University of São Paulo
Faculty of Philosophy, Languages and Human Sciences
Graduate Degrees in Philosophy Division

WORLDS AND STRINGS

Ontology and Epistemology in Fundamental Physics

Diana Taschetto

São Paulo, 2018

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Master's Degree Dissertation submitted to the University of São Paulo in accordance with the requirements of the degree of Master in Philosophy in the Philosophy Department of the Faculty of Philosophy, Languages and Human Sciences.

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São Paulo
2018

For Thales Borrely,
the true gentleman who taught me how to do physics

Preface

I am deeply enamoured to philosophy of physics. I recall it was second day of junior high school when the philosophy teacher entered class for the first time and, without greetings, introductions or further ado, wrote two questions on the blackboard: 'What's madness?' and, next to it, 'What's time?'. I fail to remember the discourse made thereafter regarding these topics, but I do recall vividly its impression upon me. I knew I'd become a philosopher that day. My fondness for philosophy back then was contrasted by my distaste for the exact sciences—what were all those nasty calculations good for? No teacher would explain to me their concrete significance, and my education in physics and mathematics was as bad as bad can get. Only a couple of years later, working at the small public library of my hometown in order to pay for English classes, a little book entitled 'Newton and his Apple' came to my knowledge, a book that would reveal the answer to such questions. As it turns out, the equations that seemed to me empty of meaning during high-school years *explained the workings of Natur.* I got thunderstruck, because that sort of knowledge I craved for. Not long after, as an undergraduate in philosophy I was introduced to philosophy of science and, devouring all books I could find on the subject, after finishing Popper's *Quantum Theory and the Schism of Physics* I was baffled at the acknowledgment of the important questions still open in physics, questions that require philosophical treatment to be answered. Intellectual thrill is too powerful a thing, and the feelings triggered by those high-school philosophy classes were back with renewed, greater force. This is when the decision to really and formally learn physics was taken.

The implications of this decision were deep and far-reaching, because learning physics, in my case, meant learning it *from scratch.* To go from high-school level trigonometry to tensor analysis, from managing to solve quadratic equations to dealing with non-linear n-order differential equations in such a short time lapse has been the most painful road I've ever taken. Its consequences to my work in philosophy, to my research during these master's years more specifically, are also extremely substantial, for time is a scarce good. Work-hours spent on studying vector calculus and its application in electromagnetism are hours not spent reading philosophy papers, or writing, for that matter. These remarks are intended to play the role of an excuse: one side effect of focusing on understanding the box in detail to, later, think out of it was the need to write this dissertation in frenetic hurry. I can't speculate how much the overall result has been harmed by this need. Of this work, the second chapter of part I and part II entirely were written within a period of about two months (a version of the former was already present at the text the examiners of my qualification in December 2016 had received. Its severe lack of clarity and objectiveness, however, later acknowledged, motivated me to replace it with a new version altogether, rewritten from scratch). The introduction and first chapter of part I differ from the qualification version only in the adding and extracting of some paragraphs. A few words here and there were also changed. The flaws I acknowledge in these are many and severe (lack of objectiveness standing the most prominent of them). If the situation were such time sufficed to rewrite them completely, Introduction of part I, for instance, would be shortened from eleven to three, four pages at most. Time also fell short for an overall conclusion and the two or three appendices it was my intention to add.

The reader may be surprised at the acknowledgment this dissertation is composed by two independent parts. Part I deals with many-worlds theories in cosmology and Part II discusses Richard Dawid's non-empirical, string theory based epistemology proposal. Detachment from experience characterizes both multiverses and string theory but, similarities notwithstanding, these are not my concern here. In fact each chapter of this work is self-contained, in the sense that one need not follow a specific reading in order to grasp the philosophical essence of the chapter in question. Each chapter deals with a specific problem, and the very philosophical tools I put to use in each are different. This is no coincidence: I've deliberately chosen to deal with different issues, issues that require different sorts of analysis, with the purpose of gaining, developing and exercising the greatest number of philosophical muscles possible. I strongly believe this to be what a master's degree program is all about and I have, driven by such belief, spared no efforts whatsoever in attempts to sharpen my philosophical skills. I've taken a lesser traveled, painful road in my research choices but it is my personal evaluation it did, as it had done for Robert Frost, make all the difference.

Regardless of the specifics of particular issues, my overarching goal here is to foster appreciation of how problems in mathematics, metaphysics and philosophy of scientific methodology are interlinked with pressing questions in fundamental physics. This entire work was written with an admittedly critical bias, an attitude for which I make no apologies. Aware as I stand no person is born a philosopher, but rather becomes one, I've attempted to expose my ideas in the clearest possible manner, for the reader to be able to easily find, and then later point out to me, the error of my ways. I am sure such errors abound, and I look forward to learn from them.

I am grateful to Fundação de Amparo à Pesquisa do Estado de São Paulo (FAPESP), grant nº 2014/25882-9, for its support in the development of this work. Einstein's *The World as I See It* came to my knowledge during my undergraduate philosophy of science studies and, ever since, it is the philosopher-scientist who has been my deepest source of inspiration. I share his feeling towards Nature and knowledge-seeking, and these sentiments have given me drive and strength to move forward the many times I saw myself amidst intellectual chaos. From contemporary philosophers, it is to the work of Lawrence Sklar inspirational feelings are directed. I admire him deeply, and I have taken ideas from his

published works without stint. I thank him for being regardful and willing to help whenever I've requested his attention. A more ubiquitous, pervasive intellectual influence, however, springs from a relationship that antecedes my philosophy of physics studies. After writing and reading this dissertation, I was impressed by the acknowledgment of how much my undergraduate supervisor, Dr. Anna Carolina Regner, has influenced how I think. I am afraid the effect of having been her student can be felt throughout this whole work. I thank her once more for how carefully she has guided me through the winding roads of philosophy of science.

I thank Jojomar Lucena, physicist, philosopher and friend, for the uncountable hours we've spent together struggling with the framework of quantum field theory. For many reasons, these studies were crucial to my intellectual development. I thank professor Elcio Abdalla, my supervisor at the Institute of Physics, for all the many times he has helped me with technical details of general relativity, quantum field theory and string theory. These were hard-won lessons, and I cannot thank him enough for his infinite patience. I thank my supervisor Osvaldo Pessoa Junior, for having accepted me as his student and for allowing me to pursue my own ideas, even though he does not share most of them. This is something I cannot put a price on. I thank his golden heart for his trust, his patience, and for helping me in my personal difficulties during this last rough year.

I thank professor Nick Huggett for the care he has had with my work. He is an extraordinary philosopher and human being, and it is my hope he will not be disappointed with the results of my efforts here displayed to the point of giving up on me. I thank my mother and my father. It is solely due to the fact I am the daughter of warriors I have come this far, their teaching me to fight for my dreams, to force heart and nerve to their very extreme, if it is for doing work I love. My last thanks is owed to Thales Borrely. The fact that it is to him this dissertation is dedicated suffices to indicate his significance to me, how deeply indebted to him I feel. I have serious doubts to the effect I would have made it were it not for him. His love and friendship are the best thing I have found in life.

Diana Taschetto

São Paulo, January 2018

'Most of them simply do not see what sort of risky game they are playing with reality.'

Einstein about quantum theorists, letter to Schrödinger, 1950

Abstract

TASCHETTO, D. Worlds and Strings: Ontology and Epistemology in Fundamental Physics. 2018. 109p. Dissertation (Master's Degree)—Faculty of Philosophy, Languages and Human Sciences. Philosophy Department, University of São Paulo, São Paulo, 2018.

This work is divided into two major topics: many-worlds (or multiverse) theories in cosmology and Richard Dawid's string theory-based epistemology, or 'non-empirical confirmation theory', as he calls it. The former is discussed in part I and the latter in part II of this dissertation. These topics are not intertwined in this work, as are not the essays that compose each chapter: in part I, first chapter, probability arguments that are presented in the literature as indications a multiverse must exist are accessed, whereas the second chapter is concerned with analyzing the metaphysical view that motivates many-world theory building, namely, the need to find unconditioned explanations in physics. Non-empirical confirmation theory is built upon three arguments, the 'No Alternatives Argument', the 'Meta-Inductive Argument from the Success of Other Theories in the Research Program' and the 'Unexpected Explanatory Coherence Argument'. Each compose a chapter in part II of this work, as they encode different philosophical issues that require for their assessment different tools from the philosopher's arsenal. Skeptical conclusions are drawn at the end of each chapter. The wide spectrum of questions this work touches are designed to give at least slight indication that critical exploration of foundational theories made upon grounds familiar to philosophers can be found as internal to scientific practice itself, if that practice is concerned with the discovery, refinement and revision of fundamental theories.

Keywords: Many-worlds theories. Physical possibility. Non-empirical confirmation. String theory. Fundamentalism.

Resumo

TASCHETTO, Mundos e Cordas: Ontologia e Epistemologia em Física Fundamental. 2018. 109p. Dissertação (Mestrado)—Faculdade de Filosofia, Letras e Ciências Humanas. Departmento de Filosofia, Universidade de São Paulo, São Paulo, 2018.

Este trabalho divide-se em dois grandes tópicos: teorias de muitos mundos (ou multiverso) em cosmologia e a epistemologia 'não-empírica', embasada na teoria das cordas, de Richard Dawid. O primeiro é discutido na parte I e o segundo compõe a parte II deste trabalho. Tais tópicos não estão ligados, e a problemática desenvolvida em cada capítulo deste trabalho é, em larga medida, independente das demais: no primeiro capítulo da parte I argumentos probabilísticos indicados a literatura em prol da existência de muitos mundos são analisados, enquanto no segundo capítulo os pressupostos metafísicos que motivam a construção de teorias de muitos mundos em cosmologia, a saber, o fundamentalismo que busca explicações não-condicionadas para os fenômenos com os quais lida a física, são discutidos. A teoria da confirmação não-empírica de Dawid, tema da segunda parte deste trabalho, tem por base três argumentos, a saber, o 'argumento das alternativas inexistentes', o 'argumento meta-indutivo do sucesso de outras teorias no programa de pesquisa' e o 'argumento da coerência explanatória inesperada'. Cada um destes argumentos é tema de um capítulo neste trabalho, posto que desvelam problemáticas filosóficas distintas que requerem, por sua natureza, ferramentas de análise diferentes. Conclusões céticas são indicadas ao final de cada capítulo. O amplo espectro de questões que aborda este trabalho é desenhado com o propósito de fornecer ao menos vaga indicação de que a exploração crítica de teorias fundamentais, levadas a cabo a partir de vieses familiares ao filósofo, pode ser vista como interna à própria prática científica, se esta prática é preocupada com a descoberta, refinamento e revisão de teorias fundamentais.

Palavras-chave: Teorias de muitos mundos. Possibilidade física. Confirmação não-empírica. Teoria das cordas. Fundamentalismo.

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Part I

Many-Worlds Theories: From Cosmology to Metaphysics

1 Introduction

This is often the way it is in physics–our mistake is not that we take our theories too seriously, but that we do not take them seriously enough.

Steven Weinberg

All problems—both in philosophy and in the mathematical and physical sciences—are problems of Consistency. The Temple of Modern Science—built upon the pillars of classical general relativity and quantum field theory—, has seen its bricks adjusted, reshaped, removed and replaced in name of the internal logic according to which physics finds knowledge: theoretical and empirical Consistency. In the domain of the experimental probing, the Standard Model of Particle Physics emerges triumphant[1] and unscrupulously opens the door for advancing our understanding of the fundamental workings of Nature at the beginnings of space and time themselves: claiming to offer an explanation of the origin of the density perturbations that seeded the formation of galaxies and other cosmic structures, by incorporating supersymmetric particle physics models inflationary cosmology has got the core of standard big-bang rearranged, astronomy books rewritten and walls of Physics departments redecorated. That is not, however, a door through which scientists can cheerfully walk: quantum fields over Minkowski space summarize a theoretical framework of immense empirical success but of undeniable physical obscurity, conceptual and ontological quarrels brought forth by quantum mechanics being inevitably inherited and taken to higher levels[2]. On the other hand, the claims which got the inflationary wagon rolling—viz., resolution of explanatory problems that beset standard big-bang cosmology like the monopole problem, the horizon/uniformity problem and the flatness problem—have not borne fruit (MUKHANOV, 2005) and, up to date, it is safe to say inflationary cosmologists have not succeeded in producing an anomaly-free model, whether theoretical, observational, or both.

Faith in the picture of Nature painted with the joint colors of inflaton fields and the Weinberg-Salam framework was not shaken, however—technical difficulties notwithstanding. Patches are, of course, needed here and there but the physical backbone is solid, it is held (POLCHINSKI, 2015; GUTH, 2007; LINDE, 2002; REES, 2000a). Such means of representing the various states of nature inexorably lead, some believe, to one important

[1] The Glashow-Weinberg-Salam electroweak theory is the unified gauge theory of the weak nuclear and electromagnetic interactions. Quantum chromodynamics is the gauge theory of the strong nuclear interaction. Together they make up a QFT for all interactions (except gravity) known as the *Standard Model of Particle Physics* and effectively predict anything we can measure (except gravitational phenomena).

[2] See (CAO, 1997). Needlessly to say, quantum field theory also has conceptual problems of its own.

implication (VILENKIN, 2013; LINDE; NOORBALA, 2010, 2010; LESLIE, 1978): the concrete existence of an infinite number of possible worlds (or a 'multiverse', if you like. I shall employ both terms interchangeably). There are two reasons for thinking so. First, some promising fields of research in quantum and early universe cosmology suggest (taking the Standard Model as a given and assuming supersymmetry holds) that physical processes which could have given birth to our universe (or to our region of the universe) from a primordial quantum configuration could have brought into existence other universes as well (ELLIS, 2007; TEGMARK, 2014; SMOLIN, 1997a). New fuel to this idea was given by superstring theory (SUSSKIND, 2005a). The second reason springs, to some extent, from a sense of 'ad hoc-ery' in the number of free parameters in the Standard Model. The experimental values of these parameters must be fed into the theory by hand. It was also acknowledged that, had other values for the constants of Nature but not the ones measured obtained, our universe would not be suitable for complexity and life to emerge (CARROLL; TAM, 2010; BARNES, 2012; REES, 2000a; SMOLIN, 2008; BARROW; TIPLER, 1986).

Scientists and philosophers alike have spent many work-hours in attempts to sharpen linked issues and elucidate the consequences of assuming an infinite number of causally disjoint worlds, forever hidden from our perception, exist. I do not wish to repeat here the work already done by abler hands. Philosophers John Earman and Jesus Mosterin, for instance, have built a strong case in their '*A Critical Look at Inflationary Cosmology*' showing the sociological pitfalls which contributed for the rise and subsequent integration of the 'inflationary programme' (Lakatosian sense implied) into the standard theoretical core of physics. They present arguments indicating how the model in its current form departure a great length from the original goal of improving the standard big-bang theory and how difficult it is to make good (in terms of self-consistency and empirical prediction) of the highly speculative elements inflation critically relies on. On the scientists' side, technical arguments of completely different flavor are put forward by Joshua Schiffrin and Robert Wald (2012). They argue there to exist intolerable inconsistencies in attempting to use the canonical measure of general relativity to make probability arguments in cosmology—such as the probability of our universe having undergone a period of inflation, for instance. Results are shown to the effect that the measure is infinite in any phase space one chooses to perform the calculation and, as such, the resulting computation of probabilities forcefully depend directly on the choice of truncation, that is, on how infinity is regulated. In fact, different choices of truncation yield different results[3].

Of particular interest is another argument presented by these physicists against retroactive probability calculations in the very same paper. They ask us to pay attention

[3] In minisuperspace models with the standard Gibbon, Hawking, Stewart (GHS) measure and using a natural regularization procedure, Gibbons and Turok (2008) obtained an extremely small probability that the universe would have undergone an era of inflation. Making use of an equally natural regularization procedure, physicists Carroll and Tam (2010) obtained a probability very close to unity that the universe would have undergone a large number of e-foldings of inflation.

to the fact that, since thermodynamics holds in our universe, holds thermodynamics *time*. Because entropy increase is a t-asymmetric operation in Nature, they insist, causality is detectable and exploitable in only one direction: forward. Thus one cannot use knowledge of current conditions of the universe to 'retrodict' the likelihood of past conditions:

> In ordinary statistical mechanics, we can use knowledge about the present macrostate of a system to successfully predict the likely future evolution of the system. This is done by considering all of the possible microstates that are consistent with the present macrostate and evolving them forward in time using the (microscopic) dynamical equations. If it is found that an overwhelming majority (in the Liouville measure) of these microstates will have some given property at some specified time in the future, then we can be confident in predicting that the physical system will have that property at that specified time. We refer to this type of argument as a 'trajectory counting argument'. The key point is that although one can use a trajectory counting argument to predict the future, one cannot use a trajectory counting argument to 'retrodict' the past. The time reversal invariance of the microscopic dynamics assures us that most trajectories through phase space that pass through a present non-equilibrium macrostate have entropy that is increasing both into the future and into the past. However, it appears that we live in a universe in which the second law of thermodynamics holds, where entropy increases only in one time direction ('the future'). In practice, we find that trajectory counting arguments do give correct results when used to make predictions; i.e., systems do not appear to be evolving towards some 'special final state.' However, because entropy in our universe decreases towards the past, trajectory counting arguments will always give incorrect answers when used to make retrodictions (SCHIFFRIN; WALD, 2012).

Attempts to assess reverse influence—illustrated in assertions such as 'primordial quantum fluctuations were such and such' and 'initial conditions of geometry and values for the scalar inflaton field were such and such with probability P of creating other universes of characteristics such and such' are, if Schiffin and Wald are correct, at best highly non trivial[4]. These are, I think, genuine methodological difficulties. Can we assume the physics brought under such schemes correspond to the workings of Nature? On what grounds? Data is found wanting. Most fashionable models of inflation are ones in which the universe is eternal, each bubble-universe budding forever new bubble-universes, in a branching process with no end (LINDE; MEZHUMIAN, 1995; LINDE; NOORBALA, 2010).There also exist stochastic inflation, axion-driven inflation, R^2 inflation, and so on. Peebles

[4] (ECKHARDT, 2006) has discussed the causal time asymmetry problem which concern us here in detail. I refer the curious reader to his paper.

once commented, correctly, 'the inflationary universe allows the imagination to roam free' (BARTUSIAK, 1986). Cosmology is the one discipline that inspires me the deepest awe, and it is with unease that I acknowledge the seriousness of assertions such as 'our universe has structure such and such' have been, more often than not, glossed over by practitioners who take joy on speculating recklessly about the properties and dynamics of inflaton fields. I appreciate intellectual fun, but I take science to be a philosophical adventure that involves more profound concerns. Alan Guth came up with the 'inflation idea' in the hope it would lead to further understanding of our universe's history and global structure. If the hope can be fulfilled, then all of us cosmology lovers should commit ourselves to labor in the vineyard Guth planted. But if the hope is a vain one, the sooner we should know so we can seek alternative ways to forward the search into the mysteries and simplicities of this strange, beautiful Universe we inhabit.

Difficulties, both technical and philosophical, in inflationary cosmology have been studied and discussed well enough already to warrant further treatment here [5]. At the end of the day, however, inflationary cosmology critiques may be written and read at will but appear as fainthearted, ghostly words to multiverse enthusiasts: an inflaton field added to a hot big bang can indeed yield eternally-branching worlds, but this is just one 'possible physical vessel', so to speak, for an old metaphysical spirit. I ask the reader to join me on focusing on other aspect of the many-worlds building story, namely, the many-worlds view motivated by the particulars of the free parameters of the Standard Model and by the apparent fine-tuning and contingency of the constants of Nature. Relative to our most well-confirmed lawlike generalizations, initial conditions and parameters remain 'free' or 'open' in their specifications. They just 'happen to be distributed in such and such ways', they amount to mere '*de facto* contingent truths' of our world. A derivation of them neither from the laws of nature themselves nor from other principles of fundamental physics can be given. And yet, had different values of these physical parameters obtained, carbon-based life would not emerge[6] —a fact that, it is believed, 'cries out for explanation'.

Some perspective can be gained, I think, if historical record is invoked. In his *Principles of Nature and of Grace Founded on Reason* (1714), German philosopher Gottfried W. Leibniz posed a problem now known in metaphysics as the 'Primordial Existential Question' (PEQ). He asks: 'Why is there something contingent at all, rather than nothing contingent?' Two assumptions ground his concerns. These are:

(1) his Principle of Sufficient Reason (PSR), and

(2) the Null Possibility Hypothesis (i.e., the ontological thesis according to which in the absence of external cause, the natural state of affairs is one which contains nothing contingent at all).

[5] See (CARR, 2007).
[6] Or so it is believed; see (BARROW; TIPLER, 1986) and references therein.

The following passage (LEIBNIZ, 1973) makes clear how (1) and (2) are ingrained in PEQ:

> 7. Up till now we have spoken as physicists merely; we must now rise to metaphysics, making use of a great principle, commonly but little employed, which holds that nothing takes place without sufficient reason, that is to say that nothing happens without its being possible for one who has enough knowledge of things to have reason sufficient to determine why it is thus and not otherwise. This principle having been laid down, the first question we are entitled to ask will be: *Why is there something, rather than nothing?* For 'nothing' [the Null World] is simpler and easier than something. Further supposing that things must exist, it must be possible to give a reason why they must exist as they do and not otherwise. 8. Now this sufficient reason of the existence of the universe cannot be found in the series of contingent things, that is to say, of bodies and of their representation in souls [. . .] Thus the sufficient reason, which needs no further reason, must be outside this series of contingent things, and must lie in a substance which is the cause of this series, or which is a necessary being, bearing the reason of its existence within itself; otherwise we should still not have a sufficient reason, with which we could stop. And this final reason of things is called God.

Leibniz tells us that (a) the existence of something contingent *is not to be expected at all* and (b) its actual existence cries out for explanation in terms of a non-contingent sufficient reason (which he articulates in his Section 8). Leibniz's ontological imperatives set the stage for 'fundamentality' and 'unification' quests in physics, dreams of explaining the existence and nature of the universe by means unconditioned on any antecedent contingent posits. Leibniz and his scions could be challenged with a counter-question, 'why *should there be nothing contingent at all*, rather than something contingent?' The idea that Reason rules Nature is, however, too productive, too elegant to be dismissed. Philosopher-scientist Albert Einstein certainly thought so (EINSTEIN, 1970):

> I would like to state a theorem which at present cannot be based upon anything more than upon a faith in the simplicity, i.e., intelligibility, of nature: there are no arbitrary constants [. . .] that is to say, nature is so constituted that it is possible logically to lay down such strongly determined laws that within these laws only rationally completely determined constants occur [. . .].

On such grounds, I ask: what motivates scientists and philosophers not only to know how contingent certain aspects of the world are, but also to want things not to be contingent, building thus theories crafted to satisfy such desire? Many theoretical puzzles and arbitrary parameters undermine the Standard Model of Particle Physics (REDHEAD, 1996), albeit it stands 'the undefeated world-champion of empirical accuracy'. Nobel-prize winner physicist Steven Weinberg seems to endorse Einstein's faith regarding the ontological structure of Nature when he asserts that

> [The Standard Model] describes everything we see in the laboratory. Aside from leaving gravity out, it is a complete theory of what we see in nature. But it's not an entirely satisfactory theory, because it has a number of arbitrary elements [...] For example, there are a lot of numbers in this standard model that appear in the equations, and they just have to be put in to make the theory fit the observation. For example, the mass of the electron, the masses of the different quarks, the charge of the electron. If you ask, 'Why are those numbers what they are? Why, for example, is the top quark, which is the heaviest known elementary particle, something like 300,000 times heavier than the electron?' The answer is, 'We don't know. That's what fits experiment.' That is not a very satisfactory picture. (NOVA Interview, 2003).

If the world is less contingent, it is felt, then less remains to be explained. It is not just the desire to reduce intellectual labor which is at work here, though. Leibniz's voice echoes loud. The thing is, the greater the contingency, the less we know why things are a certain way rather than another.

I hope to have given the reader a taste of why questions regarding the numerical values of constants and free parameters have been tenaciously asked. Not an insignificant percentage of the who's who in theoretical physics hold our very existence to be, as a matter of fact, consequence of our universe displaying certain specific contingent features:

> [...] just how probable is it that a universe created by randomly choosing parameters will contain stars? [...] The answer, in round numbers, comes to about one chance in 10229. (SMOLIN, 1997b);
>
> If Q [the amplitude of primordial fluctuations; $Q \approx 2x10^{-5}$], were smaller than 10^{-6}, gas would never condense into gravitationally bound structures at all, and such a universe would remain forever dark and featureless, even if its initial 'mix' of atoms, dark energy and radiation were the same as our own. On the other hand, a universe where Q were substantially larger than 10^{-5}–were the initial 'ripples' were replaced by large-amplitude waves-would be a turbulent and violent place. Regions far bigger than galaxies would condense early in its history. They wouldn't fragment into stars but would instead collapse into vast black holes, each much heavier than an entire cluster of galaxies in our

> universe... Stars would be packed too close together and buffeted too frequently to retain stable planetary systems.(REES, 2000b);
>
> Virtually all detailed investigations [of entropy and cosmology] so far have taken the FRW models as their starting point, which, as we have seen, totally begs the question of the enormous number of degrees of freedom available in the gravitational field . . . The second law of thermodynamics arises because there was an enormous constraint (of a very particular kind) placed on the universe at the beginning of time, giving us the very low entropy that we need in order to start things of. (PENROSE, 1989);
>
> A lumpiness [Q] of about 10^{-5} is essential for life to get a start. But is it easy to arrange this amount of density contrast? The answer is most decidedly no! The various parameters governing the inflating universe must be chosen with great care in order to get the desired result. (SUSSKIND, 2005b);

just to cite a few. But, the credo goes, critical values of physical parameters highly-adjusted for carbon-life to develop do not jump into existence *reasonlessly*. For how *could* they? Human inquiry is the Universe's means of knowing itself—how can *that* be a result of Chance's twisted ways? Dreams of avoiding the contingency of explanation seem more and more fainthearted, however, at the end of every day within the walls of the Large Hadron Collider[7]. Supersymmetric partners were predicted with constantly increasing masses, from some GeV to some 100 GeV to LHC energies of some TeV. Nothing was found. The exiting diphoton bump with its promises of opening the empirical gates for Grand Unified Theories turned out to be a fluke. The mass of the Higgs is not natural. The cosmological constant problem keeps particle physicists awake at night. And yet, quantum electrodynamics theoretical predictions for the fine-structure constant α agree with data to within ten parts in a billion (10^{-8})! How to retain scientific realism when Logics shout 'inconsistent!' while Nature yells 'saves the phenomena!'?

One can always move the goalposts, of course. One can turn PSR upside down.

The reasoning one can follow here was championed by Spinoza. Holding that 'to everything a cause must be assigned, either for its existence or for its non-existence'[9],

[7] 'Ultimate explanations' have a more modern rubric, 'Theory of Everything'. History teaches us candidates have risen and fallen, as have the notion of what counts as 'everything'. String theory—topic of part II of this work—stands the only contender in the fundamentality arena nowadays, notions of 'everything', now, considerably more wide-encompassing: gravitation, space-time structure, and weak, strong and electromagnetic interactions must be comprehended in a single mathematical scheme. Explanatory dreams include, nonetheless, the unification of all branches of science and the calculation of all parameters and constants of Nature from first principles[8]' (BARROW; TIPLER, 1986; LESLIE, 1978; SMOLIN, 1997a). (Note that, underlying this programme, there exists the idea that de jure, there should be nothing contingent. If contingency obtains, then (a) there ought to be a reason for it and (b) this reason can be expressed in causal terminology).

[9] Ethics, part I, proposition II.

he uses this in a fancy proof of God. 'A thing necessarily exists', he writes, 'if no cause be granted to prevent this' and, he goes on, there exists nothing inside or outside God to prevent His existence. Ergo He exists, necessarily. Note the assumption here is that 'the natural thing' is for every possibility to obtain. A sufficient reason for something to exist is therefore that there exists nothing preventing its existence, rather than the other way around! Our embarrassments in failing to find a principled way of deriving the values parameters and constants of Nature can be dismissed as ungrounded, if we think along these lines. Imagine Leibniz was wrong: God did not create *just the best* of all possible worlds but is, instead, perversely energetic in His creation of physically possible worlds (that is, worlds which satisfy the natural laws which obtain in the actual world; (EARMAN, 1987; LEWIS, 1973)). If that is the case, follows there must be no surprise to find in our world values of parameters such and such and of constants of Nature such and such. If *all possible worlds obtain*, *one* world would necessarily show the values we observe. Makes no sense, in this picture, to insist on 'rather-than' metaphysical conundrums.

The multiverse worldview is revealed to us by the results of contemporary physics, many suggest. But do our most fundamental theories really carry with them implications of such magnitude? The discussion I would like to propose here in attempts to answer this question has two very distinct parts. Chapter I deals with probability theory—objective, frequentist, Bayesian—and attempts to access whether the logical and mathematical basis of many-world building can be made self-consistent and free from contradiction. Chapter II has a completely different flavor. Its main purpose is to put the metaphysical assumptions underlying multiverse theories under the philosophical microscope and ask about the legitimacy of many-worlds building in light of this analysis. It raises doubts to the effect that there is really a 'contingency problem' in physics to solve.

Elegant theoretical structures (such as those provided by modal logics and chaotic inflation mathematical framework, for instance) may give the illusion that some significant matter is thereby revealed. Such elegance, however, may disclose nothing existent. We should not allow the glitter of formalizations—even when they lead to reasonable consequences—seduce us to their acceptance and unrestricted application. There exists little one cannot prove if one is well-skilled in mathematics and is equipped with, so to speak, just the 'right' premises. My concern is to call the attention to the pitfalls of relying on *a priori* intuitions and abstract mathematical arguments alone in attempts to grasp and unveil the secrets encoded by Nature: this is certainly an attitude motivated by particular epistemological and ontological views themselves not immune to dispute. At any rate, whether or not the reader agrees with the specific conclusions and morals I draw, I hope at the end she comes away at least with an appreciation of the important role played by philosophical considerations in fundamental physics. Science and philosophy are not, to my view, independent pursuits.

2 On Fitting Speculations to Equations

Eternal inflation produces not just a very large universe, but an infinite universe containing an infinite number of pocket universes, each of which is itself infinite.

Ben Freivogel

At no time do we run greater risk of foolish and elementary error than when we use probabilities.

Ian Hacking

Both in Physics and in Philosophy freedom for speculation is a *sine qua non* condition for getting the job done—theories are 'free creations of the mind', as Einstein rightfully stated, not the inductive children of impressions summed over a blank-slated brain, as Plato, Locke and Bacon wished them to be. For good speculations, however—fruitful, edifying ones—we need solid ground, which is the only ground that can serve as springboard for further speculation. This is actually the theoretical recipe for sorting out good speculations from misleading ones. If the grounds are challenged, the whole construction can go down and different grounding—or a different building altogether— ought to be looked for. If, on the other hand, the theoretical jumping-off point is solid, we have good reason to keep on, brick by brick. Note that generally, in Science, this requirement of rigor and clarity from the starts may be neglected since Nature, the Supreme Judge, gives us feedback: *experiment* tells us whether our speculations turned out to be idle dreams or excellent guesses. Within the theoretical scenario under discussion, however, we cannot count with the aid of data. Reasoning alone is our only guide. This is why to put the very grounds of many-worlds theories under the philosophical microscope from the very beginning is so important. The chances of going astray, losing ourselves in meaningless calculations and ending up nowhere intellectually interesting diminish a good deal.

All the (vast) literature on the subject have focused, to my knowledge, on the following two topics: (a) probability arguments, and (b) anthropic arguments. The grounds of many-world building are not single-layered (as we shall see in the next chapter) but the part played by probability theory is pivotal. This is why all shapes of the argument—dressed with Bayesian and frequentist probability clothing—will be addressed in this chapter. The mathematical and logical robustness of probability arguments in many-worlds building (upon which the thesis is utterly dependent to be intelligible) are here to be accessed in the following way: each variant of the argument, shaped in accordance to a different interpretation/theory of probability, shall be discussed separately. In each case, the first step will be to present the theoretical basics of the particular theory of probability invoked to bring legitimacy to the statistical argument in question. Next, the argument itself will be presented, presuppositions made clear in each premise. Finally, the robustness of the argument thus constructed will be analyzed vis-à-vis the morals spelled by the requirements of the probability theory upon which the argument is couched. The legitimacy of the argument will be granted in accordance to the extent to which these demands of consistency and correspondence are satisfied.

A few remarks regarding the anthropic principle, to the extent it is related to the statistical arguments discussed, will also be drawn in the end of this chapter.

Aristotle has taught us (*Nichomachean Ethics*, chapter III) we cannot expect from the theory as a whole more rigor than the subject-matter admits. He is right. Here, note, the subject matter is of a sort great precision is required. We cannot afford to rely on intuitions and non-rigorous concepts and calculation. It is no doubt *a priori* defenses of a scientific theory—as is the multiverse theory—must fail necessarily. Empirical legitimacy is, however, as we have seen in this specific case, beyond our epistemological powers to obtain. This is why, I repeat, a well-defined, rigorous springboard for speculation is mandatory in this context. That statistical arguments used by many-worlds theorists satisfy the posits of the probability theory on which they are predicated is the very least we can ask of them.

It is thus to them that we now turn.

2.1 THE BAYESIAN-PREDICATED ARGUMENT

2.1.1 The Machinery of Bayes's Probability Theory

The technical apparatus of Bayesian confirmation include Bayes's theorem, probability calculus, and rules for changing degree of belief via conditionalization. A sketchy survey of these will be given. In order to obtain a proper grasp of the force of Bayes's theory, however, the following definitions and propositions authored by the Reverend himself need mentioning[1] before we get our hands dirty with technicalia:

Definition 1. The probability of an event is the ratio of the value at which an expectation depending on the happening of the event ought to be computed, and the value of the thing expected upon its happening.

Definition 2. By *chance* I mean the same as probability.

Definition 3. Events are independent when the happening of any one of them does neither increase nor abate the probability of the rest.

[1] As they are described in Bayes's 'An Essay Towards Solving a Problem in the Doctrine of Chances', 1978, reprinted in Biometrika 45 (1958), p. 296-315.

Proposition 1. When several events are inconsistent the probability of the happening of one or the other of them is the sum of the probabilities of each of them.

Proposition 2. The probability that two subsequent events will both happen is a ratio compounded of the probability of the 1^{st}, and the probability of the 2^{nd} on the supposition that the 1^{st} happens.

Corollary. Hence if of two subsequent events the probability of the 1^{st} be $\frac{a}{N}$, and the probability of both together be $\frac{P}{N}$, then the probability of the 2^{nd} on the supposition the 1^{st} happens is $\frac{P}{a}$.

Proposition 3. If there be two subsequent events, the probability of the 2^{nd} $\frac{b}{N}$ and the probability of both together $\frac{P}{N}$, and it being discovered first that the 2^{nd} event has happened, from hence I guess that the 1st event has also happened, the probability I am right is $\frac{P}{b}$.

Proposition 4. The probability that several independent events shall all happen is a ratio compounded of the probabilities of each.

We are now ready to take a look at Reverend Bayes's famous theorem. Stated in a form Bayesians of all stripes can agree, the theorem is thus: if H, K and E are respectively the hypothesis under analysis, the background knowledge, and the new evidence, then

$$Pr(H/K\&E) = \frac{Pr(H/K) \times Pr(E/(H\&K)}{Pr(E/K)}. \tag{2.1}$$

If $\{H_i\}$, i = 1, 2, ..., is a set of mutually exclusive and exhaustive hypothesis, the principle of total probability enables us to rewrite 2.1 as

$$Pr(H/K\&E) = \frac{Pr(H_i/K) \times Pr(E/(H_i\&K)}{\sum_j Pr(E/(H_j\&K) \times Pr(H_j/K)}. \tag{2.2}$$

In Bayesian theories of confirmation, the explanations of conformational virtues are couched in terms of the factors on the right sides of 2.1 and 3.1: $Pr(H/K)$, the *prior probability* of the hypothesis in question; Pr (E/H&K), the *likelihood* of the evidence given the hypothesis and background knowledge; and $Pr(E/K)$, the *prior likelihood* of the evidence.

Bayesians also share the conviction that learning from experience—the main message of Bayesian statistics—is to be modeled as conditionalization. *Strict conditionalization rules* assert that if it is learned for sure that E and if E is the strongest such proposition, then the probability functions Pr_{old} and Pr_{new}, standing for degrees of belief prior and after acquisition of new knowledge from data, are related by

$$Pr_{new}(\cdot) = Pr_{old}(\cdot/E). \tag{2.3}$$

Proposition 3, above, can be understood as an attempt to justify this rule. From the point of view of strict conditionalization, equation 2.2 makes clear how the acquisition of new evidence impacts on previous degrees of belief to yield new degrees of belief.

Bayesians are also armed with probability calculus, including countable additivity. Since hypotheses are propositions and propositions are object of belief, and since degrees of belief, in Bayesian statistics, are identified with probability, probabilities will here be assigned to propositions—in particular, to propositions which express *hypotheses*. The mathematical arsenal we need is the following:

Let $\Re$ be a set of propositions. The content and structure of $\Re$ varies from context to context, but the least required is that $\Re$ be closed under finite truth-functional combinations. We define (here I follow (EARMAN, 1992)) a probability function Pr as a map from $\Re$ to $\mathbb{R}$ satisfying (at minimum) the following restrictions:

1. $Pr(A) \geq 0, \quad \forall\, A \in \Re \qquad \text{(A1)}$
2. $Pr(A) = 1 \quad \text{if} \vdash A \qquad \text{(A2)}$
3. $Pr(A \vee B) = Pr(A) + Pr(B) \quad \text{if} \vdash (A\&B). \qquad \text{(A3)}$

Here $\vdash$ A means that A is true in all models or worlds (which will of course depend upon context). (1) to (3) suffice to prove the following principles of probability calculus:

$$Pr(\neg A) = 1 - Pr(A) \qquad \text{(P1)}$$

$$Pr(A) = Pr(B) \quad \text{if} \vdash A \leftrightarrow B \qquad \text{(P2)}$$

$$Pr(A \vee B) = Pr(A) + Pr(B) - Pr(A\&B) \qquad \text{(P3)}$$

$$Pr(A) \leq Pr(B) \quad \text{if}\, A \vdash B. \qquad \text{(P4)}$$

Here A $\vdash$ B means that A implies B, in the sense that B is true in every model or world in which A is true.

Conditional probability can here be introduced as a defined concept:

Definition: If $Pr(B) \neq 0$, then Pr (A/B) $\equiv Pr(A\&B)/Pr(B)$.

Bayes's theorem follows easily from this definition.

We also need the principle of continuity, assumed by Bayesians also. The definition runs thus:

C If $A_i \in \Re$, $i = 1, 2, ...$, are such that $A_{n+1} \vdash$ A for each n and if $\{A1, A2, ...\}$ is inconsistent, that is, they cannot all be true simultaneously in a model of possible world, then $\lim_{n\to\infty}$ Pr $(A_n) = 0$.

Now take P to be a monadic predicate and let a1, a2, ... be a (countably) infinite set of constants. We then get that

$$Pr((\forall i)Pa_i) = \lim_{n\to\infty} Pr(\sum_{i\geq n} Pa_i), \qquad \text{(A4)}$$

in which $\sum_{i\geq n} Pa_i$ stands for $Pa_1\&Pa_2\&Pa_3\&...\&Pa_n$. If we need that $(\forall i)Pa_i) \vdash Pa_n$ for every n and that $\{\neg(\forall i)Pa_1, Pa_2, ...\}$ be inconsistent, then (A4) is simply a consequence of **C** if we take $A_n \equiv (\sum_{i\geq n} Pa_i \,\&\, \neg(\forall i)Pa_i)$. It is also the case that $Pr((\exists i)Pa_i) = \lim_{n\to\infty} Pr(\bigvee(i \leq n)Pa_i)$, where $\bigvee(i \leq n)Pa_i$ stands for $Pa_1 \vee Pa_2 \vee ... \vee Pa_n$. Notice that (A4) can then be understood as an extension of finite additivity principles (A3) and (P3) to countable additivity.

Endorsing continuity and countable additivity implies the following non-intuitive consequences. Consider a denumerable infinite set $H1, H2, ...$ of pairwise incompatible and mutually exhaustible hypotheses. One may think it is possible to treat them all alike by assigning to each element equal probability, but this cannot be done consistently with **C**, since **C** implies that $\sum_{i=0}^{\infty} Pr(H_i) = 1$. Continuity thus has us playing favorites (settling with finite additivity would open the gates for results of extreme evenhandedness, like $Pr(H_i) = 0$ for all i). To abandon countable additivity, however, leads us to results that, according to Earman, 'Bayesians and non-Bayesians alike find repugnant' (for a discussion of these, see appendix 1, chapter 2 of (EARMAN, 1992).

A set is denumerable if it is equivalent to the set of natural numbers in cardinality. Needless to say, uncountable sets (like the set of real numbers, as was proved by Cantor's diagonal argument) do not behave nicely enough to undergo statistical treatment. Continuity and countable additivity are not satisfied.

One last word before I close this subsection. Different nomenclature is predicated when statisticians and mathematicians speak of probability calculus. For them, a probability space is a three-tuple $(\Omega, \Im, \mathbb{P})$. Ω, a set of elements, is called a *sample space*; $\Im$, a field of subsets of Ω, is the collection of measurable sets; $\mathbb{P}$ is a non-negative (countably additive) function from $\Im$ to $\mathbb{R}$. Countable additivity here implies that if $B_i \in \Im$, $i = 1, 2, ...$ are pairwise disjoint, then $\mathbb{P}(\bigcup_{i=1}^{\infty}(B_i) = \sum_{i=0}^{\infty} \mathbb{P}(B_i)$. One can jump from Bayesian to mathematical framework of probability by taking Ω to be the set of models of the language of $\Re$, $\Im$ to be field generated by a set of models of the form mod(A) for a proposition A $\in \Re$ and, finally, $\mathbb{P}$ to be a measure satisfying the relation $\mathbb{P}\ (A) = \mathbb{P}\ (mod(A))$. Going to the opposite direction is also possible, of course (see appendix 2, chapter 2, of (EARMAN, 1992).

2.1.2 The Many-Worlds Argument – Bayesian fashioned

Cosmologist and string theorist Joe Polchinski calculated (2015) the likelihood of us living in a multiverse by means of Bayesian probability and arrived at the following answer: 94%. John Carroll, a cosmologist and a Bayesian himself, writes that 'the multiverse shows we need to change our thinking about what science is' (2016). Nobel-laureate Steven

Weinberg, in 'Living in the Multiverse' (2007, p. 40), operationalizes degrees of belief in terms of betting transactions:

> [. . .] Martin Rees said that he was sufficiently confident about the multiverse to bet his dog's life on it, while Andrei Linde said he would bet his own life. As for me, I have just enough confidence about the multiverse to bet the lives of both Andrei Linde and Martin Rees's dog.

Perhaps that could be made rigorous if one appeals to Dutch book and strict conditionalization, but I am not sure. The life of helpless pets are at stake. Betting quotients and random Bayesian percentages aside, however, the argument these respected physicists are committed to can be stated as the following:

T1 (premise). The Laws of Physics are the same everywhere in our Universe (world). This implies there exists some sort of necessity attached to them (they are 'physically necessary').

T2 (premise). The values of the constants and free parameters of our theories, as the initial conditions which obtained in our Universe, could have been different. They are contingent aspects of Nature (SUSSKIND, 2005a; BARROW; TIPLER, 1986; CARR, 2007).

T3 (premise). From all possible values and all possible initial conditions, only a small subset of those allows carbon-based life to emerge (LESLIE, 1978; BARROW; TIPLER, 1986; REES, 2000a).

T4 (conclusion following from 1, 2, 3). The Universe we observe is improbable with respect to the set of all possible values of constants, free parameters, and initial conditions (fine-tuned for carbon-based life).

T5 (corollary from 4) The improbability of our Universe cries out for explanation (LESLIE, 1978; REES, 2000a; CARROLL; TAM, 2010).

T6 (corollary from 1, 2, 3). A choice between different contingent possibilities has somehow happened; the fundamental issue is what underlies this choice. Two possible explanations present themselves. Hypothesis 1: Cosmic Coincidence (either by (1a) creation *ex nihilo* (VILENKIN, 1982; SWINBURNE, 1991)[2] or by (1b) a self-referential or self-sustaining universe (HAWKING, 1987; GOTT; LI, 1997). Hypothesis 2: Multiverse.

T7 (premise). H1 and H2 proposals are mutually exclusive.

T8 (premise). Cosmic Coincidence does not explain away why our Universe is how it is, and not otherwise. It by-passes the contingent aspects of Nature, it does not explain them (Ellis, 2007 and references therein).

[2] Vilenkin develops a cosmological theory. Swinburne argues for the existence of God along these lines.

T9 (premise). An improbable event is more likely to occur in a long sequence of trials than in a single trial (Law of Large Numbers; see Kolmogorov, 1999).

T10 (conclusion following from 4, 5, 6, 7, 8). Hypothesis 2 is a better explanation of (T4) than is Hypothesis 1 because (a) it fits the facts better and (b) it renders the existence of our Universe more likely.

Now we have all things set to put the tools handed by Bayesian confirmation theory to work. (T10) states that

$$Pr(E\,|H_2) > Pr(E\,|H_1); \tag{2.4}$$

an inequality that follows from (T4, T5, T6, T7, T8, T9), as stated. In order to verify (T5) we must set intuitions aside and calculate; so that is what we shall do. Take (T4) to be the evidence that our Universe is fine-tuned for life. (T5) implies (trivially) that

$$Pr(T4\,|H_2) \approx 1 \quad \text{while} \quad Pr(T4\,|H_1) << 1; \tag{2.5}$$

(countable additivity) and, taking (T5) for granted, follows thus from Bayes's theorem that

$$Pr(H_2\,|T4) >> Pr(H_1\,|T4). \tag{2.6}$$

We have however no means (logical, empirical, a priori) to distribute priors unevenly between H_1 and H_2; so the priors must be roughly the same. Also, standard Bayesian epistemology requires (EARMAN, 1992) that probabilities be computed on the basis of the *total* evidence; so let us begin again. Suppose E_{total} is equal to (T4) and L is the proposition that life such as ours exist. Equation (2.1) gives us the following:

$$Pr(H_2|L\,\&E) = \frac{Pr(H_2|L) \times Pr(E|H_2\,\&L)}{Pr(E|L)} = \alpha; \quad 0 \leq \alpha \leq 1; \tag{2.7}$$

$$Pr(H_2|L\,\&E) = Pr(H_1|L\,\&E) = \frac{Pr(H_1|L) \times Pr(E|H_1\,\&L)}{Pr(E|L)} = \beta; \quad 0 \leq \beta \leq 1, \tag{2.8}$$

where $(\alpha + \beta) = 1$, as required. Recall, however, that $Pr(H_i|L\&E)$ is equal to the *prior probability* of the hypothesis in question; that $Pr(E|H_i\&L)$ is the likelihood of the evidence given the hypothesis and background knowledge and that $Pr(E|L)$ is the prior likelihood of the evidence. Here's where we get stuck: $Pr(E|H_i\&L) = Pr(E|L) = 1$. The likelihood of the evidence given the hypothesis is equal to the prior likelihood of the evidence, since we know the specific characteristics of our universe beforehand. This is known in the literature as the 'old evidence' problem (see (EARMAN, 1992), chapter V). Whatever the theory under assessment says, the likelihood of the evidence—prior or not—is equal to one. Consequence: the terms cancel. We are thus left with

$$Pr(H_2|L\&E) = Pr(H_2|L) \quad \text{and} \quad Pr(H_1|L\&E) = Pr(H_1|L), \tag{2.9}$$

but setting $Pr(L\&E) = Pr(L) = 1$ we have

$$Pr(H_2)_{new} = Pr(H_2)_{old} = \alpha \quad \text{and} \quad Pr(H_1)_{new} = Pr(H_1)_{old} = \beta. \tag{2.10}$$

Evidence cannot favor one theory over another. And as we have demanded fairness in our distribution of priors, we have that

$$Pr(H_2)_{new} = Pr(H_2)_{old} = Pr(H_1)_{new} = Pr(H_1)_{old}. \tag{2.11}$$

and thus follows that

$$\alpha = \beta. \tag{2.12}$$

Countable additivity then yields

$$1 = 2\alpha \tag{2.13}$$

and therefore

$$\alpha = \frac{1}{2}. \tag{2.14}$$

Consequence: $\alpha = \beta = \frac{1}{2}$. If we do not wish to beg the question, haunted by the ghosts of arbitrariness all the way down, and distribute priors evenly, Bayes's theorem tells us our posteriors will also be even. We have arrived nowhere interesting: the Bayesian bus has left us right where we started. The forceful conclusion is that we have as many reasons to believe the universe we observe is all there exists as we have reasons to believe an infinite number of them do. (T10) is false if predicated over Bayesian grounds.

2.2 THE FREQUENTIST ARGUMENT

The open melody of this section is that prediction is a *sine qua non* condition for confirmation. When a scientific hypothesis T is laid down to explain some event of interest X which befalls under the scope of T, it is expected that T specifies the (physical) conditions under which X does or does not occur. This 'predictive power' requirement can be stated in one of the following two forms:
1 – If a (complex) set of conditions S is realized, then event E certainly occurs.
2 – If a (complex) set of conditions S is realized, then event E cannot occur.

With respect to S, in the first case E is called a 'certain' or 'necessary' event and, conversely, on the second case E is called 'impossible' with respect to S. Let S be, for instance, water under atmospheric pressure at temperature t, $0° \leq t \leq 100°$. Water then is necessarily observed under its liquid state (case 1) and cannot be observed under gaseous or solid states (case 2). But does that mean that, given T and S, grounds for a definite answer such as 1 and 2 can always be obtained for a given E? The answer is 'no' for, for instance, reflection of a photon by a half-silvered mirror. Quantum mechanics tells me the chance of this specified outcome is $0 < p < 1$. Actually, experimental physicists know that the frequency with which this question is answered by an enthusiastic 'yes, we can!' is very low in the study of realistic physical events. We thus need to enrich our nomenclature. An event E which *vis-à-vis* conditions S sometimes does occur and sometimes does not is called a *random* event. This does not mean, however, that the randomness of E allows us to infer the absence of any law-like connection connecting E to the elements of S. It may mean that knowledge of S is partial. We are not logically omniscient, we do not have crystal balls—and physical events are a quite messy thing. Since science is a human-made enterprise, we are more often than not denied complete knowledge of S and we cannot, therefore, capture univocally the facts that establish E. We thus have to proceed in an empirical way: we watch the frequency with which E occurs and we proceed on making a mathematical investigation of it. We work out the probability of Es happening in a large number of repeated trials of the same kind.

Here's how we proceed. The assertion that event E occurs under a set of conditions S is given by some function

$$Pr(E|S) = p; \tag{2.15}$$

which must be read as 'in a sufficiently long series of trials (realizations of complex conditions S), the frequencies

$$\nu_r = \frac{\mu_r}{n_r} \tag{2.16}$$

of the occurrences of E (μ_r equals the number of tests in the r^{th} series for which event E held, n_r the number of tests in the r^{th} series) will be close to one another and close to constant p' when said out loud. From (2.15) and (2.16), principles P1-P4 sketched in the above section can be proved without much effort. But let us pause and digest the meaning of (2.15) and (2.16) a bit better. In ordinary circumstances, we are acquainted with cases where n is finite: a six-faced die; a roulette-wheel with thirty-seven slots; and so on. Mathematicians and physicists however frequently have to deal with situations in which the number n is infinite. Think of the infinite number of microscopic configurations a certain gas can display. Of the infinite number of positions in a box a point particle can choose to be in. It is interesting to notice that nothing about how small is the difference $(\nu - p)$ can be for any n is suggested. It is one thing, I stress, to have a set of formal probability axioms and pursue a mathematical investigation grounded upon these. Quite another is to understand what probability *is*.

The mathematics of probability theory is impeccable; the metaphysics is murky. Here, however, I must break off the discussion in order not to stray beyond the scope of this work. What interests us in this context are a number of theorems, called 'Laws of Large Numbers', which can be proven from $(P1 - P4) \cap (A4) \cap ((2.15) - (2.16))$ and which are meant to understand the closeness between μ/n and p[3]. These theorems prompt the

[3] The Laws of Large Numbers were first proved by Swiss mathematician James Bernoulli in his Ars Conjectandi, published in 1713.

abductive reasoning followed by many-worlds theorists worried with justifying their claims without the aid of empirical evidence. Let us then comment these theorems briefly.

In the theory of probability, typical cases are such that, in an arbitrarily long sequence of trials, it is theoretically possible to obtain one of the two extremes allowed for the frequency

$$\frac{\mu}{n} = \frac{n}{n} = 1 \quad and \quad \frac{\mu}{n} = \frac{0}{n} = 0. \tag{2.17}$$

For whatever n tests an experimentalist wishes to perform, it is thus possible to assert with confidence that we shall have for instance the inequality

$$\left| \frac{\mu}{n-p} \right| < \frac{1}{9}. \tag{2.18}$$

If event A is, to take the orthodox example, the rolling of a six-faced die, then in n trials the probability of a six coming up on all n trials is $\left(\frac{1}{6}\right)^n$. This means that with probability $\left(\frac{1}{6}\right)^n$ we shall obtain some frequency of rolling a six with value *one*; and with probability $\left(1-\frac{1}{6}\right)^n > 0$ a six will not turn up; that is, the frequency of rolling a six will be zero. This holds if the trials are independent of one another. We cannot prove that in an infinite sequence of trials the sequence would converge to the probability because there is no such thing as an 'infinite sequence'. This is an idealized notion and can only be done by a mathematical fiction. We can, however, given the independence of trials, prove that the convergence is a probabilistic certainty. This is the essence of the Law of Large Numbers both in its weak form (proven by Bernoulli, it does not require countable additivity) and in its strong form (proven by Billingstey, 1979, it does require countable additivity).

Note however how delicate the matter is. The connection between frequencies and probabilities is of a probabilistic kind. The independence of events, required to make the proof hold, is a probabilistic notion. Kolmogorov (1999) emphasizes, in a very strong manner, that the property of random-like events to become stable, that is, that in long sequence(s) of experiments, repeated under fixed conditions, their frequencies are grouped around a 'standard level' which we have labelled their 'probability', we are always guilty of unavoidable vagueness and inexactness of formulation. It is not indicated, for instance, 'how long' must the sequence n be in order for it to exhibit clearly the stability in question—it is not indicated how deviations of the frequencies $\frac{\mu}{n}$ (from one another or from their standard level 'p') are allowable for sequences of trials $n_1, n_2, ..., n_s$ of any given length. A more fundamental worry is related to the manner we form the sequences of trials in which we are to examine the stability of the frequency of occurrence of any event. Take, for instance, some arbitrary event A. To say A is 'random' or 'stochastic' and to ascribe it a definite probability

$$p = \mathbf{P}\left(\frac{A}{S}\right) \tag{2.19}$$

is possible only when the class of allowable ways of setting up series of experiments *is already determined*. This class, note, *is included*—explicitly or not—in the conditions S.

2.19 expresses the 'objective traits' of the connection between A and S. Here's what this last sentence must be read as saying: there exists no event which is, per se, *absolutely random*—the event is random or not relative to the connection in which it is considered. I have said earlier I would not let ourselves get into the metaphysical muddle that surrounds probability theory but the shape of the issue at hand here requires the reader to have in mind the following important and substantive remark: in a mathematical investigation of actual events, a theoretical model of these events must always be crafted beforehand. The space of theoretical possibilities may or may not be large and the discrepancies between actual experiments and the model chosen can, by its turn, also be subject to mathematical investigation. This is important because it teaches us that no precise definition of randomness can be given. The assumption that events are probabilistic remains valid only if certain conditions, determined by us, are kept fixed for unlimited time and with absolute exactness. Thus the passage

$$\frac{\mu}{n} \to p \tag{2.20}$$

does not have an *objective* meaning (whatever that means). It is a practical guideline.

Let us now see how all this talk on frequentist theory revolves around the question of the possibility of existing an infinite number of hidden-to-our-view universes. The reader will see that this is done by means of speculations of 'inference-to-the-best-explanation' flavor. These claims are presented in a sort of 'inverted pyramid' structure, grounded upon the (frequently misleading) rubric of the 'anthropic principle'. This 'principle', as it is called, states the following: given the fact that observers exist in the world we know, it follows that such a world—improbable as it is, remarkably fine-tuned as it is—is possible. Fair enough, we say. Trivial and vacuously true. We still do not know why such a world—our world—sprung into existence. Many-worlds theorists, acquainted with the tools of probability theory and abhorred by the intelligent design hypothesis, are happy to supply the answer. Looking at the world through the glasses of the Laws of Large Numbers, they say the probability of existence of a world such as ours goes to certainty after an infinite number of trials. Physics teaches us that the Laws of Nature are true 'by means of necessity'. Free parameters, constants and initial conditions, however, have resisted all attempts to have their numerical values deduced from first principles. They are a contingent matter and could have easily been different, they suggest. Thus there is no contradiction on turning possible worlds[4] to concrete, physical worlds. If universe-creation is a random event in a Bernoulli measure—like getting an even number at a throw of a die, for instance— then the laws of large numbers guarantee that a carbon-based universe, populated by beings iteratively haunted by prisoner's dilemmas, does exist in the vastness of the multiverse. The many-worlds hypothesis, argue these theorists, at the end of the day actually *predicts* the existence of our world.

[4] 'Possible' here means 'allowable by the laws of physics'.

A more systematic presentation of these claims will have us profit in clarity. This is what these physicists are saying:

1. *Because* our world is possible, it is can be thought of as an element of a set of possible worlds.

2. The set of all possible outcomes for, say, double-dice rolling, is a Bernoulli measure. This sample set is discrete and the distribution of outcomes is represented by a frequency distribution function. There is a constant probability $0 < p < 1$ for each possible outcome of independent trials. Now consider, following (EARMAN, 1987), a Cosmic Dart Board. The points of this Board correspond to one-to-one the elements of the set of possible worlds $\Im$; the area A of the board is equal to the Bernoulli measure μ of the set. Worlds are actualized whenever the Creator decides to throw darts randomly at the board; the distribution of outcomes is also given by some function of μ. The random dart throwing method also gives probability $0 < p < 1$ for each possible big bang model α_i that may be actualized; $\alpha_i \in \Im \quad (\Im = \sum_{i=0}^{\infty} \alpha_i;\ i = 1, 2, ..., n;\ n \in \mathbb{R})$.

3. Given (1) and (2), our world is an element $\alpha_i$of $\Im$ with probability of actualization $0 < p < 1$. We know by the Laws of Large Numbers that this probability goes to 1 when the number of independent trials approaches infinity: if the Creator fires his dart at the Board promiscuously, our world is likely to spring into existence with probability 1—just like any other big bang model available at the Universe Menu crafted by the Lord.

4. Our world did spring into existence. That is notoriously difficult to explain, given the remarkable features this particular world displays, *vis-à-vis* the scientific facts known by us. It is not if we consider a random mechanism of universe creation. If we keep the many-world cake, we have the nutritional advantages of explaining (i) why our world exists (ii) why we have been unsuccessful on our attempts to explain the values of the constants of Nature and free parameters from first principles. This is an 'almost *a priori*' rationale, but it is conceptually robust and we do not have empirical reasons to dismiss it. The mathematics backs it up. Probability theory backs it up.

5. No alternatives that explain (i) and (ii) above were thought up. Isn't something better than nothing? A mechanism of random world creation fits all known facts. This mechanism is undetectable by us, but the anthropic principle has taught us something precious. It has taught us that our nature as observers conditions the kind of evidence we are likely to come upon. We must be aware we are thus biased and we must take this bias into account. It is therefore reasonable to believe in the

> many-world hypothesis. Its support from evidence is vacuous all right but it has great explanatory power. It is the best explanation for all facts given.

I acknowledge the self-seduction of endorsing this kind of reasoning. A closer look, however, reveals it is identical in structure to the 'god-of-the-gaps' inference sort. 'Inference to the best explanation' (IBE), or 'abduction', as it is sometimes known called, is one kind of inference (the others being deduction and induction). The charges held against it are many and severe and need not be covered here (I direct the reader to (VAN FRAASSEN, 1989; VAN FRAASSEN, 1985), where the charges are brilliantly exposed. Defense claims are supplied by (PSILLOS, 199)). No defense of IBE, however, is crafted along a priori lines: those who flirt with the idea that IBE is a sound reasoning type do not subscribe to a conception of truth that establishes a link between explanatory power and truth. IBE practitioners do not estipulate explanatory superiority to be a necessary condition to validate truth claims. Rather, they suggest, we ought to speak of 'pragmatic virtues', 'fallibilistic attitudes' and 'learning from experience'.

It is not difficult to see that if these are the epistemological posits that make up the metrics according to which we must evaluate the merits of IBE-framed hypotheses, many-worlds theories—designed along the procedural rationale of abduction—fail miserably to meet no less but all of them. Needlessly to say, there is no 'learning from experience': 'infinite worlds exists parallel to ours' has the same evidential support as has the 'Jehovah exists and rules the world' hypothesis. There is nothing 'fallibilistic' about it either: hidden from our sight, no piece of evidence can show other worlds do not exist. Shielded from the attack of empiricists by the anthropic principle, it is unfalsifiable thesis *ab initio*. Finally, although it is correct to say that abductive reasoning is employed routinely in everyday affairs[5] we cannot afford to invoke pragmatism when what is at stake is the 'ultimate structure of reality'. These things are not compatible with one another. Pragmatism's maxim is to work out the *practical consequences* of a theory T in order to clarify the theoretical content of T. This implies that it is solely through *experience* that fundamental concepts such as 'truth' and 'reality' can be accounted for:

> Any idea upon which we can ride ...; any idea that will carry us prosperously from any one part of our experience to any other part, linking things satisfactorily, working securely, saving labor; is true for just so much, true in so far forth, true *instrumentally* (JAMES, 1907).

Nothing instrumental, note, springs from knowing why our world exists and why the constants of Nature exhibit the values they do. Those who seek answers for this kind of questions have a completely different motivation in their hearts and a different attitude

[5] See (DASCAL, 1979) and (HOBBS, 2004).

towards knowledge-seeking, science, and philosophy than those who work on the vineyard planted by James and Pierce. The 'shut up and calculate' motto Feynman has ingrained in the minds of many scientists, teaching them how to do science (without questioning orthodoxy! without philosophy!), just does not do for those who contemplate Nature and, baffled by it, eager to know the fundamentals of the structures that govern its behavior. 'Usefulness' is not enough for them. What they seek to know, to use Hawking's apt phrase, is 'what breathes fire into the equations'.

Perhaps this is the kind of pseudo-question Wittgenstein has warned us about. I wish not to discuss this matter here, this is not the place for it. I wish solely to highlight the contradiction. If many-worlds theories are crafted by means of abduction and if this sort of reasoning is to be assessed—because so construed—by the metrics of evidential relevance, usefulness, predictive power, and so forth, it is fair to say that many-worlds theories simply does not warrant acceptance. They are, as a matter of fact, construed as IBE-type explanations; they do not, however, give to this kind of rationale any qualified endorsement.

But let us assume for the sake of the argument we live in a world in which my critiques are unsound and in which many-worlds theories do display the features we think IBE-kind explanations have and are due praise (assuming, again for the sake of the argument, that they do). It remains to ask if the explanatory purpose is served, that is, if a many-world theory backboned by frequentist probability is credible given the tenets of theory-assessment and confirmation philosophers of science of all flavors can agree on. Well, is it?

Only one response suggests itself, a negative one. The many-worlds argument rests on a specific premise to hold, namely, 'the existence of an infinite series of universes boosts the probability of existence of our world up to one'. I have worked out the specifics of this argument above: it is backed up by the Laws of Large Numbers. The existence of this infinite series of universes is, however, precisely what is at stake. She who follows this reasoning is caught up in a vicious circle.

If pushed a bit more, more epistemological flaws flow from this argument. We find ourselves puzzled when we try to understand the mathematics implied by the frequentist theory according to which the whole many-world case is built upon: if our measure is infinite and the sequence is random—'all possible worlds obtain'—then the probability of having a universe of characteristics such and such are nil. If that is the case, note, no anthropic principle can settle the issue. Such a result holds for all universes. If the addictive property holds—as it must—, then the sample space can be obtained as the union of disjoint zero-probability events. A paradoxical result, however, seems to follow:

$$P(\Omega) = P\left(\bigcup_{\omega\in\Omega} \{\omega\}\right) = \sum_{\omega\in\Omega} P(\omega) = 0 \tag{2.21}$$

when, by the axioms A1-A3, it should be $P(\omega) = 1$ (here each $\omega \in \Omega$ is a zero probability event). Mathematicians would complain the controversy is inexistent since we have mistakenly taken the set in case for a countable set. The measure of worlds must be, they will say, a finite one: end of story. But then, complains the philosopher, we are taken full circle back: why has the Creator chosen this specific series of worlds, and not any other? Why only initial conditions such and such have sprung into existence, and not others? We would have a specific infinite series of worlds to explain; and something akin to the traditional argument from Design seems to be the only way out of these conundrums.

Closing, I take these questions to be, at the end of the day, mooted. Many-worlds landscapes can be created by most inflation cosmology models; by superstring theories; by broadening one's wave-function until it embraces the entire universe. Others may exist. The 'many-worlds thesis' is, to my view, not a thesis *per se*. It is an attitude towards problem-solving. Those who like the many-worlds recipe seem willing to embrace any form of universe crafting that gives purchase to their *modus operandi* [6]. Matters do not settle here however, and there exists a more comprehensive perspective from which we can look at this issue, so far neglected by the literature. The consequences elucidated by taking this different viewpoint shall allow us to draw more far-reaching philosophical conclusions, I hope. New challenges are posed and tackled in the next chapter.

[6] Ian Hacking writes, 'buying one model of many-universes seems to help one develop a taste for others' (HACKING, 1987). His strikes me a well-motivated diagnosis.

3 On Explaining Everything

[In] our description of the space M of possible universes m, we must recognize that it is based on an assumed set of laws of behavior, either laws of physics or meta-laws that determine the laws of physics, which all m have in common. Without this, we have no basis for defining it.

Ellis et al., 2009

If we were to conceive the whole order of nature, we should find that there are many things that—by their nature—could not exist.

Baruch Spinoza, 1663

'Multiverse' or 'many-worlds' theories come in all sorts of flavors. In Philosophy, it is owed to Dawid Lewis the credit for having opened the gates to one of the most hotly pursued approaches to Metaphysics—Possible Worlds Metaphysics, that is—, the development of which led to new avenues of investigation in Logics and Semantics also. In his Counterfactuals, it is with words tinged with blatant realism that he argues in favor of this new, rather extreme attitude towards what there is:

> I believe that there are possible worlds other than the one we happen to inhabit… I emphatically do not identify possible worlds with respectable linguistic entities; I take them to be respectable entities in their own right. When I profess realism about possible worlds, I mean to be taken literally. Possible worlds are what they are, and not some other thing. If asked what sort of thing they are, I cannot give the sort of reply my questioner probably expects: that is, a proposal to reduce possible worlds to something else. I can only ask him to admit that he knows what sort of thing the actual world is, and then explain that other worlds are more things of that sort, different not in kind but only to what goes on at them. Our actual world is only one world among others. (LEWIS, 1973)

Quantum Mechanics provides a framework in which other worlds are invoked to play a causal explanatory role whose structure in nothing resembles that of the objects posited by Lewis. Theoretical physicist Bryce S. DeWitt and his colleague Neill Graham, sharing the sentiments of those discontent with the readings of quantum phenomena associated with Niels Bohr and Werner Heisenberg, open their *The Many-Worlds Interpretation of Quantum Mechanics* (1973, p. v) by asking us to withdraw orthodoxy and consider the following:

> Everett [. . .] propounded a new interpretation of quantum mechanics that denies the existence of a separate classical realm and asserts that it makes sense to talk about a state vector for the entire universe. The state vector never collapses, and hence reality as a whole is rigorously deterministic. This reality, which is described jointly by the dynamical variables and the state vector is not the reality we customarily think of, but as a reality composed of many worlds. By virtue of the temporal development of the dynamical variables the state vector decomposes naturally in orthogonal vectors, reflecting a continual splitting of the universe into a multitude of mutually unobservable but equally real worlds [. . .]. (DEWITT; GRAHAM, 1973)

Everett himself assigns no operative character, but goes as far as Lewis in attributing full-blown actuality to the entities whose existence he was the first to anticipate:

> It is [. . .] improper to attribute any less validity or "reality" to any element of a superposition than any other element, due to [the] ever present possibility of obtaining interference effects between the elements, all elements of the superposition must be regarding as simultaneously existing. (EVERETT, 1973)

This is meant by Everett to be the solution for the so-called *measurement problem* in quantum mechanics. Note the embarrassing arbitrariness attached to the reduction of the wave-packet is eradicated: as the quantum system is split into different branches (one for each term of the superposition), the dynamics of Schrödinger equation is not affected upon measurement (randomness and action-at-a-distance are consequently exorcised from the theory as well). Now, a lot of ink has been spilled lately on attempts to apply equal medicine—many worlds, that is—to cure equal disease—arbitrariness, not to the extent it infects quantum mechanics solely however, but the whole of Physics. This is the table over which the 'multiverse scenario' in Cosmology was actually forged and it is *this* particular many-world model my primary concern here. Since an understanding of how the problem came about naturally antecedes the assessment of any proposed solution, it is pressing we first set the discussion in context.

John D. Barrow and Frank J. Tipler have put a lot of effort, from the mid-60s onwards, into calling the attention of the scientific community to a torrent of anthropic coincidences responsible, they suggest, for the emergence of complexity. These 'anthropic facts' ranged from the atomic scale (the mass of the electron, for instance) to the cosmic one (the initial conditions that obtained at the infinitesimal moment of time after the big-bang initial singularity) and were interpreted as *evidence* the world shows its existence and nature to be matters of *necessity.* The mainstream view—according to which laws of nature are necessary, whilst constants and initial conditions acquire arbitrary values and are, as such, contingent aspects of Nature—are usually taken to indicate the possibility of many worlds (or, if you wish, different 'recipes' for those), differentiated by their contingent specifications. Anthropic facts were viewed, however, as an indication that only a single possible world, this one, was possible: its specifications included. Eddington's 'fundamental theory quest' and von Weizäcker 'transcendental deduction of the basic features of Nature' had their hyper-Platonism resurrected by anthropic data, and the stage was set for an overall program in physics that sought to avoid the contingency and arbitrariness of explanation by means of formulating a unique, self-consistent *Theory of Everything.* This program was given a fighting chance by the hands of the S-matrix (according to the bootstrap philosophy of the S-matrix, the general constraints of the theory ought to 'fix particle widths as well as particle masses' (CHEW, 1970)) and achieved highest momentum in the aftermath of superstring theories (in which all parameters are required to be derived by first principles and all concepts justified by self-consistency). It was, however, acknowledged arbitrariness could not be removed—let alone unconditioned explanations obtained—when string theories were found to admit $\sim 10^{500}$ vacuum solutions. The possibility to formulate an unconditioned account of the conditioned was, then, put under a cloud.

Distaste for arbitrariness in Physics remained, however. Ironically, the most commonly adopted maneuver to minimize arbitrariness amounts to going full-circle back to the previously dismissed many-worlds picture: with one possible universe, note, we do have reduction, but *some* arbitrariness (why *these* values, *these* constants, *these* initial conditions—and no others?) With an *infinity of them*, though—the whole of the multiverse preferring no specific way to any other, but giving each and every possibility its place and due—we have the *least arbitrariness possible.* Just as Everett exorcised the arbitrariness attached to the collapse mechanism by attributing world-independent existence to each term of the superposition, in a similar fashion it has been suggested the arbitrariness of contingent aspects of Nature—the values of constants and initial conditions, that is—is eradicated if to each class of physically possible worlds allowed by the cosmic recipe, 'general, necessary laws; particular, arbitrary values of constants and initial conditions', independent actuality in different space-time regions is conceded (identical rationale was applied to the embarrassment of riches found in string theory: Leonard Susskind suggests the $\sim 10^{500}$ vacuum solutions describe a 'string multiverse', the so-called 'cosmic landscape'[1]). As of this writing, the view according to which the multiverse 'explains the unexplained' within physics enjoys significant popularity in the scientific community.

1 See (SUSSKIND, 2005a).

It is my opinion the multiverse hypothesis, if so construed, can't be put to work. There exist cases drawn from physics which force us to think twice before asserting that, even if the lawlike is restricted to the realm of the necessary, the particulars, the specific values of constants, parameters and the initial state of the world at a given time, are contingent. These examples show the superficiality of the standard 'necessary laws, arbitrary parameters' view and by such means call into question the legitimacy of the very problem many universes are asked to solve. The cases we will look at below shall suffice, I hope, to support the claim that constants and initial conditions are not so contingent and arbitrary as one may think—their inter-relation with the laws being of a complex, rather delicate nature.

1

A sermonette on the meaning and importance of the constants of Nature seems to precede an analysis of their ability to 'support counterfactuals'. At a meeting of the Royal Society in 1983, Steven Weinberg gave a talk entitled 'Overview of theoretical prospects for understanding the values of fundamental constants'. There he proposed the following definition for the term 'constants of Nature', now widely invoked: 'we cannot calculate [them] with precision [sic] in terms of more fundamental constants', he says, 'not just because the computation is too complicated [. . .], but because we do not know of anything more fundamental' (WEINBERG, 1983). He concludes by claiming, 'each constant [. . .] is a challenge for future work, to try to explain its value' (ibidem). (ELLIS; KIRCHNER; STOEGER, 2003) proposes these constants are to be distinguished into two sets: 'dimensionless ratios' constants (called also 'fundamental parameters', these constants are 'pure numbers': they are invariant under change of parametrization) and 'dimensional' constants ((VENEZIANO; OKUN; DUFF, 2001) call them 'fundamental units'). Given that measurement of a physical quantity amounts to comparison with a particular physical system chosen to be reference, one must look for constant variation that does not simply amount to unit redesignation. Unit systems are arbitrary and the values the constants of nature take are subjected to this arbitrariness (e.g. the electron rest mass is 5.486×10^{-4} atomic units, and 9.109×10^{-31} kg). It is thus easier to investigate the variation of dimensionless quantities, immune as they stand to unit system choices. One way variance of dimensional constants can be made meaningful is by means of the following procedure: you may force dimensional constant x_i to vary while specifying all other constants x_j, $i \neq j$, are kept fixed. Dirac, for instance (himself inspired by Eddington's aforementioned 'fundamentality quest') has famously proposed a theory back in the 30s in which the gravitational constant G changes as the inverse of the cosmic time, but in atomic units, forcing the electron rest mass to hold fixed. His is thus a theory in which the dimensionless quantity $\frac{2\pi G m^2 e}{hc}$ changes.

We shall focus our attention on the speed of light, c. Our initial task shall be to consider the implications for the lawlike structure of physics when action at a distance is conceded to obtain—that is, if the speed of the light is said to be 'infinite' or, equivalently, if tachyons are allowed to exist. Next, the possibility of a 'varying speed of light' will be analyzed and the related consequences for the laws of nature, accessed. The simplest case scenario—the speed of light simply taking different (constant) values, greater and/or lower, in contrast with the measured 299,792,458 meters per second (SI units)—is considered at last. There are three study cases, four drawn consequences—the second of which applies to the 'action-at-a-distance' and 'dynamical variable' ones simultaneously.

1. The effects (physical, conceptual) of light propagation outside the null-cone are topic of perennial discussion in the foundations of relativity theory. (SKLAR, 1990) and (EARMAN, 1972), for instance, call our attention (sweeping aside all the technicalia involved in the inferences) to the following conclusions of interest: first, *ceteris paribus*, causality is compromised. This is how it happens: within the confines of the infinite, flat Minkowski spacetime of special relativity, any event x' outside the null cone of some reference event x is in every time order (in the past of, simultaneous to, in the future with respect with) relative to x for at least *some* observers. So, given the usual relativistic requirements to determine the simultaneity of events for observers at some distance from one another, if you make a tachyonic signal propagate from x there will exist some observers (chosen as inertial frames) relative to which the tachyonic signal will be received as if propagating from its origin event (at x) to distant events *earlier in time* relative to x. The 'shooting-myself-before-pulling-the-trigger-that-launches-the-bullet' sort of paradoxes that emerge in this scenario are easy to acknowledge. Thus if we are not willing to let go the causality principles we hold dear—event never precedes cause; causal order is objective (that is, the same for all observers)—, then these paradoxes are not problems in appearance only, but must be seriously considered. Sklar argues causal loops so constructed (constructed, that is, by means of postulating superluminal causal propagation whilst retaining Minkowski spacetime) can be avoided if, and only if, it is stipulated that all states of the world at a given time (relative to any chosen inertial frame) are self-consistent—meaning, if some initial conditions are excluded from the possibility set. Because connected to themselves as self-determiners in a lawlike manner, these initial conditions may interfere with themselves in an inconsistent way (as exemplified by the aforementioned suicide paradox or, if you prefer, by any variant of those famous time travelers paradoxingly murdering their ancestors). Sklar's conclusion, note, is thus unavoidable: if any specification of initial conditions are fed into the causal laws, then these causal loops imply some specification of initial states are themselves impossibilities.

If the variety of worlds differentiated by particular facts are to obey the laws of nature, investigation shows constraints must be imposed either on the value of c or on the specifications of initial conditions. Would this fact not suffice to cast doubt on the tenability of the 'necessary laws, arbitrary parameters' cosmic recipe?

2. More immediately, however, an unacceptable consequence of causal propagation outside the null cone is the breakdown of Lorentz invariance. From quantum field theory to general relativity, from measurements of electroweak boson masses to the redshifts of quasars light signals that reach Earth, *all physics is Lorentz-invariant.* If it is allowed to break, how this affects every bit of the lawlike structure of our theories remains to be seen (we shall look briefly at one particular example in the next section). Of course the question of what, precisely, Lorentz invariance for a set of laws amounts to also requires answer, but since coming into grips to it would lead us too far astray, I will simply pose it here for further study. What can be in fact said is that to write non-Lorentz invariant laws of Nature in covariant form, further non-trivial nomic structure must be added (for instance, for it to be done in special relativity, Minkowski metric needs to be replaced by a pseudo-Riemannian one—a move that takes us beyond special relativity in the usual sense. See (FOX; KUPER; LIPSON, 1970) for details). The descriptive and explanatory role of the Lorentz-invariant dynamical equations that we honor with lawlike status no longer makes sense in this scenario and our worldview fades in metaphysical ether. This result, note, follows from exploring the space of possibilities entailed by the 'contingent values of constants and initial conditions' picture that grounds many-world building as it goes against, by the same token, the assumed necessity attributed to the laws of Nature as currently understood. Was it not, however, the arbitrariness of the former, *given the necessity of the latter*, that motivated the construction of an infinite variety of worlds in the first place?

3. The same results apply if instead of admitting $v_{lim} \to \infty$ one shifts the role of c to that of a dynamical variable. Many scientific work-hours have been spent lately on attempts to construct a 'varying-speed-of-light theory' (VSL, for simplicity. There are 86 entries on this topic listed on arXiv from 2010 to January 5^{th}, 2018). It is offered as an alternative to inflationary cosmology in resolving fine-tuning anomalies that undermine orthodox, inflation-free big bang theory. (ELLIS; KIRCHNER; STOEGER, 2003; ELLIS, 2007) has built a strong case to the effect of showing that, in spite of the interesting motivation that trigger their efforts, VSL cosmologists' theory crafting methods are unsound: on relaxing the constant character of the speed of light, attention has not been paid to the implications of this procedure to the set of laws whose validity is assumed in deriving the theory's dynamical consequences. This is what he calls our attention to: consider a curved spacetime with line element $ds = g_{\mu\nu}dx^{\mu}dx^{\nu}$, where $g_{\mu\nu}$ is the metric tensor. How are we to measure time and distance? The most natural way to proceed is to define proper time τ by means of the relation $ds = -c^2 d\tau$ (this choice guarantees the limiting speed given by $ds = 0$ is the same in all inertial frames. It also yields the required time-dilation effect). In terms of the metric tensor, we clearly have $-c^2 d\tau = g_{00}(dx^0)^2$ for any observer at rest with respect to the coordinate system (x^{α}). In general relativity, spatial distance dl between two points, say, x^i and $x^i + dx^i$ is generally defined as the *radar distance*. This latter is

given by $c/2$ times the proper time as measured by an observer at x^i for a signal to go from that point to $x^i + dx^i$ and back. Thus dl reads

$$dl = \gamma_{ij} dx^i dx^j; \quad \gamma_{ij} = g_{ij} - \frac{g_{0i} g_{0j}}{g_{00}}, \tag{3.1}$$

$i, j = 1, 2, 3$. It is clear the determination of distance requires (a) measurement of time and (b) signal exchange between two points. For the laws as they stand, all works perfectly, for light propagates with universal speed c. If one allows c to change or go to infinity, however, light no longer follows the null geodesic of the metric—*and we no longer know what geometric meaning, if any, the metric tensor has neither how to measure time and spatial distances*. Furthermore, we know from Maxwell's theory that an electromagnetic wave in the vacuum is given by

$$(\partial_t^2 - c^2 \Delta)(\Phi, \mathbf{A}) = 0, \tag{3.2}$$

where $\mathbf{A}$ stands for the potential and Δ the Laplacian. c—the velocity of any electromagnetic wave, and thus of light, in the vacuum—is calculated by and experimentally verified with great precision as

$$c^2 = \frac{1}{\mu_0 \varepsilon_0}; \tag{3.3}$$

where μ_0 and ε_0 denote the permeability and permittivity of space respectively. It is clear that, if c is allowed to vary, then one must propose other equations than Maxwell's to govern electromagnetism. Ellis's complains are to the effect of the lack of consistency on abandoning a particular aspect of physics and neglecting the aftermaths of this maneuver on the theoretical structure in which the given particularity is embedded. Physics, he insists, is an 'integrated whole': 'you can't just alter the speed of light in one or two equations and leave the rest of physics unchanged' (ELLIS, 2007).

Now, I suppose infinite and varying values for c do strike us as possibilities compatible with the 'necessary laws *vs.* contingent specifications' cosmic recipe used to actualize counterfactuals—unless one attempts to evade criticism by claiming an impossibility by *fiat* that c cannot, in fact, take such forms. 'Only finite, constant values are within the possibility set', one may ad-hocly insist. But can the nomic structure of our theories in fact be preserved from bankruptcy if, instead of 'giving each possibility its place and due' in the multiverse, we restrict possibility space to a selective measure cognizable for yielding only finite, constant values for c smaller or greater than the velocity experience has shown light to exhibit in *our world*?

4. Let us first consider the speed of light to be different—faster—than it actually is for, say, ten orders of magnitude: make it 3.00×10^{18}m/s. For physical implications to happen, the fate of other constants ought to be also defined. Simplest case scenario considered, the electron charge e, the mass of the electron m_e, the mass of the proton m_p and Planck's reduced constant $\hbar$ are presumed not to change. e, m_e and $\hbar$ suffice to

determine physical dimensionality, so our unit of length shall be radius of the hydrogen atom (or, more technically, the Bohr radius):

$$r_B = \frac{\hbar^2}{m_e e^2}. \tag{3.4}$$

As unit of energy, the Bohr energy

$$E_B = \frac{e^2}{r_B} = \frac{m_e e^4}{\hbar^2} \tag{3.5}$$

and

$$t_B = \frac{\hbar}{E_B} = \frac{\hbar^3}{m_e e^4}, \tag{3.6}$$

$$v_B = \frac{r_B}{t_B} = \frac{e^2}{\hbar} \tag{3.7}$$

the units of time and velocity, respectively. An atom is, then, both our clock and our ruler.

Stage is set for counterfactual reasoning to start. Since we have assumed the charge and mass of the electron to hold fixed, we can speculate that chemistry would not be affected because reactions are regulated by electron exchange. Nature as we know it, however, would be denied its sublime beauty: the properties of photons would change dramatically. Think of the consequences: for the same energy that is determined by the levels of the atom, the photon emitted by it would display momentum p ten orders of magnitude smaller and wavelength λ ten orders of magnitude longer:

$$p = \frac{E}{c}; \quad \lambda = \frac{\hbar}{p} = \frac{\hbar c}{E}. \tag{3.8}$$

We know from quantum mechanics that, if an atom is excited, a photon is emitted with probability proportional to its phase space and, *a fortiori*, to $p^2 dp$. Note, however, that

$$p^2 dp = \frac{E^2 dE}{c^3}, \tag{3.9}$$

what implies the time for an atom to radiate away a photon *would exceed the age of the universe*, 4.3×10^{17}s. Contrast this information with the workings of our universe: ultrafast spectroscopy experiments (experiments in which the time for an excited electron to transition back to a lower energy level is measured), have pico-second (10^{-12}) time scale. Recall further that the Rayleigh scattering of light by an atom is

$$\sigma_R \propto \left(\frac{e^2 r_B^2}{E_B}\right)^2 \left(\frac{\omega}{c}\right)^4 \propto r_B^2 \left(\frac{v_B}{c}\right)^4, \tag{3.10}$$

where $\omega = \frac{R}{\hbar}$, as usual. The Thompson cross-section for photon scattering by free electrons is in this context reduced by around 40 orders of magnitude:

$$\sigma_T = \left(\frac{8\pi}{3}\right)\left(\frac{e^2}{m_e c^2}\right)^2. \tag{3.11}$$

3.10 and 3.11 tell us $c = 3.00 \times 10^{18}$m/s leads to a universe where photons *do not couple to matter*. It is safe to say universes so constructed are not described by general relativity, by quantum mechanics, for that matter: no stars shine in this world, no gravitational waves can be detected, no physics textbooks can be read—there can be no eyes to look at them. Is it not implicit in the 'necessity' attribute attached to the laws of Nature, however, their holding in 'all possible worlds'? Nomic necessity also faces onslaught if one goes to the opposite direction, forcing c do take low values (reference to $c = 3.00 \times 10^{18}$m/s is assumed throughout). The most obvious place to look for effects is to the fine-structure constant α,

$$\alpha \approx \frac{e^2}{\hbar c} = \frac{v_B}{c}. \tag{3.12}$$

Nature has trusted α with heavy burdens: it is the parameter that characterizes the role of relativistic effects in the hydrogen atom. When She speaks, experimentalists hear $\alpha \approx \frac{1}{137}$. Were, note, the speed of light decreased by two orders of magnitude, it would margin the velocity of an electron bounded to a hydrogen nucleus—turning quantum physics as we know it (non-relativistic and quantum field theory) into science fiction[2].

We could go on playing with the speed of light and the physical aftermaths this game implies, perhaps sharpening the results, but I think we need not: the seriousness of the implications of relying on intuitive notions of 'necessity' and 'contingency' as licenses to infer counterfactuals have been, I hope, demonstrated. We have so far studied (briefly) only the behavior of c under the presumption its value can be changed, but the conclusions drawn generalize easily to other constants. It is clear that once certain specifications for the behavior of c (or G, or $\hbar$, or α—what have you) are tolerated as possibilities that delineate 'possible worlds', one finds after closer inspection that the idea the laws found within our theories are 'necessary', valid 'everywhere and everywhen' ought to be dropped. If one insists on holding onto the latter, one must conversely admit that not every stipulation of values for constants and/or initial conditions is a legitimate stipulation. Worries to rejuvenate Leibnizianism aside, the morals is that one cannot, in fact, keep one's metaphysical cake and eat it too.

2

The nature of physical possibility is no playing field. When one strives for philosophical comprehensiveness in electromagnetism, for instance, reflection shows the magnetic field B to be necessary consequence of relativistic effects on accelerated electric charges (check (PURCELL, 1985)). Follows that there can be, note, *no electromagnetic waves in a non-Lorentz invariant universe.* In general relativity, curved spacetimes that do satisfy the

[2] In QED: The Strange Theory of Light and Matter Richard Feynman writes (1985, p. 129), 'all good theoretical physicists put [the fine-structure constant] number on the wall and worry about it'. I can make no sense of his assertion, but there does exist a vast literature on attempts to understand the constraints $\alpha \approx \frac{1}{137}$ poses on phenomena. I direct the reader to (UZAN, 2002) for a comprehensive survey on the topic.

field equations can suffer causal pathologies (as in Gödel's world model, for instance) prompted by closed timelike loops (GÖDEL, 1949), light obediently propagating inside the null-cone notwithstanding (FRIEDMAN et al., 1990). These causal degeneracies can be evaded if consistency constraints are evoked (HAWKING; ELLIS, 1973; SKLAR, 1990; EARMAN, 1995), which is another way of saying initial conditions are not up for grabs whimsically, but must be cleverly chosen. More generally, studies in general relativity have shown that, in constructing solutions to the field equations, it is not enough to find a mass-energy distribution and having it 'cause' spacetime structure: the mass-energy distribution asks for an antecedent specification of the metric structure of the spacetime in which the distribution is to be embedded. Consistency-checks are needed for physically reasonable solutions to be in order. The laws alone do not suffice.

In (FRISCH, 2004) and (FRISCH, 2005), Mathias Frisch brings forward an example from classical electrodynamics where physically reasonable initial conditions, once fed into the Maxwell-Lorentz equations, can find no sensible solution—leading him to conclude such specifications cannot possibly obtain. He infers the 'necessary laws, contingent parameters standard picture' for delineating possible worlds does not fit certain aspects of classical electromagnetism. Sharing his diagnosis, I fail to understand how come the 'standard view', as Frisch calls it, finds so great reception given the sort of counterinstance he asks us to consider is not exceptional in physics, but quite the opposite. The following example from introductory quantum mechanics is the first to come to mind: when the Schrödinger equation is solved in three dimensions (spherical coordinates) by the separation of variables technique, for instance, we learn from the solution of the θ-dependent equation that for any value of l (the azimuthal quantum number) there are $(2l+1)$ possible values of m (the magnetic quantum number). But the θ-dependent equation is a second order differential equation and admits, as such, two linearly independent solutions, for any values of l and m—and indeed one finds that $A\, ln[tan\left(\frac{\theta}{2}\right)]$ also satisfies it for $l = m$. This is said to imply 'physically unacceptable' results (check (GRIFFITHS, 1995), so we stick to the nicely behaved $(2l+1)$ aftermath. The immediate question then is, why are some initial conditions 'allowed', and others 'forbidden'? At a more profound level, (PENROSE, 1989) traces the origin of the arrow of time to the special conditions that characterized the big-bang initial singularity (for all the well-known reasons, statistical mechanics does not bear the burden of explaining away entropic increase). Arbitrary a solution as it may sound, Penrose's suggestion does pinpoint one important possibility: thermodynamics does not rule universes which have not themselves expanded from rather special initial states. Alternatively, (SKLAR, 1990) defends there to exist time-asymmetric probability constraints over initial micro-states that do determine the temporal evolution of systems (one needs not, says Sklar, go as far as the big-bang to explain the workings of the Second Law). This is, of course, no place to argue for either position. One way or another, the main philosophical consequence remains: we have accumulated results that find, once again, no accommodation in the 'fixed laws and arbitrary initial conditions' metaphysical scheme used to delineate possible worlds.

My words must not be taken to imply I take return to full Leibnizianism that says only one world, contingent specification included, is possible, to be in order. The claim of the existence of 'necessary truths' is problematic, as we all know. We do, however, have evidence that tells us our ideas of the pure 'contingency' of the aspects of physical theory we are unable to find principled, lawlike explanation for, lacks ground. The legitimate philosophical consequence to be drawn on such basis must not be 'let us embrace rationalism' but, rather, the acknowledgment that we do not have, as yet, proper understanding of the nature of physical possibility. How can one rely on intuitive notions of 'physical necessity' to discriminate between 'the nomic' and 'the contingent', at loss as we stand in our very attempts to grasp what the concept of 'law of nature' amounts to? My skepticism towards the multiverse hypothesis is not directed at the possibility of our cosmic landscape being richer than anticipated (of course there can be other universes), but rather at the view according to which there exists a 'contingency problem' within physics for which many-worlds stand a plausible solution. Our metaphysical view of Nature is rather murky, we do not know how apples and mugs of coffee fit in a world of relativistic quantum fields. Let alone we know how to consistently distribute 'necessity' and 'contingency' to aspects of physical theory, as I hope to have shown. We have seen that more worlds are 'physically impossible' than those excluded by the laws alone. Multiverse lovers could, I suppose, attempt to sustain their view by claiming laws of nature vary in other worlds also. But in this case, invoking other worlds to 'explain the unexplained' attached to the laws the govern *our world* loses meaning, intelligibility, and whatever initial plausibility the idea may, at first, have seemed to possess.

Part II

Who's afraid of Non-Empirical Theory Assessment? A Discussion of Richard Dawid's String Theory-Based Epistemology

4 Introduction

Experience without theory is blind, but theory without experience is mere intellectual play.

Immanuel Kant

I cannot believe–and I say this with all the emphasis of which I am capable–that there can ever be any good excuse for refusing to face the evidence in favour of something unwelcome. It is not by delusion, however exalted, that mankind can prosper, but only by unswerving courage in the pursuit of truth.

Bertrand Russell

'Nobody thinks it works', tells me philosopher of quantum gravity Christian Wüthrich while we have beers at a bar in Chicago. 'It is interesting and all, but few take it seriously. String and multiverse theorists love it, though'. I had just asked him what is the mainstream opinion within the philosophy community regarding string theorist and philosopher of science Richard Dawid's new ideas. Liked or not by the professionals, truth is Dawid's work has brought scientific methodology—relegated by most to museum status after the work of Feyerabend and Kuhn—back to the forefront of scientific and philosophical discussion. Intending to show scientific methodology ought to reshape itself to fit high energy physics new theoretical developments and lack of experimental import, the idea of 'non-empirical confirmation'—the sort of confirmation Richard Dawid claims science now ought to rely on—triggered the long-needed interplay between theoretical physics, experimental search and philosophy. Many papers were published to discuss the plausibility and implications of Dawid's ideas, conferences were organized, uncountable debates in scientific on-line forums and blogs popped up in the World Wide Web. A historical workshop in Munich, December 2015, headed by Dawid himself, entitled 'Why trust a theory: Reconsidering Scientific Methodology in Light of Modern Physics' was attended by prestigious scientists such as Carlo Rovelli, George Ellis, Joseph Silk, Joe Polchinski, Nobel-laureate David Gross and philosophers such as Peter Achinstein, Elena Castellani and Massimo Pigliucci. This workshop made the headlines in the media for months in a row. Strong disagreement, hot debate and no consensus amongst the participants capture the spirit of this meeting from the start to its closure. New fuel and taste to this discussion was given by particle physics when, in August 2016, the Large Hadron Collider (LCH) came up with no signs of supersymmetry. Again[1]. The implications of such results are far-reaching because string

[1] See (ARKANI-HAMED, N. et al., 2016).

theory, inflationary models in cosmology and grand-unified theories all take supersymmetry as a given. All require it by means of theoretical consistency . And Nature shouted, again, a loud 'No!'.

Supersymmetry dissolves nicely all the nasty conceptual problems within the Standard Model physicists cannot manage to solve by means of standard particle physics textbook (MARTIN, 1997). String theory requires it for structural mathematical reasons and it is *a priori* the only theoretical piece of string theory which could be experimentally checked (DINE, 2007):

> Finding such [supersymmetric] particles would corroborate an important aspect of string theory. Calabi-Yau manifolds [...] were hand-picked by string theorists as a suitable geometry for the extra dimensions partly because supersymmetry is automatically built into their internal structure. Discovering signs of supersymmetry at the LHC would thus be encouraging news, to say the least, for string theory and the whole Calabi-Yau picture. For one thing, the attributes of the supersymmetric particles could tell us about the hidden dimensions themselves, explains Burt Ovrut, 'because how you compatify the Calabi-Yau manifold affects the kind of supersymmetry you get and the degree of supersymmetry you get. You can find compactifications that preserve supersymmetry or break it completely.' Confirmation of supersymmetry would not confirm string theory per se, but it would at least point in the same direction, showing that at least part of the story string theory tells is correct. (YAU; NADIS, 2010)

Hopes the Large Hadron Collider would add some flash and bone to the bloodless ghost of testability that has been haunting string theory for decades now have, it seems, faded away[2]. Physicists at LHC have discovered in 2016 something extremely deep and important: nothing new. The long sought-for superparters which would have made particle physics beautiful again are nowhere to be found[3]. The implications of this feedback of Nature for theoretical physics are far-reaching, but the intriguing questions it raises cannot be pursued here. For now I lack the intellectual resources for doing so, thus I face the matter with humility. This is, however, already enough to make strong the suspicion that a real understanding of string theory and an appreciation of how well-founded are its promises of describing Nature cannot be achieved without previously assessing the grounds over which it is built and the philosophical conceptions and misconceptions it takes for granted.

[2] See CONOVER, Emily. Supersymmetry absence at LCH puzzles physicists. ScienceNews, v. 190, n. 7, Oct. 2016, p. 12.

[3] See CERN's Document Server, 16th June 2016, for CERN's ATLAS and CMS data results and analysis.

The epistemological problem of restricted empirical testability—or the impossibility of it, as seems to be the case for string theory and multiverse theories—is a particularly thorny one because it is not only a matter of reading the answer off the relevant fraction of physics, for the interpretation of the physics may in turn be shaped by convictions about the form the answer should take. If you are a string theorist and you are aware the theory leaves no room for empirical predictions and concrete application neither exists shadows of turning in its future development—and yet, you believe the theory must be interpreted realistically—, then you react by considering non-standard forms of epistemology, criteria of reality and truth-functions that fit and accommodate your biases nicely. You read fractions of science through the epistemological glasses you had framed in accordance with your preferences. Dawid's proposal stands as a frame-candidate, a non-standard epistemology. It claims scientific practice has changed and epistemological standards must follow through. Dawid may, of course, be right, but no philosophical doctrine of far-reaching consequences as his is must be taken seriously without careful scrutiny of the technical niceties it embodies, philosophical understanding of key epistemological concepts, assessment of potential counterexamples, appreciation of historical background, ontological attachments, heuristic power, and fruitfulness: how it advances in understanding methodological issues regarding conceptual analysis, theory choice, truth-value assessment, and so on. The purpose of the following chapters is to undertake this difficult philosophical task in a way that is both introductory and realistic.

Dawid's doctrine, presented in his '*String Theory and the Scientific Method*' and published for the first time in 2014, is backboned by three main arguments: the 'No Alternatives' Argument (NAA), 'Meta-Inductive' Argument (MIA) and the 'Unexpected Explanatory Coherence' Argument (UEC). Together they stand the pillars of the epistemological house Dawid has built and they are accessed independently in each of the foregoing chapters. The tenets of non-empirical confirmation shall be exposed, its virtues (if found) extolled and its heuristic power and fruitfulness diagnosed *vis-à-vis* its implications, scientific and philosophical. The upshot of my examination of Dawid's theory of confirmation is to be drawn in terms of the general precept that governs all human endeavors: 'by their fruits we shall know them'.

So let us begin.

5 The 'No Alternatives' Argument

We are genuinely assailed by doubts which originate in our awareness that there are more theories in Plato's heaven than have, as yet, been dreamt up by our theoreticians.
SKLAR, L.*; Do Unborn Hypotheses Have Rights?*

Who would dare assert that we know all there is to be known?
GALILEO*, Letter to Castelli*

There are more things in heaven and earth, Horatio, than are dreamt of in your philosophy.
SHAKESPEARE, W.*; Hamlet. Act 1, scene 5*

Three inter-dependent roads lead to non-empirical confirmation. They are said to be 'truth-conductive'—epistemic triumph is guaranteed; a final 'theory of everything' awaits at the end of our course—but all of them ought to be taken, says Dawid, if the destination is to be reached. As we take the first, we hear the skeptic complaining that this sort of epistemological proposal promises way too much. Tt must prove its epistemic power if it aspires to be taken seriously, she insists. We surely want the *strongest* corpus of beliefs to which we are entitled by evidence—empirical or not; assuming there is something to the term 'non-empirical evidence'—and, if we are to accept string theory as the 'final theory of Nature', as Dawid suggests, we demand conclusive grounds to rationally maintain this epistemic attitude. Dawid's first move is to say that we must believe p because there are no options for p: p is all we got. I begin by presenting this 'inverse underdetermination strategy' as Dawid himself has done in his (2014); endorsing, as I should, a philosophically healthy principle of charity. Next, I consider how well Dawid's case fares when confronted by the doubts of the skeptic. I conclude by assessing whether or not the skeptic's doubts must be our own.

1

No one is better suited to expose Dawid's reasoning at its best shape than Dawid. Borrowing his voice, in what follows I present to the unfamiliar reader the 'No Alternatives Argument', as he calls it:

> The plain no alternatives argument (NAA): string theorists tend to believe that their theory is the only viable option for constructing a unified theory

> of elementary interactions and gravity [. . .] The goal of string theory is to reconcile gravity with [. . .] advanced and successful concepts of contemporary particle physics and therefore to provide a truly unified description of all natural forces. In this endeavor, the traditional investigations of canonical quantum gravity do not constitute alternatives, which leaves string theory as the only available way to go.

And he continues:

> Why is it so difficult to find a unified description of gravity and nuclear interactions? [. . .] quantum gravity is non-renormalizable within the traditional field-theoretical framework. Non-renormalizable quantum gravity cannot be considered viable at the Planck scale, however, the scale where the gravitational coupling becomes strong. Early attempts to solve this problem applied the traditional methods of gauge field theory and tried to deploy symmetries to cancel the infinities which arise in loop calculations and therefore make the theory finite. For some time, the concept of supergravity, which utilizes supersymmetry, looked like a promising candidate for carrying out this task, but eventually the appeal to symmetry principles was judged insufficient. As it turns out, the remaining theoretical options are quite limited. One might venture into giving up some of the most fundamental pillars of present-day physics like causality or unitarity. Ideas on this direction have been considered, but did not lead to any convincing theoretical schemes. If one wants to retain these most fundamental principles, then, according to a wide consensus, there remains only one way to go: drop the idea of point particles, which univocally leads to string theory (DAWID, 2014a)

Dawid seeks to convince us that there must be a gauge field-theoretic modelled, divergence-free quantum gravity theory capable of subsuming onto its explanatory corpus all known scientific facts linked to general relativity and microphysics. The crucial point is that contemporary theoretical physics imposes severe constraints on crafting quantum gravity-like structures and the said fact that only string theory has done justice to such constraints bears on itself concrete confirmational value:

> [. . .] one might conjecture a connection between the spectrum of theories the scientists came up with and the spectrum of all possible theories that fit the available data. The observer [. . .] takes the fact that scientists have problems finding alternatives as a sign that not too many alternatives are possible in principle. In other words, she concludes that scientific underdetermination is significantly limited. Based on the traditional assumption that the phenomena

> in question can be characterized by a coherent scientific theory at all, the conjecture of significant limitations to scientific underdetermination can then enhance the trust in the available theory's viability: if a viable theory exists and only very few scientific theories can be built in agreement with the available data, the chances are good that the theory actually developed by the scientists is viable (DAWID, 2014b).

Fed underdetermination of theory by data with their mother's milk, philosophers of science ask by means of what epistemological grounds can paucity of imagination be linked to reasons for believing a hypothesis or theory. Are we supposed to commit ourselves to the believability of *p*—unaided by evidence as we stand—on the basis of our inability to craft alternatives? Should we not take possible alternatives into account, even if we cannot say what they are? It is a truism of probability-framed theories of confirmation that the probability of some preposition depends upon the possibility set in which the proposition in case is imbedded. Suppose at time t physicists are equipped with a language L and, say, a degree of belief Pr on proposition p of L. It is correct to assert that, at time t, physicists are aware of only a small fraction of the possible theories that can be formulated in L. Suppose further that, at $t + 1$, new theories are formulated and the explicitly acknowledged possibility space thereby expanded. The probabilities of previously considered theories and hypothesis may then require modification. Can Dawid discard altogether the possibility that, among the unconceived, there exists a more plausible quantum gravity theory that not only meets all requirements current fundamental physics puts on the table but that is also better supported by data than string theory? Leaving aside the familiar problems brought up by induction, we face the pressing, obvious threat of knowledge stagnation. Dawid argues such objections do not apply to the No Alternatives Argument as he designed it. His response runs thus:

> The principle of scientific underdetermination acquires its plausibility based on a specific understanding of the scientific process. According to this understanding, the scientist builds theoretical structures, which reflects the regularities observed in nature up to some precision, and tunes the free parameters contained in those structures to fit the quantitative details of observation. The successful construction of a suitable theory for a significant and repeatedly observed regularity that characterizes the world is assumed to be just a matter of the scientist's creativity and diligence. If it is always possible to find one suitable scientific theory, however, it seems natural to assume there can be others as well. There may always exist different choices of theoretical structure that have coinciding empirical implications up to some precision in the observed regime if their respective free parameters are fixed accordingly. The principle of underdetermination follows from this. [. . .] It is the *flexibility* of the scientific

> theories we are used to working with which provides the basis for making the standard understanding of scientific theory building plausible. If one considers only the class of highly predictive structurally unique theories, one encounters an entirely different situation. Compared to the general case of all possible scientific theories, the chances for being able to describe a specific empirical data set with a highly predictive structurally unique theory are strongly reduced for two reasons. First, a highly predictive theory that does not allow any freedom of choosing free parameter values or modifying qualitative characteristics in order to fit the empirical data is compatible with far fewer sets of empirical data than a conventional theory. Second, the difficulties to come up with structurally unique theories suggest that there are far fewer structurally unique theories than conventional ones. In fact, the only structurally unique theory known in science today is string theory (DAWID, 2014c).

The expression 'structurally unique theory' makes a nice sound all right, but one can righteously be skeptical about the effect of its meaning—if it has any—in confirmation theory. Its vagueness also make it difficult not to pay attention to the question begging sound that reverberates in the ear of the epistemologist whenever the term is invoked as a self-warranting theory predicate. Dawid continues:

> Considering both the generality of the principles which define a theory such as string theory and the vast range of possible phenomenological regularities and parameter values, it is most natural to assume that highly predictive structurally unique theories, if found at all, can only be found for a small subset of points within the huge space of all possible regularities. Since there is no reason for expecting that different structurally unique theories, each of them based on a different set of fundamental physical principles, have a tendency to give similar empirical predictions, the notion that the entire set of data we have collected in our experiments can be accounted for by several highly predictive structurally unique theories which are not empirically equivalent must then be considered highly improbable. The principle of scientific underdetermination, which looks convincing if applied to the set of all scientific theories, therefore lacks plausibility if applied to the set of highly predictive structurally unique theories. If we assume that structurally unique theories can only be expected to be superseded by theories which are once again structurally unique, it therefore seems most plausible to expect that a highly predictive structurally unique theory that has found empirical confirmation will not be superseded at all (DAWID, 2014a).

The burden of underdetermination is only felt by scientific theories of limited scope, says Dawid. An ‘all-things-encompassing’ theory such as string theory could only be replaced by another ‘all-things-encompassing’ quantum gravity theory—a possibility Dawid dismisses altogether on the grounds of the physical and mathematical constraints underpinning the joint properties of quantum theory and general relativity: the overwhelming technical difficulty of meeting these requirements can only be surpassed if a string-theoretic framework is adopted (POLCHINSKI, 1998a; GREENE, 1999)[1]. A lower-level theory, by its turn, cannot be seriously considered a contender when a fully-fledged final theory is at the arena (the latter accumulates a greater amount of positive warrant from evidence than the former). The conclusion, argues Dawid, is inescapable: we are warranted in believing in the viability of string theory by the very fact that we have *it* – and no other. We *cannot*, says Dawid, have any other. *Wir müssen nicht wissen, wir wissen jetzt schon.*

Let us now explore in depth the structure of Dawid’s argument and the skeptic’s reading of it.

2

What sort of facts of the matter entitle us to unequivocally state that experimental results E are evidence (for or against) hypothesis H? Suppose at hand are H and E. The meaning of H is not under dispute. Neither is the fact that E has been observed. Our doubt is to the effect of whether, or the extent to which, what has been observed *provides evidence for*, or supports, H. It is clear that the believability of H depends largely on the unambiguous objective character and meaning of the evidence at disposal. One then may ask—on a par with probability-framed theories of confirmation—whether increase in probability, if the case is such, is enough for evidence. One asks also how high the increase ought to be. Where the threshold lies. One also wants to know if little evidence is enough, or if many instances are required. Is a ‘permissive’ account sufficient—$\tilde{E}$ is such that allows me to believe H'—or can a body of rationally held beliefs not be grounded on such a weak principle? What must one do when faced with E of a sort that supports H and $\sim H$ simultaneously? And so on. These are not trivial questions. Confirmation, however, is no trivial matter.

Consider Dawid’s conception of ‘non-empirical evidence’. It is misleadingly simple: an analogy with its ‘empirical counterpart’ within the inference-to-the-best-explanation framework does the trick. Take, for that matter, the example of how the electron got its charge. In 1897 J. J. Thomson conducted a series of experiments in order to determine the nature of cathode rays. He claimed his results were evidence—indeed, conclusive

[1] Even if Penrosean fantasies were realized and a different flavor of final theory were formulated, it could only be ‘as good as’—but no ‘better than’—string theory. Endorsing a reasonable principle of methodological conservatism, the existence of a contender for T (which is just as good as, but no better than, T) is no reason for ceasing to believe in T. When it comes to final-theories belief choice ,the fact of the matter is precisely this. Check (DAWID, 2014d).

evidence—that cathode rays amount to streams of charged particles:

> As the cathode rays carry a charge of negative electricity, are deflected by an electrostatic force as if they were negatively electrified, and are acted on by a magnetic force in just the way in which this force would act on a negatively electrified body moving along the path of these rays, I can see no escape from the conclusion that they are charges of negative electricity carried by particles of matter (THOMPSON, 1897).

Thompson's reasoning is not drawn in a statistical fashion. Neither it is of a hypothetico-deductive kind: E is not derivable from H. Rather, the relation of E and H strikes us abductivelly: H is the *best*—on Thompson's eyes, the only plausible—explanation for E. On similar grounds, Dawid asks what to make of the fact that, in spite of intense work and creative energy expense, no quantum gravity theory on a par with string theory burst upon the world. His is no empirical fact—like Thompson's results on cathode ray tubes—but it is, he says, a fact notwithstanding: of a 'non-empirical' kind. How can one explain it? His answer—we've met it above—is that the case is so because string theory is the correct all-encompassing and final theory of Nature. Our inability to do better is—so the argument goes—evidence of it.

Do we have an argument for scientific realism here? Maybe, but let us bracket the question for now. I beg for the reader's indulgence and ask her to go along with me in assuming, for the time being, that (1) Dawid's conception of non-empirical evidence is a coherent one; (2) abduction is a sound mode of scientific reasoning; (3) his hypothesis is indeed an explanation for the 'non-empirical data' in question. Putting all these issues temporarily to the side, mustn't we still be cautious with respect to how well his explanation fits the relevant facts? For, the skeptic may remind us, doctrines of confirmation of all flavors agree that if hypothesis H explains a set of data X, and if hypothesis H' explains sets of data X and Y simultaneously, then H' is explanatorily superior to H and it is to be preferred on such grounds. The fact that string theory has been unable to serve any empirical purpose, has resisted all attempts to subsume it into general physical principles and has denied us conceptual understanding are as much 'non- empirical evidence'—according to the definition Dawid himself has crafted us—as is the former string-theorist's carefully-chosen 'no-alternatives' case. String theory, as it stands, was given birth (according to Dawid[2]) in the late 70s as an extension of the quantum-field-theory programme. The conceptual conundrums linked to quantum field theory are well-known in the community of experts and they cannot be left on the shelf when questions related to meaning, reality, evidence, unification and fundamentality are at stake—and they are when we speak of a final 'theory of everything'. The skeptic

[2] We shall see in the next chapters this historical description is not accurate.

may thus suggest the following alternative hypothesis: 'quantum field theory is too poorly understood a framework for a final all-encompassing theory of nature to be in its terms crafted. Proof is that the only alternative theoreticians have yet thought up is empirically meaningless and stands a stagnated research programme for over 30 years now'. It is easy to see that such hypothesis is supported by a greater amount of 'evidence' than is Dawid's NAA: it explains lack of competition *and* of theoretical advancement. If it is correct to assert that a broader theory—a theory that explains, say, $E \,\&\, E'$—is to be rationally preferred to a narrower scope theory—that explains, say, E' alone—and if scientific decision making is an evidence-driven endeavor, we may have a hard time blocking the skeptic's concern with respect to the extent to which Dawid's inference to the best explanation is, all relevant 'non-empirical data' considered, good enough to be believed.

3

Let us now leave how Dawid's NAA fares when compared to its contenders *vis-à-vis* relevant non-empirical facts to the side and evaluate its merits with respect to well-accepted standards of rationality. Keeping all options out of the arena, does NAA stand a plausible hypothesis? Philosophers of science take underdetermination of theory by data an unquestionable epistemic fact. Is it not thus incoherent or absurd to claim for the truth of string theory solely on the grounds that no other 'final-theory-like' quantum gravity theory is available for consideration? For Dawid's reasoning is of the 'inference-to-the-best-explanation' type—which, at the end of the day, amounts to just affirming the consequent—and the skeptic may complain the whole thing simply begs the question. Dawid's NAA, simply stated, asserts that string theory is true because it is the theory we have and it is the theory we have because it is true. Should we grant belief to a non-verified theory which, by historical contingency and paucity of imagination, has kept its rivals out of the arena simply because we feel having no 'theory of everything' at all so unsatisfactory an epistemic state?

An initial response to the skeptic's worries would be to assert that, as a matter of fact, one cannot deal with the unavailable. The potential existence of empirical equivalents to one's theory cannot be relevant to questions of justification: were it so, scientific decision-making would be an impossible enterprise. This issue is of the same sort of the familiar specter of skepticism about induction: to deny empirical warrant to a theory on the grounds that all data is never in is simply to misconstrue what justification in the inductive context is. Similarly, to reject a theory by invoking the existence of unimagined alternatives is to misconstrue what counts as justification in the context at stake. The whole point of induction is to allow us to make inferences on the grounds of less than an exhaustible amount of all data; to be justified in accepting a hypothesis is, the argument may go, to hold onto the best explanation one has been able to come up with *vis-à-vis* background knowledge and available evidence at a given time. For surely we do not wish to label Newton an irrational man for committing himself to his theory simply because he did not take into account the fact that future history may have better theories hidden in its womb—a fact Einstein later proved true. Can we declare a person irrational in her beliefs simply because they have turned out to be false? Would not human kind suddenly find itself with no rational beings?

Up to a point I think this argument has merit: it is as out of hand to consider as potential candidates for belief all possible hypotheses—the ones we have already brought to awareness and the ones we do not yet have—as it is to have in view all facts about the universe. At any rate, most examples of underdetermination invoked by philosophers are artificial algorithmic constructs too far-fetched to be considered a serious epistemic threat anyway. But—the skeptic can insist—even though algorithms for designing empirical equivalents can be dismissed by the sensible realist, she cannot forgo the lessons of underdetermination *history of science* has provided us with. (STANFORD, 2001a) has built a very strong case to the effect that historical record supports the claim that transient underdetermination predicament is a real, pervasive problem that recurs for each theory and each body of evidence we consider. He argues that it is enough to look at history of science to acknowledge that we have, over and over, occupied a position in which theorists were able to conceive only a single theory that was supported by all available evidence when there existed indeed—as time later proved true—alternative possibilities equally well-supported by that evidence and stronger in explanatory terms. Stanford calls this 'A New Induction over the History of Science' and he argues thus:

> [the point of the New Induction] is not that past successful theories were ultimately found false or otherwise wanting in some way, but instead that they were one at a time *the best or only theories we could come up with*, notwithstanding the *availability* of equally well-confirmed theories [. . .]. Furthermore, unlike constructing empirical equivalents, [the New Induction] does not allow us to say *which* actual theories are underdetermined by the evidence, nor anything about what the (unconceived) competitors to present theories look like. But I have tried to suggest that empirical equivalents have proved to be a Devil's bargain for advocates of underdetermination—providing convincing evidence of an underdetermination predicament only where they have transformed the problem into one or another familiar philosophical puzzle—and I would suggest that we start worrying instead about the kind of underdetermination that the history of science reveals to be a distinctive and genuine threat to even our best scientific theories (STANFORD, 2001b).

Even if the reader doubts—as does Dawid—empirical equivalent artifacts manufactured by the manipulation of predicates (exemplified in (KUKLA, 1996; EARMAN, 1993)) are a real issue in theory of confirmation, she is likely to agree that the sort of underdetermination Stanford warns us against does exist. Dawid attempts to vitiate the skepticism drawn by underdermination by appealing to 'final theory claims': he says that 'the case for non-empirical confirmation' is 'strengthened' by the 'final theory claims implied by string theory' (DAWID, 2014e) and that it is 'highly implausible' such a theory will be replaced (DAWID, 2014f). A simple glance at the historical record suffices to show that other men have reasoned like Dawid and further events have proved them wrong: the end-of-physics theme has a long history. Ranging from Maxwell to Stephen Hawking and from Hertz to Planck and Dirac, prophecies to the effect that 'all knowledge would soon converge' and 'only busy work to clear up relatively minor matters would be all there is left for physicists to occupy themselves with' have abounded[3]. Surely these are instances of the distinctive problem Stanford's *New Induction over the History of Science* asks us to pay attention to. Feelings of completeness were nourished before and exorcised by science's internal dynamics—why take the bet that Dawid's prognosis shall fare better?

The vortex atomic theory that was given birth by Lord Kelvin in 1867 and subsequently fed and grown by a whole school of British mathematical physicists is an interesting case of particularly informative value. This theory states that atoms are but vortical modes of motion of a primitive, perfect fluid—the ether. In 1882 J. J. Thompson published an essay in which he gives a sophisticated account of the vortex theory. He extended it to cover chemical problems, including affinity and dissociation. Others applied it to electromagnetism, gravitation, and optics: it was a bold attempt to formulate a unitary and continuous 'theory of everything' based solely on the dynamics of the ether. British mathematician and physicist William Hicks, in 1895, gave a confident report on the state of the art of the vortex atom theory at the annual meeting of the British Association for de Advancement of Science (BAAS). The following piece—found in historian of science Helge Kragh's *Quantum Generations*—is worth quoting:

> While, on the one hand, the end of scientific investigation is the discovery of laws, on the other, science will have reached its highest goal when it shall have reduced ultimate laws to one or two, the necessity of which lies outside the sphere of our recognition. These ultimate laws—in the domain of physical science at least—will be the dynamical laws of the relations of matter to number, space, and time. The ultimate data will be number, matter, space and time themselves. When these relations shall be known, all physical phenomena will be a branch of pure mathematics (BAAS Report 1895 in (KRAGH, 2002a).

Kragh writes that although many of Hick's colleagues endorsed his philosophy, soon enough the theory was left by most of those who first cherished it. 'Decades of theoretical

[3] For several excellent examples, see (BADASH, 1972).

work', he says, 'had led to no real progress and the great vortex program was degenerating into sterile mathematics' (KRAGH, 2002b)[4]. Knowing that the ether concept plays no role whatsoever in 20th century physics, what to make of the fact that a full-fledged 'theory of everything' could be made out of it? Does it not give enough grounds to assert that theoretical considerations by themselves are no reasonable canons for scientific decision-making? Doesn't reflection upon historical scientific experience inductively leads us to the conclusion that 'feelings of completeness' can play no role in theory justification and that one is never safe to assert that the future of physics has no marvels in store even more astonishing than those of the past?

It is true that, irresistibly armed with promises of unification, supersymmetry and extremely sophisticated mathematics, quantum field theory of strings has subdued the theoretical physics community severely. But in face of the skeptical conclusions we have been considering, is it not safe to say that, given the current state of the art and the simple standards of rationality believability in string theory fails to meet, if string theory is the best we could come up with it may simply not be, as yet, good enough?[5]

Dawid asks us to look also at the qualities of string theory itself—rather than focusing solely on the scientists' paucity of imagination on crafting alternatives—when assessing his 'non-empirical' theory of confirmation. His second argument is thus considered in the next chapter.

[4] If one follows the history told by (RICKLES, 2014a) and (CONLON, 2016) one finds the historical dynamics of string theory and vortex atom theory strikingly similar. Although I think a proper careful analysis of the analogy would be an interesting exercise, the issue will not be pursued further here.

[5] At least, note, not to the effect we have epistemic reasons to crown it 'true theory of all interactions'.

6 The 'Meta-Inductive' Argument from the Success of Other Theories in the Research Program

Concepts which have been proved to be useful in ordering things easily acquire such an authority over us that we forget their human origin and accept them as invariable. Then they become 'necessities of thought', 'given a priori', etc. The path of scientific progress is then, by such errors, barred for a long time. It is therefore no useless game if we are practising to analyse current notions and to point out on what conditions their justification and usefulness depends, how they have grown especially from the data of experience. In this way their exaggerated authority is broken. They are removed, if they cannot properly legitimate themselves; corrected, if their correspondence to the given things was too negligently established; replaced by others, if a new system can be developed that we prefer for good reasons.

EINSTEIN cited by BORN, Max
Einstein's Statistical Theories

The dialectic structure of the present chapter is similar to the previous one. An exposition of Dawid's argument is first given. Common philosophical concerns are at the heart of my assessment of Dawid's epistemology; thus after a description of his case we shall let the skeptic take the lead. I close by assessing the extent to which Dawid can make sense of anti-skeptic replies to his opponent's worries.

1

The Standard Model of Particle Physics stands undefeated the champion of empirical precision. The methods of quantum field theory have proved themselves so powerful in dealing with high energy physics conundrums it was expected all of Nature would be subsumed under its formulae: based on the theoretical innovations of gauge theories of quantum fields and quark theory, the stormy development particle physics experienced in the early 1970s led the art to be referred as 'the new physics' (KRAGH, 2002a). After decades of unexplained tracks in data chambers and nuclear emulsions, by the mid-70s the Weinberg-Salam model and the new quantum chromodynamics (QCD) were accepted as good theories for the electroweak and strong interactions (PICKERING, 1984). The Victorian feeling of 'eminent completeness' that had infected 19^{th} century physicists[1] once again arouse within the particle physics community—a feeling that was not completely unwarranted if one has a bit of theoretical work empathy. The following brief interlude on symmetries and gauge theories may make my point clearer.

A symmetry is an operation in a system that leaves some feature of that system invariant. Rotating a ball, for instance, leaves its shape unchanged. Maxwell's equations are time-reversal symmetric: one can replace the variable t for ($-t$) but the equations will remain the same because time always occurs squared. Particles, in quantum mechanics, are represented by waves. If the equations that govern the dynamics of such waves do not change when arbitrary phases are added to the waves at each point in space-time, then the theory is said to have a U(1) *gauge* symmetry.

One can think of gauge-theory crafting as a two-step algorithm. You start off with an equation that describes the behavior of matter—in the case of electrodynamics this would (simplest case considered) consist of electrons only (the Dirac equation). Next you assume that the theory displays a specific symmetry (a $U(1)$ symmetry in this case). Generally—as in the example here given—the latter requirement is not compatible with the former: when a $U(1)$ gauge transformation is applied to the Dirac equation, *it is not invariant*—you are left with extra terms. To make such extra terms go away—that is, to have your theory invariant under the operation of the symmetry—you add a new field: the *gauge* field. When such extra fields are operated on by the gauge transformation, it is postulated (!) that they give rise to precisely the terms you need to cancel the extra terms from the matter side of your theory. To our awe, in quantum electrodynamics the required gauge field to cancel the extra terms is precisely that of the photon. Thus if you start with Dirac's equation for the electron and plead a $U(1)$ symmetry, *you are led to postulate the existence of the photon*—the carrier of the electromagnetic force.

A similar rationale played a role for Sheldon Glashow, Steven Weinberg and Abdus Salam when they unified the quantum field theory of the weak and electromagnetic forces. They have built a gauge theory with $SU(2)$ and $U(1)$ symmetry. Once more one starts with matter and goes on postulating a symmetry. The $U(1)$ symmetry demands the equations of motion governing matter have a symmetry under an independent change of phase at each space-time point. One must also require that the theory stays invariant even if the neutrino and the electron fields mix in different degrees at each space-time point (this is what the $SU(2)$ symmetry is about). Thus to make the theory invariant under this more complex set of symmetry operations—the $SU(2)$ X $U(1)$ symmetry—one needs to add not only one but *four* gauge fields: the photon and the three carriers of the strong force: the

[1] I direct the reader again to (BADASH, 1972) for a wonderful historical study of 19th century physics's *zeitsgeist*.

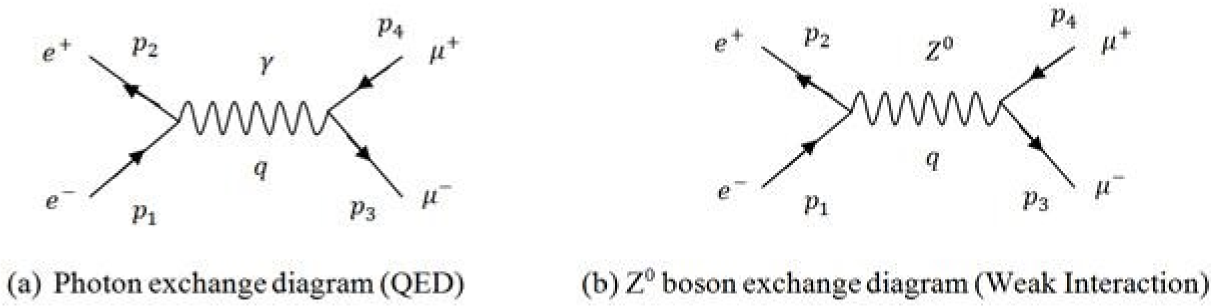

Figure 1 – Feynman Diagrams for Photon and Z^0 Boson Exchanges in Muon Pair Production. *Image source:* (TERSOO; EMMANUEL, 2015).

W^+, the W^- and the Z^0.

The extension of the electroweak theory to cover strong interactions also was directly achieved by extending $SU(2)$ X $U(1)$ symmetry into $SU(3)$ X $SU(2)$ X $U(1)$. The theory in this case deals with both leptons (such as electrons, neutrinos, muons) and quarks (such as the up quark, the top quark, the strange quark). Consistency constraints, in this case, have led again to a very strong prediction: given the six known leptons, there must exist six types of quark (PICKERING, 1984). The extraordinary success of gauge physics both in theory—in particular the discovery that, under certain conditions, quarks inside nucleons act freely on one another—and experiment—the discovery of neutral currents and, shortly after, the J/psi and other charmed particles—were taken as a sign that the remarkable mathematical-physical structure brought forward by the 'gauge revolution' that ushered in the Standard Model revealed, to some extent, the order of the world at its most fundamental level. High-energy physics had risen to the peak of 'theory-matches-experiment' figure and it could not, as it were, comprehend a downward gradient. The Standard Model works marvelously—the 'explain, correlate and organize' theory predicament has been fulfilled. The only ghost yet to be exorcised is that who whispers 'unification has not been established'.

'Superstring theory', Steven Weinberg writes (1987, p. 105), 'has the smell of inevitability about it'. It yields you the unification you crave for using the tools from quantum field theory you know work well. Thus Dawid writes:

> The Standard Model can be called a direct precursor of string theory. Indeed, string theorists view their own endeavor as a natural continuation of the successful particle physics research program. The fact that the standard model theory was at the end impressively confirmed by experiment conveys a specific message to particle physicists: if you knock all the doors you can think of and precisely one of them opens, the chances are good that you are on the right track. Scientists working on unifying gauge field theory and gravity have thought about all conceivable options, including those who drop fundamental

> physical principles. The fact that exactly one has gained momentum suggests that the principle of theory selection which have been successfully applied during the development of the standard model are still working (DAWID, 2014g).

The argument is that of a methodological conservatism. String's theory positive warrant springs from its linkage with the Standard Model and the scientists' trust on the viability of the latter: that's Dawid's 'non-empirical evidence'. *Because* the Standard Model displays so many empirical virtues and *because* string theory passes through the extreme narrow gates of consistency imposed by the quantum field theory programme— to make use of the Lakatosian jargon—the empirical virtues of the former *must be transferred to the latter*. Thus string theory is 'empirically confirmed at the 'meta-level'':

> It is important to emphasize that MIA relies on empirical tests of other theories and thereby in a significant sense resembles the process of theory confirmation by empirical data. The level of reasoning, however, differs from that chosen in the classical case of theory confirmation. In the present context, the empirically testable prediction is placed at the meta-level of the conceptualization of predictive success. We do not test the scientific theory that predicts the collected empirical data but rather a meta-level statement. The following hypothesis is formulated: scientific theories which are developed in the research program of high energy physics in order to solve a substantial conceptual problem, which seem to be without conceptual alternative and which show a significant level of unexpected internal coherence tend to be empirically successful once they can be tested by experiment. This statement if argued for based on past empirical data (in our case, largely data from the standard model of particle physics and from some earlier instances of microphysics) and can be empirically tested by future data whenever any predictions which were extracted from theories in high energy physics along the lines above are up to empirical testing [...] All data that can be collected within the high energy physics research thus constitute relevant empirical tests for the viability of MIA and thereby here implications for the status of other theories in the field which are considered likely viable based on that hypothesis (DAWID, 2014h).

Theory selection has been shifted away from specifics of coupling constants, masses and lifetimes—our claims about the physical world no longer face the tribunal of experience but it is in the self-proclaimed authority court of mathematical rigor and quantum field theory constraints that scientific speculations about the world are to be judged. MIA tells us we are committed to the believability of string theory because it is unreasonable to think the methods of gauge physics and quantum field theory would fail us at the Planck scale. Since we do know our background theory works and since we do believe in it, since string theory is our only option relative to this background, and since it is reasonable to adopt a principle of conservatism that tells us to prefer the background theory we do have to any alternatives we might have had, we are warranted in praising string theory as a final true theory of Nature. The successes of the standard model are string theory's own.

2

If Dawid's intention is to non-empirical confirmation to play a role in scientific decision-making in actual practice, assurance must be given to the effect that (1) MIA *justifies* one to grant belief in string theory and (2) although a justified belief does not imply *true belief*, we want our hopes to reach truth by means of MIA to be *prima facie* tenable. Thus there must exist a connection between having justification and being in a position to instantiate true beliefs about the world (in the sense that to be epistemically (in opposition to 'morally' and 'pragmatically' or even 'rationally') justified to endorse p or not p is the best doxastic attitude in the interests of truth). But if the question whether or not p is not one Nature can answer, can one appeal to a priori tenets such as MIA to evaluate the epistemic worth of p and not look embarrassingly like a dogmatist?

As a warm-up exercise, let us locate ourselves within the context of 'orthodox' epistemologies—to use Dawid's expression to describe epistemologies to which empirical data still has a call—and see if there are relevant lessons to be drawn from these. Suppose that, when Nature speaks, she pronounces 'not p'. If $\{H, K\} \vdash p$ and if K is taken to be background knowledge, the forceful conclusion is that hypothesis H ought to be false. Within the confines of hypothetico-deductivism (HD), such a process entails falsification. William V. O. Quine (1951, 1969) has shown us the road to knowledge is not, however, as smoothly traveled as foundationalist positivism would have us believe. It is long, winding, and hard. He asked us to pay attention to the fact that the deduction of observationally decidable consequences from scientific hypothesis requires the aid of auxiliary assumptions ξ. One cannot sweep ξ under the rug of K because ξ can be, insists Quine, as questionable as H. Hence, for Nature's pronouncement 'not p' it can be inferred, by simple *modus tollens*, that 'not $H \vee$ not ξ'. Who must be blamed for the false prediction, then? H could be maintained come what may if the only constraints operating in our belief system were those that follow direct observation and deductive logic. It could also happen of H being mistakenly thrown in the wastebasket when a small correction in ξ was all it was needed to restore adequacy. Thus a straightforward, foundationalist HD account is at the same time too broad and too narrow. The solution, claims Quine, is to embrace holism. Quine says our claims about the world face the tribunal of experience not individually but only as a corporate body. It is *holistically* that our belief corpus is shaped and re-shaped by experience. In order to appraise p (or H or ξ, for that matter), something must be said about the other beliefs someone happens to have. The propositions a person is entitled to believe are, urges Quine, a function of holistic considerations applied to such prepositions.

Non-empirical epistemology does not take a stand on internalism/externalism controversies. Dawid is silent about foundations of knowledge and justified belief in general. This silence is a high price to pay since the methodological procedures he wants us to adopt are backed up solely by principled arguments of dubious *ad hoc* flavor and require, as such, strong epistemic support (assuming it can be given) to carry weight with even a moderate inductive skeptic. Be that as it may, albeit Dawid mentions holism not once in his book and Quine is invoked solely when the underdetermination issue is brought to the table, I think MIA can only be given what it is due if read through glasses framed in an holistic style. Justification: on Quine's picture, we learn about the world and strive to accommodate novel facts and experiences by maximizing two epistemic factors: overall simplicity of the new system and conservation of old beliefs. Now, it is precisely *because we believe in the methods of quantum field theory* that Dawid asks us to believe in string theory (recall that he calls the former a 'direct precursor' of the latter' (DAWID, 2014i): in so doing we preserve aspects of our belief system. Our belief in string theory is justified by means of its connection to other beliefs we have. The favorable epistemic presumption we give to string theory is justified when the principles of coherence and conservatism that holism relies on are invoked. And that, thinks Dawid, is justification enough.

Once Quine's footsteps are followed, non-empirical confirmation no longer feels intuitively suspect to the scientifically-minded philosopher. Dawid consistently applies the commitments of a conservative, holistically-structured epistemology. Therefore, if MIA ought to be questioned, questioned must be the epistemic principles it critically relies on.

3

It sure makes a nice sound when it rolls off the tongue our beliefs about the world are to be judged not individually but only as a corporate body. But as (GLYMOR, 1980) reminds us when he sets the stage for his bootstrap confirmation theory, it is not as if scientists act as if they were at loss with respect to how to distribute praise when the tribunal says 'yea!' or blame when the tribunal says 'nay'. There is no algorithmic procedure to follow when the distribution has to be made of course but there are, says Glymor, firm intuitions—intuitions that can be explained, in Glymor's opinion, by bootstrap relations. Regardless of whether or not holism is to be abandoned in favor of bootstrapping, however, it is important to note that in Quine's picture of the world the tribunal our beliefs face is *experience*. His point is solely that the trial is way more complex than HD confirmation would have us take it. But *the world*, would argue Quine, with all the concrete material happenings that trigger our philosophical concerns, *must unquestionably be out there for grips*; anxious and eagering to test the appropriateness of our belief systems. In Dawid's scheme, note, Nature is silent. How can the tribunal yell 'yea' or 'nay' if the seat is taken from the judge? Is Dawid's suggestion to implement a new tribunal—coherence with prior beliefs *alone* parceling out praise and blame? Would it not be dogmatism in disguise calling the shots at the courthouse Dawid designed?

4

Other things being equal, one should keep old beliefs rather than adopt new ones. This is the advice Quine's heirs give us and, in the light of the skeptical challenge with which the last section was closed, the die-hard Dawidian reasoner may insist that we *know* quantum field theory works. No confirmation theory would question the extent to which we are empirically justified in granting it our belief. Dawid denies that to believe in string theory on the basis that we believe in the standard model leads us to the way of dogmatism: such a move is, he would insist, just a natural inductive leap justifiable on the degree of confidence we have in the results yielded by the machinery of quantum field theory. But let us think this through. If Dawid is right and MIA can be granted soundness, then it can be elevated to the status of a general epistemic principle that works whenever we are puzzled to choose amongst beliefs. So I invite the reader to entertain the following thought. Imagine the existence of some world W which only differs from ours to the effect that MIA has—for some unknown and irrelevant to our purposes reason—to be employed on the assessment of all scientific theories developed after the death of Aristotle. All other aspects—physical, historical, social—are preserved *holus bolus*. Suppose that MIA is consistently applied by the scientific community in W and a book, entitled 'History of Science in W from Aristotle Onwards', is written by a member of that community. Suppose further that the Spirit of Critical Examination of Theories of Confirmation leaves a copy of such book on your desk for your amusement and scrutiny. A note with a procedural tip is also left by the Spirit. It reads: 'compare the development of science in W with the history of science of your world. If it matches, MIA yields rationally and epistemically justified beliefs. If a mismatch is found, the opposite conclusion obtains'. Astonished to find such a precious gift awaiting for you, you immediately obey the Spirit's command, reaching to the closest history of science book resting at your shelf. The upshot is that the science of W and of your own world do not merge. To the contrary. Driven by coherence and conservatism, W's science has never outgrown its Aristotelian roots. No talk of black holes, of spacetime metric, of quantum jumps, of entropy, for that matter: in W, the 'terrestrial realm' is said to be made of four elements—earth, fire, air and water—and the 'celestial realm', of a fifth substance; the 'quintessence', in Aristotelian jargon. Apples do not fall due to gravity in W—they fall because they seek 'their natural place'. Similarly, planets do not orbit M's Sun due to the conservation of momentum of primordial hydrogen atoms that eventually attached to one another through mutual gravity—they in fact do so because, well, it is the natural thing for them to do.

Aristotle's physics works well enough for Aristotelian-minded scientists with

Aristotelian-like epistemic purposes. The fact that this doctrine stood undisturbed in a world in which pressure prompted by evidence for purposes of theory revision does not obtain is unsurprising: recall Aristotle's science and philosophy have reigned sovereign for many centuries in *our* world[2]. Guided by MIA, the scientists of W are not judges of theories: they are *advocates* of Aristotle. In spite of non-empirical reasoners' protests, to endorse MIA does imply going to bed with dogmatism. In a world in which Dawid's 'non-empirical' epistemic guidelines are the only tool for accessing scientific hypotheses, Kuhnian worries of incommensurability, duck-rabbit gestalt switches, theory-ladenness of observation, and the like take no place. A copy of *The Structure of Scientific Revolutions* would strike the scientific community of W as—literally—science fiction. There is no 'conceptual revolution'—to again abuse of the jargon—when to retain old opinions is taken to be the most rational course to follow. Perhaps some may say that, in that respect, the people of W are in advantage over us. That advantage, however, is the same of theft over honest toil—or, more accurately, of living an epistemic fairy tale over the struggles of climbing the mountains of truth.

When Nature is no longer heard and all one has at the confirmation equation are conservative-driven theoretical criteria—that is, 'non-empirical' ones—fights against dogmatism are considerably harder. The counterfactual exercise here suggested is, I think, enough to cure any clear-headed MIA reasoner.

5

The conclusions drawn in the previous section could be dismissed as hand-waving by the MIA reasoner who questions the grounds of counterfactual reasoning[3]. I think we need not go as far as distant worlds to acknowledge the undesirable results one is led to by non-empirical confirmation, however. The history of science of the world brought to our awareness by perception alone brings an even deeper objection, meaning, the very facts it depicts. I explain: Dawid claims MIA is framed in the historical record (that is why he states that MIA is, as it stands, an *empirical* argument (DAWID, 2014j). A brief analysis however suffices to show that, to the extent that history of science bears on MIA, the latter is simply descriptively false.

First, a bit more of stage-setting. Dawid argues that 'regular predictive success in a research field justifies the assumption that future predictions of similar kind will be correct as well' (DAWID, 2014k) and that 'the predictive success of the standard model is constitutive for the current trust [...] in string theory' (DAWID, 2014l). He makes an

[2] Aristotelian ideas influenced research even long after they had allegedly been removed by early modem astronomy and physics - any history of 17th- or 18th-century science will show that (see, for example, John Heilbronn's Electricity in the 17th and 18th Centuries, Berkeley and Los Angeles, 1979). Traditions, like old habits, die hard.

[3] I direct the reader to Alan Hájeks' 'Most Counterfactuals are False'.

analogy with work on paleontology (!) to strengthen his case: ‘let us imagine’, he suggests,

> that strategies similar to the ones that have led [someone] to believe about a number of theories about the animal that owned the [previously mentioned] discovered tooth have also led [her] to believe in various theories on other animals based on discoveries of other fossils. If some of those other discoveries end up being vindicated by later findings of more complete fossils, this not only supports those theories themselves but also the methods which generate them. The increased trustworthiness of the involved methods, in turn, also raises the trust in theories which are based on our dinosaur tooth (DAWID, 2014m).

The inductive leap that backbones MIA—in addition to the conservative principle discussed in the previous section—is springboarded by (i) limitations of scientific underdetermination and (ii) inference to the best explanation. Dawid reasons thus:

> Assuming limitations to scientific underdetermination [. . .] looks like the only satisfactory explanation of the fact that scientists regularly find theories which are predictively successful. Inference to the best explanation can then lead from the observation of regular predictive success in scientific field towards the conjecture that scientific underdetermination is limited in that field (DAWID, 2014d).

Perhaps a brief remark or analogy can illuminate our reading of the passages quoted from Dawid’s work. In the midst of the adventure entitled ‘The Sign of Four’, Sherlock Holmes rebukes his companion, Dr. Watson, for not reasoning clearly: ‘How often have I said to you’, says the detective, ‘that when you have eliminated the impossible, whatever remains, *however improbable*, must be the truth?’ To the ear of the epistemologist, Sherlock Holmes’s rhetorical question shall sound as the paradigm of eliminate induction—or inference to the best explanation, or abduction—at work. The reader must have noticed that the strategy adopted by Dawid goes along very similar lines: NAA yields us reasons to believe string theory has no competitor (all alternatives were discarded as dead-ends); MIA tells us we are justified in granting belief to *the only option we have* (for reasons of conservatism and eliminative induction). And then, *voilà*. You are right on the track of truth by means of non-empirical confirmation.

Many may read from such a reasoning an open invitation to rationalism. This (highly non-trivial) implication of Dawid’s philosophy need only concern us, however, if inferenced from solid grounds—that is, if MIA, from which it follows, can be granted soundness. I argue it cannot. It is unclear how the successes of the standard model can be extrapolated to string theory *if string theory is no child of the standard model.* String theory has sprung into the world within the duality program—which, by its turn, has outgrown from Geoffrey Chew's formulation of Heisenberg's S-matrix formalism. It was Yoichiro Nambu who, in 1969, discovered a link between duality models—which, as stated, emerged from the S-matrix program—and the field theory of a string. The quantized string theory industry has arisen *because of the duality program of S-matrix theor*—a program envisaged as a substitute for (the at the time going through a severe downward gradient) quantum field theory[4,5,6]. Indeed physicist, historian and philosopher James Cushing writes that

> without the S-matrix program, it is unlikely that quantized strings would have ever been discovered and studied. By inference, one can ask whether theoretical physics would have arrived at one of its promising constructs (superstring models) if it had not been for the existence, nearly four decades earlier, of a program (Heisenberg's S-matrix theory) which ultimately in its own right proved a dead end. It is impossible to prove that we would not have arrived at quantized string models by another route. However, the present detailed study of how this came to be seriously considered as a result of the S-matrix and duality programs make it appear implausible that they would have been discovered had it not been for the S-matrix theory. On the other hand, the road from quantum electrodynamics to modern gauge field theories owed nothing to the S-matrix program (CUSHING, 1990b).

On such grounds, the question we ought to ask ourselves is whether or not it makes sense to argue that string theory 'shares' the successes *of other theories in the research program*—QED and QCD, says Dawid—when string theory owes its very *existence* to a

[4] After the astonishing successes of quantum electrodynamics from 1947 to 1949, it was expected that similar methods of quantum field theory would also work for other fundamental interactions. Hopes were eventually fulfilled – but not soon enough. Indeed Steven Weinberg says the following about the state of affairs at that time: 'It was not long before there was another lapse in confidence – shares in quantum field theory tumbled at the physics bourse, and there began a second depression[5], which was to last for almost twenty years' (WEINBERG, 1977). Remember that there was, as yet, no unification via the concept of gauge fields and Yang-Mills Theory.

[5] The 'first depression' or crisis refers to the 'plagued with infinities, not yet renormalized' QED. Problems were in part of a logical and conceptual nature; part linked to the incapability of the new relativistic quantum theory to account for new facts. Within the first group of problems, in addition to the paradigmatic issue of the infinite self-energy of the electron (which has its counterpart in classical theory) new divergencies of quantum nature have arisen in relativistic quantum field theory[6]. Things changed after Willis Lamb, at the Columbia Radiation Laboratory, reported the experimental results now known as 'Lamb Shift' in 1947. Such a result paved the way for Julian Schwinger's, Sin Itiro Tomonaga's and Richard Feynman's technical developments that culminated at the new renormalized QED of 1948. (KRAGH, 2002b).

[6] R. Oppenheimer, for example, proved in 1930 that in addition to the classical electrostatic self-energy of the electron a quantum effect would contribute with an extra, quadratically diverging term to the self-energy of the bound electron. These divergencies would imply an infinite displacement of spectral lines – an obviously unacceptable result. See Oppenheimer's 'Note on the theory of the interaction of field and matter'. Phys. Rev. 35, p. 461-477, 1930.

completely different research program, the S-matrix, whose worldview, physical principles and methodological techniques are at odds with those of quantum field theory[7]. It is true that string theory poses a challenge to the historian: the very notion of what such a theory is has gone through aggressive transformations throughout its short lifetime. The theory has seen changed the notion of what its most fundamental objects are (open strings? closed strings?); what these objects represent (hadrons, gravity, photons…?); in what scale they operate (10^{-13}cm? 10^{-33}cm?); on what sort of space they inhabit (4D, 5D, 10D, 11D, 26D?). What physical picture is attached to these things? Does the space in which strings propagate have curled-up, internal dimensions? How are they compactified? Do strings possess worldsheet symmetry or space-time supersymmetry? Are there 'higher order' or 'lower order' strings (0-branes, 1-branes, 2-branes)? Or both? Historical record shows that answers and questions have changed dramatically. Dawid can of course argue that, in spite of its roots in hadronic physics and S-matrix theory, string theory soon developed a highly different structure, with a different intended target-system from those displayed at the time of its birth at the corridors of the S-matrix bootstrapped palace. He would be right on such respect. String theory switched from being a theory of strong interactions to a theory embodying Yang-Mills fields and gravitational interactions. But can one say its structural relationship with QFT is such that ST, QED and QCD are part of one and same research program when we have established that ST had blossomed within a QFT-independent line of inquiry, SMT—one which, by all means, has not been *falsified* (in Popper's jargon) but simply *abandoned* in favor of QFT's came back in form of spontaneously broken gauge field theory, the empirical adequacy of which followed soon enough to shift everyone's attention?[8] Let me emphasize this is *not* a trivial claim and one cannot gloss over the (possible) structure, concepts and methods string theory owes to/ has inherited from S-Matrix theory. A very sophisticated structural mapping of S-Matrix, QFT and string theory—this latter throughout its many forms—would be required before any straightforward diagnosis of the sort 'research program X has characteristics α and β'; 'research program Y displays characteristics ϕ and ψ', 'theory T has shifted from X to Y' is written down—even if you define 'research program' as broadly as the Lakatosian doctrine allows you to.

At any rate, either ST lies at the QFT research program or it does not. Only if it *does* can the question of whether or not the positive instances that confirm QED and QCD also confirm—'meta-inductively', 'non-empirically'—string theory be posed. If it does not, MIA is *prima facie* false and must be immediately dismissed. Until assurance can be given with the respect that ST obeys a strictly QFT rationale, that it shares the 'hard-core', 'protective belt' and 'positive heuristics' of such a research program, however, MIA must be considered—best-case scenario—a non-starter.

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7 This shall be our topic of discussion in the next chapter.

8 I direct the reader to Cushing's brilliant (1990) for details of S-Matrix's downfall.

6

Now I beg for the reader's patience and ask her to join me on accessing one last issue before this chapter is closed. Let us assume a hard-nosed Dawidian has tricked us into taking a magic pill that cancels out the effect of the arguments against MIA above given. In this new state of affairs—where no argument I've presented in this chapter has any epistemic force; where methodological conservatism does not have us trapped in the dogmatic carousel and where string theory does lie within QFT's research program... can MIA be granted the epistemic tenability Dawid attributes to it?

Let us bring some epistemology tautologies into the scene. It seems pretty obvious that, inasmuch as there cannot be a 'true' belief and a 'true' *disbelief* in the same proposition, *there cannot be*, on pain of logical inconsistency, a *justified belief and a justified disbelief in the very same proposition*—given, of course, the same set of facts. If the available facts are such that they justify believing in X, they will not be such as to justify disbelieving in X. It is thus safe to say, on such grounds, that if your epistemic principles (or criteria of justification) are such that they enable you to have, with respect to X, justified belief and justified disbelief alike (X being seen, I repeat, with reference to the same data), either such principles (or criteria) cannot be held by a rational person and must be dismissed altogether or, to our astonishment, rationality has somehow changed its ways and can now be identified with subjective whim (a possibility we reject). This is uncontroversial enough to avoid any skirmishing with our Dawidian friend, I hope.

Now, the problem I find with MIA is precisely that it entitles us to have opposite beliefs with respect to the same proposition. MIA tells us a theory T that has not yet enjoyed empirical confirmation reaps the empirically granted benefits of its research program companions (in our case, QED and QCD). However, if MIA 'non-empirically confirms' string theory, it confirms also, by the same token, say, loop quantum gravity (LQG)—ST's most well-developed rival in the quantum gravity battlefield. If ST is to be found in a room of the Quantum Field Theory Architecture, so is LQG[9] (indeed, in his LQG textbook Carlo Rovelli writes, 'the main formalism of this book is to develop the formalism for background independent[10] QFT' (ROVELLI, 2003)—and pretty much most approaches to quantum gravity[11]. Now imagine two individuals—say, Lomsin and Twiten—of whom the latter believes in string theory and the former does not (he grants

[9] For that to happen, note, one's notion of 'research program' must be loose enough to meaning simply 'use similar theoretical methods and mathematics': we have seen that it has not been established at all that ST is to be found in QFT's research program in Lakatos' sense. Since the reader is under the effect of the magic pill, however, I will not take issue of the abuse Dawid makes of Lakatosian jargon.

[10] For a good discussion of background independence and its implications, see (RICKLES, 2008)

[11] Yang Mills, general covariance, internal gauge invariance, high symmetry and local Lorentz invariance (to cite a few) are common traits of most approaches. See, for instance, (WITTEN et. al., 1987) and (ROVELLI, 2002).

his belief to loop quantum gravity instead). Suppose that Twiten knows all facts available to him, and that Lomsin knows all facts available to *him*. Suppose further that Lomsin knows all the (relevant) facts that Twiten knows and Twiten knows all the (relevant) facts that Lomsin knows with respect to the quantum gravity problem (on the basis of inference as regards what is the correct answer to the question of whether or not string theory is a true theory of all interactions). It is of course not possible, under such circumstances, for Twiten to be justified in believing in string theory and for Lomsin to be justified in *disbelieving it*. And yet both might be MIA reasoners; each of them justifying their opinion on whether or not string theory or loop quantum gravity holds the solution to the quantum gravity problem by reference to the fact the preferred theory shares quantum field theoretical traits with QED and QCD and must, on similar grounds, share also the empirical avail Nature yields them. Each of these individuals could also be aware of the opposite opinion held by the other and of the support of MIA to it (even though, note, it would not be neither logically nor *psychologically possible* for Lomsin to regard Twiten's opinion as justified or for Twiten to regard Lompsin's position as justified). Were MIA thus tenable, *it would be possible for both Twiten and Lomsin be justified in their contradictory opinions with regard to the same body of evidence*, empirical and otherwise—an altogether absurd result.

I think there is no escaping the conclusion MIA fails to justify one's maintaining a certain epistemic attitude towards one theory over another and cannot, for this very reason, motivate rational decisions for us. If MIA had thus been allowed to stand in spite of the considerations adduced against it in previous sections of this chapter, this last result certainly suffices to show that MIA must be charged with incoherence and contradiction and ought to be deleted as a methodological epistemic principle candidate.

7 The 'Unexpected Explanatory Coherence' Argument

We must have both felt that it must be good for something, since it was such a beautiful, tight structure.
John Schwartz

An error does not become truth by reason or multiplied propagation, nor does truth become error because nobody sees it.
Mahatma Gandhi

Dawid argues three principled-arguments form the basis of non-empirical confirmation's architecture—two of which have fallen at the weight of their epistemic pathologies, as we have seen in previous chapters. The identified difficulties are not redeemable by appealing to Dawid's last card, the 'unexpected explanatory coherence argument': a four-legged chair is useless if three of them are cut and a three-pillared house is unable to stand if two of them are demolished. I shall, however, leave no stone unturned in my philosophical examination of Dawid's epistemology—even though the lack of tenability of non-empirical confirmation has already been demonstrated and going through this one last road may imply (if it is only the soundness of his proposal taken as a whole what interests us) unnecessary expense of intellectual energy from our part. The unexpected explanatory coherence argument (UEA) has, however, its own special interest and, I think, valuable lessons can be drawn from it. Its relevance springs from its apparent *a priori* plausibility: it is, in fact, the argument most commonly invoked, both in the literature and in the media, to crown string theory 'the theory of everything'. I have saved it for last precisely because it is in my opinion the strongest weapon of Dawid's arsenal (even though the least original one)[1].

The procedural steps for my philosophical analysis of UEA take somewhat different form from those followed in previous chapters. I again borrow Dawid's voice on presenting UEA but, to assess it fairly, we must go farther than Dawid himself has gone. Two sections are added with the strict purpose of unpacking the details UEA involves. General conclusions and challenges are drawn at the end of this chapter.

[1] Dawid's own presentation follows a different order. He first discusses NAA, then UEA, and MIA at last. I do not think order makes any difference neither at introducing non-empirical ideas nor at analyzing them.

1

Dawid is concerned in locating justificatory weight on certain specific theoretical traits of string theory (a 'no miracle's argument', it seems, very similar in structure to that famously put forward by Putnam (1975) in his defense of scientific realism). In general lines, his reasoning goes like this. Suppose you have a set of problems α (empirical or not—to Dawid, the argument goes both ways) for the resolution of which there are two competing theories—S, S', say—both of which successful in resolving the targeted problems α and enjoying (with respect to α, of course) identical degree of confirmation. Take both theories to also be pairwise inconsistent. Now, epistemic balance is broken when it is discovered, after further investigation, that S unintendedly uncovers the solution to a different, additional set of problems β: S had explanatory interconnections hidden in its womb unexpected by those who constructed it and a theoretical spectrum far more reaching *vis à vis* the (more limited) role it was devised to play by the time of its birth. The upshot is that there exists an intuitively grasped 'objective character' to this 'coincidence' which changes the confirmation picture dramatically: the likelihood of S is greatly increased; the likelihood of S' quickly being driven down to 0. Now, trade 'S' for 'string theory' and 'S'' for 'any other quantum gravity theory' and *voilà*. String theory, says Dawid, stands highly confirmed in this scenario.

But what coincidences of this sort can one find in string theory? It is *these* we must assess in order to grant or deny UEA plausibility. Curiously, however, although the edifice of non-empirical epistemology depends upon the strength of UEA by a fraction of one-third Dawid gives it a very skeletal treatment, sketched in a few paragraphs only. In presenting his ideas to the reader though, I am incapable of being even briefer; so I quote, in the following, the relevant purple passages:

> It is widely held that a truly convincing confirmation of a scientific theory must be based on those of the theory's achievements which had not been foreseen at the time of its construction. Normally, this refers to empirical predictions which are later confirmed by experiment. However, there is an alternative. Sometimes, the introduction of a new theoretical principle surprisingly provides a more coherent theoretical picture after the principle's theoretical implication have been more fully understood. This kind of theoretical corroboration plays an important role in string theory. Once the basic postulates of string physics has been stated, one observes a long sequence of unexpected deeper explanations of seemingly unconnected facts or theoretical concepts (p. 33).
>
> String theory posits nothing more than the extendedness of elementary particles. The initial motivation for suggesting it as a fundamental theory of all interactions was to cure the renormalizability problems of quantum field theories that include gravity. Remarkably, string theory does not only just

> provide a promising framework for quantum gravity but actually implies the existence of gravity. The gravitational field necessarily emerges as an oscillation mode of the string (p. 33).
>
> String theory also puts into a coherent perspective the concept of supersymmetry. Initially, interest in this concept was motivated primarily by the abstract mathematical question whether any generalization of the classical continuous symmetry book was possible. As it turns out, supersymmetry is the maximal consistent solution in this respect. Soon after the construction of the first supersymmetric toy-model, it became clear that the implementation of supersymmetry as a local gauge theory (i.e. supergravity) had the potential to provide a coherent field theoretical perspective on gravity and its interaction particle, the graviton. In the context of string theory, on the other hand, it had been realized early on that a string theory that involves fermions must necessarily be local supersymmetric (p.34).
>
> All of these explanations represent the extendedness of particles as a feature that seems intricately linked with the phenomenon of gravity and much more adequate than the idea of point particles for a coherent overall understanding of the interface between gravity and microscopic interactions. The subtle coherence of the implications of the extendedness of elementary objects could not have been foreseen at the time when the principle was first suggested. It would look like a miracle if all these instances of delicate coherence arouse in the context of a principle that was entirely misguided (p.34).

Dawid's exposition of string physics is seductive and provides an explanation for the number of practitioners enamored of its structure (amongst of which we can undoubtedly count Dawid in). Whether or not the described features entail confirmation is, of course, a completely different matter. Before we begin examining critically the epistemological extrapolations and conclusions Dawid derives from the aforementioned 'theoretical facts', however, given the vagueness with which he chooses to present them, it is pressing we check whether his description of such 'facts' are not misconstructions or oversimplifications which, after closer inspection, allow solely less bold interpretation in contrast with those Dawid has presented us with. This shall be our task in the next two sections.

2

The point of this section is to strip UEA from rhetorical decoration and make it more concrete. In trying to decide whether or not 'surprising theoretical interconnections', embodied by Dawid in UEA as an 'epistemic principle', allow rational justification for belief, we are in deep water for we must go through the related formalism ourselves and check if the mathematical structure of string theory does springboard the physical and ontological interpretation Dawid has clothed it with. There exists the suspicion UEA stands up a straw man; backed up by theoretical flimflam—if proven correct, such a principle is to be discarded as hand waving; if not, philosophical analysis can proceed. To review all the relevant formalism is of course too gigantic a topic for the compass of this work and setting an appropriate level for the presentation is an additional problem of gargantuan proportion. I shall attempt to make the treatment of this topic sketchy, but sufficient for our purposes of analysis—hoping this does not result in an assault upon straw man of *my part*. I am forced by the difficulty of the subject to assume the reader is familiar with the tools of mathematical physics (although I shall try to simplify as much as I can and use verbal presentation of the results of physical theory whenever possible). A listing of resources will be given in footnotes, in the hope they can make up for the lack of extensiveness and detail of my part.

One more brief additional note on procedure before we begin. Focusing here on Dawid's assertion 'string theory implies the existence of gravity', investigation shows two independent formal results might justify it. One is our object of study in this section, the other is dealt with in section 3. These results cannot, however, be appreciated if not placed in the context in which they appear—and their appearance happened as results of theoreticians' work hours in their struggles to understand the *strong interactions* (!) back in the 60s. A historical snapshot of the chain of developments that culminated in the birth of string theory is our departure point in this section but, I hasten to add, it so due to no coincidence: the birth of strings and the results eventually associated with the graviton are complexly intertwined, as the reader shall see.

Our narrative starts in the best empiricist fashion: data-driven. We know that quantum mechanics (in contrast with its classical counterpart) does not provide us with a deterministic evolution of isolated states from initial to final states but only allows us to compute, given the initial state, certain probabilities of transition for various later states. When one's worries are upgraded to the physics of many particles, things are no different and the observables are likewise infected by probabilistic considerations. One of the observable quantities is the *scattering cross-section*, defined as the transverse area relative to a particle's motion that guarantees it will collide. The scattering cross-section allows you to compute the likelihood of a collision given that two particles are moving towards each other: the magnitude of the cross-section is proportional to this likelihood. This cross-section is itself a function of the energy of the incoming beams; and if the behavior of the cross-section as a function of the energy is examined, it is possible to identify peaks from which one may infer the existence of particles.

Data analysis of scattering experiments performed in the early sixties indicated the production of a large number of new short-lived particles, or 'hadronic resonances' ('fleeting', strongly interacting particles which correspond to sharp peaks in the total

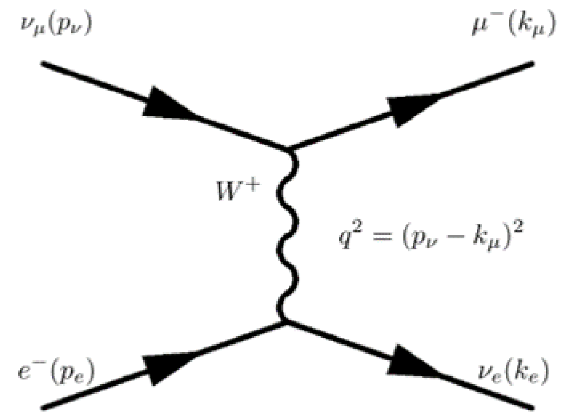

Figure 2 – Diagram of 2-body scattering between an incoming muon neutrino with 4-momentum p_ν and an electron at rest with 4-momentum p_e. *Image source:* (FORMAGGIO; ZELLER, 2013).

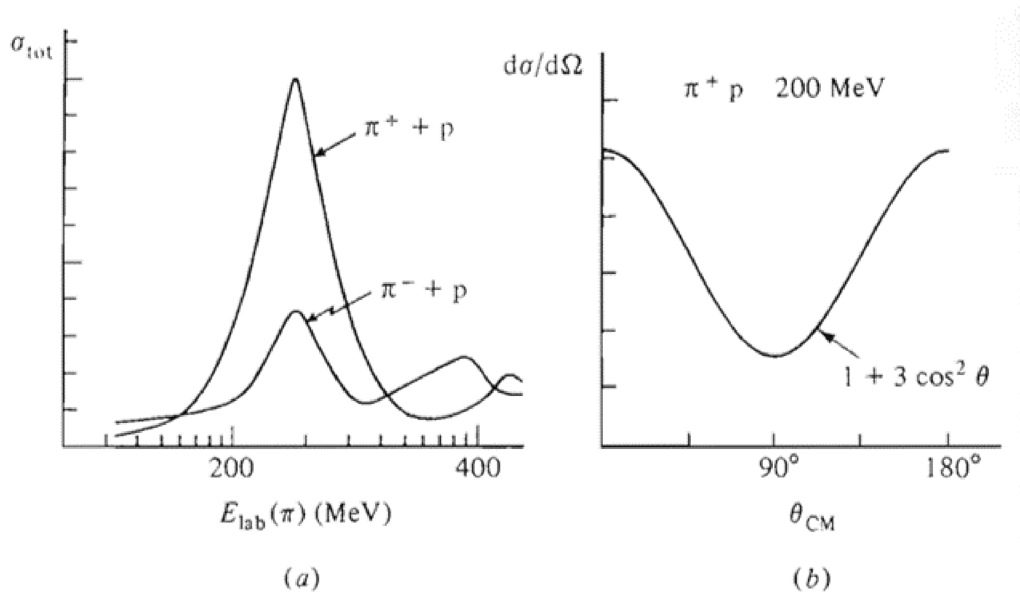

Figure 3 – The 3,3 resonance in pion-muon scattering. *Image source:* (CUSHING, 1990a).

cross-section as a function of the energy).

The strong force is, well, strong—this implies the greater the energy, the greater the flow of particles. Indeed, from the early sixties onwards more and more entries were added to the list of hadronic resonances, all decaying on a time scale of 10^{-23} seconds (historian of physics Andrew Pickering calls this ever-growing list of hadrons 'population explosion'. I direct the reader to (PICKERING, 1984) for details). These hadronic resonances seemed to exist with a rather high spin, the mass squared of the lightest particle of spin J being $m^2 = \frac{J}{\alpha'}$, where the quantity $\alpha' \sim 1GeV^{-2}$ is a constant called the 'Regge slope'. The resonances were so numerous physicists started thinking they could not possibly be all fundamental (i.e., elementary) and their spin values posed a real problem because they were seemingly 'unbounded from above', to use Rickles's (RICKLES, 2014a) apt expression.

Spin behavior was tested up to about $J = \frac{11}{2}$ and there was no reason it should not continue going indefinitely. No consistent field theory, at the time, could explain this feature; the spin values were inferred from the angular distribution of the decay products in the various reactions[2].

As the experimenters generated increasing amounts of data on increasing number of particles, so theorists set out to apply some order on what was being found. They faced, at the beginning, extreme difficulties. We know—as they did—that interactions among particles are classified into gravitational, electromagnetic, weak and strong. With exception of the latter, these were well-understood by the scientific community of the sixties but were pursued, on the whole, as independent lines of inquiry[3]. One important common feature, however, distinguished the electromagnetic and weak forces from gravity: both were well-described by quantum field theory; their coupling constants are small so that expansion in perturbation theory are relevant; only a small number of particles (for fields) are described by the framework; they possess a gauge group that leaves the Lagrangian invariant. It would be nice, particle physicists thought, if the strong force would also follow the pattern. Strong interacting particles, however (hadrons, that is), true to their name, have large coupling constants which determine how strongly they interact with one another. The standard field-theoretical tool of expanding quantities in powers of these constants no longer works in this case. The perturbation expansion breaks down[4]. Physicists tried to model the reported phenomena with the weaponry of quantum field theory they knew so well but, as it turned out, it was useless for such. The failure to apply QFT to the study of the strong interactions forced theorists to doubt the orthodoxy and search for new ways of thinking[5]. One approach gained momentum and, for about two decades, took centre stage: the so-called *S-matrix theory*. The S-matrix, also known as 'bootstrap theory', involved focusing squarely on just those properties of the scattering process (or, if you wish, of the probability amplitude for such a scattering) that had to

[2] Consistent (meaning renormalizable) quantum field theories seemed to be limited to spins zero, one-half, and one (examples being Abelian gauge theories, scalar and Yukawa theories, Yang-Mills theory).

[3] The reader must note that, although the Glashow-Weinberg-Salam theory of weak and electromagnetic unification had been developed and published in 1967, it stagnated for some time before conditions were sufficiently ripe for its importance to be recognized by the community of physicists.

[4] The uncertainty principle, once located within quantum field theory, implies that particles (i.e., quanta of the field) can be either created or destroyed at a rate that is related to the energy of the system in question. This implies that whenever special relativity and quantum theory are combined you are faced with many-body physics—a situation that is compounded as the energy is increased. If the coupling constant is less than 1, then you can deal with the increasing number of particles popping up as negligible 'corrections' to the lowest order terms; if greater than 1, however, going higher in the perturbation of the coupling constant (and adding an increasing number of particles) means the corrections will not be inconsequential and the first terms will not, as in the small coupling constant case, yield a for-all-purposes good approximation of the whole series. It makes no sense to rely upon a first-order term which is followed by an infinite series of 'perturbations' of increasingly greater magnitude.

[5] See (WEINBERG, 1977) for a nice historical survey of the ups and downs of quantum field theory presented from a particularly privileged perspective. Another comprehensive historical treatment can be found in (CAO, 2010).

be followed by a (physically) reasonable relativistic quantum theory[6] but dealing solely with *observable* quantities[7,8]. The axioms of S-matrix theory are the following: (i) Lorentz invariance[9]; (ii) unitarity[10]; (iii) CPT symmetry (or 'crossing')[11]; (iv) analyticity in the complex plane of the Mandelstam variables[12]; (v) maximal analyticity in the complex

[6] Although the S-matrix had sprung into the world within the confines of QFT, it soon broke free from its field-theoretical roots and was seen, in Chew's 'bootstrap' formulation, as an explicitly anti-field-theory approach.

[7] The reader may have heard this sort of talk before: the S-matrix shares Heisenberg's and Bohr's positivistic strategy of ignoring what happens between quantum events. In the S-matrix case what is ignored (as unphysical or meaningless since too 'short-lived' to be observed) are the unmeasurable processes occurring between initial and final states of a process: what one does is to sketch a 'black box' around the innards of the process and only gives attention to the particles entering and leaving the box; and their probabilities of doing so (one has got to split the Hamiltonian into two parts $H = H_0 + H'$; the former describes a world of 'non-interacting particles' and the lattes a world of interacting ones. When the experiment begins, H' is zero; during the interaction it is non-zero and some mysterious things happen; at the end of the experiment, H' is zero again. The operators time-evolve according to the free part H_0 of the Hamiltonian solely. See (LANCASTER; BLUNDELL, 2014) chapter XVIII for a very clear exposition of the subject).

[8] With respect to the 'fundamentality question' raised in the preceding paragraph, to Geoffrew Chew (S-Matrix's most prominent figures) it was simply a misguided approach to question which hadrons are 'elementary' and which ones are 'bound states': one should treat them all *democratically* (a concept Gell-Mann labeled 'nuclear democracy'). This is indeed the core of the notion of 'bootstrapping' within the S-Matrix program: hadrons are bound states of other hadrons, which by their turn held together by hadron exchange forces. They were supposed to 'pull themselves up by their bootstraps' as a self-consistent solution to the S-matrix equations—a purely endogenous mechanism (note the difference with the 'aristocratic' character of quantum field theory; in which each particle is assigned its own quantum field).

[9] Lorentz invariance is satisfied when physical quantities are unchanged by Lorentz transformation (of form $x'^{\mu} = \Lambda^{\mu}_{\nu} x^{\nu}$ for all 4-vectors $x^{\nu} = (x^0, \mathbf{x}) = (t, \mathbf{x})$ and Lorentz tensors Λ^{μ}_{ν} (this guarantees energy, angular momentum and momentum conservation).

[10] Unitarity amonts to the condition that the scattering matrix S is unitary—namely, $S^{\dagger}S = 1$ (this guarantees that probability—the modulus squared of the amplitude—is conserved over time).

[11] Crossed processes correspond to continuing energies from positive to negative values (whence the particle-antiparticle switch). Thus 'crossing' is a symmetry that relates pairs of processes that differ by the exchange of one of the input and output particles (thus mapping particle to antiparticle and the other way around). For instance, $a + b \to c + u$ and $\bar{u} + b \to c + \bar{a}$ (in which $\bar{a}$ and $\bar{u}$ are a and u's antiparticles).

[12] An analytic function is a function that is locally given by a convergent power series (that is, if its Taylor series about x_0 converges to the function in some neighborhood to every x_0 in its domain). An analytic complex function is differentiable at all points in some region. Analyticity is satisfied only if a scattering amplitude A is an analytic function of the Lorentz invariant objects used to represent the physical processes in question. This scattering amplitude A is a function that yields probabilities for the outcomes of collision particle experiments and possesses properties of the particles as argument. For instance, the function may depend upon the energy E of the collision event and upon the scattering angle ϕ that represents the particle's deflection. $f(E, \phi)$ therefore represents the nature of this interaction. The more general representation of it involves the incoming energy s and the momentum that is transferred in the collision t, which are defined in this context in the following way:

– t is the square of the difference between the initial and final momenta of the particles of the system in question; $t = (p_a - p_c)^2 = (p_b - p_d)^2$;

– s is the square of the sum of the momenta of the initial state in one hand and of the final state on the other; $s = (p_a + p_b)^2 = (p_c + p_d)^2$.

– u is obtained by means of switching particles c and d from t. Thus $u = (p_a - p_d)^2 = (p_b - p_c)^2$.

plane of the angular momentum (nontechnically referred to as 'Regge behavior')[13]; (vi) factorizability of the residues of particle poles[14] with real coupling constants (particles that couple by means of imaginary coupling constants are referred to as 'ghosts' in the literature. If your theory yields it, you better find a way to get rid of it).

Steven Weinberg, one leading field theorist, wrote in 1964 that 'it is not clear whether field theory will continue to play a role in particle physics, or whether it will ultimately be supplanted by a pure S-matrix theory' (CUSHING, 1990c)—such was the status the S-matrix enjoyed before the phoenix-rise of chromodynamics and its eventual crowning as the correct theory of the strong interactions[15]. The aforementioned problem of explaining the high-spin behavior of hadrons was, within the confines of the bootstrap

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The incoming momenta of the particles, p_a and p_b, are identified with outgoing momenta $-p_c$ and $-p_d$. There is, in this process, conservation of 4-momentum (as it should, of course). The scattering amplitude is then a function of certain conserved invariant properties (called 'channel invariants'). Take for instance a process in which we have two incoming and outgoing particles of same mass M. Then $a + b \to c + d$. A 4-point amplitude $A(s, t)$ is involved in such a process. This is then written as $A(s,t) \sim \beta(t)(s/s_0)^{\alpha(t)}$. Here β is the so-called 'residue function'; both it and s_0 are scale factors; $\alpha(t)$ is the spin of the appropriate Regge trajectory (see next footnote). The squared modulus of these things yields the desired scattering cross-section and all the physics required. The Mandelstam variables s and t define reaction channels in the following way (adopting a metric signature $\{-,+,+,\dots,+\}$):

– the reaction $a + b \to c + d$ occurs in the s-channel, with the physical regions defined by values $s \geq (m_a + m_b)^2$;

– the 'crossed' reaction $a + \bar{c} \to \bar{b} + d$ happens at the t-channel. The physical regions are defined in an analogous way by $t \geq (m_c + m_b)^2$. Finally, the u-channel is the reaction $a + \bar{d} \to \bar{b} + d$; in which the physical region is $u \geq (m_a + m_d)^2$ (see figura 4).

13 A singularity of a complex function (that is, a point in which the value of the function is either zero or infinity for some argument) is called a 'pole' (a 'tree graph', to use Feynman diagram's nomenclature; with loops corresponding to branching points). Now think of a case in which amplitudes become poles for values of angular momentum. The locations of such points are determined by the energy of the system and the poles themselves are taken to correspond to propagation of particles. When you tune the energy parameter you get a graph—a Regge trajectory—that describes the properties of resonances and scattering amplitudes. When you transfer yourself to the relativistic world, you must introduce another class of singularity in angular momentum, one of which is at $j = -1$ (Stanley Mandelstam overcame this difficulty by introducing another Riemannian hyperplane of the complex j-plane and by performing branch cuts in it). Thus a Regge pole is a singularity that appears when you deal with angular momentum J as a complex variable. Physically, this refers to a sort of particle that 'lives' in the complex momentum plane—a particle whose spin is linearly related to its mass. Thus, if you tune the particle's energy to a value which would yield a half-integer or integer spin, this would produce a particle you can detect by experimentation! Following these lines, types of particles could be classified by their Regge trajectories; each trajectory possessing a number of resonances that differ solely with respect to their spin values (quantum numbers remaining equal). High-energy cross-sections depend smoothly upon s (for instance, at zero momentum transfer, $A(s) \sim s^{\alpha(0)}$). Another key prediction that follows from this is that cross-sections ought to be 'soft', that is, falling off exponentially fast with t: $A(t) \sim e^{\alpha(t) ln(s/s_0)}$; in which a linear form $\alpha(t) = \alpha(0) + \alpha' t$ was assumed to match the observed Regge trajectories. These predictions have enjoyed empirical success: they were found to be characteristic of high-energy hadronic scattering (see figure 5).

14 A pole is a mathematical singularity that behaves like the singularity $\frac{1}{z^n}$ at $z = 0$. For details, see (LANCASTER; BLUNDELL, 2014), appendix B.2.

15 Erosion in confidence in quantum field theory can in fact be traced back to the 50s, when great names of the 'old guard' of physics (Heisenberg, Landau, Klein) were debating over the infinities that plague it; more specifically, over whether or not one natural cut-off could be introduced (hopefully, a gravitational one). Of course, at that time, non-Abelian theories and asymptotic freedom had not yet been introduced.

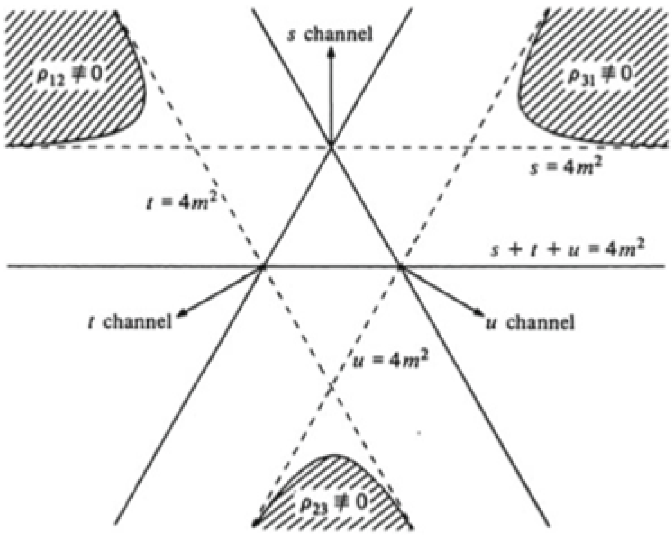

Figure 4 – A Regge trajectory function $\alpha(t)$ representing a rotational sequence of states (of mesons) of ever higher spins. The relationship with resonances (and bound states) comes about from the fact that when $\alpha(t)$ is a positive integer for some value of the argument t, then a bound state or resonance exists at that t-value, with spin read off the horizontal. For example, in this picture we have at $t = 3$ the resonance $\alpha(3) = 3$ and at $t = -1$ the bound state $\alpha(-1) = 0$. The various states given in this way generate a family: the Regge trajectory. A horizontal trajectory, $\alpha(t) = const.$, would represent particles of constant spin (elementary particles), while a non-zero slope represents particles of varying spin (composite particles). *Image source:* (RICKLES, 2014a).

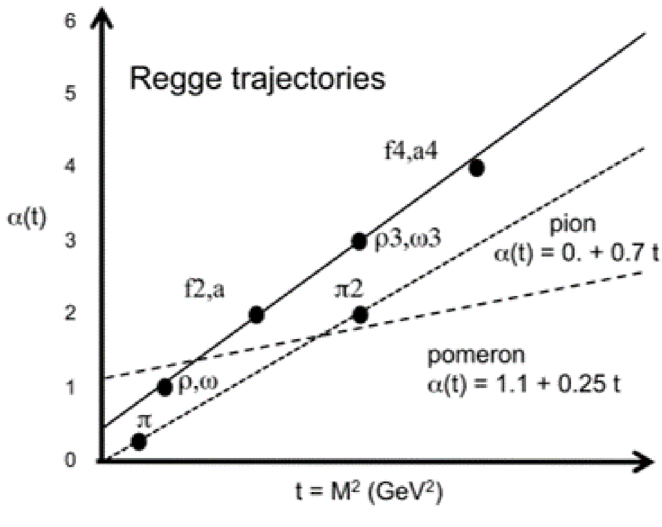

Figure 5 – Example of Regge trajectories in the complex angular moment $(J) - t$ plane: whenever $t = m^2$ (where m is the mass of a particle in the trajectory), then $\alpha(t)$ corresponds to the spin of the particle. *Image source:* (CARTIGLIA, 2013).

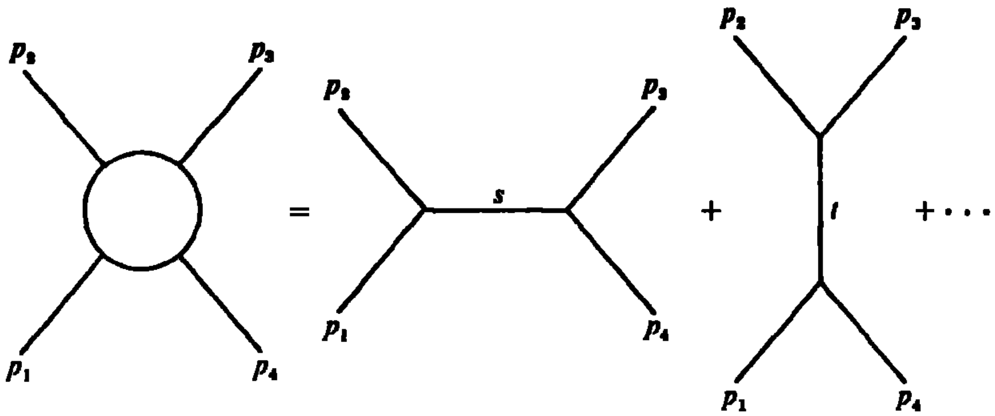

Figure 6 – An elastic scattering process with incoming particles with momenta p_1, p_2, and outgoing momenta $-p_3$, $-p_4$. s-channel and t-channel are both indicated. In field theory, the amplitude is found by summing s- and t-channel diagrams. *Image source:* (GREEN; SCHWARZ; WITTEN, 1987a).

formalism, solved in the following manner: take an elastic scattering process with incoming spin zero particle momenta p_1, p_2 and outgoing particles of momenta p_2, p_3; adopting a metric $\{-,+,+,...,+\}$ in order to the mass squared of the particle to be $m^2 = -p^2$. The Mandelstam variables s, t, u (see footnote 11) are defined as

$$s = -\left(p_1 + p_2\right)^2; \quad t = -\left(p_2 + p_3\right)^2; \quad u = -\left(p_1 + p_3\right)^2. \tag{7.1}$$

These obey the rule $s+t+u = \sum m_i^2$. In quantum field theory, the main contributions to the scattering amplitude of this system of particles come from tree diagrams like those depicted on figure 5. The reason this approach fails on dealing with particles of high spin is that diagrams with the exchange of high spin particles do not behave nicely in high energies (the successively more complicated diagrams in strong-interaction quantum field theories grow by a factor of 15 rather than diminishing by a factor of 137 as it happens with the weak interaction).

I invite the reader to now take a look at the t-channel diagram in figure 6. Call the external particles ϕ; call the exchanged particle σ. Note that, if σ possesses zero spin, the process depicted in figure 6 involves a simple $\phi^*\sigma\phi$ interaction; the resulting amplitude being $A(s,t) = -\frac{g^2}{t-M^2}$ (here g is the coupling constant and M the mass of the σ particle). This amplitude vanishes as t goes to infinity and we have nothing to worry about because we know how to calculate that sum then. If, however, the sigma particle is a spin J field $\sigma_{\mu 1,\mu 2,...,\mu J}$, the coupling of the interaction shown in figure 6 will rather look like $\phi^*(\partial\mu_1\partial\mu_2...\partial\mu_J\phi)\sigma^{\mu_1\mu_2\cdots\mu_J}$. Now, note, we have two factors of momenta $2J$. If we take the external particles to be scalars, the contribution to the scattering amplitude of the exchange in the t-channel of this spin J particle—at high energies, recall—looks like this:

$$A_J(s,t) = -\frac{g^2(-s)^J}{t-M^2}. \tag{7.2}$$

It is easy to see the behavior of this amplitude gets worse and worse as J increases. But we have, as yet, not considered an important fact. There is a large number of hadrons of several different masses and spin that might be exchanged in the t-channel. We must then rewrite equation (7.2) more generally, as

$$A_J(s,t) = -\sum_J \frac{g_J^2(-s)^J}{t-M_J^2}. \tag{7.3}$$

Here the couplings and masses of the particles are allowed to depend on J also. But given what was said about the nasty divergences that appear on high-energy collisions between hadrons, is there something to be made out of equation (7.3)? If (7.3) is a finite sum, the high-energy behavior is determined by the hadron of greatest J that contributes on (7.3). When Nature speaks, however, that is not what she says and the actual behavior of hadron scattering amplitudes is much softer than any individual term of (7.3) (in fact, Regge asymptotic behavior of the sort described by (7.2) fits well enough to the data of experiment. See (PICKERING, 1984) and (RICKLES, 2014b)). But it does not make sense to think (7.3) is a finite sum—we know of no limits concerning spin values for hadrons and there exists no entity that corresponds to the description 'hadron of highest spin'. If (7.3) is thought of as an infinite sum however, it is possible that it is well-behaved at high energies—indeed, better behaved than any of the individual term in the series (an analogy suffices to understand why this is so: recall that the function e^{-x} is smaller, as x approaches infinity, than any individual term in its power series expansion $\sum_{n=0}^{\infty} \frac{(-x)^n}{n!}$.

If we begin our analysis with the resonances exchanged in the s-channel instead, similar conclusions hold. To work out the amplitude in this case we just have to replace the s-channel poles by the t-channel poles:

$$\tilde{A}_J(s,t) = -\sum_J \frac{g_J^2(-t)^J}{s-M_J^2}. \tag{7.4}$$

Symmetry under cyclic permutation of the external momenta demands equal masses and couplings in (7.3) and (7.4). Again, we would reach the conclusion that a finite sum of the (7.4) kind has a nasty asymptotic behavior—indeed a very different one from that Nature shows to be the case by means of experiments. That is not, however, necessarily true if we take (7.4) to be an infinite sum (for the same reasons given for (7.3).

The reader may, at this point, be wondering: what has all this to do with gravity, with string theory, for that matter? I beg for the reader's patience, for this injection of

Whiggish history will make us make better sense later of some important features of string physics. Now, if one thinks further about equations (7.3) and (7.4) and their interpretation, one may start imagining if, given we choose the couplings g_J and the masses M_J wisely, the s-channel and the t-channel amplitudes $A(s,t)$ and $\tilde{A}(s,t)$ do not coincide. If they do, then the entire amplitude could be either written as a sum over only the s-channel poles (as in (7.4)) or over the t-channel poles (as in (7.3))—at striking odds with the ways of quantum field theory (GREEN; SCHWARZ; WITTEN, 1987a), where the so called 'interference models' demand that the two descriptions (s- and t- contributions) are added together like Feynman diagrams (yielding, of course, empirically inadequate results). But let us pause and digest what this identification implies. Assuming this reasoning is correct, different physical situations have the same asymptotic behavior—Regge pole exchange at high energy (t-channel) and resonance at low energy (s-channel), that is—and yield, as such, equivalent physical description!

This suggestion—the 'duality hypothesis', as it was called—was an important theoretical move within the confines of the bootstrap approach and was put forward by Dolen, Horn and Schmidt in 1967 (James Cushing has described the conjunction of S-matrix and duality as 'the ultimate bootstrap'. See (CUSHING, 1990b)) and experimental data showed the equality $A(s,t) = \tilde{A}(s,t)$ to indeed hold for small values of s and t. Could, however, further elaboration find resonance masses and couplings to follow exactly the equality $A(s,t) = \tilde{A}'(s,t)$—turning duality into an 'a priori link', so to speak, amongst s- and t-channels? Gabriele Veneziano showed it so in 1968—opening the gates for a whole new world of theoretical possibilities. He postulated a formula for the scattering amplitude,

$$A\left(s,t\right) = \frac{\Gamma\left(-\alpha\left(s\right)\right)\Gamma\left(-\alpha\left(t\right)\right)}{\Gamma\left(-\alpha\left(s\right)-\alpha\left(t\right)\right)}, \tag{7.5}$$

where Γ represents the Euler gamma function

$$\Gamma\left(u\right) = \int_0^\infty t^{(u-1)}e^{-t}dt \tag{7.6}$$

and $\alpha(s)$ is the 'Regge trajectory' (see footnote 12); for which Veneziano proposed the linear form $\alpha(s) = \alpha(0) + \alpha' s$ (corresponding to the Regge pole and the intercept, respectively)[16]. Equation (5) satisfies Regge asymptotics and also all the aforementioned principles laid down by the S-matrix (with the exception of unitarity—i.e., the preservation of probabilities summing up to one)[17].

In spite of its guesswork origin and blatantly *ad hoc* character, Veneziano's model

[16] It is not prima facie evident that the Veneziano amplitude (equation 7.5) satisfies duality, but a few steps suffice to show that it does. See (GREEN; SCHWARZ; WITTEN, 1987b). See also (KAIDALOV, 1972).

[17] See (VENEZIANO, 1968). It amounts to an (almost) complete solution of the bootstrap—the demands of S-matrix were satisfied and no quantum field theory was employed.

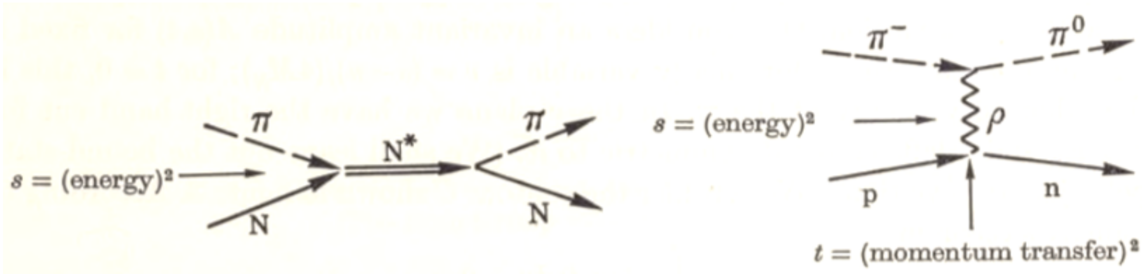

Figure 7 – Graphical representations of two descriptions of the hadronic scattering amplitude: In the left diagram, resonance is depicted (with π and N colliding to generate N^*, which decays after a short time back into π and N). On the right diagram there is Regge pole exchange (i.e. an interaction in which π^- and p exchange a ρ-meson, transforming quantum numbers to become π_0 and n). The two descriptions are different: resonance formation denotes poles in the s-channel; Regge exchange to poles in the t-channel. Duality says there is a straightforward relationship between these two descriptions. *Image source:* (SCHMID, 1970).

resolved many thorny difficulties in hadronic physics[18]. No other model had the explanatory power it did—it was, due to that fact, seriously studied and many took up the challenge of elucidating the involved physics (although no support came from experiments). A chain of surprises quickly emerged from the work hours of skilled physicists: generalizations of the Veneziano formula for N-point functions describing scalars were soon discovered (BARDAKÇI; RUEGG, 1968; VIRASORO, 1969; GOEBEL; SAKITA, 1969; BARDAKCI; RUEGG, 1969); soon after, it was proved these N-point functions satisfy the requirement of factorizability of all particle poles, an essential step to guarantee (the until then missing) unitarity (FUBINI; VENEZIANO, 1969; BARDAKÇI; MANDELSTAM, 1969); and an operator formalism in Fock space was developed (FUBINI et al., 1969; SUSSKIND, 1970). These culminated in the realization (BARDAKÇI; MANDELSTAM, 1969; FUBINI; VENEZIANO, 1969; NAMBU, 1970) the Veneziano formula admitted a factorization in terms of an infinite set of harmonic oscillators. That is to say, note, the spectrum of the resonances described by the Veneziano model correspond to the spectrum of an oscillating system. Like that of a string.

Just as a great and troublesome river deposits several strange objects in its shores, in a very similar fashion the great and troublesome river of dual models produced numerous precise but problematic, ill-understood results. A particularly pressing problem was the following: Veneziano's spectrum of resonances seemed to contain—as an inevitable consequence of the Lorentz covariance of the factorization procedure required by S-matrix's philosophy—both positive and negative norm states (i.e., the so-called 'ghosts'). (VIRASORO, 1970) however showed an infinite set of gauge symmetries were satisfied in the model if we force the intercept of the leading trajectory—$\alpha(0)$, that is—to equal

[18] For an excellent, detailed presentation of the dynamics of this theoretical development, I direct the reader to (RICKLES, 2014c).

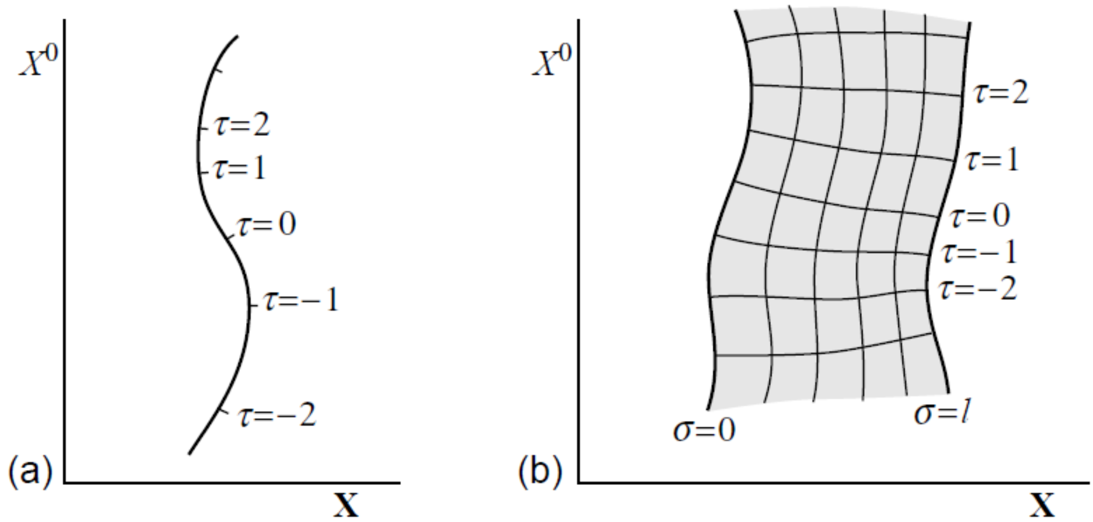

Figure 8 – (a) Parameterized world line of a point particle. (b) Parameterized world sheet of an open string. *Image source:* (POLCHINSKI, 1998a).

one[19]. It is possible to exorcise ghosts this way (one can then prove all negative-norm states are decoupled; see (BROWER, 1972; GODDARD; THORN, 1972)[20] but $\alpha(0) = 1$ implies an unwelcome side effect compared to the disease it was supposed to cure: it requires the ground state had $M^2 < 1$—a tachyon. This is clearly a step away from reality. Consistency checks also determine no negative norm vectors to hold—in addition to the aforementioned fixed intercept—in a spacetime dimension D of 26. (SHAPIRO, 1969) showed that if we force the intercept $\alpha(0)$ of the leading trajectory to be equal to 2 instead of 1 (again, $D \leq 26$), its spectrum was also ghost-free. That means a spin 2, massless particle—properties, it is thought, the graviton satisfies.

Let us see how that works out in practice. Strings come in two flavors—open (with endpoints) and closed (which from a topological view are circles). Take the first, and place yourself within the confines of a covariant Lagrangian formalism. The string moves in a D flat spacetime, with metric $\eta_{\mu\nu} = diag\,(-,+,+,...,+)$. In analogy with the relativistic action of a point particle, (NAMBU, 1970) suggested the relativistic action for a free string ought to be proportional to the area surface spanned in spacetime by the evolution of the string. For a 2-D surface embedded in spacetime ($x^\mu = x^\mu(\sigma,\tau)$), the area element looks like this:

$$d^2A = \sqrt{\left(\frac{\partial x^\mu}{\partial\sigma}\frac{\partial x_\mu}{\partial\tau}\right)^2 - \left(\frac{\partial x^\mu}{\partial\sigma}\right)^2\left(\frac{\partial x^\nu}{\partial\tau}\right)^2}\, d\sigma d\tau. \tag{7.7}$$

[19] This infinite set of conditions was isolated by Virasoro with an infinite set of operators that gives birth to what is known today as 'Virasoro algebra' (see (POLCHINSKI, 1998b)). This amounts to, at the end of a day, a complex Lie algebra. See (VIRASORO, 1970)

[20] These physicists have independently proven what is now known as the 'no-ghost theorem'.

It is possible to introduce a 'more symmetric' notation for the space of two parameters σ and τ (an important thing since parametrization is arbitrary and a distinct parametrization must be equivalent to it. Physical quantities must be independent of this choice). So, we make the following trade:

$$\left(\xi^0, \xi^1\right) = (\sigma, \tau) \,. \tag{7.8}$$

The metric-tensor of this two-dimensional space is given by

$$-g_{\alpha\beta}\left(\xi\right) = \partial_\alpha x^\mu \partial_\beta x_\mu; \tag{7.9}$$

the indices α and β take values 0 and 1. Checking, one sees that

$$d^2 A = \sqrt{-det\, g\left(\xi\right)} d\sigma d\tau; \tag{7.10}$$

as it should. The geometric significance of the surface element is reflected in its invariance under change of parametrization. If we set

$$\tilde{\xi}_i = \tilde{\xi}_i\left(\xi_0, \xi_1\right), \tag{7.11}$$

then

$$g_{\alpha\beta}\left(\xi\right) = \frac{\partial x^\mu}{\partial \xi_\alpha}\frac{\partial x_\mu}{\partial \xi_\beta} = \sum_{i,j} \frac{\partial x^\mu}{\partial \tilde{\xi}_i}\frac{\partial \tilde{\xi}_i}{\partial \xi_\alpha}\frac{\partial x_\mu}{\partial \tilde{\xi}_j}\frac{\partial \tilde{\xi}_j}{\partial \xi_\beta}. \tag{7.12}$$

This can be written as

$$g_{\alpha\beta}\left(\xi\right) = M_{\alpha i} g_{ij}\left(\tilde{\xi}\right) M_{j\beta}; \tag{7.13}$$

but since $M_{ab} = \partial\tilde{\xi}_a / \partial \xi_b$,

$$-det\, g = -det\, \tilde{g}\left(det\, M\right)^2 \tag{7.14}$$

and

$$\partial^2 \xi = \partial\xi_1 \partial\xi_2 = \frac{\partial\tilde{\xi}_1 \partial\tilde{\xi}_2}{|det\, M|}, \tag{7.15}$$

this tells us the infinitesimal $d^2 A$ is invariant. This guarantees physical quantities—such as the action—*depend only on the embedding on spacetime and not on the parametrization.* Thus the following action is postulated:

$$S = -\frac{1}{2\pi\alpha'}\int_{\tau_1}^{\tau_2} d\tau \int_0^\pi d\sigma \sqrt{det\, g} \tag{7.16}$$

$$S = -\frac{1}{2\pi\alpha'}\int_{\tau_1}^{\tau_2} d\tau \int_0^\pi d\sigma \sqrt{\left(\dot{x}\cdot x'\right) - x'^2 \dot{x}^2}; \tag{7.17}$$

where

$$\dot{x}^\mu = \frac{\partial x^\mu\left(\sigma, \tau\right)}{\partial \tau} \quad \text{and} \quad x'^\mu = \frac{\partial x^\mu\left(\sigma, \tau\right)}{\partial \sigma}. \tag{7.18}$$

τ and σ are thus dimensionless parameters and the initial and final positions of the string are given by $x^\mu(\sigma, \tau_1)$, $x^\mu(\sigma, \tau_2)$ and $0 \leq \sigma \leq \pi$. The quantity α', we have met it before:

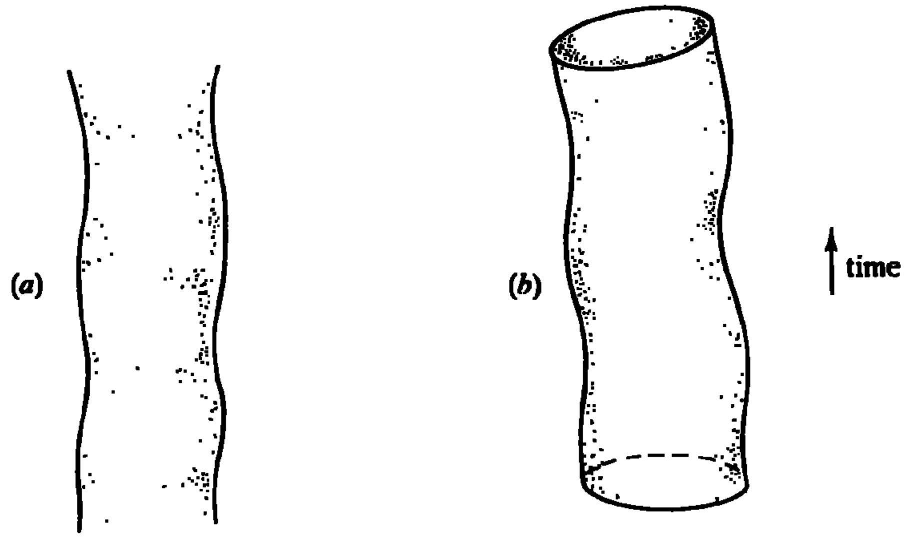

Figure 9 – An open string (a) and a closed string (b), propagating in Minkowski spacetime, sweeps out a 2-D surface known as the *world sheet* of the string (see figure 8). The classical equations of a free string demand this should be a 'minimal surface', or a surface of least area (hence the described actions).*Image source:* (GREEN; SCHWARZ; WITTEN, 1987a).

it measures the slope of Regge trajectories. This holds for a free, massless, open string. When the string is closed, however, the difference is that we integrate from $[0, 2\pi]$. This is actually the string we are interested in, for reasons which shall soon be clear. So let us move forward, now with the closed string. The boundary conditions which express that the string is closed are

$$x_\mu (\tau, \sigma + 2\pi) = x_\mu (\tau, \sigma), \tag{7.19}$$

of course. The action then is

$$S = -\frac{1}{4\pi\alpha'} \int_{\tau_1}^{\tau_2} d\tau \int_0^{2\pi} d\sigma \sqrt{(\dot{x} \cdot x') - x'^2 \dot{x}^2}. \tag{7.20}$$

Now, (7.20) can be simplified (that square root is hard to be dealt with!) by introducing an independent world-sheet metric, $h_{\alpha\beta}(\tau, \sigma)$. Take it to have a Lorentzian signature, $(-, +)$. The more convenient form of the action looks like

$$S = -\frac{1}{4\pi\alpha'} \int_M d\tau d\sigma \, (-h)^{\frac{1}{2}} \, h^{\alpha\beta} \partial_\alpha X^\mu \partial_\beta X_\mu; \tag{7.21}$$

where $h = deth_{\alpha\beta}$ and $h^{\alpha\beta}$ is the inverse of $h_{\alpha\beta}$. The equation of motion that we extract from (7.2) is the simple, linear, wave equation

$$\left(\frac{\partial^2}{\partial \tau^2} - \frac{\partial^2}{\partial \sigma^2} \right) X^\mu = 0. \tag{7.22}$$

The constraint equation[21] that arise from (7.22) is the field equation $\delta S/\delta h_{\alpha\beta}$. Invoking quantum field theory's tools, we have that in a 2-D quantum field theory the energy-momentum tensor is defined in the following way:

$$T_{\alpha\beta} = -\frac{2\pi}{\sqrt{h}}\frac{\delta s}{\delta h^{\alpha\beta}}. \tag{7.23}$$

We can then say the constraints equations are simply $T_{\alpha\beta} = 0$. In its simplest form—which appear in light-cone world-sheet coordinates, $\sigma^{\pm} = (\tau \pm \sigma)$—, the energy momentum tensor has the following properties: $T_{++} = \partial_+ X^\mu \partial_+ X_\mu$; $T_{--} = \partial_- X^\mu \partial_- X_\mu$; $T_{+-} = 0$. This last one is the trace of the energy-momentum tensor and the fact that it is zero (which in technical language means it is *conformally invariant*) brings many surprising consequences[22]. The energy-momentum tensor possesses, when expanded, the following Fourier modes (evaluated at $\tau = 0$):

$$L_m = \frac{T}{2}\int_0^\pi e^{-2im\sigma}T_{--}d\sigma = \frac{T}{2}\int_0^\pi e^{-2im\sigma}\dot{x}_R^2 d\sigma = \frac{1}{2}\sum_{-\infty}^{+\infty}\alpha_{m-n}\cdot\alpha_n \tag{7.24}$$

$$L_m = \frac{T}{2}\int_0^\pi e^{2im\sigma}T_{++}d\sigma = \frac{T}{2}\int_0^\pi e^{2im\sigma}\dot{x}_L^2 d\sigma = \frac{1}{2}\sum_{-\infty}^{+\infty}\tilde{\alpha}_{m-n}\cdot\tilde{\alpha}_n; \tag{7.25}$$

in which the indices R and L refer to the left- and right-moving modes (boundary conditions (7.19) yield a general solution in which Fourier modes α_n^μ appear. These are interpreted as oscillator coordinates. More specifically, $\alpha_0^\mu = \tilde{\alpha}_0^\mu = (p^\mu/2)(2\alpha')$—in which p stands for the momentum and α' the Regge slope when said out loud[23]). The constraint equations that ask us to make T_{++} and T_{--} vanish amount, then, to the vanishing of their Fourier components [24]. Thus our gauge conditions read

$$L_m = \tilde{L}_m = 0. \tag{7.26}$$

When this theory is covariantly quantized—we've been at the classical realm so far!—one has two sets of mutually commuting annihilation and creation operators: α_m^μ, $\tilde{\alpha}_m^\mu$. Now the gauge conditions are

$$L_m \left|\phi\right\rangle = \widetilde{L}_m \left|\phi\right\rangle = 0 \quad \text{for}\, m \geq 1; \tag{7.27}$$

[21] As usual in 2-D, the general solution to the massless wave equation can be written as a sum of two arbitrary functions: $X^\mu(\sigma) = X_R^\mu(\sigma^-) + X_L^\mu(\sigma^+)$. Here $\sigma^- = (\tau - \sigma)$; $\sigma^+ = (\tau + \sigma$; X_R^μ describes the 'right moving modes' of the string and X_L^μ describes the 'left moving modes'.

[22] The constraint equations imply that a physical state, say, $\left|\phi\right\rangle$, ought to obey $T_{\alpha\beta}\left|\phi\right\rangle = 0$. When one computes the commutation relations of $T_{\alpha\beta}$, one finds that

$$[T_{++}(\sigma), T_{++}(\sigma')] = i\left[T_{++}(\sigma) + T_{++}(\sigma')\right]\delta'(\sigma - \sigma') + \tfrac{i}{24}(26 - D)\delta'''(\sigma - \sigma');$$

which holds similarly for T_{--}. D, here, is the number of dimensions of spacetime (i.e. the number of X^μ). Note that for the physical state $\left|\phi\right\rangle$ that respects $T_{++}\left|\phi\right\rangle = 0$, the first term of the right-hand side of the commutation relation equation disappears instantly. The second term however does not vanish *unless the number D of dimensions is 26*—this is the only case in which there exists physical states! It is for *this reason* that the bosonic string model only makes sense in a rich number of dimensions.

[23] For a closed classical string, $J \leq (\alpha'/2)M^2$.

[24] L_m and $\tilde{L}_m$ are called 'Virasoro operators'.

$$\left(L_0 - \tilde{L}_0\right)|\phi\rangle = 0. \tag{7.28}$$

The mass-shell condition reads

$$\left[L_0 + \tilde{L}_0 - \alpha(0)\right]|\phi\rangle = 0. \tag{7.29}$$

(7.29) is obeyed if, and only if, $D = 26$ and $\alpha(0) = 2$. Hence the theory contains a massless, spin 2 particle—a 'graviton'. Carrier, it is held, of the gravitational force[25].

3

Before the results extracted from equation (7.29) are discussed, let us press string theory harder and check whether it can yield more fruit of gravitational flavor (I anticipate that gravity does show up elsewhere—although under new disguise). Now, one of the main reasons for the excitement that triggered much of frenzied work in string theory is the fact that *this theory is free of the ultraviolet divergences that plague quantum field theory*. In figure 10 one can find a one-loop diagram sketched (which does diverge in the ultraviolet in an orthodox, quantum-field-theory-based quantum gravity theory) and also the corresponding closed-string diagram. What differs the string diagram of 10b from the field theory diagram of 10a is the following: every world line (or propagator) of a point particle was replaced by the world tube of a propagating closed string. These diagrams are evaluated by integrating over the trajectories in the space-time of the propagating strings or points. But why, the reader must be thinking, are there infinities popping up in figure 10a and not in 10b? Well, the crux of the matter is that there are very well defined interaction vertices in 10a (these are denoted by letters p, q, r, s). Ultraviolet divergences happen because when $p = q = r = s$ the propagators connecting the vertices go to infinity all at once—blowing up everything while at it. In the string diagram shown in 10b, however, *there is no well-defined analog of such interaction vertices p, q, r, s of the field theory counterpart*. Follows that there is also no dangerous region $p = q = r = s$. This does not prove finiteness for string theory, of course—but a nice intuition of why it is so is no longer lacking, I hope. We will not, however, elude ourselves from proving finiteness—and the process shall reward our efforts by forcing mathematics to yield just that bit of gravitational physics we crave for.

Now we roll up our sleeves and force math to speak with us once again. The physics we have dealt with in section 2 took place in a 26-dimensional Minkowski space, as the reader recalls. When all parametrizations, gauge choices and quantization are done we are left with the following action:

[25] The reader may complain we are within the confines of an 'unrealistic', interaction-free, bosonic string theory. The result also holds, of course, for interacting string theories that include fermions. I have chosen this path because it is the simplest, most straightforward manner to show the desired result.

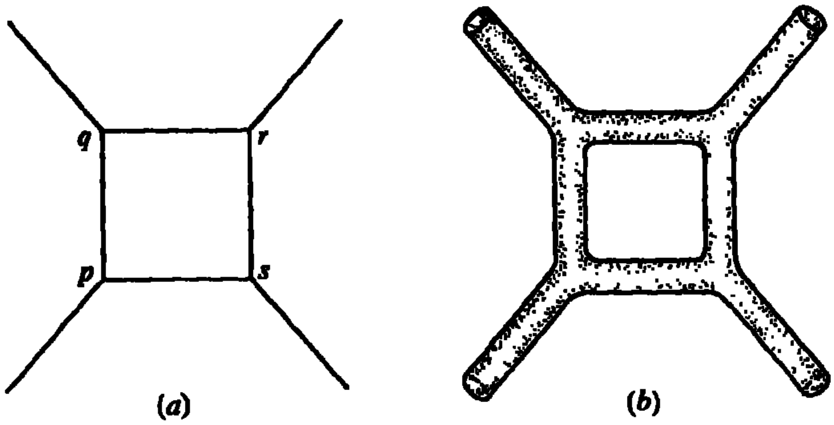

Figure 10 – In (a) a Feynman diagram is depicted; with interaction points markes p, q, r, s. In (b), the corresponding string diagram for closed strings is shown. *Image source:* (GREEN; SCHWARZ; WITTEN, 1987a).

$$S = -\frac{1}{2\pi}\int d^2\sigma\,(-h)^{\frac{1}{2}}\,h^{\alpha\beta}\partial_\alpha X^\mu \partial_\beta X^\nu \eta_{\mu\nu}. \tag{7.30}$$

Here $h_{\alpha\beta}$ is the familiar world-sheet metric for the string and $\eta_{\mu\nu}$ is Minkowski space-time. It would be nice to generalize this thing, though. We want the string to move in *any manifold*; not only Minkowski's. We thus replace the Minkowski metric $\eta_{\mu\nu}$ with the metric tensor $g_{\mu\nu}$:

$$S = -\frac{1}{2\pi}\int d^2\sigma\,(-h)^{\frac{1}{2}}\,h^{\alpha\beta}\partial_\alpha X^\mu \partial_\beta X^\nu g_{\mu\nu}\left(X^\rho\right). \tag{7.31}$$

String theorists take (7.31) to be a natural generalization of (7.30): $g_{\mu\nu}(X^\rho)$ is simply $\eta_{\mu\nu} + f_{\mu\nu}(X^\rho)$—the last term representing a perturbation. The idea is that we have just taken our initial Minkowski metric $\eta_{\mu\nu}$ and curved it by 'shaking it up' a little. Our next task is to study the properties of the action described in equation (7.31).

Both actions (7.30) and (7.31) are two-dimensional quantum field theories. An essential difference, however, distinguishes them: although the action depicted in (7.30) becomes a non-interacting field theory in the conformal gauge $h_{\alpha\beta} = \eta_{\mu\nu}$, (7.31) does not. If we impose such a conformal gauge in the action (7.31), this is what we are left with:

$$S = -\frac{1}{4\pi\alpha'}\int d^2\sigma\,\partial_\alpha X^\mu \partial^\alpha X^\nu g_{\mu\nu}\left(X^\rho\right). \tag{7.32}$$

Now the theory depends only of gauge-invariant objects built out of the metric and other fields. The fields X^μ can be thought of, if one wishes, to be coordinates on a manifold. This is called 'target space': the jargon applies because the X^μ define an embedding

$$\text{world-sheet} \to \text{target.}$$

In the string theory case, *the target is space-time itself.* A field theory which one finds in equation (7.32)—meaning, a theory in which the kinetic term is field-dependent and so the field space is effectively a curved manifold—such a theory is named, for historical reasons, a *non-linear sigma model.* Note the dependence on the Regge slope α' is back.

Now here's the kicker. An important property a consistent string theory ought to possess is scale invariance—which in our context means that if we stretch our string (imagine yourself picking up a 10 cm string and stretching it up to 50 cm) nothing happens to its properties (all we must do is, perhaps, multiply the action by a global factor). Another way of saying it is that we do not wish our physical observables to depend upon choice of coordinates. Our Minkowski-framed string action was scale invariant all right but in the more-general, $g_{\mu\nu}$ metric case, scale invariance may break down—and there is no way to preserve it without destroying world-sheet scale or conformal invariance. In quantum field theory jargon, the 'scale invariance breaking problem' is mathematically described in terms of a function called 'the β function'. In QFT, a non-zero beta function appears from ultraviolet divergences. We do not need to go into the details of QFT renormalization group methods here (which is the context in which you see the nice things the β function does for us) because they are technical, tiresome and not really at stake. What I want the reader to have in mind is simply that, if we manage to set the beta function to zero, scale invariance is guaranteed. We are not, however, as we are in quantum field theory, really worried about divergences (for the reasons with which I started this section). What really interests us is whether or not the action depicted in (7.32) is *Weyl invariant.* Weyl invariance requires of our theory more than mere symmetry under change of coordinates—it requires our physical quantities to be invariant under change of *metric* (isn't that what we have done as we moved from equations (7.30) to (7.31)?). Luckily for us though, finiteness and Weyl invariance are deeply connected to one another. *Weyl invariance implies global scale invariance; which in turn implies vanishing the β function; which implies ultraviolet finiteness.* A rich seam of physics is revealed by the exercise of imposing Weyl invariance to (7.32), so let us now see how this works out in practice.

Step one: impose the gauge

$$h_{\alpha\beta} = e^{2\phi}\eta_{\alpha\beta}. \tag{7.33}$$

Step two: regularize[26] equation (7.32). In this case, the standard procedure is to regularize the dimension of our parameters: instead of making it 2, let us make it 2 and a

[26] To cure a divergence we can introduce a finite cut-off in momentum space (call it 'Λ') in such a way that we integrate up to Λ rather than infinity. This strategy is called *renormalization*, meaning we simply forget to mention Λ from then on. This involves expressing amplitudes in terms of physical coupling constants, which are the ones we measure at some agreed point in momentum space. Note that this forces us to define coupling constants with respect to this point in momentum space. Now, physicist Kenneth Wilson had another strategy: rather than hiding the cut-off, he says, 'let's live with it'. In Wilson's view, to properly define the theory, we need to admit that we do our integrals up to a maximum momentum Λ, which we are entirely free to choose. This is regularization. The question is, then, of course, how to choose one value for Λ over another? How to justify such a choice? Physicists actually get away with this technique by choosing a length scale Λ^{-1} far smaller than that of the physics in question. Suppose you are interested in sound waves. Such waves are oscillations in the

bit more—say, $2+\epsilon$. Substituting equation (7.33) in equation (7.31) and regularizing it[27], we have the action

$$S = -\frac{1}{2\pi}\int d^{2+\varepsilon}\sigma\, e^{\varepsilon\phi}\partial_\alpha X^\mu \partial^\alpha X^\nu g_{\mu\nu}\left(X^\rho\right). \tag{7.34}$$

The condition that makes the theory free from divergences is connected to making the dependence on ϕ the action now has go away. If we treat equation (7.32) as a quantum field theory in which the quantum field is $X^\mu(\tau,\sigma)$, the next move is to get ourselves a vacuum expectation value, say, X_0^μ, and expand the quantum field around it in the following manner:

$$X^\mu\left(\tau,\sigma\right) = X_0^\mu + x^\mu(\tau,\sigma). \tag{7.35}$$

The last term (left to right) is the quantum fluctuation. The expansion of X^μ around X_0^μ is a thorny one, advise us string theory textbooks—a piece of work that can be avoided if one acknowledges equation (7.32) is invariant under redefinition of field variables

$$X^\mu \to \tilde{X}^\mu\left(X^\rho\right). \tag{7.36}$$

The redefinition is handy because it empowers you to assume that, on the more general space-time manifold you are at now, the coordinates X^μ are locally inertial coordinates at the point X_0^μ. When we do the expansion, our metric $g_{\mu\nu}(X^\rho)$ differs from the 'nothing ever happens' flat Minkowski one only in the order $(x^\mu)^2$. We cannot set the other terms to zero when we redefine the field variables—but we can simplify them if we are smart and choose, from the set of all possible coordinates, those known as *Riemann normal coordinates*. So, let's be smart and do that. In these coordinates, one can expand $g_{\mu\nu}$ as

$$g_{\mu\nu}\left(X^\rho\right) = \eta_{\mu\nu} - \frac{1}{3}R_{\mu\lambda\nu\kappa}\left(X_0^\rho\right)x^\lambda x^\kappa - \frac{1}{6}R_{\mu\lambda\nu\kappa}\left(X_0^\rho\right)x^\rho x^\lambda x^\kappa + O\left(\left(x^\mu\right)^4\right); \tag{7.37}$$

where $R_{\mu\lambda\nu\kappa}$ is understood as the Riemann tensor of the manifold at the point X_0^μ. The expansion of the ϕ-dependent exponential $e^{\epsilon\phi}$ is the usual thing (i.e., equal to $1+\epsilon\phi+...$). We now plug both the expansion of the metric and of $e^{\epsilon\phi}$ into the action (7.34):

$$\tilde{S} = -\frac{1}{2\pi}\int d^{2+\varepsilon}\sigma\,\left[\partial_\alpha X^\mu\partial^\alpha X^\nu\left(1+\varepsilon\phi\right)\eta_{\mu\nu} - \frac{1}{3}R_{\mu\lambda\nu\kappa}\left(X_0^\rho\right)x^\lambda x^\kappa\partial_\alpha X^\mu\partial^\alpha X^\nu\left(1+\varepsilon\phi\right) + O\left(x^5\right)\right]. \tag{7.38}$$

Now, recall we have regularized action (7.34) by adding a factor of ϵ to the 2 dimensions we had at first. In dimensional regularization, it is expected that poles[28] arise

density of the constituents of the gas involving mols of atoms ($\sim 10^{23}$). Applying regularization, the scale in which you are interested, in p^{-1}, is of the order of centimeters and then you can take Λ^{-1} to be a few microns.

[27] I cannot help to mention how astonished I was the first time I encountered this procedure: what is that, $2+\epsilon$ dimensions? Needlessly to say, it boggles my mind still.

[28] See footnote 14 of this chapter.

solely from logarithmically divergent integrands—something that happens in equation (7.38) when $x^\lambda x^\kappa$ are contracted:

$$\left\langle x^\lambda(\sigma) x^\kappa(\sigma')\right\rangle_{\sigma\to\sigma'} = \pi\eta^{\lambda\kappa}\lim_{\sigma\to\sigma'}\int\frac{d^{2+\varepsilon}k}{(2\pi)^{2+\varepsilon}}\frac{e^{ik(\sigma-\sigma')}}{k^2}\approx\frac{\eta^{\lambda\kappa}}{2\varepsilon}. \tag{7.39}$$

Equation (7.39) shows there are ε poles in the action derived from the theory defined by (7.38). These poles both (i) yield non-zero beta functions, and (ii) lead to ϕ-dependences that do not vanish in the limit $\varepsilon\to 0$. That is precisely what we *do not* want. What, then, can we do to cure this disease? We renormalize the ϕ-independent ε pole terms that contribute to action (7.38). We are lucky to have on our hands a non-linear sigma model though; known for possessing the property of being able to absorb the infinities these poles yield in a wave function renormalization that looks like

$$x^\mu\to x^\mu+\frac{1}{6\varepsilon}R^\mu_\nu\left(X^\rho_0\right)x^\nu+O\left(x^2\right) \tag{7.40}$$

and a renormalization of the space-time metric that yields

$$g_{\mu\nu}\to g_{\mu\nu}-\frac{1}{2\varepsilon}R_{\mu\nu}\left(X^\rho_0\right). \tag{7.41}$$

When one substitutes the renormalized terms (7.40) and (7.41) into the action (7.38), one finds that the terms with coefficients $\varepsilon\phi$ that had been cancelled by the poles are back in business. Adding up all ϕ-dependent contributions yield the following effective action ($D=26$ is assumed):

$$\tilde{S}=-\frac{1}{4\pi}\int d^2\sigma\phi R_{\mu\nu}\left(X^\rho\right)\partial_\alpha X^\mu\partial^\alpha X^\nu. \tag{7.42}$$

Here X^μ is given by equation (7.35). Action (7.32) therefore leads to a ϕ-independent, Weyl invariant quantum theory if, *and only if*, the Ricci tensor is set to zero:

$$R_{\mu\nu}\left(X^\rho\right)=0. \tag{7.43}$$

It is equations (7.43) that are supposed to inspire us awe: they are the Einstein equations for a massless universe.

4

A lot has happened in the preceding pages, so let me recapitulate. Dawid tells us we must believe in string theory because it depicts 'surprising explanatory interconnections'—a characteristic Dawid wishes to elevate to the status of an epistemic principle. What backs up his faith is, as we have seen, the astonishing fact that string theory 'requires gravity'. Our beliefs are cherished and carefully chosen so we have decided to check ourselves what sort of theoretical facts motivate Dawid's assertions. We have gone through the mathematics and we have reviewed the inevitable amount of Whiggish history that joins hands with it; we have, guided by historical record, seen that string theory sprung into the world as a dual resonance theory of hadrons, subjected to the strong-forced chains of the S-matrix; we have solved the equations of motion for a closed string and we have applied boundary conditions and gauge choices—an exercise that yielded us a spin 2, zero mass particle. We have moved forward by asking of string theory's equations of motion to move from a flat 2-D Minkowski metric to a curved, generalized one. We have seen that imposing Weyl invariance (or vanishing the β-function) was needed for a consistent generalization to hold—consistency only achieved if the Ricci tensor is set to zero (or, in stringy jargon, if Einstein's vacuum equations are satisfied). Joining facts together, string theorists argue the dynamics of string theory determine coherent states of the graviton to satisfy Einstein's field equations.

First, some comments on the graviton. It might not have escaped the reader I have left unspoken an important mismatch between historical record and the 'quantum gravity results' we have worked out in section 2 of this chapter. Clarification: dual string models have burst into existence in the *hadronic world*. String theory was born, grown and milked to describe strongly interacting systems—but there are no massless spin-2 hadrons. Hadrons are heavy beasts; and yet there are massless particles of many kinds in the spectrum of dual models. At its inception string theory not only predicted the wrong particles as it also required (for consistency reasons, see footnote 19) a number of dimensions that is immediately falsified by experience and for which there is no conceivable physical justification. It is no wonder that, when quantum chromodynamics emerged, string theory was left behind by hadronic physicists[29]. Love, however, is not love which alters when alteration finds and stringy practitioners enchanted by its framework did not give up working on it[30]. And not in vain, as a matter of fact: vice was turned into a virtue by Scherk and Schwartz who, in 1974, authored a paper entitled 'Dual Models for Non-Hadrons'. This paper is one mark of what Dean Rickles calls 'theoretical exaptation in string theory' (in direct reference to evolutionary biology; see (RICKLES, 2014a)) and its opening lines are worth quoting:

> A dual model may eventually be formulated that described the hadronic world. We do not wish to review the basis for this belief, but simply to mention some of the problems that must be overcome if this goal is to be achieved: (i) The existing model seem most natural in an unphysical dimension of spacetime (ii) These models contain massless particles and tachyons. It is widely believed that spontaneous symmetry breaking mechanisms should exist for dual models, similar to those of field theories, and that these could

[29] Although it is not clear-cut that the string dual model would not have yielded a good model of hadrons. I direct the reader to (CUSHING, 1990a) for details of this discussion.

[30] The story of string theory's rise, fall, and phoenix-like comeback is brilliantly described in (RICKLES, 2014a).

> eliminate the massless particles and tachyons [. . .]. If one supposes that these obstacles can be overcome leading to a satisfactory theory of hadrons, it is tempting to speculate as to whether a dual scheme might also be appropriate for the nonhadronic world (including leptons, photons, gravitons, and other gauge fields). Obviously, there is no empirical evidence of duality or Regge behavior in nonhadronic interactions. However, [. . .] viewing existing dual models as candidates for nonhadronic schemes, one notices that at least one defect, namely the presence of massless particles, becomes a virtue (SCHERK; SCHWARZ, 1974).

A new path was then forged for dual models: from hadron phenomenology to a 'theory of everything'. A shift in function—a really extraordinary episode in the history of science indeed—took place: previously insurmountable problems were to be later re-interpreted as some of the theory's most appealing features[31]. It was acknowledged that dual models reduced to field theories in a specific limit—namely, when α' is set to zero (the so-called 'Scherk limit'). Structural shifting methods involved modifying the Mandelstam-Regge trajectory slope from $\approx 1/GeV^2$ to $10^{-38}/GeV^2$.In terms of length scale, strings were lowered 20 orders of magnitude: from 10^{-13} (hadron scale) to 10^{-33} (Planck scale)[32]. Dual models morphed, by such means, into the string theory we know today—the previously 'embarrassing massless states' were given a new-found realistic meaning, and the stage was set for unpacking the mysteries hidden by the new temptress of physics: the theory of strings[33].

31 The tachyon problem was ironed soon after, with space-time supersymmetry offering nice manners to control the issue. Although dimensions were lowered from 26 to 10 after fermions were added to the theory, 10 was certainly closer to experience—but not close enough. The difficulty did not make faiths shake, though. Leading string theorist John H. Schwartz, for instance—who played a main role in turning string theory to the research field it today is—, in his paper (SCHWARZ, 1982), says the following: 'Superstring theories [. . .] require that spacetime has 10 dimensions [. . .]. The spatial dimensions have been treated as flat and unbounded [. . .] If we hope to make physical sense of these theories, it is clearly necessary to make a different choice. One possibility is to reject them as unphysical and seek new ones in four dimensions. Systematic searches for new string models have been carried out, and certain uniqueness theorems have been proved. While it is always difficult to know for sure that some unnecessarily restrictive assumption has not slipped into the analysis, these studies suggest that the string theories with critical dimensions 2, 10 and 26 are the only simple possibilities [. . .] any attempt to define the superstring theories for $D < 10$ is likely to lead to serious problems. *Even if this were possible, I would still argue that the $D = 10$ choice is so much more elegant and beautiful that it may be more fruitful to try to interpret the extra dimensions physically rather than reject them summarily in favor of less attractive alternatives.*' (italics mine). The quoted passage in italics is one way of saying that, if the world does not match the description yielded by string theory, then so much worse for the world. Here Earman & Mosterin's critical view towards inflationary cosmology applies also: 'The trouble with ideas that are 'too good to be wrong'', they write, 'is that they tend to engender an almost religious faith in their advocates'. (EARMAN; MOSTERÍN, 1999).

32 As one approaches the Scherk limit, non-zero mass particles go to infinity and are discarded. Only massless states survive at that point; these corresponding to known field theories.

33 I call the reader's attention to the fact that there is no 'quantum gravity problem' strings were advanced to solve. String models were refashioned from 'dual models of hadrons' to 'dual models of everything'—a clear case of survival prompting exaptation.

At this point, the reader has every right to ask: what entitles physicists to think the existence of a massless, spin 2 particle implies the attraction of gravity? Have not Einstein's hard-won geometrical insights shown us it is through the highways and few byways of General Relativity's geometrodyamics that we truly understand what Nature intends to say when she, by making matter warp geometry, speaks 'gravitation'? It need not, in fact, be necessarily so. Most particle physicists have no training in General Relativity and, used to the idea of fields, fermions and bosons, prefer to think the whole of Nature follows the pattern familiar to them. Famous particle physicist Richard Feynman, for instance, opens his *Lectures on Gravitation* by saying Einstein's way is 'unnecessary'[34] (FEYNMAN, 1995a) and a 'more suited approach' (FEYNMAN, 1995b) is to take the universe to be made up

> of twenty-nine or thirty-one [. . .] fields all in one grand equation; the phenomena of gravitation add another such field to the pot, it is a new field which was left out of previous considerations, and it is only one of the thirty or so; explaining gravitation therefore amounts to explaining three percent of the total number of known fields. (FEYNMAN, 1995b)

It is indeed possible to approach gravity by adopting either a 'geometric viewpoint' or a 'field viewpoint'. If one endorses the latter, the natural choice is to begin by considering the gravitational field to be a scalar (as in Newton's system), or a vector (by making an analogy with electromagnetism). These choices soon lead to inconsistencies (if you take gravity to be a scalar, for instance, you are left with no light bending. If you take it to be a vector, you have negative-energy waves and no light bending). A symmetric rank-2 tensor potential $h_{\mu\nu}$ (more indices, note) does the trick. This object is required to be a 'Lorentz covariant, massless, spin 2 field'[35]. It possesses a boson attached to it—the 'graviton'[36]. (FIERZ; PAULI, 1939) were the first to write down a Lagrangian that satisfied $h_{\mu\nu}$ requests and unpack the details; the resulting theory is now found in the literature under the name *'linearized theory of gravity'*. If general relativity is taken as *the* theory of gravity, flat-spacetime linearized gravity can be studied as an approximation of it: it works in the weak-field limit of general relativity.

[34] Feynman gave his lectures on gravitation at Caltech in 1962-1963. He was going through his 'quantum gravity phase', as he called it.

[35] The techniques of general relativity necessary to translate this object into a set of field equations is found in any quantum field theory textbook.

[36] Credit is historically given to Soviet physicists Dmitrii Blokhintsev and F. Gal'perin for coining the term in their 1934 paper 'Neutrino hypothesis and conservation of energy' (original version in Russian). Although at this point the term is already familiar to the reader, it is interesting to understand why gravitons are thought to display these characteristics. I direct the curious reader to take a look at the first two chapters of (FEYNMAN, 1995c): guessing the properties of gravitons is the starting point of Feynman's speculations (an approach to gravitation that stands indeed in stark contrast with Einstein's sophisticated *'ubi materia, ibi geometria'* distinctive style.)

In weak-field situations, the metric tensor can be given by

$$g_{\mu\nu} = \eta_{\mu\nu} + h_{\mu\nu}, \tag{7.44}$$

where $\eta_{\mu\nu}$ is Minkowski spacetime and $h_{\mu\nu}$, a perturbation. The requirement is that $|h_{\mu\nu}| << 1$ (perturbations must be really small, so the overall field is almost untouched by gravity and the baseline metric is flat— Minkowiski's); the field equations are expanded in terms of $h_{\mu\nu}$ in a coordinate frame in which (7.44) is obeyed. Note this is precisely what we have done when we morphed action (7.30) into action (7.31) in section 3, above—recall we have called this procedure 'generalization of the metric'—and, in fact, linearized gravity is what one obtains in string theory. Allow me to emphasize one important fact: *there is no sense in which general relativity is quantized in string theory. Rather, a classical theory of spinning strings is quantized that possesses low-energy limits—when α' is set to zero, that is—in which a massless spin-2 particle emerges.* These are taken to be the correct properties the graviton must display in linearized gravity theory, as we have seen. Linearized gravity, however, is no general relativity—just as classical mechanics is no quantum mechanics—but a theory on its own right. And, as a matter of fact, a self-inconsistent one: it requires energy-momentum to be locally conserved; which in physical terms means no body could be accelerated by gravity (for derivation and discussion of linearized gravity's inconsistency, check box 7.1 of (MISNER; THORNE; WHEELER, 1973b)). It predicts, for instance, a mass placed on Earth's gravitational field, if started at rest, does not fall. Our planet, linearized gravity says, is not attracted by the Sun: it flies off interstellar space.

Just as quantum mechanics is empirically reduced to classical mechanics when the limit $h \to 0$ is taken (h denotes Planck's constant) and special relativity shares ground with Newton's mechanics at low velocities, one can 'descend' from general relativity to linearized theory by linearizing about flat spacetime and, conversely, can 'bootstrap one's way up' from linearized gravity to general relativity by imposing consistency between the equations of motion and the linearized field equations. This is, in fact, how the aforementioned difficulties that plague linearized gravity are exorcised: you must shift frameworks and translate yourself to full-blooded, non-linear general relativity (I direct the reader to (THIRRING, 1961) for details). There exists a common empirical ground between linearized gravity and Einstein's general relativity, yes; but that these are different theories must be held constant in the reader's mind. A comparison between them can be found in figure 11.

Now, with a firmer grasp on what has string theory to offer us, let us look at Dawid's UEA once again. We have reached mathematical climax at the end of section 3 when, setting the Ricci tensor to zero for Weyl invariance to obtain, Einstein's equations for the vacuum emerged—a result that adds up well with our 'spin-2 linearized gravity limits of application' discussion. It is indeed a theoretical fact that, in Riemann normal

DERIVATIONS OF GENERAL RELATIVITY FROM GEOMETRIC VIEWPOINT AND FROM SPIN-TWO VIEWPOINT, COMPARED AND CONTRASTED

	Einstein derivation	*Spin-2 derivation*
Nature of primordial spacetime geometry?	Not primordial; geometry is a dynamic participant in physics	"God-given" flat Lorentz spacetime manifold
Topology (multiple connectedness) of spacetime?	Laws of physics are local; they do not specify the topology	Simply connected Euclidean topology
Vision of physics?	Dynamic geometry is the "master field" of physics	This field, that field, and the other field all execute their dynamics in a flat-spacetime manifold
Starting points for this derivation of general relativity	1. Equivalence principle (world lines of photons and test particles are geodesics of the spacetime geometry) 2. That tensorial conserved quantity which is derived from the curvature (Cartan's moment of rotation) is to be identified with the tensor of stress-momentum-energy (see Chapter 15).	1. Begin with field of spin two and zero rest mass in flat spacetime. 2. Stress-energy tensor built from this field serves as a source for this field.
Resulting equations	Einstein's field equations	Einstein's field equations
Resulting assessment of the spacetime geometry from which derivation started	Fundamental dynamic participant in physics	None. Resulting theory eradicates original flat geometry from all equations, showing it to be unobservable
View about the greatest single crisis of physics to emerge from these equations: complete gravitational collapse	Central to understanding the nature of matter and the universe	Unimportant or at most peripheral

Figure 11 – Comparison between spin-2 field approach to gravitation and Einstein's geometrodynamics. The former reproduces the results of the latter in the vacuum and/or in very weakly interacting systems (such as light bending). The equations of linearized theory are written as if space were Minkowski flat; the connection to experiment is however made through the curved-space formalism of general relativity. *Image source:* (MISNER; THORNE; WHEELER, 1973a).

coordinates—the coordinates chosen in order to make Einstein equations come out in section 3, recall—with Ricci zero tensor (and hence the Riemann zero tensor), this coordinate system is that of ordinary Minkowski spacetime (check (HIRATA, 2012) for proof)[37]. String theory thus requires spacetime to be flat. And one can pretend it is flat, locally, in some weakly interacting systems, to simplify math work. But the reader can anticipate that, in a correct quantum gravity theory, idealizations do not suffice. It is *truth* we are interested in. Note it is one thing to say spacetime is Minkowski flat, it is quite another to view geometrodynamics and matter as spacetime. The implications of asking about the basic structure of spacetime are so, so great—in physical, epistemological, and metaphysical terms—, years of PhD studies shall not suffice to work out the basic questions and rehearse answers. I am unable to, now, even scratch the surface of the most basic philosophical perplexities involved. The only thing I *can* say is that pragmatism allows you to switch from one framework to another, geometrodynamics to spin-2 fields—*realism* doesn't.

But isn't there some truth to Dawid's UEA slogan, 'string theory requires gravity'? We have, in fact, done ourselves the necessary theoretical spadework to check where in the formalism that assertion finds support. This study motivates the conclusion that, if taken literally, Dawid's assertion is misleading in its superficiality. It takes a specific reading of gravity that is by no means established: recall we have seen in section 1 he describes 'the extendedness of particles as a feature that seems intrinsically linked with the phenomenon of gravity'. This is, *at best*, an exaggeration. The limited applicability of string theory's results to the vacuum case—where there is no matter distribution to affect geometry, to curve space—is not mentioned by Dawid. How much a spin-2 field approach to gravity differs in terms of physics, ontology, and epistemology from a geometric viewpoint—and why, by all means, is the former to be preferred to the latter—is also a pressing issue that Dawid leaves untouched. Given the boldness of his proposal, I do not think that is something he can get away with. Deviations from flatness have been empirically observed—what are the shortcomings of requiring a flat Minkowski spacetime rather than a curve, Riemannian one? Are we supposed to be conventionalists about space—thus invoking Poincaré's arguments to justify our holding onto flat space come what may[38]? Linearized theory of gravity gets in touch with experiments only when translated to the language of Einstein's curved-space formalism. If conventionalism is not the answer and yet geometrodynamics is misguided, is spacetime an 'a priori given' entity? How can this question be investigated? Or it is no entity at all, but rather a set of

[37] In fact, any spacetime metric can be regarded as satisfying Einstein's field equations,

$$R_{\mu\nu} - \frac{1}{2}Rg_{\mu\nu} + \Lambda g_{\mu\nu} = 8\pi T_{\mu\nu}$$

since once the left-hand side of it is determined by the metric-tensor of the spacetime of your like, one can define $T_{\mu\nu}$ as the right-hand side of the equation. Because of the complexity of Einstein's equations, no exact solutions of it are found except in cases of very high symmetry—like a flat space, without matter. Einstein's equations in Minkowski spacetime is the simplest empty spacetime there is. For a very nice mathematical description of it I direct the reader to (HAWKING; ELLIS, 1973).

[38] Poincaré's conventionalist thesis are depicted in three famous papers on geometry (POINCARE, 2017).

relations that obtain amongst material happenings? If gravity is no longer identified with the dynamics of spacetime but with a quantum field, do we direct to it the conceptual problems and ontological conundrums that characterize quantum field theory? How must this last question be re-written, in the context of a theory of strings? Or, given that recent developments have shown string theory is not a theory of string but of D-branes... what is the treatment of these questions in this new landscape?

Closed strings moving in a fixed background spacetime were considered in section 2, above. One mode of the string corresponds to a massless spin-2 particle when the appropriate limit is set, the 'graviton'. Graviton-scattering amplitudes can be calculated in string theory's framework—but only perturbatively. Other massless particles are also at the quantized closed string spectrum, side by side with the graviton and differing from it only with respect to spin value—the dilaton and the axion. These are massless, spin 0 particles. They are well-known to phenomenologists and share with the graviton, in addition to their mass values, another important feature: they have never been observed. The Einstein equations for the vacuum, displayed at the end of section 3, were also found by perturbing about the background flat space—specific choices of gauge and coordinates made in order to force the desired result to come out. Now all other worries aside, are these approximations enough to convince us string theory is a theory of all interactions, the long-sought 'Holy Grail of Physics'? That these results *exist* is in itself, I think, an astonishing fact; indicative of a framework worth working on. But one cannot be overanxious to interpret the related framework *realistically*—let alone forge epistemic principles out of it—when consensus with respect to what the theory is *about* does not obtain[39].

I was lucky to attend *the Summer School on the Philosophy of Quantum Gravity* held in Wisconsin, USA, June of 2016. At that occasion, string physicist Djorge Minic, in a lecture about string theory, said something like the following: 'We don't have a specific problem we want to solve in string theory. We do not philosophize about it. We are pragmatists. String theory is not like communism, you know—we want to get *there*. We are just adventuring ourselves through the mathematics and checking if it leads us somewhere interesting.' His words left a great impression on me[40]. Historical record has indeed shown us string theory was not born to be a theory of quantum gravity—*it morphed*

[39] This 'schizophrenic' feature of string theory was briefly discussed in the last chapter. It is a fact that how one finds string theory generally depicted in the literature (specially in popular presentations), that is, in terms of a 'pristine', 'unique', 'ineluctable' framework is too romanticized, almost completely false a description of it. (RICKLES, 2014a) opens his book with the following assertion written in bold: 'string theory [...] does not exist!'. The volatile history of string theory is in his book brilliantly displayed.

[40] They are at odds with how the theory is presented by Richard Dawid, by Leonard Susskind, by Brian Greene—forcing one to wonder to what considerable extent these authors' presentation of string theory rely on rhetoric rather than on theoretical description. (CONLON, 2016), however, describes the art in a manner that is compatible with Minic's utterances.

into it; saved from the dark fate to which succumbed its foremother S-Matrix by the hands of practitioners who exapted it from describing hadrons (with 'strong photons' and 'strong gravitons') to a theory of quantum gravity (electrodynamics, Yang-Mills theory, and gravitation—'everything'). It is thus the authority of history that forces us to identify an important common feature between arguments UEA and MIA: *both are descriptively false.* We have seen in the first section of this chapter Dawid's utterances to the effect that 'the initial motivation for suggesting it as a fundamental theory of all interactions was to cure the renormalizability problems of quantum field theories that include gravity'. *This is clearly not what happened.* I hasten to add that the 'unexpected explanatory coherences' Dawid speaks of, looked at by us in sections 2 and 3, were discovered before the 80's and, to this day, when theorists say 'string theory requires gravity', these are the arguments they invoke; no further developments added[41]. Perhaps this stagnation is symptomatic of an incompatibility between 'pragmatism' and 'attempts to craft a theory of everything'. This incompatibility tends to be overlooked when you take philosophical considerations to be useless and allow math alone guide you—the standard *modus operandi* of string theorists, according to Minic.

The reader may now be under the impression I shall be counted amongst 'string critics', whatever that amounts to. I do not see myself as such. I have spent so much time with string theory I was too seduced by her charms; I do think it stands a framework worth fighting for. But I also think pragmatism is an unfruitful attitude towards quantum gravity and it is not by ignoring foundational questions, by thinking 'math alone will supply us the answer' that the secrets of Nature will be uncovered. Perhaps string theory can provide correct answers to the quantum gravity problem—it is the right questions that are missing. Perhaps it is a failed research program and completely different ideas are needed to the merging of quantum theory and general relativity. All possibilities must be explored. I fail to see how to assume string theory's truth, however—as Dawid suggests we should do—does any good to the field or to quantum gravity research in general.

I conclude this chapter by suggesting not only Dawid's last argument does stand up a straw man as his whole 'non-empirical epistemology', as he presents it, a red-herring. His philosophy not simply fails to get at the heart of the most pressing epistemological questions in quantum gravity investigation: it prevents us from asking them. It aims to prove string theory displays certain traits which warrant unrestricted belief in it: we need not investigate further, he says; we need not check the theory's credentials; we need not check its implications for ontology; we need no indorsement from experience. Some of his

[41] I recall attending a lecture by famous string theorist Nathan Berkovits at the Institute of Physics of the University of São Paulo (IF-USP), winter of 2015. The topic was 'why work on string theory' (he had just received a major funding for a project on string theory to be developed in Brazil). One of my professors, mathematical physicist A. F. R. de Toledo Piza, attended it also. In class the next morning I asked him how had he liked the lecture and he replied, 'these guys say the same thing over and over since the seventies'.

arguments are *simply false* and they amount to a misconstruction of the meaning of the word ‘confirmation’. It is an epistemology crafted in a bed of Procrustes, tailored to make the gears of confirmation turn for string theory and for it only. Like all sorts of dogmatic thinking, it harms science instead of making it grow.

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