Physics without Experiments?

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Abstract

Many scientific theories in fundamental physics are faced with the problem that they lack empirical support. This has led to alternative methods of theory assessment that do not rely on experiments. For instance, Dawid [2013] has proposed a non-empirical method of theory assessment which strongly relies on the concept of theory space. We will argue that the lack of empirical data as well as this new proposed methodology require a change in scientific practice, namely towards an active search for alternative competing theories. We further argue that this change in practice would face at least three challenges, which illustrate the difficulty to implement this change of focus in practice.

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1 Introduction

Consider the following three theories: cosmic inflation, supersymmetry and string theory. Cosmic inflation provides explanations, for instance, for the large-scale homogeneity and isotropy of the universe, the flatness of the universe and the absence of magnetic monopoles. Supersymmetry extends the symmetry of the Standard model of particle physics by introducing a new supersymmetric partner for each particle we know. It solves many open problems, like the infamous hierarchy problem and provides possible candidates for dark matter. String Theory is a proposed unified theory of all fundamental forces, i.e. gravity, the electromagnetic force, and the strong and weak nuclear forces. It is a theory most relevant at very high energy scales, the so-called Planck-scale, where one expects all fundamental forces to unify. These three theories are exemplar theories in modern fundamental physics and they have two things in common. First, they either lack any empirical support (String theory and Supersymmetry) or rely on scarce empirical support (inflation)[1] and second, they are, nevertheless, being defended by scientists for several decades despite the lack of empirical support.

But why do scientists trust their theories in the absence of empirical data? In a recent book Richard Dawid [2013] addresses this question. The reason, he argues, is based on the idea that one can assess theories non-empirically by assessing the extent to which scientific underdetermination is limited, or to put it differently, the extent to which the space of theories is constrained. This shift in fundamental physics towards regimes where empirical data is hard to come by, will necessarily have an impact on scientific practice. We will present what this change in practice amounts to and what the problems will be that the practicing scientist then faces.

We start in Sect. 2 by discussing scientific methodologies with and without experiments and their normative consquences. The normative consequences of the methodologies without experiments discussed are faced with several problems that we discuss in Sect. 3. These problems provide us with additional normative implications for scientific practice, which we discuss in Sect. 4. We conclude in Sect. 5.

[1]See Ijjas et al. [2017] for a recent discussion.

2 Scientific Methodologies and their Normative Consequences

2.1 Scientific Methodology With Experiments

Scientific theories are supposed to at least provide us with empirically adequate accounts of the world. It is therefore not surprising that when we want to assess[2] them, we confront them with the world. The history of the scientific method has, therefore, been a history of finding the right methodology to assess theories based on empirical data.[3]

One line of reasoning followed an inductivist methodology, where one tries to identify reliable methods to infer inductively from observations (the empirical data) to generalisations (the theories). Other methodologies were not concerned with the origins of the theories, i.e. it did not matter, whether the inductive method was used in the development of the theory or whether the theory appeared in someone's dream. The method of assessment relied on the consequences of the theory only. Popper's falsificationism and the hypothetico-deductive account or Bayesian account of confirmation fall under this category.[4]

Most methodologies were not only (or at all) about providing descriptively accurate accounts of how scientists reason, but aimed at providing a normative component, as to how scientists ought to reason. That is, they do not only aim to provide the framework to rationally reconstruct how scientists obtain scientific knowledge but also to provide the rules and guidelines of how they should go about obtaining it.

Let us consider some examples. Newton formulated, within the inductivist tradition certain rules that scientists, i.e. natural philosophers at that time, should follow. His Rule III, for instance, says:

> Those qualities of bodies that cannot be intended and remitted [i.e. qualities that cannot be increased or diminished] and that belong to all bodies on which experiments can be made, *should* be taken as qualities of all bodies universally. [Quoted in [Harper, 2011, p.272],

[2]I use "assess" when I do not want to consider a specific kind of assessment, i.e. I leave it open whether the assessment is via Popperian corroboration, H-D confirmation, Bayesian confirmation etc. See (Dardashti and Hartmann, this volume).

[3]See [Nola and Sankey, 2014] for a textbook introduction or [Andersen and Hepburn, 2015] for an overview article on the scientific method.

[4]See (Pigliucci or Dardashti & Hartmann, this volume) for some discussion on these.

my emphasis.]

John Stuart Mill's inductive account was about finding regularities in the observations. He tried to identify methods by which to identify the causes.[5] He proposes in his "Systems of Logic", for example, his famous method of difference:

> If an instance in which the phenomenon under investigation occurs, and an instance in which it does not occur, have every circumstance save one in common, that one occurring only in the former; the circumstance in which alone the two instances differ, is the effect, or cause, or a necessary part of the cause, of the phenomenon. [Mill, 1843, p.483]

Karl Popper claims that "[e]very genuine test of a theory is an attempt to falsify it, or to refute it." and that "[c]onfirming evidence should not count except when it is the result of a genuine test of the theory" [Popper, 1989, p.36]. So rather than showing that your theory makes correct predictions one should focus scientific practice on trying to refute the theory.

The details or the viability of these normative claims do not matter at this point. However, all have in common that if taken seriously they have direct implications for scientific practice. That is, they are not solely (or at all) descriptive accounts of how science generates new knowledge but normative accounts of how scientific practice should proceed in order to reliably produce scientific knowledge.

2.2 Scientific Methodology Without Experiments

As mentioned in the introduction, the situation of fundamental physics has changed in the last 2-3 decades. For most theories beyond the standard model of particle physics and theories of quantum gravity, empirical data is either scarce or completely absent. Nevertheless, theories like String theory, Supersymmetry or cosmic inflation are all theories that have been defended for decades now, although, none of the previously developed methodologies seem to straightforwardly apply.[6] So there are two questions we need to

[5]See for instance [Wilson, 2016, Sect. 5].

[6]See (Dardashti & Hartmann, this volume) for a discussion of Bayesian confirmation theory and its flexibility to account for new proposed methodologies. See (Carroll, this volume) for a criticism of considering falsificationism as the only method scientists rely on.

answer: (i) Are there new methodologies scientists can rely on? and (ii) What are the consequences for scientific practice? Note, that there might already be consequences for scientific practice, even if one does not want to rely on any of the new proposed methodologies, as the lack of empirical data by itself may require for a change in scientific practice. In what follows we will argue that the same change in scientific practice is both warranted on the basis of a new proposed methodology, Dawid's non-empirical account of theory assessment, as well as due to the lack of new empirical data. As the lack of experiments by itself already requires from the practicing scientist to make the most out of the empirical evidence already available.

Dawid's account of non-empirical theory assessment

An answer to question (i) was proposed by Richard Dawid [2013] in his book "String Theory and the Scientific Method", where he argues for the possibility to confirm theories even in the absence of direct empirical evidence. The approach is based on the idea that one can assess how constrained theory space is and that in cases where one has (non-empirical) evidence that the theory space is strongly constrained one can use that to confirm the theory.[7] The reason for this is that a more constrained theory space increases the probability of those remaining theories to be empirically adequate.

So one crucial element for Dawid's proposed assessment is the idea of a theory space. Theory space, however, is a very difficult construct to assess. How can we possibly have access to it, in order to analyse how constrained it is. Dawid discusses three arguments that, in case one can establish them, provide non-empirical evidence for how constrained it is [Dawid, 2013, Sect. 3.1]. One of the specific arguments is the No Alternatives Argument (NAA). Consider the following observation: "the scientific community, despite considerable effort, has not yet found an alternative to theory T fulfilling certain constraints" [Dawid et al., 2015, p.217]. One can understand this observation about the status of the theory as providing non-empirical evidence for there not *being* any alternatives or at least a very limited number of them. The conjunction of all three arguments is then suppose to provide relatively strong evidence for how constrained theory space is. The details of the other two arguments, however, do not concern us for the purpose of this paper as we are already able to identify an important normative consequence of the account.

[7]We refer the reader for the details of Dawid's non-empirical theory assessment to Dawid [2013], (Dawid, this volume) and Sect.4 of (Dardashti & Hartmann, this volume)

The evidence in the case of the NAA, as well as in the other arguments, depends crucially on how well theory space has actually been explored. For, if scientists have not actively looked for alternatives, the observation that they have not found any alternatives does not probe the actual number of alternatives in theory space. However, scientists usually do not actively search for alternative theories. That is in order to assess theories non-empirically, scientists will have to change their focus in research towards the active search for alternatives. This provides a first hint at the normative implications of non-empirical theory assessment for scientific practice.

Making the Most out of the Available Empirical Data

Even if scientists do not want to rely on non-empirical methods of theory assessment, it is still advantageous for scientists, or so we will argue, to actively explore theory space. In the situation where new empirical data is hard to come by it is crucial to make the most out of the already available empirical data, i.e. to maximize the amount of information we may gain from the available data. The active search for alternative theories does exactly that. See also Smolin [2006] or (Oriti, this volume) for further arguments in support of theory space exploration.

Let us assume we are interested in whether we can trust the predictions of some confirmed theory T. We have made a large set of observations E, which are in agreement with the predictions of theory T and therefore confirm it. T also makes the predictions G and H. We usually will have some confidence in these predictions of T, as it has so far been an empirically successful theory. So the previous empirical success warrants an increase in our trust regarding the novel predictions G and H of T. Now assume, for some reason we will not be able to conduct the experiment, which would allow us to probe whether G and H obtain. In this circumstance we cannot further assess the theory T based on empirical data. Now assume further that someone comes up with an alternative theory, say T', which happens to also predict the set of observations E and is therefore similarly confirmed by it. In addition it predicts G but disagrees about H. Let us denote the predictions by $Predictions(T) = \{E, G, H...\}$ and $Predictions(T') = \{E, G, H'...\}$. How will the existence of this additional theory impact ones believe regarding the predictions G and H? The same available empirical data, i.e. E, confirms two competing theories, which agree with respect to one prediction, G, and disagree with respect to another prediction, H. If we have no reason to trust one theory more than the other, then the proposal of the competing theory T' should lead to an increase in our trust regarding the prediction G, while it leads to a decrease with respect to the prediction H. We learn through the proposal of the competing theory T' that we should not have taken the available empirical data E as providing as much support to prediction G as it does to H, as the same evidence confirms just as much a theory T' that does not predict H. Now imagine further, theorists come up with theories T', T'', ..., all of which agree with respect to the prediction G and disagree with respect to H. It is reasonable to assume that we would slowly become more and more certain about G being a feature of the world we live in and H not. So the proposal of competing alternatives allows us to better assess the untested predictions of the theory.

This simple example illustrates, how the exploration of theory space, the search for alternative theories, allows us to make the most out of the empirical data we already have. Note, of course, that in cases where we could just conduct the experiments necessary to test G and H we would not need to rely on competing theories to assess the viability of G and H. But in cases where you do not have empirical data, whether you intend to assess theories non-empirically á la Dawid or simply want to learn as much as possible from the already available empirical data, exploring theory space may be the change in scientific practice necessary to make progress.

3 Problems of Theory Space Assessment

To explore theory space is, obviously, easier said than done. For that purpose, it is crucial to identify certain pitfalls that one may face. In the following we will address several problems that arise in determining a theory's status with regard to its position in theory space. The approach is to consider specific cases from the history of physics where scientists have mistakenly constrained theory space. This will allow us to identify what the elements are that constrain theory space and what the possible pitfalls are in using them prematurely. The three discussed problems are the theoretical problem, the structure problem and the data problem. While we discuss them separately, they are actually intricately intertwined.

3.1 The Theoretical Problem

When physicists develop new theories, they necessarily rely on certain theoretical assumptions. Further, the problem they may wish to solve, may not be an empirical problem but a conceptual one. The difficulty to justify these two theoretical components of theory development I call *the theoretical problem of theory space assessment*. We will illustrate that problem with a case study from 70s particle physics.

Georgi and Glashow proposed in 1974 a grand unified theory based on the mathematical group $SU(5)$. They provide a unification of all fundamental interactions of particle physics. They start the paper by claiming

> "We present a series of hypotheses and speculations leading inescapably to the conclusion that $SU(5)$ is the gauge group of the world." [Georgi and Glashow, 1974, p.438]

They then go on to develop the theory and end with the claim:

> "From simple beginnings we have constructed the unique simple theory." [Georgi and Glashow, 1974, p.440]

So here we have an example, where scientists make explicit statements with regard to theory space. They say they have provided a theory, which "inescapably" gives rise to the "gauge group of the *world*"[8] and it is a "unique" theory. Whether they meant it as strongly as it is suggested by these quotes is not relevant for our purposes.[9] We are only interested in reconstructing the necessary constraints that would lead to the conclusion they draw.

Let us start with the gauge group of the standard model of particle physics from which Georgi and Glashow develop their $SU(5)$ theory. The gauge group of the standard model is $SU(3)_C \times SU(2)_W \times U(1)_Y$. The particle content in their respective representations of the gauge group is listed in Table 1. What is important is that any future theory should accommodate the particle content of the standard model and contain the gauge group as a subgroup. This is crucial to guarantee the empirical adequacy of any future theory with respect to the evidence that already confirmed the Standard model.

The standard model gauge group is a Lie group. Luckily, simple Lie groups have been completely classified. That is we have a fixed set of possible groups

[8] My emphasis.

[9] It is even doubtful that they considered it in that strong sense as Georgi himself went on to propose SO(10) as a possible group for unification.

Names		$SU(3)_C \times SU(2)_W \times U(1)_Y$
Quarks	$Q = \begin{pmatrix} u \\ d \end{pmatrix}$	$(\mathbf{3}, \mathbf{2})_{+1/3}$
	u^c	$(\bar{\mathbf{3}}, \mathbf{1})_{-4/3}$
	d^c	$(\bar{\mathbf{3}}, \mathbf{1})_{+2/3}$
Leptons	$L = \begin{pmatrix} \nu_e \\ e \end{pmatrix}$	$(\mathbf{1}, \mathbf{2})_{-1}$
	e^c	$(\mathbf{1}, \mathbf{1})_{+2}$
	$[\ \nu_e^c$	$(\mathbf{1}, \mathbf{1})_{0}]$
Gauge Bosons	G	$(\mathbf{8}, \mathbf{1})_{0}$
	$A^{\pm}, A^3$	$(\mathbf{1}, \mathbf{3})_{0}$
	B	$(\mathbf{1}, \mathbf{1})_{0}$
Higgs	ϕ	$(\mathbf{1}, \mathbf{2})_{+1}$

Table 1: The Standard model gauge group and its particle content (first generation only) in their respective representations. Any future theory of particle physics should contain these. See e.g. [Griffiths, 2008].

to choose from. Let us consider several of the constraints on any future gauge group G [Georgi, 1999, p.231-234]:

(1) Group G must be rank ≥ 4: this is needed to contain the four commuting generators of the standard model gauge group.

(2) Group G must have complex representations: some of the particles, like u^c, are in the complex representation of $SU(3)_C$ and so need to be accommodated by any future group.

(3) Group G should be a simple[10] group: this ensures that the gauge couplings are related.

(1)-(3) will put strong constraints on the set of possible groups that come into consideration. For instance, if we restrict the groups to simple groups we rule out for instance groups like $SO(7)$ (rank 3) or F_4 and $SO(13)$ (both have no complex representation) to mention a few. However, groups like $SU(5)$, $SU(6)$, $SO(10)$ and others remain. So it seems not in any way to lead us "inescapably" to $SU(5)$ as the gauge group "of the world". The only way $SU(5)$ could be considered the unique group, is by adding an additional constraint:

(4) Group G should be the simplest group satisfying (1)-(3).

So given constraints (1)-(4) we may say that theory space allows for only one possible group, namely $SU(5)$.

[10] A group is simple, if it does not contain any invariant subgroup.

Let us start by evaluating the different constraints. It is obvious that the different constraints differ in strength of justification one can give for them. It seems restriction (1) and (2) are reasonably supported theoretical constraints on G, as they represent minimal requirements to account for the successes of the standard model gauge group. That is they are empirically supported. On the other hand, (3) seems not to be necessary in a similar sense. Having a simple group has the nice feature that the three gauge couplings of the standard model are then related to each other after the symmetry is spontaneously broken. There is however, nothing empirically requiring this to be the case. Slansky, for example, even says with regard to simple groups that the "restriction is physically quite arbitrary" [Slansky, 1981, p.14]. A couple of months after Georgi and Glashow, Pati and Salam [1974] actually provided a unification based on a group, which was not simple. The last constraint we considered, that we pick the simplest out of the simple groups, lacks any empirical justification (except maybe the non-empirical justification of Ockham's razor). So we see, how one may have been led to think one has found a "unique" theory but this assessment will only be as strong as the constraints that led to the uniqueness. At least in this case not all constraints are well justified.

There is a further problem. While we saw why we may need to worry about wrong constraints, we may also need to worry about the very problem the theory aims to solve. Consider for a moment that all the theoretical constraints from before are empirically well confirmed (imagine this is possible) and based on these constraints there are no alternatives. We would be lead to believe that this theory is empirically adequate, as there simply are not any other theories. However, it can now simply be the case that the problem we are considering is simply not a genuine problem. Unlike empirical problems, conceptual problems like unification are on shaky grounds. In the above example one considers the problem in need of a solution to be the unification of all the standard model interactions. But how do we know this is really a problem in need of a solution? The danger is that we find a theory, which has no alternatives to a problem that is not really a problem. In this case we would have developed a theory towards the wrong "direction" of theory space.

Physicists have the impulse to develop theories without rigorously trying to justify every step of the way all the assumptions involved in theory construction. This is, of course, fine if one can then do experiments to test the theory. Georgi and Glashow's $SU(5)$ theory predicted, for instance, that the proton would decay. However, there has not been any observation of proton decay and the bound put on the lifetime of the proton disagrees with the predictions of $SU(5)$. If, however, experiments are not possible, one needs to address two theoretical problems: (i) to carefully assess all the constraints used in the development as well as (ii) provide good reasons that the proposed problem is genuine. These are necessary ingredients for the reliability of the theory space assessment.

3.2 The Structure Problem

Consider some physical assumption. While one may phrase that physical assumption colloquially in words, one usually has a more precise formal representation of it in mind. When I, for example, talk of probabilities and certain features they should satisfy, I have more formally the mathematical structure of Kolmogorovian probabilities in mind, which satisfy certain specific mathematical axioms. That is I link the physical concepts with specific mathematical structures, which represent them. In the previous example we considered symmetries of some internal space, which are represented in terms of the mathematical structure of Lie groups. The conceptual problem of unification, together with the other constraints, like the particle content and their representations, then translated into the attempt to unify these different Lie groups, into one bigger Lie group. The assessment of theory space was explicitly determined by the classification of all simple Lie groups. That is we could just rule out certain points in theory space by ruling out Lie groups that do not satisfy the constraints. But is this the right place for the constraining to take place? The possible non-uniqueness of the mathematical representation of the physical assumptions is what I call *the structure problem f of theory space assessment*.

To illustrate the problem that arises with this let us consider an example. The strongest kind of assessment of theory space are no-go theorems, i.e. impossibility results. One proves that, given certain assumptions, there are no theories in theory space that are able to satisfy them. In the 1960s, scientists aimed to find a unification of internal and external symmetries. They did not only fail but even claimed it is impossible. Later, and for independent reasons, scientists were actually able to non-trivially unify internal and external symmetries. Not by changing certain physical constraints but by changing the mathematical structures used. The mathematical structure used in the impossibility results were Lie algebras. It is instructive to consider it in more detail.

Let us quickly remind ourselves of some definitions[11].

> A **Lie Algebra** consists of a vector space L over a field ($\mathbb{R}$ or $\mathbb{C}$) with a composition rule, denoted by $\circ$, defined as follows:
>
> $$\circ : L \times L \to L$$
>
> if $v_1, v_2, v_3 \in L$, then the following properties define the Lie algebra:
>
> 1. $v_1 \circ v_2 \in L$ (closure)
> 2. $v_1 \circ (v_2 + v_3) = v_1 \circ v_2 + v_1 \circ v_3$ (linearity)
> 3. $v_1 \circ v_2 = -v_2 \circ v_1$ (antisymmetry)
> 4. $v_1 \circ (v_2 \circ v_3) + v_3 \circ (v_1 \circ v_2) + v_2 \circ (v_3 \circ v_1) = 0$ (Jacobi-identity)

Both the internal and external symmetries were represented as Lie algebras. The aim, similar to the previous example, was to find a Lie algebra that would non-trivially combine them. However, mathematicians already in the mid-1950s found a more general structure. The idea is that one can circumvent the pure commutator structure apparent in the anti-symmetry claim by defining the vector space as the direct sum of two vector spaces with different composition properties. This is, for example, achieved in what one calls $\mathbb{Z}_2$-graded Lie algebras:

> A $\mathbb{Z}_2$**-Graded Lie Algebra** consists of a vector space L which is a direct sum of two subspaces L_0 and L_1, i.e. $L = L_0 \oplus L_1$ with a composition rule, denoted by $\circ$, defined as follows:
>
> $$\circ : L \times L \to L$$
>
> satisfying the following properties:
>
> 1. $v_i \circ v_j \in L_{i+j mod 2}$ (Grading)
> 2. $v_i \circ v_j = -(-1)^{ij} v_j \circ v_i$ (supersymmetrisation)
> 3. $v_i \circ (v_j \circ v_l)(-1)^{il} + v_l \circ (v_i \circ v_j)(-1)^{lj} + v_j \circ (v_l \circ v_i)(-1)^{ji} = 0$ (Gen. Jacobi-identity)
>
> with $v_i \in L_i$ (i=0,1).

[11]See [Corwin et al., 1975] for a review article and [Kalka and Soff, 1997] and [Mueller-Kirsten and Wiedemann, 1987] for elementary discussions.

This new structure has several interesting features. First, the grading gives rise to the feature that the composition of two L_0 elements gives an L_0 element, the composition of an L_0 element with an L_1 element gives an L_1 element and finally the composition of two L_1 elements gives an L_0 element. So L_1 by itself is not even an algebra, since it is not closed, while L_0 by itself is. Second, the so-called supersymmetrisation leads to a commutator composition for all cases but when two elements are taken from L_1. In those cases the composition is given by an anti-commutator. From this it follows that Lie algebras are special cases of graded Lie algebras in the case where L_1 is empty. This generalization of the mathematical structure simply allows one to do more. In the above example, the non-trivial unification of internal and external symmetries is now possible with this new mathematical structure. This is problematic, because it seems that any assessment of theory space will depend on the choice of the mathematical structure within which the different theoretical and empirical constraints are represented with. This *structure problem* of theory space assessment is difficult to assess with crucial implications for the interpretation of no-go theorems[12].

3.3 The Data Problem

A final and maybe less obvious problem is what I call the *data problem of theory space assessment*. There seems to be nothing more uncontroversial then the constraints that come from empirical data. But what one may consider as empirical data once it is implemented explicitly into a theory is far from obvious. Take one apparently obvious example, the dimensionality of space. One may say it suffices to open the eyes to see that as an empirical fact space is three-dimensional. The three-dimensionality of space though may seem to be a strong empirical constraint on physical theories. In formulating a theory it seems obvious to simply start with three space dimensions as it was done for many centuries. Even with the advent of the general theory of relativity, where space itself became a dynamic entity, $D = 3$ was not under discussion. However, starting in theory development with the strong empirical constraint of $D = 3$ may put too strong of a constraint on theory space. Let us consider this example a little bit further, by considering examples from physics where extra-dimensions were introduced.

Unsurprisingly, they did not consider higher dimensional theories for the

[12]See Dardashti [2018].

sole purpose of having a higher dimensional theory. They had different motivations. For instance, Nordström [1914] and Kaluza [1921] were hoping to unify gravity with electromagnetism and realised that a theoretical option would be to introduce an additional space dimension.[13] More recently Arkani-Hamed et al. [1998] proposed a solution to the hierarchy problem, the unexplained difference in strength between the electroweak force and the gravitational force, by introducing curled up extra-dimensions. The idea is that the gravitational constant is a dimension-dependent quantity and in case we have more dimensions we can change the gravitational strength and thereby bring it closer to the electroweak scale. In case the radius of the curled up dimension is chosen to be small, we might not have been able to observe it by any experiment. At the time of Arkani-Hamed et al.'s [1998] proposal, millimetre sized extra-dimensions were still empirically possible.

At this point one may think that one may not be able to rule out curled up extra-dimensions by observation, but one can at least be sure that there are no additional extended non-compactified extra-dimensions. Randall and Sundrum [1999], just like Arkani-Hamed et al. tried to solve the hierarchy problem and showed that one does not need to require the extra-dimensions to be curled up if one allows for warped extra dimensions. As long as the electroweak interactions are constrained to three-dimensional space, the additional dimensions could be extended. The gravitational interactions, unlike the electroweak interactions, extends into these additional dimensions and that explains why it is weaker. Again, at the time of the proposal, there was no empirical evidence to rule these higher dimensional spaces out. So although one may have thought that the dimensionality of space is an empirical fact in no need for any further justification, it turned out to be much more complicated.

The three-dimensionality of space, a seemingly obvious empirical fact about nature, turned out to be an incredibly flexible element in theory development. When we observe the world around us we see three space dimensions. But as our vision is restricted to only electromagnetic interactions with the world and as the resolution of the eye is approximately one arc minute[14] there is plenty of leeway for theory development. Further experiments will, of course, put further limits on this leeway but will never rule them out irrefutably. It is important to realise that this is not a unique example but is a

[13]Since Einstein had not yet developed his theory of general relativity in 1914, Nordström used his own empirically inferior scalar theory of gravity.

[14]One arcminute corresponds to a resolution of about a millimetre at a distance of 30 cm.

general feature of empirical data that is used in theory development. The empirical data used by scientists in theory development never consists of protocol statements in the Carnapian sense but extrapolations thereof. Whether we consider the three-dimensionality of space or the homogeneity and isotropicity of the universe, none of these are irrefutable empirical statements.

4 The Normative Impact on Scientific Practice

In science we find both reliable and unreliable methods of theory development and assessment. When scientists and philosophers of science develop scientific methodologies they want to identify the reliable methods. Once you consider your methodology reliable, you are imposing normative rules on scientific practice to guarantee that the scientists follow the reliable method as closely as possible.

As we argued, in cases where we cannot rely on empirical data to assess theories a shift towards an assessment of theory space is a way to make progress, whether you want to rely on Dawid's non-empirical theory assessment or not. We have considered several examples from physics to illustrate different ways one can mistakenly constrain theory space. In these cases, one considered theory space to be more constrained then what was warranted by the available empirical evidence. If we want to assess theories based on non-empirical theory assessment we need to address these problems as they threaten the reliability of our assessment. The success of non-empirical theory assessment will therefore strongly depend on how well we can address these problems. However, there does not seem to be a straightforward way to address these issues. So the aim of this chapter and formulating these problems should rather be understood in the sense of the phrase: Recognizing a problem is the first step towards solving it.

We saw that by developing a theory we need to make certain assumptions to get started. Based on empirical, theoretical and mathematical assumptions we develop a theory able to solve some problem. Whether these are well justified or not, does not matter as long as there is empirical evidence that can be used to test the theory. If, however, there is no empirical evidence, it is crucial to consider the legitimacy of each constraint and assumption. An assessment of theory space to a large extent depends on the legitimacy of the assumptions involved in theory development and the constraints they impose. The theoretical, structure and data problems of non-empirical theory assessment suggest that it will be very difficult to assess theory space. The question of how to address these problems needs to be addressed case by case. It will require a careful analysis of how the constraints act on theory space in each case and the development of possible strategies of inductive justification of the constraints involved.

In order to assess theories non-empirically a conscious shift of focus in scientific practice is needed. The most obvious change of perspective in scientific practice will be the focus on what is usually called the context of discovery. Rather than coming up with new theories and then considering their empirical consequences, the focus should be on justifying the very assumptions that led to the theory in the first place. Let us now consider each of the problems in turn and discuss some possible ways to address them.

Theoretical Problem It is important to consider the scientific problem one wishes to solve carefully. If, for instance, I have some fine-tuned element in my theory, I may want to try to solve it, but one should also recognise the possibility that the theory does not solve the problem at the next energy scale. Assuming and comparing only theories at the next energy scale that solve the problem may already constrain theory space too strongly. This, of course, makes it necessary to analyse the very question of what constitutes a genuine scientific problem. A highly non-trivial problem. On the other hand one has to be aware of imposing strong theoretical constraints, even when they are well-confirmed. Consider for instance the theoretical principle of CPT-symmetry or Lorentz-invariance. Lorentz-invariance has been a successful ingredient of many well confirmed theories in physics, from classical electrodynamics via special relativity up to the standard model of particle physics. This may provide good reasons to consider it a meta-principle that needs to be required of all theories. However, this again may prematurely constrain theory space. Therefore, an approach has been to test violations of the Lorentz symmetry, by e.g. considering preferred frame effects, by extending the Standard Model of particle physics with Lorentz-symmetry breaking operators or by what is called doubly special relativity, where it is shown that only a subgroup of the Lorentz group is needed to account for all the standard predictions [Mattingly, 2005, Liberati, 2013]. These are explicit methods to address the problem of constraining theory space based on unwarranted theoretical constraints. In this case there has been an extensive set of experiments testing various possible violations of Lorentz invariance within the different proposed frameworks [Russell and Kostelecky, 2009]. So if the theory one is developing is at an energy scale where the empirical evidence does not show a violation of Lorentz invariance, requiring the Lorentz symmetry, and thereby constraining theory space, is empirically justified. If, on the other hand, one develops a theory several orders of magnitude beyond the empirical bounds, theory space is not yet constrained by the data, and one may prematurely constrain theory space by requiring it. These are possible effective methods that are required to test the viability of theoretical constraints. Scientists, in the absence of empirical data to support their theories, should focus on these scientific practices to epistemically justify their guiding principles.[15]

Structure Problem This problem seems less accessible. The problem is that scientists in practice do not recognise it as a constraint. When trying to solve some problem, one uses, understandably, the mathematical structures one always used. They were successful in the past, why should they not be in the future? Only if we recognise that we cannot solve a problem by using that structure, one may look for an alternative structure that allows one to solve the problem. If one finds a structure one stops looking, as there is no need to continue the search as one has solved the problem. Once one has solved the problem, with a specific mathematical structure their is no incentive in continuing the search for other mathematical structures that may also do the job. In this case one may be lead to think, based on the well supported theoretical constraints etc. that theory space is constrained. However, now there is an incentive. In case I do not have empirical data to test my theory, I may want to rely on theory space assessment. So if I would like to argue in favour of my theory I need to actively pursue the possibility of alternative mathematical structures. As we mentioned, the mathematical structure of graded Lie algebras was developed in the mid-1950s. If, O'Raifeartaigh or Coleman and Mandula would have actively searched for alternative mathematical structures, even within the existing mathematical literature, they would have recognised their unwarranted constraint on theory space.

Data Problem The data problem can be addressed similarly to how the problem of theoretical constraints can be addressed. Namely by not extrap-

[15]See Crowther and Rickles [2014] for a special issue focused on the principles guiding theories of quantum gravity.

olating the available data to energies, where we have no evidence for it, but by approaching the question heads on by testing how far one can extrapolate. Take for instance the number of particle generations in the standard model. Three generations have been observed amounting to 12 matter particles. Could there be any more particles, a fourth generation, we have not yet observed? Requiring future theories to have only three generations may put too strong of a constraint on theory space. However, researchers have recently combined experimental data on Higgs searches from the particle accelerators LHC and Tevatron to conclude that a fourth generation of the standard model can be excluded with 5 σ. Similarly, take the example of the speed of light. It has been measured using a range of different methods, from astronomical measurements to interferometry. So its value is well tested and so for any future theory, one may require it to have that fixed constant value. However, do we really have good reason to believe that it is actually constant? Some physicists have suggested the possibility of theories with a varying speed of light [Magueijo, 2003]. There has been, however, controversy about the viability of these theories. As Ellis and Uzan [2005, p.12] point out in arguments against these variable speed of light theories: "[t]he emphasis must be put on what can be measured", which leads to the constraint that "only the variation of dimensionless quantities makes sense". So we see that a seemingly simple concept like the speed of light is actually "complex and has many facets. These different facets have to be distinguished if we wish to construct a theory in which the speed of light is allowed to vary" [ibid.]. These examples illustrate that the topic of how data can constrain theory space is a highly non-trivial matter that needs much more careful analysis.

There is, obviously, much more one can say about each of these problems. We only wanted to illustrate how the different problems could in principle be addressed. While each of these problems need to be discussed in much more detail, we can already recognise some more general features for scientific methodology. Most importantly, it requires a careful reorientation in scientific practice. When I cannot test the novel predictions of the developed theory, I have no other choice but to assess carefully the elements that led me to it. This then may allows a confirmatory assessment in terms of theory space assessment. Alternatively, one can just wait and hope that further progress in technology will soon catch up with the energy scale, where the novel empirical predictions are being made. The trouble with this second possibility is that many of those theories where non-empirical theory assessment is necessary, i.e. theories of quantum gravity, are by construct theories at the Planck scale and it is unclear whether in the foreseeable future experiments will be able to probe these scales.

5 Conclusion

In modern fundamental physics we have many instances where we are lacking empirical evidence. In most of the twentieth century, it did not matter how theories were developed. After all, once we developed the theory we could just test it by experiments. Scientific practice itself was not affected so much by scientific methodology. As long as experiments can be done there is enough external guidance, that the developed methodologies did not need to affect the practice much. But if experiments are lacking, a whole new approach of theory development and assessment needs to be implemented.[16]

We discussed how the lack of empirical data leads to two possible strategies: either one relies on Dawid's non-empirical method of theory assessment or, more conservatively, one does not and try to make the most out of the available empirical data.[17] Independent of what strategy one chooses, scientific practice should shift, or so we argued, towards an assessment of theory space (see also Oriti, this volume). Any evaluation of a theory then relies on how reliable the assessment of theory space as a whole is. We considered three problems of theory space assessment that any practicing scientist may face. Each of these problems may lead to an unreliable assessment of the extent to which theory space is considered to be constrained.

One may argue that these problems just show that it is impossible to assess theories reliably based on non-empirical methods. This might very well be the way it turns out, but it does not have to. Scientists have not aimed at systematically exploring theory space. If they would, there is of course still the possibility that they find many possible alternatives that are currently not excluded and in those cases there will be no non-empirical support for any particular theory. That is, in those cases, the available empirical evidence does

[16]Another promising route, which is not discussed here, is of course the use of the often quite complicated relation of empirically unconfirmed predictions of empirically more or less confirmed theories, like black hole entropy (see Wüthrich this volume) and Hawking radiation (see Thébault this volume), to test currently empirically inaccessible theories.

[17]Of course, there can be further possible non-empirical strategies, none of which, however, have been significantly developed so far.

not suffice to constrain theory space significantly. But it also may be the case that a wide range of theory space can be excluded and that this suggests that there really cannot be any alternatives.

Let us end on an important point. Nothing in the above suggested in any way that one should move away from trying to empirically test these scientific theories (See Quevedo or Kane, this volume). The claim is that, if there is no empirical data to test these theories, an exploration of theory space provides a method to make the most out of the available empirical data.

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