claim of his, and followed its consequences through the intricate web of statements that conform Dawid's system. What we ended up with is a number of fairly specific statements concerning the manner in which both the Standard Model and String Theory were developed. And so, if one wants to claim that significant non-empirical confirmation has been achieved for String Theory, she needs to be able to defend Dawid's views on how physical theories are developed over time. Whether this is immediately obvious or not, in sum, Dawid's take on this topic has consequences for the ultimate success of his framework, including the fulfillment of its own avowed goals.

This should suffice to convince us that the stakes behind my critique of Dawid are high. The question still remains, of course, of whether my critique is sound at all. In the next section, I will try to show that this is indeed the case by making use of a case-study.

between arguments required to obtain significant non-empirical confirmation would not obtain either.

My goal in doing that will consist of showing that Dawid's account of the development of the Standard Model is at odds with the historical record. I shall take a look at the manner in which theories of high-energy physics were developed during the 1950s and 1960s, paying particular attention to the role that Dawid's arguments may have played in that process. This will allow me to argue, in Section 4, that Dawid's take on the subject is mistaken, and that his views on theory development ought to be revised.

3 Sakurai's 1960 Theory of the Strong Force

As I mentioned above, my case-study concerns a theory of the strong force that Jun Jon Sakurai introduced in 1960. Sakurai's goal, as we shall see, consisted of building a quantum field theory that could account for the strong interaction. His was, in fact, one of the first gauge theories to be introduced as a serious candidate to describe the dynamics of a nuclear interaction. As I advanced before, Sakurai offered a number of non-empirical arguments in support of his theory, one of which very closely resembled Dawid's own UEA. In order to understand the reasons that led Sakurai to do this, and the logic of his arguments, we need to introduce some background concerning the development of high-energy of physics during the 1950s and 1960s.

3.1 Historical Background: High-Energy Physics during the 50s and 60s

The origins of contemporary high-energy physics can be traced back to the late 1940s. Although many factors contributed to the creation of the new discipline, two events deserve our attention here. The first one is the renormalization of quantum electrodynamics at the Shelter Island and Pocono conferences in 1947 and 1948, and physicists' failure to apply this same technique to the weak and the strong interactions (Schweber 1994). As is well known, the quantum field theories that Fermi and Yukawa had introduced during the previous decade turned out to be intractable by means of the renormalization scheme that Dyson, Feynman, Tomonaga, and Schwinger had devised for QED. This left open the question of whether consistent quantum field theories for the strong and the weak nuclear interactions could be developed at all (Cao 1998).

The second main event leading to the genesis of high-energy physics is the discovery of the first strange particles in 1947. In this year, a cosmic ray experiment conducted by Rochester and Butler registered a number of V-shaped and fork-shaped tracks in its photographic plates (Rochester and Butler 1947). The anomalous tracks could be readily interpreted as evidence for the existence of two new kinds of particles—neutral and charged, respectively—that did not seem to correspond to any known entities. The new particles possessed a number of contradictory properties, including an anomalously long lifetime that seemed hard to square with their being sensitive to the strong force. And although the meta-stability of the "V" particles turned out to be relatively easy to explain, Rochester's and Butler's was only the first of a long series of similar discoveries. As particle accelerators came into operation during the early 1950s, indeed, more and more particles kept being discovered. This raised the question of whether the members of the growing particle zoo could be classified in some sort of useful, productive way (Pickering 1986).

We thus have that, towards the early 1950s, the nascent high-energy physics community faced two main challenges. First, there was the problem of cataloguing the new particles and making sense of their properties. And secondly, there was the problem of providing an account of the dynamics governing the interactions between them. These two problems would find their eventual resolution with the formulation of the Standard Model during the late 1960s and early 1970s. But during the two decades prior to that, physicists struggled to find a way to address these two interrelated difficulties. Sakurai's 1960s theory, as we shall see, constituted one of many attempts to do so.

Before we go on to review the details of Sakurai's theory, there are two aspects of the history of high-energy physics that will be useful to highlight. The first one concerns the role that experimental data played during the 50s and 60s in helping physicists formulate a consistent theory elementary particle physics. As we shall see, empirical evidence proved to be only of limited use in this process. This might be surprising, as we tend to think of the 50s and 60s as a period in which empirical data was particularly abundant. This is indeed true, and some research programs did try to take advantage of this fact and build their theories from the ground up, by taking empirical evidence as their starting point. This is the case, for instance, of the so-called S-matrix program. These kinds of approaches got some traction during the late 50s and early 60s, but they were gradually overthrown as the QFT-based Standard Model rose to prominence towards the late 1960s (Cushing 1990).

Quantum field theoretic approaches stood in a much more complicated relationship with empirical data. As I just mentioned, the renormalization techniques that had rendered QED calculable failed to extend to other domains, and this made the extraction of empirical predictions out of most quantum field theories impossible. Throughout the 50s and 60s, therefore, those interested in developing quantum field theoretic descriptions of the interactions between elementary particles had to proceed largely in the dark. Once a quantum field theory purporting to describe either the weak or the strong interaction had been formulated, there was very little that physicists could do to confirm it. It was these kinds of difficulties that forced Sakurai, as we shall see, to make use of non-empirical evidence to argue in favor of his theory.

A second point that is worth mentioning concerns the crucial role that symmetry principles played in the construction of the Standard Model. Part of the reason why symmetry principles proved so important during the 50s and 60s is that they provided a link connecting the two main problems that stood at the center of the discipline. On the one hand, symmetry principles could be used as tools to taxonomize the various members of the particle zoo. And on the other hand, the resulting taxonomies could be used to discern hints about the dynamics that governed the interactions between them.

Although this much is well-known, there is one aspect of the role that symmetry principles played during the 1950s and 1960s that is often overlooked. Although most physicists agreed that symmetries would play an important role in leading to a successful theory of elementary particles, different authors had different views as to why exactly that was. Different research programs coexisted at the time, in fact, all of which conceived of the role of symmetry principles differently. Sakurai's own path to his 1960 theory, to which we now turn, will also be illustrative of this fact.

3.2 Sakurai's "Cosmic Symmetry" Approach

Sakurai introduced his new theory of the strong force in an article that he published in *Annals of Physics* in 1960 (Sakurai 1960). Sakurai's 1960 theory, however, represented a change of heart for the Japanese-American physicist. During the late 1950s, he had developed a different theory of the strong interaction, which he referred to by the informal name of “cosmic symmetry” (Sakurai 1959).

The cosmic symmetry approach constituted Sakurai’s first attempt to make use of symmetry principles to inquire into the dynamics of the strong force. By the mid-1950s, physicists had already made some progress in identifying a number of purported symmetries of the strong interaction. It was already known, for instance, that all strongly interacting particles—which at the time included both the baryon and the meson octets, with the exception of the η meson—could be classified by making use of the quantum numbers of isospin, hypercharge, and baryon number (Borrelli 2015). Additionally, the experimental data available back then seemed to suggest that the behaviour of the strong force was approximately the same for the members of at least some of the resulting multiplets. The question, then, was how to make use of that information to build a coherent theory of the strong force.

Sakurai’s cosmic symmetry approach attempted to do that by generalizing Yukawa’s meson theory. The Yukawa theory, after all, provided the most promising field theoretic treatment of the strong interaction. The theory had been formulated during the 1930s, however, and it therefore described the interactions between nucleons and pions only. A natural way of proceeding in the study of the strong force, then, consisted of incorporating the new particles discovered during the 40s and 50s into the Yukawa framework, and of doing so in a way that was consistent with the observed symmetries of the interaction. Other physicists made similar attempts at around the same time, as a matter of fact, Gell-Mann’s and Schwinger’s “global symmetry” scheme being perhaps the most well-known (Gell-Mann 1957).

All of these approaches took the Yukawa theory as a starting point, and they all faced a common difficulty. As it soon became clear, the presumptive symmetries on which they were based were only approximate. Isospin symmetry, for instance, worked reasonably well as an approximate symmetry of the strong force. Although the members of a given isospin multiplet had slightly different masses, they all seemed to behave similarly when subject to the strong interaction. It was less clear how well strangeness would hold up as an approximate symmetry, however, as particles in different isospin multiplets seemed to couple differently to the strong force.

One way of reacting to this problem consisted of saying that the symmetries of the strong force were "broken" by the presence of other interactions. Most strongly interacting particles were also sensitive to the electromagnetic interaction and so, one could attribute any deviations from the expected symmetry to electromagnetic effects—or even to the possible existence of further interactions, a possibility that was seriously considered at the time (Gell-Mann 1957; Sakurai 1959). If all other interactions could be "turned off," then the exact nature of the symmetries of the strong force would be revealed. Both Sakurai as well as Gell-Mann and Schwinger toyed with this idea during the late 1950s, while data on the behavior of the new strange particles was still largely incomplete.

Unfortunately, this basic idea turned out to be difficult to implement. One important problem was that, when incorporated into the Yukawa theory, the symmetries of the strong interaction were broken in a deeper sense than one may have thought. The problem, indeed, was not just that the presence of other interactions somehow masked the supposedly exact nature of these symmetries. The problem was that, additionally, these interactions had the effect of renormalizing the values of the coupling constants of the various strongly interacting particles. This process of renormalization was different for different particles and this broke whatever symmetries one may had written into the Lagrangian. Those symmetries would only obtain for the bare, unrenormalized fields and this made them impossible to observe.

The situation would have perhaps been tolerable if the supposed symmetries of the strong force had only been slightly broken. As more experimental evidence came in, however, it became clear that no symmetries broader than isospin symmetry could be discerned empirically. And thus, towards the end of the decade, the idea that one could find any deeper but ultimately broken symmetries became increasingly chimeric. As Salam noted in 1959, those kinds of symmetries would not only be unobservable in principle, but they also seemed to provide no "useful relations" (Salam 1960, p. 550). Salam's analysis meant the end of Sakurai's cosmic symmetry approach as well as the end of other similar approaches (Polkinghorne 1989). It did not constitute, however, the end of Sakurai's efforts to make use of symmetry principles to study the strong force.

3.3 Sakurai's 1960 Theory

Disappointed by the fate of the cosmic symmetry approach, Sakurai decided to develop a new theory of the strong force some time around 1959. Sakurai's starting point was an analysis of the reasons behind the failure of his previous theory. As we have seen, these had to do with Sakurai's inability to ensure that the symmetries that he had built into the Yukawa Lagrangian would remain observable. In an effort to solve this problem, Sakurai turned his attention away from the Yukawa theory, and focused on QED instead.

In quantum electrodynamics, the photonic field A_μ couples to the charged current J_μ

as described by the QED Lagrangian:

$$\mathcal{L}_{QED} = -f_{QED} A_\mu J_\mu. \tag{1}$$

In this expression, f_{QED} is a coupling constant and the value of the current J_μ is given by

$$J_\mu = i\bar{\psi}_e \gamma_\mu \psi_e, \tag{2}$$

where ψ_e represents the electron field.

As is well known, Lagrangian (1) is invariant under local "gauge" transformations of the form of:

$$\begin{aligned} \psi'_e &= e^{i\theta(x^\mu)}\psi_e \qquad (3) \\ A'_\mu &= A_\mu + \partial_\mu \theta(x^\mu), \end{aligned}$$

where the transformation parameter θ is allowed to take different values at different locations of space-time.

As it turns out, QED possesses a property that would prove key to Sakurai's project. The coupling given by (1), indeed, is *universal.* This means that the strength with which an electrically charged particle couples to the electromagnetic field is independent of whether any additional interactions might be present, and of whether the particle under examination is sensitive to them. This is a result of the fact that, in QED, unlike in the Yukawa theory, the current J_μ that couples to the photonic field is a conserved quantity—it is, in fact, the Noether current associated to transformation (3). Since this quantity is conserved, renormalization effects can at most affect the charge distribution of the different particles, but not the charge itself. This, in turn, guarantees that the coupling constants of the various particles remain the same.

The notion of universality provided Sakurai with an alternative method for incorporating the symmetries of the strong force into a quantum field theory. In order to avoid the pitfalls of the cosmic symmetry approach, one could develop a theory in which the various strongly interacting particles coupled universally to the strong force. Pursuing the analogy with QED, Sakurai started out by requiring that fields possessing the quantum numbers of isospin, hypercharge, and baryon number transformed as:

$$\psi'_T = e^{i\theta_{\mathbf{T}}\cdot\tau}\psi_T \tag{4}$$

$$\psi'_Y = e^{i\theta_Y}\psi_Y \tag{5}$$

$$\psi'_B = e^{i\theta_B}\psi_B, \tag{6}$$

where $\theta_{\mathbf{T}}$, θ_Y, and θ_B are the transformation parameters associated to each transformation, and T, Y, and B are used to label isospin, strangeness, and baryon number, respectively. Note that, just like in (3), the three parameters are allowed to take different values at different points of space-time. Note also that the boldface print in $\theta_{\mathbf{T}}$ denotes it to be an isospin vector with three components. This reflects the fact that the group associated to isospin rotations, $SU(2)$, is non-abelian.

Sakurai's goal was to find a Lagrangian that remained invariant under transformations (4)—(6) in the same way in which the QED Lagrangian remains invariant under (3). Building on previous work by Yang, Mills, and Lee, Sakurai went on to write (Yang and Mills 1954; Lee and Yang 1955):

$$\mathcal{L} = \mathcal{L}_T + \mathcal{L}_B + \mathcal{L}_Y, \tag{7}$$

where each one of those three Lagrangians read

$$\mathcal{L}_T = -f_T \mathbf{B}_\mu{}^{(\mathbf{T})} \cdot \mathbf{J}_\mu{}^{(\mathbf{T})}, \tag{8}$$

$$\mathcal{L}_Y = -f_Y B_\mu^{(Y)} J_\mu^{(Y)}, \tag{9}$$

$$\mathcal{L}_B = -f_B B_\mu^{(B)} J_\mu^{(B)}. \tag{10}$$

The expressions for the three currents $\mathbf{J}_\mu{}^{(\mathbf{T})}$, $J_\mu^{(Y)}$, and $J_\mu^{(B)}$ above were given by

$$\mathbf{J}_\mu{}^{(\mathbf{T})} = \mathbf{i}\bar{\psi}_\mathbf{N}\frac{\tau}{\mathbf{2}}\gamma_\mu\psi_\mathbf{N} - \bar{\psi}_\mathbf{\Sigma} \times \gamma_\mu\psi_\mathbf{\Sigma} + \mathbf{i}\bar{\psi}_\mathbf{\Xi}\frac{\tau}{\mathbf{2}}\gamma_\mu\psi_\mathbf{\Xi} + \phi_\pi \times \frac{\partial\phi_\pi}{\partial\mathbf{x}_\mu} +$$
$$+ i\Big(\frac{\partial\phi_K^+}{\partial x_\mu}\frac{\tau}{2}\phi_K - \phi_K^+\frac{\tau}{2}\frac{\partial\phi_K}{\partial x_\mu}\Big) + \mathbf{f}_{\mu\nu}^{(\mathbf{T})} \times \mathbf{B}_\mu{}^{(\mathbf{T})}, \tag{11}$$

$$J_\mu^{(Y)} = J_\mu^{(Y)} = i\bar{\psi}_N\gamma_\mu\psi_N - i\bar{\psi}_\Xi\gamma_\mu\psi_\Xi + i\Big(\frac{\partial\phi_K^+}{\partial x_\mu} - \phi_K^+\frac{\partial}{\partial x_\mu}\Big), \tag{12}$$

$$J_\mu^{(B)} = i\bar{\psi}_N\gamma_\mu\psi_N + i\bar{\psi}_\Lambda\gamma_\mu\psi_\Lambda + i\bar{\psi}_\Sigma\gamma_\mu\psi_\Sigma + i\bar{\psi}_\Xi\gamma_\mu\psi_\Xi, \tag{13}$$

with

$$\mathbf{f}_{\mu\nu} = \frac{\partial\mathbf{B}_\mu^{(\mathbf{T})}}{\partial\mathbf{x}_\nu} - \frac{\partial\mathbf{B}_\nu^{(\mathbf{T})}}{\partial\mathbf{x}_\mu} - \mathbf{f_T}\mathbf{B}_\mu \times \mathbf{B}_\nu. \tag{14}$$

Note again that, in all these expressions, boldface print has been used to label isospin vectors.

It was then easy to show that Lagrangian (7) remained invariant under transformations (4)—(6) as long as the five fields $\mathbf{B}_\mu^{(\mathbf{T})}$, $B_\mu^{(Y)}$, and $B_\mu^{(B)}$ transformed as:

$$\mathbf{B}_\mu'^{(\mathbf{T})} = \mathbf{B}_\mu^{(\mathbf{T})} + \mathbf{2B}_\mu^{(\mathbf{T})} \times \theta_{\mathbf{T}} + \partial_\mu \theta_{\mathbf{T}} \tag{15}$$

$$B_\mu'^{(Y)} = B_\mu^{(Y)} + \partial_\mu \theta_Y, \tag{16}$$

$$B_\mu'^{(B)} = B_\mu^{(B)} + \partial_\mu \theta_B. \tag{17}$$

Note, however, that the invariance of Sakurai's theory under these transformations relied crucially on the fact that the B fields had been assumed to be massless. Lagrangians (8)—(10), indeed, lack mass terms for the five vectors bosons, and the addition of suitable mass terms would have spoilt the symmetry of the Lagrangian under the transformations given by (4)—(6) and (15)—(17). This fact will turn out to be important later on, when it comes to understanding the manner in which Sakurai deployed the UEA to defend his theory.

Lagrangian (7) provided the theory of the strong interaction that Sakurai was looking for. In this theory, which we would nowadays describe as a Yang-Mills theory with $SU(2)_T \times U(1)_Y \times U(1)_B$ as its gauge group, there were strictly speaking not one but three different strong interactions, one for each of the three symmetry transformations in (4)—(6) and (15)—(17). These interactions were mediated by the five vector bosons $\mathbf{B}_\mu^{(\mathbf{T})}$, $B_\mu^{(Y)}$, and $B_\mu^{(B)}$, which jointly acted as the carriers of the strong force.

Crucially, Lagrangian (7) was such that the couplings of the various strongly interacting particles to the vector bosons $\mathbf{B}_\mu^{(\mathbf{T})}$, $B_\mu^{(Y)}$, and $B_\mu^{(B)}$ were universal (Sakurai 1960, p. 11). The coupling constants f_T, f_Y, and f_B in (8)—(10), in other words, were not renormalized differently for different fields. This resulted from the fact that, just like in QED, the three vector currents $\mathbf{J_\mu}^{(\mathbf{T})}$, $J_\mu^{(B)}$, and $J_\mu^{(Y)}$ are the conserved Noether currents associated to the theory's gauge symmetries. The universality of the couplings guaranteed that all particles with a given kind of charge—T, Y, or B charge—coupled to the corresponding B field in the exact same way, and that the sameness of these couplings was open to empirical inspection.

3.4 Defending the 1960 Theory: Sakurai's Use of the UEA

Sakurai had thus achieved his goal of incorporating the symmetries of the strong force into a quantum field theory while avoiding the difficulties that had led him to abandon the cosmic symmetry approach. Having done that, he had to show that the resulting theory provided a correct account of the dynamics of the strong force. In what remains of this section, I will focus on showing that the arguments that Sakurai offered in defense of his theory amounted to a slight variation of the UEA. I will also attempt to show, furthermore, that Sakurai did not use the UEA to show that his theory was viable, but to show that it was worth developing further.

How could Sakurai show that his theory was correct? The natural thing to do, of course, would have consisted of testing the theory empirically. As I mentioned above, however, the precarious state of renormalization theory at around 1960 made this impossible. As things stood, all but the lowest order estimates of any predictions obtained from (7) were infinite. Lacking any technique that would allow him to bypass this difficulty, Sakurai was unable to extract any empirical predictions out of his theory. In spite of the very abundant empirical data at his disposal, then, he could not confirm his theory empirically.

Unable to rely on empirical testing, Sakurai had to resort to a more elaborate line of argumentation. His first step consisted of showing that his theory was at least compatible with the few successful phenomenological models available at the time. These models, of which the so-called Chew model is perhaps the most famous, did not provide much insight into the inner workings of the strong force, and they covered a very limited experimental regime only (Chew 1953; Ruiz de Olano et al. 2022). Still, Sakurai had to make sure that there was no in principle incompatibility between his theory and these kinds of phenomenological approaches.

More importantly, Sakurai developed a strategy to provide positive arguments in favor of his theory. Even if he could not derive explicit quantitative predictions from his Lagrangian, Sakurai was still able to extract a few qualitative predictions out of it. These predictions could not get him very far in systematically comparing his theory against experience, but they were still of use in a more intricate, indirect manner. Qualitative predictions could be used, Sakurai contended, to address outstanding "mysteries" in the study of the strong interaction. If explanations to many different, seemingly unrelated problems could be offered this way, this would surely count as a consideration in favor of his theory.

The main core of Sakurai's 1960 paper was devoted to unpacking this very line of reasoning. Sakurai, in fact, was explicit about the strategy that he used in order to defend his theory. As he himself put it (Sakurai 1960, pp. 16–17):

> Of course, we never know how our predictions become affected by the "phenomenological" Yukawa couplings, and in most cases what we can predict are qualitative "yes-no" propositions. For instance, we can tell only whether the sign of a certain phase shift is positive or negative, or whether or

not the theory can offer a qualitative explanation for a certain "mystery." Yet, if the theory makes correct yes-no type predictions ten times, the probability that this agreement is fortuitous is one part in $1,024$.

It is illustrative to consider some of the qualitative predictions that Sakurai offered, and the manner in which those could be used to shed light on open questions in the study of the strong force. Sakurai's first prediction, for instance, concerned the isospin dependence of s-wave pion-nucleon scattering. Experimentally, one found that the pion-nucleon interaction was attractive in the s wave when the total isospin of the system added to $T = 1/2$, and repulsive when it added to $T = 3/2$. At the time of Sakurai's writing, no explanation existed for this kind of behavior. Sakurai, however, could offer a very straightforward explanation for this phenomenon since, in his theory, the interaction between nucleons and pions was proportional to $\mathbf{T}_\pi \cdot \tau_\mathbf{N}/\mathbf{2}$ (Sakurai 1960, p. 17). This allowed him to account for the observed anomaly without having to provide a precise quantitative treatment. The rest of Sakurai's paper proceeded in that same mood, by showing how similarly qualitative arguments could be used to address other unresolved puzzles in the discipline.

We are now in a position to appreciate the sense in which Sakurai's argument was analogous to the UEA. The ease with which Sakurai derived qualitative predictions, admittedly, introduces a bit of a disanalogy with Dawid's own formulation of the argument. Still, note that the force of Sakurai's arguments did not reside on the extent to which these qualitative predictions agreed with empirical data, but on the theoretical import of the problems that they resolved. The persuasiveness of Sakurai's claims, furthermore, stemmed from the fact that his theory had not been developed to address any of those mysteries, and that the mysteries in question seemed to bear no obvious relation with each other.

All of these considerations are non-empirical in nature and thus, it makes sense to say that Sakurai's line of reasoning was non-empirical as well. Sakurai's arguments, furthermore, used non-empirical evidence in the same way as the UEA. As we saw in Section 2, the UEA relies on the observation that it is very unlikely that a theory that was developed for independent reasons should provide answers to a number of outstanding theoretical puzzles just by chance. It then makes the point that, therefore, any theory that satisfies that basic description deserves to be trusted—at least to some extent. This is the precise manner in which Sakurai's own arguments mobilised non-empirical evidence in favor of his theory, and it is in this sense that those arguments count as slight variations of the UEA.

3.5 The Mass Problem and the Question of Pursuit-worthiness

Before we move on to the next section, there is one last aspect of Sakurai's argumentative strategy that needs to be discussed. This last point concerns the masses of the five vector bosons $\mathbf{B}_\mu^{(\mathbf{T})}$, $B_\mu^{(Y)}$, and $B_\mu^{(B)}$ that acted as the carriers of the strong interaction.

As we saw, the masses of those five vector bosons were required to be zero, in order for Sakurai's Lagrangian to remain invariant under the local gauge transformations given by (4)—(6) and (15)—(17). The local gauge invariance of the Lagrangian, in turn, was essential to Sakurai's conception of his own theory and it had played, as we saw, a crucial role in its genesis.

Unfortunately, the masslessness of the five vector bosons was in contradiction with the entire line of argumentation that Sakurai had developed to support his theory. The qualitative predictions that were at the heart of Sakurai's argumentative strategy, indeed, assumed that all five of the vector bosons were massive. This assumption was acceptable in itself, of course, but it undermined the grounds on which Sakurai had erected his theory, and it contradicted the rationale behind it. And thus, Sakurai had to choose between the reasons that he had for finding his theory attractive, and the arguments that he had for believing that it was correct.

Sakurai had no solution for this problem, and he acknowledged as much explicitly. Soon after introducing the Lagrangian of his theory he pointed out (Sakurai 1960, p. 12):

> By this time an intelligent reader must have made the following objection: The B fields cannot be massive because the mass term $\mu^2 B_\mu^2$ in the Lagrangian certainly does not satisfy your gauge principle. This is a valid objection, perhaps the most serious objection to our theory.

Shortly afterwards, Sakurai added (Sakurai 1960, p. 13):

> We admit that we lack satisfactory answers to the questions of the masses of the various B quanta. We must assume that they are all massive lest the whole edifice of our theory should crumble down. One of the reasons why the present work is submitted for publication in spite of the B mass problem is that the author hopes that the publication may prompt some clever ideas along this line.

Sakurai's candid language raises the question of what the goal of his argumentative strategy was. More precisely: did Sakurai think that his theory was viable? If not, why did he bother publishing it? And most importantly, what did he hope to show in deploying the UEA in its favor? As the quotation above suggests, Sakurai's perspective was that, while his theory could not possibly be viable in the form that it then had, it deserved to be developed further. Again, Sakurai was explicit on this point (Sakurai 1960, p. 14):

> Since our ideas are rather novel, it is not too surprising that there are difficulties associated with our theory. It would be a pity to give up our theory on account of the B mass problem just as it would have been a pity to give up Bohr's atomic model on account of the difficulties associated with the notion of "quantum jumps."

The entire point of his paper, then, and of the battery of arguments that he had offered, consisted of showing that his theory was worth developing further. Again, Sakurai was explicit about this (Sakurai 1960, p. 43):

> These theoretical arguments, together with the experimental indications mentioned earlier, seem to the author to be strong enough to suggest that this theory might not be complete nonsense and that, even if the theory turns out to be wrong in the end, it is at least worth trying to work out various consequences of it. There are a number of new experimental and theoretical directions to be explored.

Sakurai went on to provide a list of twelve specific suggestions as to how the research program that he had outlined in the paper could be further developed, which he detailed over the course of three full pages. There can be little doubt, then, that Sakurai's goal in deploying the UEA consisted of showing that his theory was worthy of pursuit.

As we know, Sakurai's theory of the strong force turned out not to be viable. Neither isospin nor hypercharge and baryon number, so far as we can tell, give rise to gauge interactions. Our current understanding of the strong force, furthermore, is based on a different kind of gauge theory, namely QCD. And yet, there is a sense in which Sakurai's theory was pursuit-worthy after all. Sakurai's theory, indeed, was the first one to include multiple gauge interactions in the same Lagrangian, and this allowed him to draw a number of important lessons. More precisely, Sakurai was able to show that different gauge interactions could coexist without affecting the values of each others' coupling constants, and this constituted his theory's most enduring legacy. This insight would prove important in the development of the Standard Model which, after all, also results from the amalgamation of three different kinds of gauge interactions into a single Lagrangian.

4 Viability, or Pursuit-Worthiness?

Having introduced the details of my case-study, we can now go on to discuss the manner in which those support the two main claims in the paper.

My first claim was that non-empirical methods can legitimately be employed in physics, and that these methods can in fact play an important role in situations in which empirical testing is impossible. This first claim was formulated so as to include Dawid's three arguments, which I introduced in Section 2.

It is easy to see how my case-study supports this first claim. As we saw, the impossibility of deriving predictions from his theory forced Sakurai to rely on non-empirical evidence in order to argue in favor of his theory. Sakurai, furthermore, made use of an argument that very closely resembled one of Dawid's three arguments, namely the UEA. This supports the idea that non-empirical arguments were indeed used during the 1950s and 1960s in the context of high-energy physics. Even more importantly, the case-study sheds light on the reasons why the employment of these kinds of arguments is legitimate. As Sakurai's efforts to develop a workable theory of the strong force illustrate, there is no reason to expect that empirical confirmation will always be available. The kinds of difficulties that get in the way of empirical testing, in fact, are more varied than one may have expected and they include, as we have seen, the practical impossibility of deriving empirical predictions. Given that these kinds of difficulties are bound to recur, it is hard to see why recourse to non-empirical evidence would have to be discarded out of hand.

My second claim was that non-empirical methods, and Dawid's three arguments in particular, can be deployed for purposes other than the confirmation of scientific theories. More precisely, my claim was that Dawid's three arguments can be used to ground pragmatic choices concerning what theories are worth developing further. The idea here was that this constitutes an important aspect of the role that non-empirical methods play in science, which cannot be reduced to whatever work these methods might do in confirming scientific theories.

The case-study also provides support for this second claim of mine. As we saw, Sakurai's goal in making use of the UEA did not consist of showing that his theory was viable, but of showing that it was worth developing further. Sakurai, in fact, was well aware of the fact that his theory could not possibly be viable as it stood, owing to the problem posed by the masses of the three gauge bosons $\mathbf{B}_\mu^{(\mathbf{T})}$, $B_\mu^{(Y)}$, and $B_\mu^{(Y)}$. His strategy, therefore, did not consist of showing that his theory was pursuit-worthy *because* it was likely to be viable. Sakurai, on the contrary, attempted to show that his theory was pursuit-worthy *in spite of* it not being viable.

The case-study, in fact, does more than offering support for my second claim. It also gives us insight into the kinds of reasons why a theory might be worthy of pursuit in spite of its not being viable—independently, in fact, of its chances of being viable. The point here is that, as Sakurai's case illustrates, a theory may very well lead to a viable theory without being itself viable. Assessing a theory's chances of being viable and assessing its chances of leading to a viable theory, in other words, are two different, if often closely related endeavours. And given that decisions concerning what lines of research to pursue will often have to be made without the aid of empirical evidence, it seem perfectly reasonable to make use of non-empirical evidence in order to ground those choices.

For these reasons, I take the case-study to support my two main claims in the paper. Having established this much, we can go on to think about what exactly this means for the overall debate on non-empirical physics. As we saw in Section 2, the main point of disagreement between Dawid and me concerned the question of reduction that is present in my second claim. Dawid's own position was that the work that the three arguments do in guiding theory development can be understood solely in terms of the mechanism of non-empirical theory confirmation. We also saw how this seemingly small disagreement had repercussions for questions of considerable importance, some of which concerned the very goals of Dawid's framework. Now that my case-study has called Dawid's views on theory development into question, it is time to ask ourselves what this might mean for his overall system.

It seems clear that my take on the role of non-empirical methods raises certain concerns for Dawid. If grounding pragmatic decisions is part of what the three arguments have typically been used for, and if this manner of using them does not reduce to their ability to confirm theories, then it is possible that Dawid's construal of the debate on String Theory is mistaken. Perhaps showing that String Theory is worth pursuing is all that its advocates have attempted to do over the years—or perhaps that is all that they could have possibly succeeded at doing, given the manner in which the three arguments actually work.

Of course, my own position is not that the role of the three arguments reduces to the purely pragmatic either, and this leaves Dawid with space to maneuver. Since Dawid's framework comprises so many different moving parts, furthermore, it should be possible to make adjustments so as to incorporate the criticism that I presented here. Still, I do take myself to have shown that the topic of theory development deserves more attention, and that Dawid's position on the matter is not tenable as it stands. At the very least, a defender of Dawid's views would have to explain how is it that his take on the role of non-empirical methods can tackle questions that seem orthogonal to the topic of confirmation, and have a lot more to do with the question of how theories evolve over time. Without that, and given the kind of historical evidence that we presented here, it is hard to see how Dawid's position can be defended. His claim that significant confirmation has been achieved for String Theory, in particular, stands in need of further justification.

Before I conclude, I would like to comment on some of the potential solutions at Dawid's disposal. Clearly, the question hangs on how one thinks of the role of the three arguments, and of the multiple tasks that they can accomplish by making use of non-empirical evidence. In my view, Dawid offers compelling reasons for believing that the three arguments are capable of non-empirically confirming theories, although perhaps not to a significant degree. As we saw in this paper, however, there are also good reasons for thinking that the three arguments can play an independent role in guiding theory development. As far as I can see, these two uses of the three arguments can neither be completely independent of each other, nor can one of them reduce to the other. The challenge, then, consists of developing the tools to think about the ways in which these two roles of the three arguments relate to each other, and about the various ways in which non-empirical evidence can be mobilized for different purposes.

The situation that we are in reminds one of similar debates in other areas of philosophy. A question that recurs in the literature in epistemology, but also in adjacent debates in moral philosophy, philosophy of religion, action theory, and philosophy of science concerns the distinction between belief and acceptance (McKaughan 2007; Maher 1990; Cohen 1989; Alston 1996; Harman 1988; Lehrer 2000; Levi 1997; Stalnaker 1987; Engel 2000; McKauhan and Elliott 2015; Elliott and Willmes 2013). While belief entails sincere commitment to the truth of a proposition, its acceptance merely requires its adoption for some practical purpose—one might accept a proposition, for instance, for the sake of proving it wrong, or as a working hypothesis. And while grounds for belief and grounds for acceptance are clearly related to each other, and belief will typically lead to acceptance, the opposite will not generally be true. The idea is that, by introducing a finer-grained spectrum of possible cognitive attitudes, we will be better equipped to think about the complex ways in which the epistemic and the pragmatic interact with each other.

My suggestion is that the tool of a cognitive attitude could be useful in thinking through some of the questions that I raised in this paper. In the picture that I have in mind, Dawid's notion of non-empirical confirmation would provide part of the epistemic basis on which assessments of a theory's pursuit-worthiness ought to rest. A theory's chances to be viable, then, would constitute part of the rational ground on which sound pragmatic decisions are made. This information, however, would have to be complemented with a number of other considerations, including the potential of the theory to lead to other viable theories, the availability of the resources required to explore those other options, and the relative merits of other potential competitors—to name just a few obviously relevant factors. [8] The point that I want to emphasize,

[8]Another possibility would consist of using the three arguments to assess the viability not just of a single theory, but of an entire class of theories. In our case, for example, one could argue that Sakurai should have applied the UEA not just to the one theory singled out by the Lagrangian that he provided, but to the larger family of theories that that theory belongs to—the larger class of Yang-Mills theories with an $SU(2) \times U(1) \times U(1)$ symmetry, say. The suggestion here is that this would allow the three arguments to assess the chances of Sakurai's theory eventually leading to a viable theory solely in virtue of Dawid's mechanism of non-empirical theory confirmation, against what my second claim in this paper states. While this suggestion does have some merit, I do not believe that it suffices to answer the challenges that I introduced in this paper all by itself. The main difficulty, of course, consists of identifying the relevant family of theories that the theory under evaluation belongs to. The most obvious way of amending Sakurai's theory, for one thing, consists of adding an explicit mass term to the Lagrangian, and this breaks the gauge symmetry of the theory. Sakurai still thought that this option was worth taking seriously, which shows how tricky defining the relevant family of theories that Sakurai's theory "belonged" to can be. I still find this suggestion, which I owe to C. D. McCoy, useful in clarifying the role of non-empirical evidence in assessing a theory's pursuit-worthiness.

however, is that there is no reason to expect that the role of non-empirical evidence ought to be limited to producing an estimate of a theory's chances of being viable, nor should that be assumed to be the only role of the three arguments.

It seems possible to me to integrate the notion of a cognitive attitude into Dawid's framework. This strategy would perhaps resemble Dawid's use of the resources of Bayesian confirmation theory and of the notion of an Inference to the Best Explanation for his own purposes. What I hope to have shown in this paper is that some effort to incorporate these kinds of considerations is required, and that some changes are needed to respond to the challenges that I have presented here.

5 Conclusions

In this paper, I used a case-study from the early history of high-energy physics in order to engage critically with Dawid's work on non-empirical physics. To do that, I introduced Dawid's work on non-empirical physics in Section 2. I then went on to introduce the details of my case-study in Section 3. After that, I discussed the implications that the case-study has for Dawid's position in Section 4.

This investigation allowed me to draw the following conclusions. First, we saw how Dawid is correct in claiming that non-empirical methods have a legitimate role to play in physics. However, we also saw that Dawid is wrong in assuming that the methodological import of non-empirical reasoning can be fully accounted for in terms of his notion of non-empirical confirmation. The manner in which non-empirical methods can be used to determine what theories ought to be developed further, in particular, cannot be understood solely in these terms. This raises a number of difficulties for Dawid's overall framework and, in particular, for his contention that a significant degree of non-empirical confirmation may be attained for String Theory. Finally, the notion of a cognitive attitude was introduced as a promising philosophical tool for investigating the manner in which non-empirical methods may be used for guiding theory development.

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