ON THE POSSIBILITY OF STABLE REGULARITIES WITHOUT FUNDAMENTAL LAWS

ALDO FILOMENO

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CCIL program, department of catalan philology.
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THESIS SUPERVISOR:
Dr. Carl Hoefer

author's signature:

Aldo Filomeno

director's signature:

Dr. Carl Hoefer

Autonomous University of Barcelona,
Faculty of Philosophy and Arts,
CCIL program, department of catalan philology,
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"All religions, nearly all philosophies, and even a part of science testify to the unwearying, heroic effort of mankind desperately denying its contingency"

— Jacques Monod [Monod, 1971]

Dedicado a mis padres, María del Mar y Agustín

PREFACE

This doctoral dissertation investigates the notion of physical necessity. Specifically, it studies whether it is possible to account for non-accidental regularities without the standard assumption of a pre-existent set of governing laws. Thus, it takes side with the so called deflationist accounts of laws of nature, like the humean or the antirealist. The specific aim is to complement such accounts by providing a missing explanation of the *appearance* of physical necessity.

In order to provide an explanation, I recur to fields that have not been appealed to so far in discussions about the metaphysics of laws. Namely, I recur to complex systems' theory, and to the foundations of statistical mechanics. The explanation proposed is inspired by how complex systems' theory has elucidated the way patterns emerge, and by the probabilistic explanations of the 2^{nd} law of thermodynamics. More specifically, this thesis studies how some constraints that make no direct reference to the dynamics can be a sufficient condition for obtaining in the long run, with high probability, stable regular behavior. I hope to show how certain metaphysical accounts of laws might benefit from the insights achieved in these other fields.

According to the proposal studied in this thesis, some regularities are not accidental not in virtue of an underlying physical necessity. The non-accidental character of certain regular behavior is only due to its overwhelming stability. Thus, from this point of view the goal becomes to explain the *stability* of temporal patterns without assuming a set of pre-existent guiding laws. It is argued that the stability can be the result of a process of convergence to simpler and stable regularities from a more complex lower level. According to this project, if successful, there would be no need to postulate a (mysterious) intermediate category between logical necessity and pure contingency. Similarly, there would be no need to postulate a (mysterious) set of pre-existent governing laws.

Part I of the thesis motivates part II, mostly by arguing why further explanation of the notions of physical necessity and governing laws should be welcomed (chapter 1), and by studying the plausibility of a lawless fundamental level (chapters 2 and 3). Given so, part II develops the explanation of formation of simpler and stable behavior from a more complex underlying level.

Keywords: Laws of Nature, Physical Necessity, Randomness, Statistical Mechanics, Typicality, Method of Arbitrary Functions, Symmetry principles

PREFACIO

Esta tesis doctoral investiga la noción de necesidad física. Concretamente, estudia si es posible explicar las regularidades no accidentales sin la asunción habitual de un conjunto preexistente de leyes que gobiernan la Naturaleza. Al dispensar de dicha asunción se pone del lado de las llamadas teorías deflacionistas sobre leyes de la Naturaleza, como la humeana o la antirealista. El propósito principal de la tesis es el de complementar dichas teorías con una explicación —ausente a día de hoy— sobre la *apariencia* de necesidad física.

Para proveer dicha explicación, recurro a campos a los que no se había recurrido en la literatura sobre metafísica de leyes: a la teoría de sistemas complejos y a los fundamentos de mecánica estadística. Concretamente, la explicación propuesta está inspirada por la forma en que la teoría de sistemas complejos da cuenta de la emergencia de patrones y por las explicaciones probabilísticas de la 2ª ley de la termodinámica. Más en detalle, esta tesis estudia cómo ciertas restricciones —que no hagan referencia directa a cómo debe ser la dinámica del sistema— puedan ser suficientes para obtener a largo plazo, y con alta probabilidad, regularidades estables. Espero mostrar cómo ciertas teorías metafísicas sobre leyes pueden beneficiarse de los resultados obtenidos en estos otros campos.

Según esta propuesta, la razón por la cual ciertas regularidades son no accidentales no es debido a una necesidad física subyacente. Es sólo debido a su inmensa estabilidad, fruto de un proceso de convergencia a regularidades estables. Así pues, el objetivo viene a ser el de conseguir explicar la *estabilidad* de patrones temporales sin presuponer un conjunto de leyes preexistente. Si esta propuesta es correcta no haría falta postular una (misteriosa) categoría intermedia entre la necesidad lógica y la contingencia pura. Del mismo modo, no haría falta postular un (misterioso) conjunto de leyes preexistentes.

La parte I de la tesis motiva la parte II argumentando por qué las nociones de necesidad física y de leyes gobernantes son efectivamente misteriosas, y que una explicación debería ser bienvenida (capítulo 1), y estudiando la plausibilidad de un nivel fundamental sin leyes (capítulos 2 y 3). La parte II desarrolla la explicación de la formación de comportamiento estable más simple desde un nivel subyacente más complejo (en última instancia, caótico o aleatorio).

Palabras clave: Leyes de la Naturaleza, Necesidad Física, Aleatoriedad, Mecánica Estadística, Tipicalidad, Método de Funciones Arbitrarias, Principios de simetría

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ACRONYMS

QM Quantum Mechanics

QFT Quantum Field Theory

BSA Best System Account

Part I

METAPHYSICS OF FUNDAMENTAL LAWS

1

NON-ACCIDENTAL REGULARITIES WITHOUT PHYSICAL NECESSITY

Abstract

This first chapter exposes what motivates the whole dissertation: the apparent existence of non-accidental regularities. In particular, I explore the plausibility that physical necessity is not a genuine category of our ontology. I take side with deflationist accounts of laws, like the humean or the antirealist, and aim to provide a missing explanation of why some regularities in the world seem to be non-accidental. In this chapter, after spelling out what this problem exactly amounts to, and justifying that it is indeed an important problem, I take a survey of the answers in the extant philosophical literature about laws of nature. It is shown that none of them provides a satisfactory answer to this trait of laws. Then, I finish the chapter by looking at current physics, i.e. at whether our current best empirically tested physical theories provide any hint on such metaphysical questions. Specifically, I pursue the strategy of understanding laws in terms of symmetry principles, given the ubiquitous and prominent role of the latter. I present an interesting candidate in Quantum Field Theory that might fill the desired role.

Contents

1.1 INTRODUCTION

The goal: non-accidental regularities,

THE OBJECTIVE The riddle that motivates this whole dissertation is the apparent existence of *non-accidental regularities* in the world. It is the well-known fact that some regular behavior around us seems *not to be a cosmic coincidence*. Some classic examples are:
(i) All emeralds are green ;
(ii) Massive bodies attract each other;
(iii) Sugar gets dissolved in water ;
(iv) Stars end up going out ; or
(v) All uranium spheres are less than a mile in diameter.
There seems to be *some reason* behind these regularities, unlike others that we would take as accidental, as mere contingencies. For instance, Reichenbach and Hempel compared (v) with the celebrated:
(vi) All gold spheres are less than one mile in diameter.
While (vi) clearly appears to be an accidental true generalization, (v) is not: it is not that we have merely found (v) to be the case, there *cannot* be uranium spheres bigger than a mile in diameter (cf. [Van Fraassen, 1989, 27]).

Tradition has it that the apparently non-accidental regularities from (i) to (v) are explained by appeal to the existence of *laws of nature*, or to some *causal* mechanism responsible of such regularities, or to both. In fact, this dissertation reflects about something we are very used to —used since the scientific revolution—, that is: the fact that scientists have been discovering regularities that, at least apparently, seem to be not accidental, and explain them in virtue of the existence of laws of nature. In particular, my focus is on the laws of physics. Presumably, these are the fundamental laws to which the rest of laws reduce (this reductionist view won't be necessary, though).
What is the default orthodox view on physical laws? It is roughly this: the laws are a set of rules, written in mathematical language, presumably exceptionless and holding universally across space and time, which ultimately guide the behavior and temporal evolution of the whole universe and all the entities therein. (Not all physical laws are laws of temporal evolution, but leave that aside for now). Purportedly, it is due to these laws that some regularities are not accidental.

and physical necessity...

The notion of law yields the idea that there are some events or things in Nature that are *physically necessary*, that in the actual world they cannot be otherwise: it is not possible in the actual world that massive objects do not attract each other, or that something travels faster than the speed of light (assuming in the examples that our current laws are true, which of course might not be correct). Some philosophers and physicists, then, have claimed that there is, between pure contingency and logical or mathematical necessity, an intermediate degree: physical necessity, also named 'natural necessity'. Phys-

ical necessity is thus intrinsically related with the concept of non-accidental regularity and with the concept of law of nature.
Consider another of the examples above: we think that the fact that all emeralds are green is not a mere contingency that just happened by chance. But it is not either grounded by any logical truth: logical necessities are compatible with emeralds being blue. In fact, we think we can conceive a possible world where emeralds are blue, or where massive bodies repel each other. There is something between logical necessity and mere contingency that makes the emeralds in this world to be green: physical necessity.

However, ever since David Hume famously cast the doubt on the notions of causation, physical necessity and laws[1], no satisfactory philosophical account has been provided (I shall elaborate this claim in sec. 1.4).

One can tackle this situation only in terms of causation or in terms of laws, not necessarily of both. Thus, one saves the need to explain one of the two. And presumably, if you manage to account for one of them, a related notion of physical necessity follows. Bertrand Russell famously advocated for the exclusion of the notion of causation [Russell, 1953], while Nancy Cartwright [Cartwright, 1999] has been a pioneer in dispensing with laws and place causation in its stead[2].

...without fundamental laws

This thesis takes side with the former. The move of postulating genuine causation to account for regular behavior is not contemplated here.
Specifically, this thesis takes side with deflationist accounts of laws, like the humean or the antirealist. This means that there are not genuine laws as a primitive category of our ontology. Then, I study whether it is possible to obtain stable regularities. The existence of overwhelmingly stable regularities is something that has not been properly answered by deflationist accounts, as I argue in 1.4.

The philosophical questions stemming from the postulation of laws

ON THE NOTION OF LAW OF NATURE Physicists, for centuries until the present day, have been aiming to discover how are the laws that describe and (allegedly) prescribe the evolution of the world — Maxwell's equations, Einstein's field equations, the Schrödinger equation, and so on. Appropriately, *what* are these laws is not something the physicist answers; it could be said that it is not his job. Yet, from the associated worldview, that is, from the scientific image describing a world ruled by laws, at least three characteristically philosophical questions arise:

1. What are the laws,

1 [Hume, 1896, book 1, part 3, sec. 14] and [Hume, 1748, ch. 7].
2 Other advocates of each side are the following. As to skeptics of genuine causation, [Earman, 1976], [Maudlin, 2007, ch.5], [Hoefer, 2004] (or my draft [Filomeno, 2010]). As to skeptics of genuine laws, see p. 18, and 1.4.2 p. 25.

2. Why there are laws,

3. Why there are these laws and not others.

At risk of being too optimistic, I would say that certain progress has been achieved in contemporary philosophy in what respects question '1', fleshing out some proposals that were suggested long before in modern philosophy[3].

The orthogonality or not between the three questions is not evident at first sight. Be it as it may, most contemporary accounts give an answer to question '1', but ignore question '2': laws, understood one way or another, are just assumed to exist.

In philosophy it is desirable to commit only to the necessary premises —in order to show the bare logical form of the argument, without irrelevant premises that in turn could be disputable. So if it were possible to answer '1' without committing to an answer to '2', this should be welcomed. Nevertheless, I shall argue that deflationist accounts must answer '2' to count as minimally satisfactory accounts[4].

Thus, what this thesis attempts is to complement deflationist answers to '1' with an answer to '2'. The answer to '2' in a deflationist scenario, i.e. a scenario without governing laws, amounts to answer why there is *the appearance* of laws.

Regarding question '3', I shall argue in chapter 3 how it is not orthogonal to '1' either, but it also turns out to bear an influence upon the answer to '1'. In a nuthshell, the choice of a set of laws rather than other set is argued to be suspicious —an "insoluble problem" quoting J.A. Wheeler—, so this indirectly supports a qualitatively different alternative —an alternative not consisting of a finite set of equations[5].

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3 The contemporary accounts are surveyed in 1.4. The predecessors I refer to are [Hume, 1896], [Mill, 1884]; previously, partakers in the scientific revolution like [Bacon, 1620], [Galilei, 1632], or [Descartes, 1644, II, 37]. [Dorato, 2000, ch. 1] overviews the historical genesis of the idea of law of nature since greek antiquity. The echoes of the notion of laws presiding over Nature trace back to Lucretius, in *De Rerum Natura* [Lucrezio, Ist c. BC, Book V, 56-57]. For a more thorough historical survey see [Ruby, 1986].

4 Let me note that I am also going to ignore a bunch of other interrelated topics. On the one hand, this can be welcomed for the reasons mentioned. On the other hand, it is obvious that some of the topics ignored might bear a crucial import on what is discussed. Unclear metaphysical notions like 'law of nature' probably depend on the understanding of other equally mysterious notions, as that of time, space, or motion, to name the most immediate and unfathomable.

5 Some other discussions have been made around question '3', mostly related with the contingency or necessity of laws; cf. [Swoyer, 1982], [Armstrong, 1983], [Lewis, 1983], [Sidelle, 2002], [Chalmers, 2002], [Schaffer, 2008], or [McKenzie, 2013]. Roughly, a side argues that laws could not have been otherwise (in any possible world), and other side that they could have, i.e. that laws are contingent. Other front of discussion related with question '3' comes from the apparent fine-tuning of the actual laws. Scientific arguments in its support are found in detail in the book [Barrow and Tipler, 1986], or in [Susskind, 2003]. Serious critical assessments (i.e. that are well aware of some flagrant fallacies that justly annoy some skeptics on the topic) can be found in [Ellis, 2006, sec. G], [Smeenk, 2013], [Mosterín, 2004], or in the book [Bostrom, 2002].

Thus, the main objective of the thesis is to remark and attempt to provide an answer to what I take to be the main flaw of deflationist accounts of laws. As I will further elaborate in 1.4, I refer to their inability to distinguish between accidental from non-accidental regularities. This flaw can also be phrased as their silence regarding the counterfactual predictive power of laws. I will address the problem by means of explaining the *stability* implicit in the very definition of law. This explanation is carried out, in part II, by appeal to probabilistic arguments. These arguments are mainly based on convergence theorems of sequences of random variables, like the law of large numbers.

The overwhelming stability of certain regularities, for a deflationist metaphysics

To convey the central worry, take the point of view of the humean. If the Best System Account is just the best description of the patterns of the humean mosaic, why it is that some of those patterns display such an *overwhelming stability* across all history? Are we just to flatly accept such a *highly special* feature —i.e. the stability of certain regularities, allegedly holding across all the humean mosaic— merely as a brute fact? Or is there some explanation, some reason behind, that would complement such a point of view? An explanation is what this thesis aims to develop, i.e. an explanation of why the human mosaic displays such overwhelmingly stable patterns.

The same worry can be phrased in the framework of the antirealist on laws [Cartwright, 1999]: How is it that there are, in certain contexts, something like "nomological machines" that display, even if not universally, *stable* temporal patterns?

Likewise, in the framework of the dispositionalist like [Mumford and Anjum, 2011]: while the causal role of dispositions is arguably capable of substituting the need for laws, how is it that there are *stable* dispositions in the world? And, how are we to understand such primitive dispositions in our ontology?[6]

All in all, the results presented along this thesis can hopefully complement any of those deflationist accounts of laws.

Thesis: no physical necessity between logical necessity and pure contingency

The project, then, is to study the scenario in which there is no physical necessity between logical/mathematical necessity and pure contingency. Therefore, the *appearance* of physical necessity *must* be explained. Ultimately, the idea is that laws of nature are not a primitive genuine constituent of our ontology, but they can be derived from something else, something more plausible to postulate as primitive.

Is a random dynamics meaningful ?

A first reaction on the picture I am setting forth could be the following: To what amounts a lawless scenario? Is it, at the very least, meaningful? Again, this is something that deflationist accounts on laws have overlooked. Presumably they want to take side with the actual, highly stable, state of affairs; however, the lack of fundamen-

6 And, to complete the analogy with the three questions above, the analogous to question '3' is: why there are these dispositions and not others?

tal laws seems to yield a scenario of pure contingency, of a random dynamics, that is, of something very different from the actual world (see 1.4.2 for further elaboration).

In fact, it has been historically difficult to tackle with precision a scenario of pure contingency. A secondary aim of the thesis is to contribute to an improvement in the comprehension of such scenario. To this end, annex A walks through the recent mathematical insights into the formalization of the notion of randomness, and annex B summarizes C. S. Peirce's metaphysics, based on an evolution and reinforcement of regular behavior from a random origin.

Likewise, a negative moral is vindicated: an account of lawful behavior from pure contingency, from pure randomness, is (arguably) impossible unless further constraints are postulated.

Thesis (again): Non-dynamical constraints sufficient for stable behavior...

What is the minimum number of assumptions postulated, and more importantly, whether these assumptions can be *non-dynamical* (i.e. not being the direct postulation of a law) is the main object of study of part II.

If there can be non-dynamical conditions *sufficient* for (the emergence of) regular behavior, the possibility of an account of non-accidental regularities without assuming fundamental laws would be vindicated.

...or general dynamical principles more plausible than current laws

This being said, there is an alternative weaker formulation of the main goal, in which the dynamical assumptions are not avoided. In such case, the project can be seen as the study of how a smaller set of dynamical laws can lead to a bigger set of dynamical laws, where this smaller set is more plausibly justifiable than the bigger set —the bigger set being the current physical laws. If there are not reasons for preferring the smaller set, then the project would be of little interest.

In a nutshell, I will recall the doubts raised in the philosophical literature against the idea of a governing view of laws. I add a critique in next chapter 2, focused on the particular form of current laws. Thus, the thesis seeks to arrive at something different from both 1) such type of governing laws (sec. 1.4.2), and 2) such unnatural and contrived current physical laws (ch. 2). As said before, the candidates entertained are either non-dynamical constraints, or different dynamical but more plausible principles, from which stable regularities can be accounted[7] (ch. 4 and 5).

1.2 OUTLINE OF THE CHAPTERS

The thesis is divided in two parts. Part I revolves around the metaphysical literature on laws, as well as on the contributions that our current physics might provide (specifically, Quantum Field Theories (QFT) and the predominant role of local gauge symmetries therein).

7 *Prima facie*, to give a rough idea, candidates like the principle of conservation of energy, or (other) global continuous symmetry principles might fit the bill.

This part exposes the departure point of the thesis: the problem of the apparently non-accidental character of regularities, i.e. the apparent existence of physically necessary facts, i.e. the stability built in the very definition of the laws of nature. As such, it aims to motivate the interest of the proposals of part II. The second part, as previously explained, consists in the study of the constraints sufficient for the emergence of regular behavior. Let's survey every chapter more in detail.

In chapter 1, after further introductory remarks, I show in section 1.4 the lack of answers of the problem at stake, and why this is a serious and important flaw of all the accounts. In section 1.5 I take a look at how contemporary physics might help in our comprehension of the nature of laws. Whereas it has been historically difficult to flesh out the details of an account that explains physical necessity as deriving from logical necessity, I suggest in sec. 1.5 that some arguments, stemming from contemporary physics, might be driving in this direction. A serious candidate to account for laws *solely* in terms of symmetry principles might be found in Quantum Field Theory (QFT). According to a presumed sort of *a priori* status of symmetry principles (which I later discuss), this could be an account in the line of the long-sought logical inevitability of laws (in the line of Plato's "Phaedo", Leibniz, or the more recent [Kneale, 1949] or [Swoyer, 1982]). However, the hope of having the candidate of 1.5 as a full blown account of laws through symmetries is undermined in chapter 2. Even so, I will defend that (naturalistic) metaphysics should pursue the general strategy of understanding laws by understanding symmetry principles.

Chapter 1

Chapter 2 reflects on whether anything can be said about the dynamics of the fundamental level in light of current physics. It takes into consideration certain metaphysical criteria, like the simplicity of a theory, or the naturalness (a scientific criterion, so far barely discussed among philosophers). More specifically, it assesses to what extent such criteria should be taken into account in light of how current physics is. Two conclusions follow from the analysis. The most relevant conclusion suggests a qualitatively different physics in the fundamental level, and in particular it supports a highly complex fundamental dynamics.

Chapter 2

Chapter 3 defends the scenario of a fundamental highly-complex underlying dynamics. Basically the chapter aims to show the consistency of such metaphysical scenario with current physics, and how it is in harmony with deflationist accounts of laws. Then, in 3.3 I analyze a parallel project, carried out by some physicists, highly similar to this dissertation: the possibility that all (or most) symmetries of laws are not fundamental but derived. This can be the case by considering a process of formation of symmetries from an underlying non-symmetric (highly-complex) lower level. In a nutshell, symmetries

Chapter 3

would typically emerge in the low energy limit *for almost all complex lagrangians*. This line of research has been pursued, among others, by a team of physicists led by H. B. Nielsen [Froggatt and Nielsen, 1991]. That Lorentz symmetry in particular is not fundamental but emergent has also been studied by Ted Jacobson [Jacobson and Wall, 2010]. Before them, physicists John Wheeler [Wheeler, 1983b] and Steven Weinberg [Weinberg, 1981] have been precursors of the underlying idea.

Further, the idea of emergence of order from randomness was developed at length in the XIXth century in the 'evolutionary cosmology' of C. S. Peirce [Peirce, 1867–1893], [Reynolds, 2002]. I have elaborated a brief overview of Peirce's metaphysics in annex B.

Then, Part II *assumes* this highly-complex dynamics motivated in chapters 2 and 3, and studies the *emergence of simpler stable regularities from an underlying more complex dynamics*. This procedure is backed by probabilistic arguments, in the same line as the statistical explanations of the 2nd law of thermodynamics.

Chapter 4

Chapter 4 argues that, if a system displays a chaotic trajectory in phase space and certain non-dynamical conditions hold, stable regularities arise without needing a specific dynamics guiding the evolution of the system. This is mostly based on the mathematical method known as method of arbitrary functions. In a nutshell, I follow the research of Michael Strevens (mostly in [Strevens, 2003] and [Strevens, 2013]), and "translate" his insights into the fundamental level. That is, his analysis is generic and presumably applicable to many high-level complex sciences (like meterorology, or biology). What I do is to argue for the applicability of those insights in the fundamental physical level.

More in detail, he explains how simpler regularities can arise from more dynamically complex lower levels. I argue how the method works for any chaotic dynamics, and then I discuss how this extension to all chaotic trajectories can be significant to the case of a scenario without fundamental guiding laws. To this end I appeal to the recent insights regarding the shared properties of the notions of randomness and chaos.

Chapter 5

Finally, the last chapter 5, tackles the same issue of chapter 4 from a similar point of view. Specifically, it aims to explain certain stable behavior, namely the tendency to equilibrium in classical statistical mechanics, *without needing any details as to the particular form of the dynamics*. In the literature on the foundation of statistical mechanics there is one account that *arguably* does so, the approach called 'typicality'. Roughly, this approach states that as long as the initial conditions of the model are typical (where the intuitive meaning of 'typical' is cashed out with precision in measure-theoretical terms), then there will be a tendency to equilibrium. I follow [Goldstein, 2001] and [Frigg, 2009] and explain how they argue that typicality makes

no reference to the dynamics —in an analogous way as to how, in ch. 4, the method of arbitrary functions did not mention how the dynamics had to be, besides being chaotic. If the details of the dynamics are irrelevant, this opens the door to consider that such conclusion —i.e. the tendency to equilibrium in the coarse-grained level— would obtain for any dynamics, therefore also for a random dynamics. The chapter aims to support the claims of the independence of the dynamics with computer simulations that I have programmed. Specifically, the simulations aim to show that the tendency to equilibrium, in the hard-sphere model of classical statistical mechanics, actually holds for almost any dynamics. The simulations model a hard-sphere group of particles that move according to a certain set of laws in a container. I have programmed the simulation in a way that is very easy to change the laws that rule their behavior. Thus, for each different set of laws it can be verified whether the tendency to equilibrium obtains.
I include the results in 5.4, and the code is in the annex C. A brief illustration of such simulations can be found online in the links:
https://vimeo.com/90044328
https://vimeo.com/90863487

1.3 FURTHER INTRODUCTORY REMARKS

THE FOCUS ON FUNDAMENTAL PHYSICS This thesis focuses on the physical necessity in the fundamental level. Assuming reductionism, the physical necessity of the high-level non-accidental regularities, as those stated before in (i)-(v), ultimately comes from (is reducible to, supervenes on) the physical necessity contained in the most fundamental level, object of study of theoretical physics.
So, I will focus on the specific form of the elementary interactions of contemporary physics. Now, they might display a different form than the regularities (i), (iii), or (v). This difference, perhaps, turns out to be informative and provides a better comprehension of laws and of physical necessity. In general, all regularities were roughly schematized in 1^{st} order logic as $\forall x(Fx \to Gx)$, being F and G two universals —like F being an emerald and G being green. This formalization has been clarificatory, and the philosophy of science based on it has contributed to a better comprehension of the nature of laws. However, the physical necessities expressed by fundamental physics are stated in a different way, and this perhaps turns out to make a difference.

"ORDER" AND "CHAOS": WHAT IS TO BE EXPLAINED? Let me make an important strategical observation; an observation about an implicit choice regarding what is going to be the *explanandum* in these sort of metaphysical inquiries. The point is on *whether laws, or the lack of them, is what has to be explained*. The choice crucially bears upon the resulting metaphysics.

What has been usually chosen along the history of physics?
Since the modern conception of science, dating it back at least to the time of Galileo Galilei, tradition has it that scientists aim at discovering underlying patterns in the world. The universe seemed to follow a determined behaviour, following certain rules, principles, or laws. In this respect, Einstein famously remarked that the most incomprehensible thing about the universe is that it is comprehensible [Einstein, 1936]. The universe is indeed following a pattern, and we can describe it in the language of mathematics. Galilei's metaphor of the book of the world written in mathematical terms has been confirmed for centuries ever since[8]. But at the same time, Einstein himself, who was wondering in the previous quote about the order of Nature (and our ability to grasp it), was feeling uncomfortable with the possibility of objective indeterminism in the dawn of quantum theory: his famous quote "God does not play dice" conveys such worry. This zeitgeist has been ubiquitous along the history of physics. It can be said that there is a strong predilection for determinism and a reluctance for the presence of randomness[9]. To illustrate what I take to be a generalized attitude, read this fragment in which the *first* virtue mentioned of the many-worlds interpretation of Quantum Mechanics (QM) is the removal of randomness:

> "The existence of the other worlds makes it possible to remove randomness and action at a distance from quantum theory and thus from all physics." [Vaidman, 2014, 1]

This attitude, worth is saying, makes much sense: it is grounded in the highly reasonable principle of sufficient reason.

Indeterministic laws "for a philosopher"

However, this affection for determinism notwithstanding, randomness has inevitably infiltrated in modern physics. It turns out that, at the present day, *genuine* objective indeterministic laws are endorsed by many (but not all) interpretations of the mathematical formalism of QM[10]. Likewise, physics abounds of the term 'spontaneous': "spontaneous vacuum fluctuations", "spontaneous symmetry breaking", etc.[11] This means that, if one really commits to an indeterministic interpretation of QM, *objective chance*, that is, *a certain degree of randomness*, is unavoidably introduced in the picture of the world. So, already in the standard orthodox scientific worldview, the notion of randomness is present.

8 "Philosophy is written in this grand book, the universe [...] It is written in the language of mathematics, and its characters are triangles, circles, and other geometric figures; [...]" [Galilei, 1623, ch. VI]

9 Why else there would be so many papers from philosophers of physics on whether determinism is violated in this or that area of physics?

10 Though the issue is not straightforward, it can be roughly said that indeterminism is endorsed in the following interpretations: Copenhagen, Von-Neumann, stochastic, objective collapse theories (like GRW), and transactional interpretations.

11 In these cases, though, there can be the hope that such spontaneity is some day explained otherwise.

The objective of this observation is twofold. One is to bolster the meaningfulness of scenarios involving a certain degree of randomness in their dynamics —given that the degree of randomness in the dynamics is going to be extended in coming chapters.

In the second place, I want to remark that the commitment to indeterministic (or 'stochastic') laws unavoidably suffers from *both* philosophical worries: the *lawful* behavior of the probabilistic laws (cf. the first of Einstein's quotes), *and* the *objective chance* present in such laws (cf. the second of Einstein's quotes). This second observation allows me to come back to the initial point regarding what should be explained, the laws or the lack of them. As said above, it is mostly held among the scientific community that what is unsatisfactory is the presence of randomness, not the existence of laws. But, to what extent is this preference justified? An underlying motivation of the next chapters is that what is more natural is to take as primitive a state of randomness, without a specific order dictated by some rules. This thought is elaborated in Peirce's metaphysics, whose central points I summarize in the annex B; there the interested reader will find more suggestive arguments for this alternative approach.

What is satisfactory to postulate, laws, or the lack of them?

An analogous observation can be made from the point of view of contemporary particle physics. Not in terms of determinism or indeterminism as to what is to be explained, but in terms of symmetry or asymmetry. In fact, the widespread idea is that the more we approach to more fundamental levels —which amounts to smaller scales or to higher-energy regimes—, there is a bigger symmetry group which describes in a unified way all elementary interactions. That is, it is presumed a process of symmetry restoration in smaller scales / higher energies. Then, much of the attention in physics and philosophy of physics centers around the converse process, that of symmetry breaking towards lower energies. This standard picture is philosophically motivated in [Brading and Castellani, 2013, 4.2]:

What is satisfactory to postulate, symmetry or asymmetry?

> "there may exist symmetries of the laws of nature which are not manifest to us because the physical world in which we live is built on a vacuum state which is not invariant under them. In other words, the physical world of our experience can appear to us very asymmetric, but this does not necessarily mean that this asymmetry belongs to the fundamental laws of nature. Spontaneous symmetry breaking offers a key for understanding (and utilizing) this physical possibility. (. . .) Spontaneous symmetry breaking allows symmetric theories to describe asymmetric reality. In short, spontaneous symmetry breaking provides a way of understanding the complexity of nature without renouncing fundamental symmetries."

Like order and determinism before, what seems to be reasonable, expected, justified, is symmetry.

Nevertheless, the complaint which I want to raise awareness of is also stated in [Brading and Castellani, 2013, 4.2]:

> "why should we prefer symmetric to asymmetric fundamental laws? In other words, why assume that an observed asymmetry requires a cause, which can be an explicit breaking of the symmetry of the laws, asymmetric initial conditions, or spontaneous symmetry breaking?"

The rationale behind this observation connects with what [Weinberg, 1981] once illustrated. According to him, one can take two paths regarding symmetries: it is a question whether or not you think the job of physics is to explain symmetries or to explain their absence. The conventional response to the successes of the Standard Model and its symmetries corresponds to the second of Weinberg's branches, that is, physics has to explain their absence. It assumes that the most fundamental laws of nature must have a large degree of symmetry and many of these symmetries, as said before, would then be broken as one descends to the present experimentally studied energy regime. This is the point of view manifested by Grand Unified Theories and by supersymmetry or supergravity models. As [Froggatt and Nielsen, 1991, 3] remarks:

> "this philosophy amounts to postulating the observed gauge symmetry group, since the symmetry is only explained by the existence of an even bigger gauge symmetry group, which is itself not explained".

Next chapter 3 takes side with the first of Weinberg's branches, that in which symmetries are what requires explanation and what is plausible, what is assumed, is an originally asymmetric, "chaotic", state. Let me insist that further reflections on this choice can be found in my comments on Peirce's metaphysics, in annex B.

MECHANISMS AS AN EXPLANATION IN METAPHYSICS This paragraph is a critical argument against an alternative path not pursued in this thesis. Namely, I argue for the insufficiency of mechanistic explanations for a science or a metaphysics whose aim is to provide a *fundamental* account of Nature. This argument becomes more attractive when one realizes the quantity of philosophical papers devoted in recent years to the notion of mechanism. Specifically, the argument is contrary to some literature that purports to vindicate mechanistic explanations as a primitive metaphysical framework sufficient for explaining eveything else e.g. [Glennan, 2005] or [Machamer et al., 2000]. Within part of this literature, it is shared the central point of this thesis, i.e. providing an explanation of regularities without assuming fundamental laws. However, this strategy is flawed, for one does not really gain much by substituting something puzzling —laws— by something equally puzzling —mechanisms.

The reason for my claim is that a mechanism, while can be plausibly considered the *explanans* of the phenomena that brings about, is itself unexplained. As such, it does *require* explanation, for the particular mechanism invoked as *explanans* will necessarily display two basic features that must not be taken for granted: 1) it will be *stable* and 2) it will have a particular composition. How is the stability obtained is not at all a trivial question, in spite of the lack of literature facing the issue[12]. This question is, indeed, the driving question of this thesis. And how does it have the particular composition is not a trivial question either. This is analogous to the worry raised in chapter 2 regarding the particular, "whimsical" features of symmetries (and laws). As such, the description of a mechanism as the final answer will always be an unsatisfactory *explanans* of a fundamental level.

THE MANAGEMENT OF DIFFERENT POSSIBLE LAWS The management of different possible laws is a subtle matter, and this is one of the things that this thesis has had to address (mostly in part II). Philosophers have entertained clear cases in which, for instance, the law of gravitation is inversely proportional to the distance cubed rather than the distance squared: $F = G\frac{m_1 m_2}{r^3}$

But, how to account for all the space of possibilities that such law could take?

In this thesis I am going tackle this issue in three different ways. One is taking advantage of the facilities that the abstraction of the *phase space* gives to us. It will be by means of managing the corresponding *trajectories* in phase space.

The second way is by taking advantage of what *numerical simulations* allow us to do. I have programmed a model ruled by some laws, newtonian laws, and then I have enabled a variation of those laws in a way that covers all possible variations (they won't be really *all*, though). This corresponds to the numerical simulations that I include in chapter 5, whose code is annexed in C.

Finally, a third way is to take advantage of the abstraction of the *lagrangian formalism*, written it in its most generic form and analyze possible general properties that are derived. The details of the last strategy have been carried out by some physicists and I will briefly reconstruct their arguments in chapter 3.

12 [Glennan, 1996] faces explicitly the issue but acknowledging that mechanisms (and the notion of causation) rely on the existence of fundamental laws. This is stated at the very beginning as well as in his definition of mechanism: *"A mechanism underlying a behavior is a complex system which produces that behavior by the interaction of a number of parts according to direct causal laws"* [Glennan, 1996, 5].

CONCLUDING REMARKS Let me emphasize that the approach to the metaphysics of laws carried along this dissertation rests on two pillars:

1. *contemporary* physics; this means that I pay attention not only to the laws of the form of differential equations, like for instance $F = ma$ (Newton's second law), or $\nabla x E = -\frac{\delta B}{\delta t}$ (Faraday's law of induction), but to the laws in the form they appear in our most recent physics; and it turns out that nowadays they exhibit particular features that might shed some light to the metaphysics of laws. As said before, I refer to the predominant role of (global and local) symmetry principles in the lagrangian of the Standard Model of elementary fields/particles. This is done in sec. 1.5, ch. 2 and ch. 3.

2. the insights coming from probabilistic explanations (basically from the foundations of statistical mechanics) together with the recent mathematical formalizations of the notion of randomness (ch. 4, 5, and annex A) —with the aim of elaborating a study of the emergence of stable behavior from a lawless scenario.

1.4 THE LACK OF A SATISFYING EXPLANATION OF PHYSICAL NECESSITY AND WHY SHOULD BE SOUGHT

This section surveys the extant philosophical accounts of laws of nature in order to show the lack of a satisfying explanation of physical necessity, be it real or apparent. I focus on the present-day accounts; for an historical evolution of the concept of natural law, see [Dorato, 2000, ch.1], [Ruby, 1986]; see also footnote 3.

Overview of present-day accounts

Regarding the contemporary accounts, an initial overview is the following. There is, on the one hand, a group of proposals, under the label of 'necessitarians', that have sought to account for the notion of law in harmony with the common understanding of the term and with the scientific preconceptions. Necessitarians believe in the existence of genuine physical necessities. Necessary behavior is explained in virtue of necessity relations between universals [Armstrong, 1983], [Dretske, 1977], [Tooley, 1977], or of dispositions [Swoyer, 1982], or of essences [Bigelow et al., 1992], [Ellis, 2001], [Bird, 2005]. In the vicinity of this group, there are the antireductionists [Maudlin, 2011, ch.2], [Carroll, 1994], which just postulate laws as a genuine non reducible primitive constituent of our ontology.

Then, a main alternative is that in tune with the empiricist tradition, the so called 'humean' account. Roughly, it relegates the notion of 'physical necessity' as a mere human illusion, in the same vein of understanding causation as a mere constant conjunction. Laws of Nature are just statements describing the regularities of the world, they are not prescriptions, but mere descriptions of the way the world is. [Mill, 1884] and [Hume, 1896] are the pioneers, together with [Ramsey, 1978][13]. A sophisticated development has been carried out by David Lewis, in what has come to be known as Best System Account of lawhood [Lewis, 1973, 72-77], [Lewis, 1999, 8-55,224-247]. It is also known as Mill-Ramsey-Lewis account. Following [Lewis, 1973, 73], suppose you knew everything about the past, present, and future, and you organized your entire knowledge as simply as possible in various systems ('systems' are a set of statements, some of which are the axioms, and then there are the theorems, which are logical deductions of the axioms). A contingent generalisation is a law of nature if and only if it appears as an axiom or theorem in the system that achieves a far better combination of simplicity, strength and fit than any of the other competing systems[14]. Cf. also [Lewis, 1983], [Earman,

13 Though David Hume actually believed in laws as physical necessities, so he was closer to the necessitarian point of view [Swartz], [Wright, 1983], [Beauchamp, 1981]. Hume was not 'humean' as we understand it now; his skepticism was epistemological, not metaphysical. I will maintain the received term 'humean', even if 'regularist' [Swartz] or 'neohumean' [Dorato, 2000] would be more appropriate.

14 Where strength, simplicity and fitness are defined as follows. To have strength is to bear a great deal of informational content about the world; to be simple is to state everything in a concise way, not to be redundant, etc.; and to fit is to accord as much as possible with the actual outcomes of world history.

1984], [Lewis, 1994], [Loewer, 1996], [Beebee, 2006], and [Cohen and Callender, 2009].

Finally, in the vicinity of the humean there is the antirealist stance, that shares the deflationist spirit of the humean, and denies the existence of laws as we usually understand them; in a nutshell, they deny not only the existence of physical necessity and of a set of governing laws, but also deny the existence of universal or exceptionless regularities. [Cartwright, 1999] is a classic reference, and more recently [Mumford, 2004]. Cf. also [Van Fraassen, 1989], [Cartwright, 1980], [Cartwright, 1989], [Blackburn, 1984], [Blackburn, 1993], [Giere, 1999] and [Ward, 2002].

LEVELS OF MODALITY A clarification must precede the assessment of the modal status of each view. There are two levels on which laws can be contingent or necessary. One level regards *whether there is physical necessity in the actual world*. This is what I have been mostly talking about so far. Then, an orthogonal level is whether laws could have been otherwise or are as they are necessarily. Being otherwise means that the actual world could have been ruled by different laws or, in terms of possible worlds, that there are metaphysically possible worlds with different laws. That physical necessities are as they are necessarily means that the laws could not have been different, or that, in every metaphysically possible world, what is physically necessary coincides (so the only differences between worlds would be due to different initial conditions, and to the outcomes of the laws iff they are stochastic).
The main objection to the view of laws as metaphysically necessary points out that we are able of conceiving worlds with different laws. Discussions regarding whether conceivability can be a reliable guide to possibility are found in [Yablo, 1993], [Chalmers, 2002], [Fine, 2002], [Sidelle, 2002] (for introductions see [Gendler and Hawthorne, 2002] or [Vaidya, 2007]). Defenders of this stronger sense of necessity of laws are [Kneale, 1949], [Harré and Madden, 1975], [Shoemaker, 1980], [Swoyer, 1982], [Tweedale, 1984], [Fales, 1993], [Shoemaker, 1998].

In order to frame the diverse philosophical discussions existent in the literature, let me notice that these two levels of modality mostly correspond to the three questions on laws raised in 1.1 p.6: the first level corresponds to questions '1' and '2' ('What are the laws', and 'Why there are laws'), whereas the second level corresponds to question '3' ('Why there are these laws and not others'). As said there in terms of the 3 questions, here we can also acknowledge that probably the two levels of modality are hardly separable, and an answer to one will probably bear an influence on the other.

That said, let's proceed to assess more in detail how the accounts explain the subject of research of this thesis, i.e. the first of the levels: whether there is physical necessity and, if so, what it is.

1.4.1 *Realist (i.e. non deflationist) attempts to explain physical necessity*

1.4.1.1 *Relations among universals*

Since the rise of scientific realism in analytic philosophy in the 60's, efforts have been undertaken to provide an account of laws in accordance with the scientific preconceptions, thereby aiming to account for the existence of physical necessities. In this line, a summarized answer to the question "What are laws of Nature?" is the following: laws are relations among universals. Some maintain that these relations are contingent, others that are necessary —in the second level of modality aforementioned. The former has been argued mainly by Armstrong [1983], Dretske [1977] and Tooley [1977]. The latter by Shoemaker [1980], Swoyer [1982] (and, as Swoyer remarks, it can be traced back to Plato's "Phaedo", Plato [4th c. B.C.]). The contingency here means that the relations could have been different, so that there are other possible worlds with different relations among universals, hence with different laws. The metaphysical necessity instead means that the relations could not have been otherwise, so that there cannot be other possible worlds with different relations among universals.

As to the first degree of modality: How does the necessitarian spell out the the notion of physical necessity? Armstrong defines a relation of necessitation between universals that he labels as 'N'. That is, for two universals F and G we may have that they stand in a necessitation relation, so in that case we can ascribe to them the relation N(F,G). This relation, whenever it holds, entails the obtaining of regularities among the instantiated universals involved. Such a relation explains the observed regularities given that $N(F, G) \Rightarrow \forall x(Fx \rightarrow Gx)$. Notably, the other direction of the entailment does not hold, and this is just the difference that distinguishes nomical regularities from accidental ones.

However, the lack of explanatory power of this approach has been notoriously criticized. The mere ascription of a label 'N' to the physical necessities does not really provide an understanding of what they are. David Lewis transmitted this worry with his usual wit:

> "Whatever N may be, I cannot see how it could be absolutely impossible to have N(F,G) and Fa without Ga. (Unless N just is constant conjunction, or constant conjunction plus something else, in which case Armstrong's theory turns into a form of the regularity theory he rejects.) The mystery is somewhat hidden by Armstrong's terminology. He uses 'necessitates' as a name for the lawmaking universal N; and who would be surprised to hear that if F 'necessitates' G and a has F, then a must have G? But I say that N deserves the name of 'necessitation' only if, somehow, it really can enter into the requisite necessary connections. It can't enter into them just by bearing a name, any

> more than one can have mighty biceps just by being called 'Armstrong' " [Lewis, 1983, 366].

The necessitarian solution is thus unsatisfactory, for the same reason that Moliere's 'virtus dormitiva' is unsatisfactory as an explanation of opium's sleep inducing properties.

1.4.1.2 *The antireductionist*

Similarly uninformative seems to be the answer of the antireductionist, which just postulates the *explanandum* as primitive.
The quality of his works notwithstanding, it is not unfair to summarize the view of lawhood expressed in Carroll [2008a] with the concise quote:

> "Laws of nature are exactly those regularities that are caused by nature. [...] They hold because of nature." [Carroll, 2008a]

In the case of [Maudlin, 2011], not only traditional deterministic laws, but even stochastic laws, of which I have argued (p. 11) that their need of explanation is twofold (one regarding the lawful element, other regarding the objective probability involved), are taken to be primitive. With respect to the probability at stake in stochastic laws, Maudlin says:

> "I cannot deny the possibility of a sort of cognitive blindness that would make someone unable to comprehend the notion of probability being used here, and I cannot offer a remedy for such blindness, since the notion appears as an irreducible posit. But still, on the one hand, such cognitive blindness appears to be rare: When the notion of a stochastic dynamics is introduced to the uninitiated, the result is not blind incomprehension. Some, like Einstein, might not like the idea, but they understand it. Furthermore, it is not at all clear what is wanted to provide the needed clarification. It is clear how hypotheses about stochastic dynamics are to be formulated, used, and tested. It is clear how to do experiments and draw conclusions. No reductive analysis is offered because the notion is not a derivative one, and there surely have to be non-derivative concepts. What more, exactly, could be wanted?"

The fragment cites a list of pragmatic virtues of stochastic laws, from which an understanding of them presumably follows. You might dislike the notion, but it can be comprehended. However, the strong rhetoric notwithstanding, the fragment is not really providing any substantial argument against the idea that stochastic laws contain philosophically puzzling issues, and that further explanation *should be welcomed*. To operate with something ('formulate', 'use', 'test', 'do experiments', and 'draw conclusions' is the partially redundant list

stated) does not imply that you fully understand, not even that you just understand, something. The foremost example is the quantity of puzzling interpretational riddles of the most empirically tested physical theory, quantum mechanics.
So, in brief: as to "*What more, exactly, could be wanted?*", my previous section, and likewise next sections, argue what *should* be wanted.

1.4.1.3 *Do dispositions and powers explain physical necessity?*

Eventually, within the necessitarian stance, a solution pursued is that which associates universals to the discourse of natural kinds, dispositions and essential properties. The revival of these neglected metaphysical categories was posed in order to ground the notion of physical necessity[15]. This move was proposed e.g. by Swoyer [1982], Bigelow et al. [1992], [Ellis, 2001], [Bird, 2007]. The idea is that an electron, for instance, has as part of its essence the causal power to repel other electrons. Likewise, the salt has the disposition to get dissolved or, in other words, the solubility is an essential property of salt. Thus, we are able to assert that laws are entailed by the dispositions of entities Bird [2005].

The discourse on essences was historically repudiated since british empiricism (and even before, since medieval nominalism), but recently it is much more tolerated (perhaps even widely accepted among philosophers) after the striking influence of Saul Kripke's work (Kripke [1971], Kripke [1980]). This discourse of essences takes back the parlance about essences and substances. Roughly, substances are what they are given the essential properties that define them. So this discourse takes back questions as to what is for something to be what it is, what is to be identical with another thing, and so on. In that way, it introduces famous insights about metaphysical necessity in terms of essences, as well as the alleged necessities *a posteriori*, of which I will refer later.

THE SAME WORRIES IN A DIFFERENT FRAMEWORK Thus, a strategy in order to ground the relation 'N' —which, in turn, amounts to ground physical necessity— is to admit the existence of essences, dispositions and/or powers, responsibles of the things being as they are, and responsibles for the relations among properties obtaining as they obtain, given the essence —the "intrinsic nature"— of these properties. [Dorato, 2000, ch.4], for instance, defends a dispositional account of laws:

> "In what respects the character of necessity of laws, we have seen how that is inherited either by the properties that characterize a natural kind or a type, or by its causal powers".

15 As I will comment later, also the antirealist side is complemented by a discourse on Nature's capacities and causal powers, though that is not strictly a necessary condition for antirealism [Cartwright, 1999], [Mumford and Anjum, 2011].

The move of appealing to dispositions, at least *prima facie*, is not as the case of 'virtus dormitiva', given that it makes a step in some direction; however, it is in the vicinity: the burden of proof has now shifted to another equally unclear place. The dispositionalist faces the worry as to *what* are supposed to be these dispositions —something that seek to treat books like [Molnar, 2003] or [Marmodoro, 2010].
Even so, to know what are intrinsic dispositions in Nature, without further ado, still says nothing about *why it is the case that actually there are regularly repeated dispositions in Nature* (and this is not the job of scientific discovery), and *why there are these dispositions and not others*. That is, I am rephrasing the same three questions stated at the beginning (p. 6), now in the language of the dispositionalist.
Moreover, one could pose the question: Are there not such intrinsic dispositions because of an underlying stochastic law? In that case, all the role of dispositions could be interchanged by stochastic laws and then explained (as usually) only in terms of the latter.
John Earman was also counting with laws as partial responsibles of dispositions; e.g. for the case of the disposition to dissolve:

> "We are confident that the secrets of dispositions to dissolve are to be found jointly in (a) ocurrent facts about the microstructure of salts and crystals and (b) laws couched purely in terms of ocurrent properties" [Earman, 1986, 95].

If that were the case, he can reaffirm the humean deflationist stance, according to which (being *W1* and *W2* possible worlds):

> "For any W1, W2, if W1 and W2 agree on all ocurrent facts, then W1 and W2 agree on dispositional facts regarding solubility (and other garden variety pure dispositions)" [ibidem]

If those questions are not properly answered, the dispositionalist framework can be hardly considered a complete metaphysical account of physical necessity and laws of nature.
Likewise, as it rests on essential properties, the next objection also threatens to undermine this type of account.

THE IRRELEVANCE OF THE METAPHYSICAL NECESSITY OF SUBSTANCES Related to the discourse of dispositions there is abundant literature arguing for the metaphysical necessity conveyed by essential properties. Kripke emphasized the metaphysical character of this necessity, in contraposition to the epistemic necessity, so it seemed that it was really stating something substantial about the world. Here, though, I will show why it is not a substantial type of necessity, not relevant for any of the two levels of modality of laws laid down above. Specifically, the necessity that comes from [Kripke, 1980][16] is more

16 And [Putnam, 1975], [Kripke, 1971], and all the abundant literature that followed ever since; some representative references being [Fine, 1994], [Fine, 2002], [Leeds, 2007], or [Nolan, 2011].

tied to our language conventions than what it intends to be. If this critique is correct, the sort of necessity involved will be true, but trivial and of little utility. At the very least, while accepting that there are necessary a posteriori truths, they do not bear any influence on whether laws could have been otherwise or not.

Let's unfold the argument, which coincides with what is more thoroughly developed in [Sidelle, 2002], [Sidelle, 1989], and similar to what is said in [Jackson, 1998], or [Chalmers, 1996, ch.3]. It is important to make the critical argument explicit, because nowadays metaphysical necessities of this sort are still considered a valuable stronghold of analytic philosophy (actually, when I was starting the PhD, four years ago, it was assumed to be a sophisticated substantial subject, an insight allegedly useful for the resolution of philosophical puzzles).

Laws as metaphysically necessary

Many elements made especially attractive Kripke's proposal. Among them, that a class of necessary a posteriori truths was the class of (a subset of) scientific empirical discoveries. For instance, 'Water is H_2O', 'killer whales are mammals', or 'Tilikum is a killer whale' (Tilikum is the killer whale of San Diego's "Sea World"). Now, we can perfectly conceive that Tilikum was a dolphin, or that killer whales are not mammals. But this, Kripke argues at length, is a case of false conceivability: to conceive is not a reliable guide to a real metaphysical possibility; it is only an epistemic possibility, and it is crucial not to get confused and differentiate them. The same train of thought might be extended to laws of nature in general, thus becoming paradigmatic examples of necessary a posteriori truths. Here 'necessary' is meant in the stronger sense of metaphysical necessity; this is to say that the laws are physically necessary in the actual world and any other metaphysically possible world.

More in detail, we have found out some laws of nature are in a certain way —think of a simple case: Newton's law of universal gravitation: $F = G\frac{m_1 m_2}{r^2}$. The immediate thought is that those laws, fruit of empirical, hence *a posteriori*, discovery, could have been otherwise; therefore the laws are contingent. In fact, we can conceive those laws having a different mathematical expression, or different values for those constants —I put before the law of universal gravitation having the distance cubed: $F = G\frac{m_1 m_2}{r^3}$; or you can easily conceive that the value of the gravitational constant $G = 6.67384 \times 10^{-11}[m^3 kg^{-1} s^{-2}]$ is just another.

But again, contrary to this standard view, Kripke argues that we must not confuse the possibility of having *found out* otherwise with the possibility of things having *been* otherwise. That is, those empirical discoveries are metaphysically necessary.

So it is essential for an electron to repel charges as it does (or in terms of dispositions: to have the disposition, or tendency, or capacity, to repel charges); otherwise it would not be an electron. Therefore, in all metaphysically possible worlds, electrons behave as they do in the actual world, they repel like charges, have spin 1/2, and so on; it is a metaphysically necessary (and discovered a posteriori) truth. Likewise, any other elementary behavior of Nature discovered and stated in the laws could not have been otherwise: the law of universal gravitation cannot have the distance cubed. It would not have been the law of gravitation anymore.

Appealing as this might seem, it can be flatly asserted that it is incorrect. Rather than unveiling metaphysical features of reality, the necessari a posteriori truths rest on a linguistic convention, so the necessary truth is necessary only in virtue of the meaning of the terms employed. What is made is an individuation of a certain empirical finding with a certain name, so the necessity involved is analytic. Take any example of the above. In the case of water's being necessarily H_2O, there is an assumption made that is that *nothing counts as water unless it has the same explanatory features as the stuff we call 'water'* [Sidelle, 2002, 319]. Then, there is a discovered empirical fact, namely that the explanatory feature of the stuff we call 'water' turns out to be composed of H_2O. The modal force in this process is only coming from the analytic individuation made in that assumption. Therefore it can hardly reveal anything at all about reality[17].

The conclusion exposed is of importance because it bears upon discussions in the nature of laws, as well as in other fields, like philosophy of mind (blatant examples are [Chalmers, 1996], or [Chalmers, 2009][18]). Namely, it bears upon the notions of the 'essences' and the 'natures' of things. [Sidelle, 2002, 321] expresses exactly what I want to convey:

> "If I am right, much of the rhetoric that has gone with, and followed upon, the acceptance of such truths involves misinterpretation. Metaphysically, it is misleading to speak of essences and natures, as if they were more than semantically determined; by the same token, it is at best misleading to say—as many philosophers often do— 'well, of course you can imagine that a is F, or some F is G—but perhaps *the very nature* of a, or G, makes this really impossible'. This is especially important, because the sort of 'real natures' talk is often what underwrites the sense that considerations of what we can imagine should not be expected to shed any light on what is genuinely possible."

17 This argument is elaborated at length in [Sidelle, 2002, sec.2]. Cf. also the aforementioned references. I leave aside many subtleties to focus in the central point stated.

18 In the abstract of this book we can explicitly find what is here criticized: *"My view is that one can legitimately infer ontological conclusions from epistemic premises, if one is very careful about how one reasons. To do so, the best way is to reason first from epistemic premises to modal conclusions (about necessity and possibility), and from there to ontological conclusions"*. And I have argued that the sort of modal conclusions at stake does not allow the step to any ontological conclusion.

In sum, the conclusion of this subsection is that none of the aforementioned accounts, all of them realist about laws —the necessitarian, the antireductionist, and the realist dispositionalist—, exhibit the sufficient explanatory power to account for the notion of physical necessity. Now I move to two antirealist accounts, the humean and the antirealist, where the conclusion will be the same.

1.4.2 *Deflationist attempts and the humean acceptance of contingency*

FUNDAMENTAL GOVERNING LAWS? Besides the specific objections raised, there is an objection common to all previous realist accounts that drives much of the deflationist proposals. The objection regards the highly mysterious status of laws understood as *a set of pre-existent, guiding, governing rules*. While this governing view of laws is the default pre-theoretic stance, both philosophers and, as I will show in chapter 3, some physicists, have disputed this viewpoint. Among philosophers, the critiques can be found in different forms in [Cartwright, 1980], [Lewis, 1983, 23], [Van Fraassen, 1989], [Giere, 1999], [Beebee, 2000], [Mumford, 2004], or [Schaffer, 2008].
For instance, [Beebee, 2000, 580-1] traces the governing view to a theological conception of laws, while [Schaffer, 2008, 16] underlines how

> "the notion of lawhood in use is a direct descendant of the theological views of Descartes, Newton, and Leibniz, who viewed laws as divine decrees concerning the clockwork of the world"

concluding that the intuitions involved in such understanding are remnants of a dubious theology. This thesis shares this critical stance, and aims to enhance it in upcoming chapters. Especially in the next chapter 2, where I remark how especially puzzling is the notion of governing laws when carefully looking at the particular, contrived form displayed by our current physical laws.

THE LACK OF PHYSICAL NECESSITY However, the alternative philosophical accounts, within the deflationist non-governing side, also fail in providing convincing answers to physical necessity: they deny its existence, but there is still an *appearance* of physical necessities and the subsequent apparent "non-accidental" regularities. *An explanation of this appearance has to be provided*.

I phrased this flaw in the very first paragraph of this chapter in terms of the "cosmic coincidence" of "ordered behavior". Let me phrase it from a complementary point of view: the humean laws lack any explanation of the counterfactual predictive power of laws [Swoyer, 1982, 209], [Fales, 1990, 85]. In a nutshell: if laws are only the best summary we can make of the history of the universe —of the humean mosaic—, then how can we be so confidents (as we are, even if we declare ourselves as convinced humeans) that the current laws are also

The cosmic coincidences that the BSA describes

How is it that laws support counterfactuals?

predicting what is going to happen in the future? Similarly, how is it that (humean) laws support counterfactuals? The humean, however, says nothing about this. Papers abound in the philosophical literature dealing with sophisticated issues around the humean account. However, little or none attention is paid to the central flaw I am recalling, which I take to be the most pressing threat to such account (cf. [Carroll, 2008b, sec.8], [Swartz, sec.7])[19].

How to expect a humean mosaic?

Let me remark a neglected feature that arguably follows from the humean framework that bolsters this central objection. Leaving aside how the world actually is, and focusing on what the humean framework says, how one should expect a humean mosaic to be? My answer is that one should expect that a humean mosaic, which lacks any genuine necessary connection, would look like chaotic, random-looking. A random-looking humean mosaic is what one should expect, given the barely restricted space of possibilities that the humean mosaic can take. The unconstrained space of possibilities —not constrained by any necessary connection— entails that *there are overwhelmingly more ways in which the humean mosaic is random-looking than highly regular, as it actually is*.

If (temporal) patterns appear, this is due to a coincidence —there are no necessary connections, there are only accidental regularities—; thus is much more likely that such regularities last not "much time" —which is to say: there are much more scenarios in which temporal patters just do not hold across all time and space. This is contrary to the highly stable regularities we daily experience, and even more at odds with the standard scientific image according to which laws of physics never change across time and space.

So, assuming the stability acknowledged in our actual world (from our daily experience and from the scientific image) but, crucially, not assuming the existence of physical necessity, should not we demand some explanation of this stability, of this highly special actual humean mosaic? Yes, we should.

This is, I argue, a very prominent problem of deflationist accounts on laws. So, later on, this dissertations studies whether is possible in this lawless scenario, adding hopefully plausible assumptions, the emergence in higher levels of *stable* regularities, that would correspond to what we would label as 'non-accidental'. That is, I will seek to answer the question: *What assumptions are needed in order to have stable regular-*

19 For instance, a sample of the topics dealt in current literature about humean laws revolves around: how to spell out the balance of simplicity and strength of the "best system account" [Woodward, 2014]; counterexamples of laws that do not show the presumed balance [Maudlin, 2011, 16], [Roberts, 2008, 23]; how to incorporate indeterministic laws and the notion of chance [Briggs, 2009]; or whether a best system account can be provided for special-level sciences [Schrenk, forthcoming]; among other topics, as e.g. [Lange, 2011], or [Cohen and Callender, 2009].

ities in a lawless scenario?

Nonetheless, the humean might not feel the urge to find an answer: he has a previous response[20]. To begin with, the humean is satisfied with taking as a brute fact the whole humean mosaic. This leads him to accept as primitive the overwhelmingly stable regularities that the mosaic displays.

A reply of the humean: accepting the humean mosaic... however it shows up

Here it is, however, where I want to argue that the humean, in his apparently natural acceptance of the humean mosaic as a brute fact, has been taking far too much for granted. It does not sound wrong to say that "*at some point explanations must come to an end. Regularists place that stopping point at the way-the-world-is*" [Swartz, sec.7]. But it is probably wrong when a layperson with curiosity realizes that many of his age-old philosophical worries are suddendly "dissolved" by their mere assumption. A dissolution would be of course welcomed, if it were not because the worries are just postulated as primitive brute facts. The *massive* postulation of the whole humean mosaic carries implicit in itself the postulation of any specific behavior that could have been displayed —so, not only the very existence of overwhelmingly stable regularities is "dissolved" but plausibly also any other sort of philosophical worry, as the time-asymmetry, the beginning and end of spacetime, and so on.

2nd humean reply: ascribing probabilities to the humean mosaic makes no sense

There is, yet, a second reply: it is not more likely for the humean mosaic to be in one way or another; it is meaningless to talk of probabilities as applied to the whole humean mosaic. Thus, the humean does not take as meaningful to say that the world displays "cosmic coincidences".

This reply is disputable too. There certainly are problems in the interpretation of probability, in the axioms of probability calculus, and especially in the assignation of real numbers (representing a probability) to single events, as in this sort of cosmological contexts. However, this does not entail that a prior landscape of possibilities does not exist, and that something substantial cannot be said about it —as in fact our intuition does. In this landscape of conceivable possibilities the scenarios in which the temporal patterns are so extremely ordered as in the actual world sum an overwhelmingly inferior number over the whole set of possible scenarios; they are far from being *the most typical case*. Typicality is a notion that does not recur to probabilities. Therefore, it does not assign any problematic value to any possibility. Typicality refers to a measure of the number of elements that share a property. The 'typicality' approach in Statistical Mechanics exploits this idea, and it is precisely defined in meature-theoretic terms. I spell

20 I thank here the humeans Craig Callender, Jonathan Schaffer, and David Albert for the advices given in personal conversations. While Callender acknowledged this as a problem of the humean account, the following reply is based on Albert's comments.

out this approach in chapter 5.
This thought is expressed in [Swartz, sec.7]:

> "there has to be some reason, some explanation, why the world is as it is and is not some other way. It can't simply be, for example, that all electrons, the trillions upon trillions of them, just happen to all bear the identical electrical charge as one another – that would be a cosmic coincidence of an unimaginable improbability"[21].

Without having to necessarily commit to problematic interpretations of probability, it is meaningful to wonder and *ask for a reason* behind such highly ordered, stable humean mosaic. *It is reasonable to expect that there is an explanation* behind such highly-ordered and frequent patterns. And this is something the humean account misses. This thesis aims to suggest a possible resolution, and (hopefully) begin to fill the gap. This thesis is thus perfectly compatible with the humean; indeed, it can be seen as a complement to any other deflationist account.

ANTIREALIST DISPOSITIONS Some dispositionalist accounts that are antirealist on laws, like [Cartwright, 1989] or, more recently, [Mumford and Anjum, 2011], recur to a discourse about causation, which is accounted in terms of capacities, dispositions, or powers. So, like the Best System Account, there are no genuine laws; there are, at most, the best descriptions of the patterns of the world. But even so, for the antirealist these description are hardly achievable. Because the regularities that arise are local, context-sensitive, and intrinsically not exceptionless[22].
In [Mumford and Anjum, 2011] this situation is cashed out in terms of the aforementioned dispositions. Causation and the appearance of lawful behavior, then, are explained in terms of fundamental dispositional properties. In this sense, they *do* provide an account of what is physical necessity [Mumford and Anjum, 2011, ch.3 and 8], and give an answer about the other two explananda —laws and causation. As such, this would be a great candidate, if it were not for the obvious objection, raised before when referring to the realist dispositions: what are supposed to be these dispositions? The burden of proof has moved somewhere equally mysterious. Perhaps it has not even moved, if according to the case suggested before, probabilistic laws is what dispositions really are. Furthermore, the other objections directed before towards the dispositionalist hold for the antirealist versions too.

21 Of course it could be the case that this apparently unlikely possibility was the necessary inevitable way the world had to be. This, anyway, does not preclude the legitimacy of the reasoning above, involving a landscape of possibilities.
22 For arguments against, see [Hoefer, 2003].

I have recovered the discussion around the dispositional account, this time within the antirealist side, for a specific reason. As well as I have claimed that this dissertation can be seen as a complement to a humean account of laws, likewise it can be a complement to a dispositional antirealist. This can be so since my project might, if correct, provide an answer as to *why there are stable dispositions*. In fact, in chapter 4 is studied how, under certain conditions, probabilistic outcomes of a system —i.e. what we could think of as propensities/dispositions— are obtained from a chaotic or random input. This thesis, then, can be understood as a possible complement to ground the ontology of dispositions.

1.5 COULD SYMMETRIES LET US DISPENSE WITH FUNDAMENTAL LAWS?

This final section takes an initial look at our most fundamental physics and the laws therein expressed. Specifically, given the prominent and ubiquitous role of global and local symmetry principles, this section formulates the following question: Could symmetry principles constrain the space of possibilities (of possible time evolutions of a system) in a way so as to dispense, when accounting for lawful behavior, with fundamental laws? After motivating in section 1.5.2 the explanatory virtues of a positive answer to the question, I set forth in 1.5.3 objections made to such project. However, in section 1.5.4 I cite some contemporary candidates that might fill the desired role. Namely, the so called renormalizable local gauge theories of Quantum Field Theory. The investigation on the relation of symmetries with laws will continue in chapter 2.

1.5.1 *Introduction*

Could symmetries let us dispense with fundamental laws? This section suggests that this can be a plausible scenario; yet, in the way it also raises several flaws that should be fulfilled. As can be guessed, the motivation behind such question comes from the spirit of the whole dissertation of accounting for regular behavior without the assumption of a pre-existent set of governing laws. Section 1.5.2 states the virtues of the move of recurring to symmetries as an alternative to a traditional view of laws. In 1.5.3 I put forward a first negative answer to the project, citing John Earman's reply [Earman, 1993] to Van Fraassen's "Laws and symmetry" [Van Fraassen, 1989]. However, in section 1.5.4 I propose what could be a positive answer: I cite a candidate that (somehow surprisingly) might fill the desired role. That is, the role of a theory constituted exclusively by symmetry principles that determines the time evolution of the system[23]. With such a theory it would be redundant to postulate genuine fundamental laws in order to account for regular behavior. Notably, the candidate proposed in 1.5.4 is found among our best physical theories with empirical support. Namely, among renormalizable local gauge theories of Quantum Field Theory (QFT). However, appealing as this project might be, in chapter 2 I turn again with a negative upshot: roughly, I highlight that the symmetry principles at stake are already too many, too complex and too unnatural as to be taken as any sort of *a priori*

23 That the determination is univocal or not —hence deterministic or not— is indifferent for our purposes. So it is legitimate if the constraint of degrees of freedom by the symmetry principles does not univocally determine *one* time evolution —so that the resulting dynamical evolution is indeterministic.

"*superprinciples*" —the ontological interpretation originally advocated by Eugene Wigner [Wigner, 1980].

1.5.2 *Preferability of symmetry principles over governing laws?*

> *"As far as I see, all a priori statements have their origin in symmetry".*
> H. Weyl — [Weyl, 1952, 126]

Symmetries as super-principles or meta-laws

Decades ago, symmetry principles could have been plausibly considered as some sort of "superprinciples", in the sense of *a priori* truths of our world. Some of the foremost examples are the invariances under time and space translation. As is known, these two global continuous symmetries correspond to two principles of conservation, of energy and linear momentum respectively. Furthermore, the relation of symmetry principles with principles of conservation is generalized by Noether's theorem: for every continuous global symmetry of the Lagrangian there is a conservation law (and *vice versa*). If some principles at all could be assumed as necessary or *a priori*, these could be reasonable candidates[24]. Whoever was puzzled by the mere existence of laws of nature might find a candidate explanation in symmetry principles. One finds mathematical truths stated in those principles —therefore metaphysically necessary truths— grounding the very existence of laws and their stability. This point of view is expressed in [Wigner, 1980] and [Weyl, 1952].

Finally in the way for the logical inevitability of laws?

So, at least *prima facie*, it seems that symmetry principles are deepest principles that cannot be violated in any metaphysically, or even logically, possible world. Under this point of view, symmetry principles could represent the long-sought explanation of the mysterious nature of laws as logically necessary. This is a point of view that always lacked satisfactory arguments in its support, but that has been cherished by many physicists and contemplated since modern philosophy by Leibniz, Spinoza, or Kant, with roots in Plato. Resting on the mathematical foundation of the symmetry principles, an argument seems to be finally glimpsed in support of the necessary logical/-mathematical inevitability of the laws. In spite of the conceivability of different laws —the main argument against the view of laws as logically (or metaphysically) necessary—, the mathematics discovered by modern physics seems to lead us to such metaphysical approach. Furthermore, the advent of more symmetries of a new type, local (also called 'internal') and following the so called gauge principle, has been taken as a sign of the "*elegance of nature*" [Wilczek, 2008, 63][25]. [Martin, 2003, 41] describes this point of view:

24 For a clear survey of the notion of symmetry in the history of physics, see [Lederman and Hill, 2004]. Excellent overviews from philosophers are [Brading and Castellani, 2013] or [Bangu, 2013].

25 The gauge principle specifies a procedure for obtaining an interaction term in the Lagrangian which is symmetric with respect to a continuous symmetry. The results of localizing (or 'gauging') the global symmetry group involves the introduction of

> "the 'gauge philosophy' is often elevated and local gauge symmetry principles enshrined. Gauge symmetry principles are regularly invoked in the context of justification, as deep physical principles, fundamental starting points in thinking about why physical theories are the way they are, so to speak. This finds expression, for example, in the prominent current view of symmetry as undergirding our physical worldview in some strong sense"

and [*ibidem*, p.52]:

> "gauge invariance is often invoked as a supremely powerful, beautiful, deeply physical, even undeniably necessary feature of current fundamental physical theory".

Thus, sophisticated "elegant" mathematics provide a foremost unified account of the fundamental interactions. Further, they have proudly achieved so as a result of conceptual (mathematical) work much before the posterior (solid) experimental support[26]. Moreover, the gauge paradigm exhibits an appealing simplicity in that few inputs are required to specify full theories [Martin, 2003, 53]. This leads to one of the most attractive features of this new physics: its unificatory role. All elementary interactions (though gravitation only in theory) are described in terms of local gauge symmetries.

Interpretations of symmetries

It must be remarked that I am assuming an ontological interpretation of local gauge symmetries, not representing a mathematical redundancy in our description of the world. While my assumption is widespread, the issue is nevertheless unsettled and the alternative interpretations exist since long ago (e.g. in [Wigner, 1967], [Redhead, 1975], or [Redhead, 2003]).

1.5.3 *Symmetries were not enough*

Roughly, part of [Van Fraassen, 1989]'s account can be taken to be representative of the project I have put forth, of symmetries letting us dispense with laws. It can be labeled as a deflationist account: the notion of law is to be substituted by higher, more fundamental, principles of symmetry that can account for lawful behavior.
There is, though, an immediate critique to the completeness of this type of project. [Earman, 1993] attenuated Van Frassen's enthusiasm about the role of symmetries by pointing out that the form of the

additional fields so that the Lagrangian is extended to a new one that is covariant with respect to the group of local transformations. Remarkably, it turns out that nowadays all the fundamental interactions of the Standard Model can be described according to this procedure. This is shown in detail in 2.3.

26 See [Bangu, 2013] or more thoroughly [Bangu, 2008] for the study of impressive historical cases, like the prediction of the Ω^- boson.

laws of motion might follow from symmetry considerations, but the content of the laws does not:

> "Symmetry considerations allow us to deduce important results without knowing the details of the dynamical laws" [Earman, 1993, 5].

But then:

> "The form of the law of motion follows from symmetry considerations; but the content of the law, which is specified by the Hamiltonian, does not. The content may, perhaps, be further delimited by symmetry properties –such as invariance under time reversal and parity" [ibidem].

In sum, the details of the dynamics cannot be deduced from symmetry considerations *alone*. In general, Earman's observation is true, and not only in classical and relativistic physics but also in particle physics: a law is not uniquely determined by the symmetries associated with it. Still, in the two decades that followed Earman's observation, relevant advances within particle physics have taken place. Some, perhaps, might overcome his claim. This is what the next section explores.

1.5.4 *Univocal determination in QFT: Will symmetries be enough?*

As I have said, one of the most salient features of the new physics is the increasing role of symmetry principles in the constitution of the theories. This section puts forward a physical theory that, arguably, overcomes Earman's previous statement recovering the legitimacy of Van Fraassen's original "radical" claim[27]. That is: *a theory whose symmetries univocally determine which laws there will be*. Arguably, this is the case in one of our fundamental most empirically tested physical frameworks, QFT. Specifically, in renormalizable local gauge theories. There it turns out that, once we have specified the relevant symmetry, the theories are uniquely specified for a given matter content [McKenzie, 2013][28]. In fact,

> "on the assumption that the fields concerned are specified, the laws are thereby also uniquely specified but for the values of the constants appearing in them. Determination of these constants is therefore a matter of matching them to experiment" [McKenzie, 2013, p.13].

It is thus remarked an essential link between particle types and symmetries.

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27 'Radical', I would say, only because we are used to other radical version, that of the existence of a finite set of (governing) fundamental laws (no matter if expressed in the lagrangian formalism or in any other formalism).

28 For further details see [Wilczek, 2000] or [Griffiths, 2008, ch.10].

Behind this procedure there is a Lie algebra that determines certain symmetry groups that correspond to the known fundamental interactions. Specifically, the groups are U(1), SU(2) and SU(3) for the electrodynamic, weak and strong interaction respectively. The elements of the groups are embedded in a certain structure, the 'multiplets'. From these multiplets the possible types of particles are univocally determined (see how the particles are graphically represented within the multiplet in figure 1, p. 55).
Crucially, this implies that if we reverse the usual direction of explanation and start by assuming a certain set of particle types, this yields to certain symmetries, and this, in turn, yields to a unique set of corresponding laws. Therefore, it turns out that *if the fields are specified, the laws are also uniquely specified*.
Thus, fundamental laws can be seen as exclusively constituted by symmetry principles[29]. So the former can be accounted exhaustively in terms of the latter. Following this interpretation, laws of nature need not be understood as a set of genuinely fundamental, "preexisting", "governing" laws. What we have now are mathematically grounded principles, so logically necessary truths, that suffice to constrain the time evolution of every elementary field or entity.
Given the ubiquitous far-reaching role of symmetries, we could now explain what grounds the non-accidental regularities and the very notion of law.

The plausibility of the assumption of symmetries, though, is what is going to be discussed and criticized in the next chapter.

29 There is the non-trivial exception of the values of constants appearing in them. This can be interpreted as a threat against the upshot I am defending (the unique determination of laws and symmetries by a set of particle types).

2 NATURALNESS AND FUNDAMENTALITY

Abstract

This chapter reflects upon which requirements, if any, should be met by a theory to be considered as fundamental. Some traditional criteria for fundamentality, naturalness and simplicity, are shown to be inconsistent with certain aspects of our best physics. The argument rests on features of local gauge symmetries that constitute the core of the Standard Model and, crucially, of its candidate successors. It concludes that local gauge symmetries, in spite of their elegance and unificatory power, are non-natural, complex, and far from anything like *a priori* "super-principles", as firstly proposed by Wigner. Then, the chapter assesses how this conclusion bears on the metaphysics of fundamentality. On the one hand, criteria of naturalness and simplicity ought to be abandoned if the fundamental level is structurally similar to our best physics. On the other hand, if no assumption is made about the fundamental level, an alternative metaphysical picture gains plausibility alongside the existing ones, one that would better preserve the criteria of naturalness and simplicity.

Contents

2.1 INTRODUCTION

Framing the chapter in the literature

In order to properly engage with a metaphysics of fundamental laws, this chapter reflects upon which requirements, if any, ought to be met by a theory to be considered as fundamental. This chapter, then, is about metaphysics of fundamentality; as such, it can be framed along with recent literature on this area, e.g. [Sider, 2011], [Schaffer, 2003], [Fine, 2012], or [McKenzie, 2011]. It can alternatively be framed along with the literature that has been reflecting upon the state of theoretical physics, e.g. [Cao and Schweber, 1993], [Maudlin, 1996] [Cat, 1998], [Castellani, 2002], [Shifman, 2012], [Feng, 2013], or [Morrison, 2013].

Of course, an answer sympathetic to some physicists as to what should be required for a (scientific) theory to be fundamental is 'nothing besides empirical adequacy and self-consistency', the guiding idea being that empirical research will provide us with the fundamental theory[1]. Naturalistic metaphysics is supposed to adhere to this attitude. Another common option, however, involves certain metaphysical criteria —whose endorsement is not always explicit— that guide the interpretation of the data and the mathematical structure with which the data is organized: some well known criteria are simplicity or naturalness. In sec. 2.3 I will point out the inconsistency of such traditional criteria with our current best physics. Crucially, the inconsistency also appears in any future candidate that will share certain structural characteristics of the current theories. Specifically, the focus revolves around the local gauge symmetries that essentially constitute the Standard Model and its candidate successors. The conclusion is that, focusing on the form of gauge symmetries, even the allegedly most natural fundamental candidate theories in modern physics are not natural.

First conclusion of the chapter

The argument is independent of, and complementary with, other arguments, laid out on other grounds, that point out the lack of naturalness of certain physical theories. The most salient cases are the fine-tuned values of the constants [Ellis, 2006, sec. G] and the gauge hierarchy problem [Morrison, 2013, 404][2]. Notably, while certain theories solve such critiques to naturalness (for instance, supersymmetry solves the gauge hierarchy problem [Feng, 2013]), all theories are threatened by the arguments put forth in this chapter.

Second conclusion

Then, resting on such conclusion, a second goal of the chapter is to advocate for the plausibility of an alternative metaphysical picture alongside the existing ones. That is, advocate for the legitimacy and

1 Later, when in touch with the current quantum field theories, this viewpoint is more faithfully represented by including the requirement of the so called 'asymptotic safety'.

2 The gauge hierarchy problem concerns the different order of magnitude between the weak scale ($0.1 - 1TeV$) and the (reduced) Planck scale ($2.4 \cdot 10^{18} GeV$). More on this later.

plausibility of an alternative of the fundamental level *and*, additionally, support a specific alternative, which is developed and critically assessed in the next chapter. Drawing an initial landscape of possibilities, the proposal amounts to a third scenario besides other two, which roughly stated are 1) the picture of a *unified* set of *few* and *simple* guiding principles —in the line of Grand Unified Theories (GUT's)—, and 2) the picture directly derived from what the current best physical theories say (leaving aside the vexed philosophical issue about what they say, i.e. what is the interpretation of a physical theory, as well as what is the best of the candidate physical theories) [3].

The specific alternative proposal, in brief, is one that takes at face value the complexity and unnaturalness unveiled in section 2.3 and induces that the dynamics of the fundamental level is highly more complex. This alternative metaphysics of the fundamental has been defended and studied by some physicists ([Wheeler, 1983b], [Weinberg, 1981], [Froggatt and Nielsen, 1991], among others cited in ch. 3) whose results I critically assess in the next chapter. There I also defend the virtues of such alternative for metaphysical accounts of laws of a deflationist sort, like the humean or the antirealist.

Third conclusion

A third conclusion from this chapter regards the modality of laws; it consists in the support to the contingent view rather than the necessitarian, given the link (fleshed out later) between unnaturalness and contingency.

Fourth conclusion

Lastly, a fourth conclusion concerns the philosophical accounts of laws on the market: the presumably unnatural (contrived, whimsical) aspect of the current laws puts pressure on most extant accounts. In a nutshell, what the philosopher has to postulate in his ontology (especially if he is realist about laws, as the necessitarian, the antireductionist, and certain dispositionalists are) is more puzzling and less economical, now that we look at the (complex and unnatural) details.

All in all, the ultimate goal of the chapter is to offer a particular metaphysical landscape of possibilities about the fundamental level, drawing special attention to the dynamics, informed by and consistent with our best physics. In the resulting landscape, one possibility is refuted (i.e. the first scenario above), one is added (the third scenario) and one is maintained though criticized (the second scenario). Before this, the present introductory section and the next section 2.2, characterizing the notions at play, pave the way for a precise discussion.

PSEUDO-PROBLEM? Let me note in this introduction a general objection that could be raised. It can be easily conceived a working

3 A rough overview of some of the main candidate physical theories comprehends: supersymmetry, in its many variants [Feng, 2013], string theory [Becker et al., 2006] (also abundant of different versions, and optionally linked with the multiverse theory [Susskind, 2006]), loop quantum gravity (the canonical and the covariant "spin foam" version) [Rovelli, 2004], and causal sets [Reid, 2001], among others.

Physics without metaphysics?

physicist reluctant of the pertinency of the metaphysical considerations I am putting forward. The attitude I refer to is represented in its extreme version by the quantum physicists that refuse to appraise any interpretation of the (highly puzzling) mathematical and empirical formalism. Not only some physicists but also some philosophers defend not to worry about what they say are only pseudo-problems (Ludwig Wittgenstein being a famous example of this attitude). They could think so for the philosophical questions here raised regarding the nature of laws and symmetries. While I sympathize with an attitude of not creating problems where there are not, there is not a recipe to know where is good or wrong to have this attitude. For instance, it cannot strike me as more unsatisfactory the attitude of giving for granted, without further explanation, what are laws [Maudlin, 2007, ch.2], or what is the objective probability of a stochastic dynamics [Maudlin, 2011, p.2]. I have argued in 1.4.1.2 why these two issues are objectively puzzling and an explanation should be welcomed.

2.1.1 *Fundamentality metaphysics for a scientific realist and an antirealist*

Terminological clarifications

First of all, as to the meaning of the term 'fundamental', I refer to a final physical theory of a bottom level; I assume an ordered hierarchy of ontological levels in which each of them ultimately supervenes on the lowest, called 'fundamental'. Notably, I refer not only to the matter or stuff of the *fundamental* level of reality without reference to its dynamical evolution —as some recent philosophical literature does, e.g. [Fine, 2012], [Schaffer, 2003]—, but also to its time evolution. Mine is the usual meaning among physicists[4] and so it should be among philosophers, especially after the tight interconnection of matter-content with its possible interactions –and so, its time evolution– revealed by our most fundamental scientific research (i.e. by General Relativity and by QFT, as remarked in 1.5).
When I talk about a 'metaphysical picture' (or 'metaphysical image', or 'worldview', or 'scenario'), I will be meaning the ontology and dynamics of the fundamental level of reality (what can also be dubbed as the physics of the fundamental level)[5].
Let me also dispel any doubt as to the fact that my concern is ontological, not epistemological, although my parlance is about 'how a fundamental *theory* should be', for the theory is aimed to describe how Nature should be.

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4 So[Castellani, 2002, 1] characterizes their meaning, too.

5 Interestingly, there is a link always assumed among physicists and naturalistic metaphysicians between high-energies and fundamentality. I am also going to follow such a link; however, worth is remarking that this link is not a (metaphysically) necessary truth (as probably a non-naturalistic metaphysician, say a scholastic, would have rapidly noted).

This being said, I want to start by raising the following question: How does the debate of scientific realism/antirealism bear upon the metaphysics of fundamentality?

A realist and an antirealist on the fundamental level

The discussion among scientific realists and antirealists is not directly addressed as to how the fundamental level is or should be. The main point in their debate is epistemological, addressed towards the faithfulness and reliability of scientific knowledge. The scientific antirealist is clearly in tune with the idea that the latest scientific theories are not necessarily close nor closer to the final truth; therefore, he is agnostic about the fundamental as long as there is no inconsistency with empirical evidence. As to the realist, it is not so clear that he is independent of fundamentality commitments. For he believes that the current best physics is tracking truth with an increasing accuracy and that the true final theory will somehow resemble the best candidates developed so far. Thus, the standard position of the realist is to endorse "literally" what our best physics states (again, leaving aside the quantity of complications involved in this process). This coincides with the second of the metaphysical pictures laid down before.
Still, within the realist framework, is there room for alternative metaphysical pictures as long as they are consistent with current scientific knowledge? Yes; nothing in the realist doctrine prevents these alternatives. Next chapter considers one of these alternative metaphysical pictures, that of a highly complex underlying dynamics. Here I will argue that it is a reasonable candidate for the fundamental level when the best physical theories display a lack of naturalness and simplicity. In such case it could be said that the empirical evidence suggests the overcoming of reading the theory as being exhaustive on what there is; that is, another metaphysical picture stems from the features of current physical theories along with the usual picture based on their literal reading. Crucial for the plausibility of this additional option is that we are referring to *fundamental* theories, and that the interpretation of the current physics is taken to be not fundamental. The idea behind is that the unnatural and the complex features of the current theories suggest that the fundamental level is qualitatively different from what the theories themselves state and, in particular, that the fundamental level can be best described by a highly complex dynamics. Equally valid is a weaker version of both claims, which is to state that current scientific theories just do not rule out such possible fundamental scenario[6].

However, a tension shows up in the case that the realist position is understood as strictly following what the physicist states and the

6 On the contrary, some physicists defend that the final theory has to be very similar to what has been proposed so far, given the astonishing empirical success of the Standard Model. This is another possibility, and I will explicitly take it into account. Let me underline that the alternative defended is also consistent with such empirical success.

physicist states that such scientific theory purports to be fundamental. In such a case the realist presumably has to stick with the default option (the second scenario of p. 37), not seriously considering other alternatives.

Problems for the realist?

This tension can be phrased through the following question: Does the realist have to commit to the allegedly fundamental physical theory *qua* fundamental, or is he entitled to allow other possibilities, as long as they are compatible with the empirical content of such a theory? Let me note, before facing the question, that the pressure that the question raises does not really apply nowadays, because there is, arguably, not any actual physical theory that the scientist flatly considers fundamental. Therefore my forthcoming dicussion is unaffected. Let me answer the question, though, in order to gain some insight into the relation of fundamentality with scientific realism.

The detachment from science of the realist

The answer can be plausibly said to be that the authority in settling the fundamentality of a theory will be physics itself; likewise, though, scientific arguments can be put forth against some candidate(s) [7]. In sum: from the point of view of the antirealist the *legitimacy* of any fundamental proposal is not controversial, as long as it is empirically adequate: it is a valid metaphysical possibility since the antirealist is barely committing to what there fundamentally exists. The realist will have at his disposal any metaphysical picture that commits to the ontology of the best scientific theory (i.e. the observed and the unobserved theoretical entities, as well as the structures unveiled). Then, regarding the alleged fundamentality, there can be scientific arguments (or philosophical arguments informed by science, as you prefer) that entitle the scientific realist to believe that what a scientific theory says does not correspond to what there fundamentally exists. To make this point clearer, think of one specific argument that in fact will be raised later: there is a large order of magnitude between the Planck scale (arguably assumed to be the lowest possible scale) and the scales probed in high-energy particle physics. From the electroweak $W^{\pm}$, Z^0 scale of 10^{-18}m to the presumably fundamental Planck scale of 10^{-35}m there are 17 orders of magnitude, a change in scale similar to that in passing from macroscopic physics, at the 1m scale, to the electroweak scale [Froggatt and Nielsen, 1991, 12]. This is in fact labeled as the 'big desert'. Current fundamental theories are silent about what happens in such energy levels. Notably, it is rather uncontroversial among physicists that is implausible "*(...) to believe that there is no new physics and we completely understand everything exactly up to scales like the GUT or Planck scale. Fan of the Standard Model that I am, that's too much for even me to swallow as plausible*" [Woit,

7 It becomes somewhat obscure whether these sort of arguments, appealing to scientific facts, pondering one choice or other, are philosophical or scientific.

2013][8]. In conclusion, the realist is entitled to believe that there are more *fundamental* phenomena lying in such a 'big desert' range scale.

2.1.2 *Naturalness*

An aim of the present chapter is to bring to the fore of metaphysical discussions the notion of naturalness, mostly discussed in theoretical physics. Such notion can shed some light on issues of fundamentality and, as argued later, on the modality of laws.

During the last decades until the present day there is a discussion in physics regarding the naturalness of candidate fundamental theories. This is mainly related with the apparently *ad hoc* and fine-tuned values of many parameters. A constant is usually settled to a value *ad hoc* because of what has been measured experimentally, ignoring why it is that it has such value. Most theories in history of physics contain such constants, basically enabling the model of the theory to fit with the empirical data.

The several definitions of the term

In intermediate levels, an explanation of their values can be provided (by appeal to lower levels), but the issue becomes more puzzling when dealing with fundamental theories.

The fine-tuning, on the other hand, refers to the fact that the specific value of some of those constants cannot *slightly* change without dramatically contradicting the way the universe actually is[9]. Both characteristics are considered unnatural for a fundamental theory (cf. [Susskind, 2006]). Here I will leave aside the fine-tuning sense to center around the first sense, related with contingency[10].

Unnatural, in general, is often related with being very unlikely [Ellis, 2006, sec.G], [Penrose, 1989, 343]. However, whether it makes sense to talk of 'likely values' of fundamental constants is (also) a disputed matter (e.g. [Smeenk, 2013, 27], [Albert, 2012, 28], [Myrvold, 7]). Thus I will be neutral on this link, for one can arguably recur to non-naturalness without committing to unlikeliness.

Once the fine-tuning sense is excluded, there is still not a straightforward officially accepted definition of 'naturalness'. Not the least, the loose meaning of the term in natural language allows for a wide variety of uses.

All in all, the idea behind is, as said above in other terms, that of not putting in by hand the parameter [Borrelli, 2011].

Not putting in by hand the parameter

In the context of particle physics, some have tried to flesh out the idea

8 On the opposite side, one can extend the validity of the Standard Model to such a domain by an inductive reasoning, based in the fact that the tested validity of the model is of 30 orders of magnitude on the energy scale (from 10^{-18}eV of the upper limit of the photon mass, to the 10^{+12}eV tested at LHC) and so an induction to the remaining 17 orders of magnitude might be justified.

9 It can be said that the universe seems to *sensitively supervene* on the values of those constants.

10 It is disputed whether the fine-tuning is not really a mislead criterion (see e.g. [Mosterín, 2004]).

with precision. To begin with, some take naturalness to hold when the free parameters or physical constants take values of order 1. More precisely, all the terms in the effective action that preserve the required symmetries must appear with coefficients of order 1 [Seiberg, 1993]. A weaker version is to maintain that the values of the different parameters ought be of the same order of magnitude. [Borrelli, 2011] overviews the different definitions in recent physical literature. According to her, the origins of the definition of 'natural' date back to [Georgi and Pais, 1974]:

> "In a theory with spontaneously broken symmetry (...) the masses and coupling constants appearing in the Lagrangian will not be independent phenomenological parameters. Rather there will be zeroth-order relations between these quantities, the corrections to which are finite. We will call such relations 'natural'".

Another sense is that defined by Gerhard 't Hooft in [Farhi et al., 1982, ch.24], the idea being that a parameter is natural as long as it can be reduced to 0 by the introduction of a symmetry [Wilczek, 2000, 6] (for example, the parameter labeled θ in a simplified version of QCD is set to 0 and this is "*tantamount to assuming the discrete symmetries P or Q*" [*ibidem*]).

The specific sense of the term in this chapter

From the latter definition follows the widespread idea that the introduction of a symmetry is not unnatural. The rationale motivating this move is based on the fact that it removes an unnatural parameter by means of the introduction of a dynamical principle. However, a further step characteristic of my analysis is that I will focus on the unnaturalness not of parameters but of the structures of the theory, that is, of the terms constituting the Lagrangian corresponding to fundamental interactions, which turn out to be expressed in the mathematics of Lie groups and algebras. Specifically, in this chapter 'non-natural' will refer to a *puzzling ad hoc contingency*. Puzzling because no reason is provided as to the choice of a feature of a theory, among a large space of possibilities, other than the match with experiment. And the features scrutinized, as I said, will be the symmetries that constitute the mathematical formulation of the theory.

Hence, worth is remarking that this is a new stronger sense, in the vicinity of the previous ones, but not to be necessarily endorsed by those who endorsed them. There is a specific goal in this new sense; the goal is to underline that the Standard Model and all candidate theories on the market, are messy and contrived, and therefore they all turn out to be far from any of the traditional metaphysical criteria for fundamentality (namely: naturalness, simplicity, unification, aprioricity, or necessity). How to deal with this tension constitutes another argument of this chapter.

One might find more, or only, problematic the unnaturalness of parameters, accepting the *ad hoc* contingency of symmetries. According to what is argued in this chapter, the resolution of an unnatural parameter leaves us with a still puzzling situation —because its resolution leaves us with new unnatural symmetries (following 't Hooft's characterization). While it is appropriate to bear in mind that there is also the unnaturalness of parameters, the new focus is important because, unlike the unnaturalness of parameters, that referred to gauge symmetries will be shared by any future account exhibiting such symmetries (e.g. any one that solves the unnaturalness of parameters by the introduction of symmetries and, in general, any future account resting on the gauge principle).

The argument applies to all current fundamental physical theories, notably also those allegedly most natural, like the Minimal Supersymmetric Standard Model (MSSM).

There is, thus, an argument directed towards the very idea of gauge symmetry, to be understood as something far from anything like *a priori* or necessary superprinciples, its elegance and unificatory power notwithstanding.

Regarding my specific use, this fragment from [Hossenfelder, 2013] conveys its "raison d'etre" (she uses the term 'finetuned' in the sense I talk of 'unnatural'):

> "the definition of finetuning itself is unnatural in its focus on numerical parameters. (...) the theories that we use are finetuned to describe our universe in many other ways. It's just that physicists tend to forget how weird mathematics can be (...) We work with manifolds of integer dimension that allow for a metric and a causal structure, we work with smooth and differentiable functions, we work with bounded Hamiltonians and hermitian operators and our fibre bundles are principal bundles. There is absolutely no reason why this has to be, other than that evidence shows it describes nature. That's the difference between math and physics: In physics you take that part of math that is useful to explain what you observe. Differentiable functions, to pick my favorite example because it can be quantified, have measure zero in the space of all functions. That's infinite finetuning. It's just that nobody ever talks about it".

Last, but not least, the fact that recent experiments in LHC are discarding the most natural versions like MSSM bolsters my point. At the end of the chapter I will return to this.

An objection

At this stage, one might raise an objection: given that most, if not all, scientific theories have displayed such a non-natural structure, one might wonder how a theory, *even if fundamental*, could ever possibly be no non-natural. That is, in a sort of (too optimistic, I would say) induction, one concludes that any fundamental physical theory will be non-natural as defined above. Is this a compelling reason to neglect such criterion as here idiosyncratically defined? No, this induction does not imply that this criterion should be neglected; one could perfectly maintain that unnatural scientific theories have been perfectly accepted so far because they were not thought to be fundamental. An indisputable testimony of this thought is the widespread endorsement of Grand Unified Theories. It is implicit in the very motivation for a unified theory the dissatisfaction with the current form of laws. I am on the one hand spelling out this dissatisfaction, and on the other hand I am pressing towards a direction different from that of the unified picture.

Another objection

LOCAL GAUGE SYMMETRIES In front of this initial overview, a skeptic reader might properly raise another objection: how could one say that current particle physics is *ad hoc* while some of its most salient characteristics are the *theoretical* prediction of new particles much before any experimental evidence and a unificatory strength resulting from (almost) all interactions being under the sway of the same principle (the local gauge principle)? Not to say the beauty of the sophisticated mathematics describing the elementary fields/particles and their interactions (the Lie groups and algebras). Furthermore, the gauge paradigm exhibits an appealing simplicity in that few inputs are required to specify full theories [Martin, 2003, 53], and *"the fact that all non-gravitational interactions fit into the gauge framework then lends this simplicity to a large part of fundamental physics"* [*ibidem*]. All these have been central reasons for the enthusiasm towards this new physics and, more specifically, for the judgments of elegance and of (a vaguely stated) necessity. This attitude has been described already in 1.5.2 p. 31.

Contrary to this positive attitude towards the symmetry principles, I will point out that, in spite of their astonishing theoretical and empirical success, their resulting quantity and form should induce in us a suspicion of such theories *qua* fundamental theories, due to their puzzling *ad hoc* contingency, i.e. due to their non-naturalness. This is one of the reasons to advocate in 2.4 for a consistent but *qualitatively different physics at the fundamental scale*. Among the possibilities, an option that would preserve the naturalness requirement is that of a fundamental highly complex dynamics, as argued in 2.4.

Additionally, two other points follow from this tension. One —the third conclusion of 2.4— regards the higher pressure onto philosophical accounts of laws, since the postulation of such laws in their ontology now becomes more mysterious, due to their contrived form. The fourth conclusion of 2.4, regarding the modality of laws, is the support for a contingent account rather than a necessitarian. This last observation threatens something defended in the previous chapter, i.e. the necessary character of an account of laws in terms of symmetry principles. Conversely, many layers of contingency in QFT will be highlighted.

Regarding the ontological interpretation of symmetries, if one takes local gauge symmetries to be mere mathematical redescriptions void of physical content, as it is contended in the references cited in 1.5.2 p. 31, makes the first of the conclusions irrelevant for the rest. That is: it does not matter whether gauge symmetries are neither natural nor simple (the first conclusion), given their lack of physical significance. The second conclusion, the proposal of an alternative metaphysical picture, was made in reaction to the unnaturalness of gauge symmetries together with the preservation of such criteria. So the second conclusion also loses steam. Likewise the third and the fourth aforementioned. Thus, the present chapter defends the conclusions under the scenario in which local gauge symmetries do have an ontological "active" role (paraphrasing Wigner) and do provide physically significant claims about the carvings of Nature (which, I would say, is the standard interpretation).

The interpretation of the symmetries

2.2 SOME CRITERIA FOR EVALUATING A FUNDAMENTAL THEORY

In this subsection, certain criteria about what should count as a proper fundamental theory are made explicit.
There are criteria narrowed to the domain of contemporary physics. For instance, since the advent of the interpretation of physical theories as effective (field) theories (something I will talk about in next chapter), a quantum field theory can be fundamental only if displays what is called as 'asymptotic safety'. It means that in the ultraviolet regime all the coupling constants that appear must possess a finite value; that is, the quantum field theory has to be well defined at all energies without being perturbatively non-renormalizable. This is tantamount to saying that a quantum field theory is asymptotically safe if it corresponds to a trajectory of the renormalization group that ends at a fixed point in the UV regime. This is, arguably, not really a metaphysical criterion but a technical one; either way, it is one of the few criteria that has to be respected also by those within the second scenario drawn in 2.1 (p. 37).

UNIFICATION AND SIMPLICITY Yet, other criteria are at stake. For instance, for those who defend the first of the metaphysical pictures of p. 37, i.e. those who advocate for a *unified* metaphysics[11]. Especially in the last decades there has been a search for grand unified theories, labeled as 'GUT' ('Grand Unified Theories') or 'ToE' ('Theories of Everything') depending on the scope. The drive that guided this search is explained e.g. in [Weinberg, 1992] or [Greene, 2011]. Some of the

Unification

11 My discussion is obviously not exhaustive of all the possible criteria. Basically, my attention revolves around naturalness and simplicity, with mention of the related necessity/aprioricity and of unification.

recent precursors can be traced back to Felix Klein, Sophus Lie, Hermann Minkowski, Hermann Weyl, Henri Cartan, Albert Einstein and Werner Heisenberg[12]. This metaphysical view is nurtured by *"the unreasonable effectiveness of mathematics in the natural sciences"* [Wigner, 1980]. The target of this chapter is a subset of those who believe in a final simple unified theory of everything; namely, those who, "too much" nurtured by current mathematics, find hints that we are approaching a fundamental theory because of the virtues displayed by symmetry principles.

Simplicity

Related but different to unification there is the criterion of simplicity, not to be confused either with naturalness. The latter has been spelled out before, and I will come back to it later. The former, simplicity, is the well known criterion secularly followed in scientific theory choice, in line with Ockham's spirit. It is fair to say that most metaphysics across the history of science and philosophy have entertained a world ruled by *few* and *simple* rules or principles. Take, for instance, Kant's metaphysics [Kant, 1786] according to which *two* opposite forces are ultimate responsibles of all motion. Likewise, scientific research has been historically inspired by the criterion of simplicity. This can be translated both to the expectation of *few* guiding principles and of simplicity in the principles themselves; this amounts to a few number of equations of motion and each involving few degrees of freedom (the simplicity thus refers to the equations, not to the solutions of the equations)[13].

It is well known the vagueness and relativity of 'simple' when comparing certain competing theories. Likewise, it is disputable that this is a reliable criterion, insofar as the real world need not be simple at all nor, especially, describable by a simple theory. The latter is, in fact, a moral drawn in this chapter (i.e. it makes sense to believe that the world is not guided by few simple principles, but rather by a highly complex dynamics). In fact, while sec. 2.3 argues that the current theories are not simple nor natural, ch. 3 considers that this is so *because the true fundamental dynamics is not simple nor natural*. Thus this chapter motivates the rest of chapters of the dissertation, all of them assessing scenarios of a fundamental highly complex underlying dynamics —as elaborated later, this high complexity, in the limit,

12 I follow here [Cao and Schweber, 1993, 74].

13 Aside from this trend, a less frequent metaphysics is that of a cosmos subject to chaotic, random behavior. This "opposite" option is in clear minority —cf. [Bohm, 1957]. An exponent of this is C.S. Peirce's evolutionary cosmology (see annex B), or any theory that argues for the emergence of order from "chaos". Yet, few literature seems to have provided substantial arguments in its support. Most if not all of the new attempts resting on the insights of 'chaos theory' do not get rid of an implicit (deterministic) dynamics; theirs is an account of order from deterministic chaos, not from randomness (e.g. [Wolfram, 2002], [Prigogine and Stengers, 1984]). The references cited later and the specific arguments along the dissertation aim to hopefully begin to fill this gap.

is more reasonable to associate it with a truly random dynamics, i.e. with the lack of guiding laws.

NECESSITY AND NATURALNESS Independent of the criteria of unification and simplicity, another *desideratum*, probably too strong, is that a fundamental theory (or at least, part of it) be *a priori* true. The extent to which this is a meaningful demand is critically discussed in 3.2, following a discussion between John Wheeler and David Deutsch. It could be less controversial if there could be informative *a priori* truths. While aprioricity belongs to the field of the epistemology, the motivation of its demand comes from its alleged relation with the metaphysical notion of (logical or metaphysical) necessity[14]. The philosophical accounts of fundamental laws as necessary in some sense (logical or metaphysical) have been discussed in ch. 1. Thus, another (strong) criterion is that the theory be necessary, irrespective of whether its discovery is *a priori* or *a posteriori* (a possibility also brought to light by [Kripke, 1980]).

Aprioricity / necessity

Then, there is in the vicinity another *desideratum*, the already introduced notion of naturalness. Among all the senses spelled out before there is a common trait: some feature of a theory is required to be natural because it is assumed the existence of *a reason* underlying such feature. Thus, it is taken to be unnatural the choice of a certain feature, among a large space of possibilities, with no more reason than empirical adequacy. In brief, naturalness, for all the senses spelled out before (p. 41) can be arguably considered a weaker version of the demand of necessity[15]. I already introduced a specific definition of unnaturalness in terms of *ad hoc* contingencies, the opposite of necessity, which renders more evident the previous link.

Relation between 'necessary' and 'natural'

RECAPITULATION A recapitulation of the criteria at stake in this chapter is the following. To begin with, a requirement in a joint version could be:

(A): 'A candidate fundamental theory requires further explanation if it is not natural nor simple'

When it is said that further explanation is required it is meant that the theory does not possess the expected properties of a fundamental theory, so it is not really a proper candidate, and a better theory ought to be sought. It is appropriate to divide (A) in two different

14 *Pace* the distinctions famously brought to light since [Kripke, 1980]. The idea is that a scientific theory knowable *a priori* would be necessary —true in this and any other possible world—, in spite of the meter bar postulated in Paris being a contingent *a priori* truth. Anyway, whether this link is correct or not will not affect the central discussion.

15 The primary sense employed in this chapter can be thought to be idiosyncratic (even if meaningful, clearly defined, and in the vicinity of the usual senses). This is why I remark that the present observation applies to all senses.

claims, one for naturalness and one for simplicity, so:

(A1) : 'A candidate fundamental theory requires further explanation if it is not natural'

And:

(A2) : 'A candidate fundamental theory requires further explanation if it is not simple'

Finally, another criterion, more disputable:

(B) : 'A fundamental theory suggests further explanation if it is not *a priori* true'.

Arguably, [Wigner, 1980] endorsed something like (B). As said before, given the definition of naturalness, there is a connection between (A1) and (B), being the former a less demanding version of the latter[16].

While these ancient criteria are reasonable, at least intuitively, the aim of the next section is to highlight the inconsistency of all of them with current physics. This is done by focusing the attention to the unnatural structural characteristics of the current type of physical theories, essentially grounded on the gauge invariance principle; thus one is lead to conclude the inconsistency of the fundamentality of such theories with (A1) (therefore with (A) and (B)). It is also claimed the inconsistency with (A2)[17]. A resolution of the inconsistencies faces alternative possibilities. One is to hope that future theories will achieve more simplicity and naturalness. But crucially, given the sense of non-naturalness at stake, also future theories sharing the same structural characteristics will lack the criteria of fundamentality of (A1). Re-

16 It might help to schematize the statements in first order logic. They correspond to:
(A): $\forall x(\neg(Nx \land Sx) \to \neg Fx)$
Or a weaker version of (A) (I think that both are reasonable) would be:
(A'): $\forall x((\neg Nx \land \neg Sx) \to \neg Fx)$
where the domain of variable 'x' ranges over theories (I am remaining neutral as to how to characterize theories), F is the property of being fundamental, N is the property of being natural, S is the property of being simple.
As to (B), let's assume that something knowable *a priori* entails that it is independent of the actual world and holds in every possible world, therefore it is metaphysically necessary, defined with $\Box$. Thus, we could state a variant of (B) like:
(B'): $\forall x(Fx \to \Box x)$
So aprioricity, here conflated with necessity, implies that the fundamental theory will be the same in every possible world, as something like the logically unique self-consistent possibility. It does not imply the existence of a unique possible world, since laws need not be deterministic, and the initial conditions can also vary. Anyway, it is obviously not a trivial demand. Regardless of whether it is or not a path worth pursuing, I will not discuss such a strong requirement. Still, remember that a defense of a substantive degree necessity (but not of aprioricity) of current physical laws has been elaborated in the previous chapter.

17 As can be noticed, I am assuming as meaningful the idea of simplicity in absolute terms, not only in relative terms.

garding the belief about the structural similarity of future candidate fundamental physical theories, cf. [Weinberg, 1992, 242] describing a widespread attitude:

> "Although the standard model was clearly not the whole story (since it left out gravity, and also relied on a number of arbitrary parameters), its theoretical success and many empirical confirmations made it quite natural to expect fundamental physical theories to have the shape of a renormalizable quantum field theory".

If this is the case, the compatibility with such criteria is undermined.

To frame the discussion remember that, on the opposite direction, the previous chapter argued that there are some reasons to think that QFT displays especially natural and even necessary features. However, the goal of this chapter is to show that QFT exhibits a high degree of unnaturalness. This means that there are two different dimensions in the analysis of QFT that suggest opposite conclusions regarding the modality of its laws. Does this new dimension of contingency trumps or supersedes the previous one, undermining any necessity of such laws? This will depend on what is assumed, as will be assessed in chapter 3.

This inconsistency is the first of the conclusions stemming from 2.3. It is uncontroversial to those who have never endorsed (A), (A1), (A2) or (B), but there have been good reasons to believe in them and many (if not most) scientists and philosophers have been on this side, as previously remarked (p. 45).
Parallel to my argument, some physicists press on the non-natural values of the fundamental constants (e.g. [Susskind, 2006] or [Kawai and Okada, 2012]). Hence, this chapter is in tune with them while the arguments for the non-naturalness are of another kind. Here the attention is driven to properties of gauge symmetries, whereas they focus in the unnaturalness and fine-tuning of parameters. Not least, the proposals of solution differ; roughly, they appeal to a multiverse whereas I will not[18].

The second conclusion stemming from the state of affairs described in 2.3 has to do with the resulting landscape of metaphysical pictures that can be considered after abandoning one of the most noteworthy, i.e. the option (1) of p. 37, inspired by the criterion (A). In particular, it is defended the addition to the landscape of a new possibility along with the remaining ones, a possibility plausible and consistent with current physics, indeed a reasonable continuation. A remaining option (number (2) of p. 37) was that of those who do not appraise this sort of metaphysical considerations and take at face value what the

18 Strictly, they do so because they are also concerned with the problem of fine-tuning of constants. I am leaving it aside, so if this is a substantial problem their strategy is better in this respect.

best physical theory is currently saying (an option widely extended in the philosophical community [19]). Other extant options in the scientific literature that instead react to the unnaturalness are the aforementioned multiverse or the cosmology of an eternal chain of big bangs and big crunchs (*a la* Whitehead's epochs).
The legitimacy of a new possibility in the landscape is appealing for those forced to avoid options that coincided with criteria like (A1) (due to the unnaturalness described in 2.3), but also not content with the unnaturalness (unlike option (2)). In addition to the general defense of a new possibility, a specific characteristic is proposed. Essentially, as already advanced, this option goes in the opposite direction of (A1) and (A2) to its ultimate consequences, postulating a scenario of high dynamical complexity (being neutral about the fundamental entities, if any). From this fundamental level the simpler stable actual laws and symmetries presumably emerge in higher levels. As has been said, the next chapter critically assesses how this scenario has been entertained by certain physicists, and the rest of chapters assume this complex underlying level and study to what extent simpler stable regular behavior could *in fact* emerge.

2.3 ELEGANT BUT NON-NATURAL LOCAL GAUGE SYMMETRY

2.3.1 *Widening the frame*

The present section argues for the contingent and *ad hoc* character of local gauge symmetries. To frame the discussion with the dialectics of the end of the previous chapter, consider the scenario entertained in 1.5.4 : our universe is not genuinely constituted by any mysterious set of fundamental laws. Yet, the scenario is able to account for lawful, regular behavior, given that symmetry principles determine the fundamental interactions and so constrain the time evolution of every elementary entity. Renormalizable local gauge theories of QFT have been shown to be a real example of this scenario —an example from one of our best empirically tested physical theories. Appealing as this might be, the description of the symmetries carried out along this section threatens such a view.
The univocity between matter-content and symmetry principles — hence laws— hinted that the account was of a necessitarian sort, that is, the necessity of actual laws was justified because of the inevitable *univocal determination* of the specific gauge symmetries, given a certain matter-content. This necessary character is especially appropriate for the completeness of such scenario as a *philosophical account of laws*, for if symmetries are neither necessary nor *a priori*, their lack of explanatory power is unveiled: neither necessary nor *a priori* symme-

19 The problems of interpreting what theories say, underdetermination, etc. are, of course, other independent (philosophical) problems.

tries can be hardly considered as undisputable irreducible primitives of an ontology. So it becomes pressing to answer questions about the nature of the *explanans*, i.e. about the nature of symmetries. In such a case, then, it turns out that *symmetries also demand explanation*. This section threatens such a "necessitarian" interpretation and therefore its associated virtues as a metaphysical account of laws. Whether this obstacle can be dealt with is a matter for the next chapter. I advance that two options will be proposed: one that posits the matter content as a brute fact from which the symmetries might be derived, and other that aims to explain the contingency of symmetries as emergent from a more fundamental non-symmetric, highly complex, level.

To show this prior general need of explanation is a main goal of this section. It is an important goal because nowadays the widespread attitude in physics and philosophy of physics is to assume bigger symmetry groups in higher energy scales, without any worry as to their explanation. Roughly, the worries revolve around the process of (spontaneous) symmetry breaking towards lower energies, whereas the other way around, towards higher energies, symmetries are assumed to be restored. In fact, in the compendium [Brading and Castellani, 2003], in the encyclopedia entry [Brading and Castellani, 2013], or in the handbook chapter [Bangu, 2013] any demand of explanation of the highly symmetric picture assumed at high energies is barely cited.
The underlying rationale connects with what [Weinberg, 1981] once illustrated, which I already introduced in 1.3. He envisaged two paths that scientific inquiry could take: explain symmetries or explain their absence. The conventional attitude laid out above corresponds to the second of Weinberg's branches, i.e. physics has to explain their absence. Quoted already in 1.3, [Froggatt and Nielsen, 1991, 3] remarked:

What is to be explained, symmetry or asymmetry

> "this philosophy amounts to postulating the observed gauge symmetry group, since the symmetry is only explained by the existence of an even bigger gauge symmetry groups, which is itself not explained".

Next chapter 3 takes side with the first of Weinberg's branches, that in which symmetries are what requires explanation and what is *natural*, what is assumed, is an originally asymmetric, "chaotic", state (cf. the same attitude in Peirce's words in annex A).

Regardless of the particular success of the next chapter I think that, in general, it is worth pursuing this appealing strategy of understanding laws through symmetries, especially after the hardly disputable lack of answers in the extant philosophical accounts, as argued in chapter 1.

That said, let's proceed to a specific case study of one of the several local gauge symmetries, and then generalize the conclusions to the whole picture.

2.3.2 *Non-naturalness of color strong interaction*

Take this specific case: the color local gauge invariance of quarks. This corresponds to the symmetry group SU(3) (the Special Unitary group of degree 3)[20]. This group is the mathematical description which represents one of the fundamental interactions, the color strong interaction[21]. Let me stress, through the description of how this force works, the whimsical aspect of both the force itself and the mathematical structure that describes it. More precisely, with 'whimsical' I mean its non-naturalness as defined in 2.1.
The background is this: the mathematical description SU(3) has been chosen following empirical adequacy and consistency with the rest of theories of particle physics, constituting a highly unified model of the fundamental interactions. Unified mainly because the same principle, the local gauge principle, is shared by every model of each type of field/particle [22] [23]. More in detail, the theory of Quantum Chromo Dynamics (QCD) successfully describes the color strong interaction: the property of color is conserved due to a certain type of bosons (force carrier particles) called gluons, exchanged in the interactions[24]. Each of them carries one unit of color and one unit of anticolor. Thus they guarantee the conservation of the initial color that changes in the quark in a 'strong' interaction with another quark. To preserve the color eight gluons do the work. The structural representation of (the properties of) those eight gluons is the symmetry group SU(3). The gauge fields/particles are associated with a set of vectors that are the so called 'generators' of the group. All this representation is translated into a new term in the lagrangian so that the lagrangian becomes invariant under the operations of the group. Later I will write down the specific generators of SU(3), correspondent with the gluons.

WHIMSICAL NATURE The first observation regards the *eight* gluons that appear to exist in Nature. It is not a number so specific as the fine-structure constant $\alpha = 7.29735257 \cdot 10^{-3}$ nor it is fine-tuned but, even so, it is the first of the features I want to remark regarding the non-natural aspect of this fundamental interaction. An immediate

20 For a more detailed presentation of the mathematical machinery behind see [Griffiths, 2008, ch. 8] and [Robinson et al., 2008, Part II - Algebraic foundations (esp. 2.2.15)]. A more comprehensive guide is [Cottingham and Greenwood, 2007, ch. 16]. I will introduce only the essential technicalities in due course through the presentation of this particular case.

21 Formally, SU(3) is a real group in complex dimensions of degree N=3 of the classical Lie groups. The dimension of SU(N) groups, as real manifolds, is $N^2 - 1$. Therefore for SU(3) the dimension is 8.

22 See footnote 25 of ch. 1.5.2 for a definition of the principle.

23 More precisely, some global models have solid empirical support but are not so unified as to, for instance, incorporate gravity, while others are more unified but lack empirical evidence. The Standard Model and Supersymmetry are the respective examples.

24 Whether I refer to fields or particles is irrelevant for our purposes.

reply is just to flatly deny any specialness or non-naturalness: eight is a number as any other, and this observation should be no more than a marginal worry, probably solved in the future by a better theory. Those who accept the contingency of laws would be in this side, without the need of any future solution. Further, a bit of basic (and I would say, acceptable) numerology attenuates any worry, as '8' is not any special number —as a high prime number might be— but is a power of 2. This reply can be perfectly the case, and further arguments will be added to consider the strong interaction as non-natural, but the observation is perfectly legitimate and is a frequent rationale in theoretical physics[25]. In fact, one can start to be suspicious of this immediate reply when bears in mind that the discussion is about *fundamental* theories —the criteria (A2) and (A1) above were in fact only referred to fundamental theories— and then one compares with the sort of simple unified metaphysical pictures drawn by scientists, as cited before (2.2 p. 45). Likewise, philosophers have never in history drawn a metaphysics minimally similar to *eight* particles accounting for *one* of *three* fundamental interactions. And less clear appears such a reply when we will take into account the global picture, with the "zoo" (as it is sometimes dubbed) of types of elementary particles plus force carriers of the Standard Model. So, according to principles like (A1) or (A2), as a part of a *fundamental* theory the eight gluons do not look like part of the expected type of story. This observation purports to be the first to bolster the case that, even if elegantly described within a unified symmetric pattern, the theory hypothesizing eight gluons to describe one of the three fundamental interactions does not seem to be a *natural fundamental* theory.

The prospect of a future unification is what might resolve the tension with both (A1) and (A2), because an unification of the several interactions would be obviously simpler, and perhaps also more natural.

However, this hope has been seriously undermined in extensive studies of philosophers of physics: in Maudlin's "On the unification of physics" [Maudlin, 1996], in Margaret Morrison's book "Unifying Scientific Theories: Physical Concepts and Mathematical Structure" [Morrison, 2000] and in her recent handbook chapter "Unification in physics" [Morrison, 2013].

The next observations differ from the previous one in that they point out not how Nature appears to be but how it is described. They are introduced through a second objection to the first point: it could be said that the eight bosons of the strong interaction, even if seeming a contingent and non natural number, are nevertheless gathered in a single mathematical structure (the multiplet of the adjoint representa-

25 As this fragment from [Lykken and Spiropulu, 2014, 36] recognizes: *"(...) there are three different types of leptons (a type of fermion): the electron, muon and tau. Why three? Why not two, of four or 15? (...) Theoretical particle physicists spend a lot of time thinking about such questions"*.

tion of SU(3)) that exhibits their hidden unity, beauty and naturalness. So the eight gluons should not be a worry. The detailed description of how the interaction is mathematically represented should refute this objection, contrary to a widespread belief (ref. 1.5.2 p. 31)[26].

So let's look more in detail how the interaction is described. As previously stated, each gluon, to preserve the internal symmetry, carries one unit of color and one of anticolor (the "colors", 'red', 'green', and 'blue', name the charges of this interaction). Therefore, there are nine logically possible combinations of the 3 colors: $r\bar{r}$, $r\bar{g}$, $r\bar{b}$, $b\bar{r}$, $b\bar{g}$, $b\bar{b}$, $g\bar{r}$, $g\bar{b}$, $g\bar{g}$[27]. This, *in principle*, amounts to the possible existence of *nine* types of gluons [Griffiths, 2008, 284]. The mathematical structure describing the interaction is, as said before, the symmetry group SU(3), the Special Unitary Group with a representation in three complex dimensions. Every symmetry group has the so called 'representations'. It is always the 'adjoint representation' that describes the force carriers, in this case the gluons[28]. The adjoint representation of SU(3) is not nine but eight dimensional. In this representation the nine states are structured in an octet and the other is a singlet element apart. The state of a particle is given by a vector in a vector space on which elements of SU(3) act as linear operators. The linearly independent base vectors that constitute the octet are:

$$
\begin{aligned}
|1\rangle &= (r\bar{b} + b\bar{r})/\sqrt{2} & |5\rangle &= -i(r\bar{g} - g\bar{r})/\sqrt{2} \\
|2\rangle &= -i(r\bar{b} - b\bar{r})/\sqrt{2} & |6\rangle &= (b\bar{g} + g\bar{b})/\sqrt{2} \\
|3\rangle &= (r\bar{r} - b\bar{b})/\sqrt{2} & |7\rangle &= -i(b\bar{g} - g\bar{b})/\sqrt{2} \\
|4\rangle &= (r\bar{g} + g\bar{r})/\sqrt{2} & |8\rangle &= (r\bar{r} + b\bar{b} - 2g\bar{g})/\sqrt{6}
\end{aligned}
$$

and the singlet element is:
$|9\rangle = (r\bar{r} + b\bar{b} + g\bar{g})/\sqrt{3}$
The combination $r\bar{r} + b\bar{b} + g\bar{g}$ is not verified in experiment [Griffiths, 2008, 285]. Later I will come back to this detail. Thus, the eight gluons that exist in Nature are described by the eight so called 'generators' that compose the octet, the set of linearly independent vectors above

26 Let me repeat, for sake of clarity, where my argument is to be framed, as said in p. 44 and in 1.5.2 p. 31. I am not interpreting gauge symmetries as redundant mathematical structure without ontological significance (as e.g. [Redhead, 2003]). Within this point of view it immediately follows the lack of any necessity or naturalness. I am assuming a substantial ontological status to symmetries, then criticizing any naturalness, necessity or *a priori* reasonableness.

27 More exactly, the logical possibilities have been constrained to the nine pairs because a neutral combination of the 3 particles is expected. This is so because baryons, the particles composed of three quarks, appear to be color-neutral.

28 An adjoint representation of a Lie group G is one of the ways of representing the elements of the group as linear transformations of the group's Lie algebra, where the elements constitute a vector space (this is what a representation is in general). Specifically, the adjoint is the representation in which the structure constants themselves form a representation of the group. (And the 'structure constants' of a Lie group determine the commutation relations between its generators in the associated Lie algebra).

that form a vector base of the 8 dimensional group SU(3)[29]. Each generator aims to represent the color state of a certain type of gluon[30]. The situation is beautifully illustrated in figure 1. The octet of the

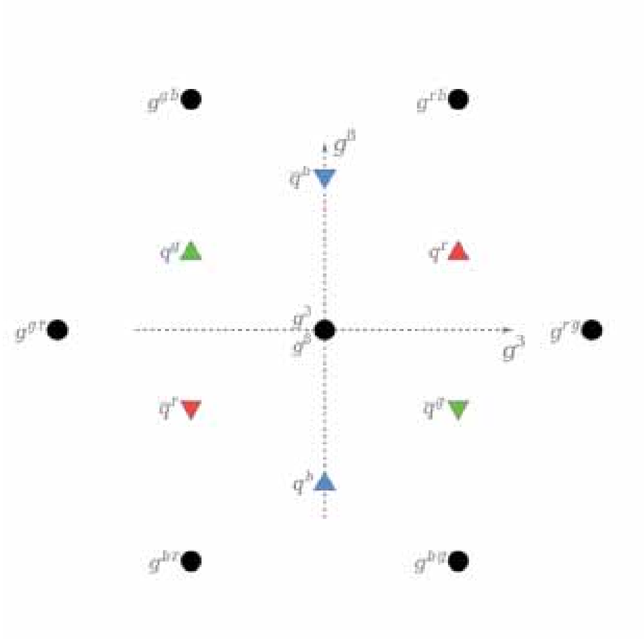

Figure 1: The pattern of strong charges for the three colors of quark, three antiquarks, and eight gluons (in black) with two of zero charge overlapping in the center. The vertical axis is strangeness and the horizontal is isospin.

figure illustrates indeed the existence of a tight pattern between the gluons (and also with the quarks), as the second objection was pointing out. But, to what extent should we "celebrate" the beautiful and unified pattern exhibited between gluons? The next subsections suggest we should not celebrate too much, as the theory is not so natural as these patterns might suggest.

2.3.3 *The symmetry space*

First of all, consider the space of possibilities of symmetry groups. This space has been explored and classified in the 'Cartan classification'. The full classification of all possible 'simple' Lie algebras is divided in four types [Lederman and Hill, 2004, 315]:

1. Rotational symmetries of spheres that live in N real coordinate dimensions: O(2) = U(1), SO(3) = SU(2), SO(4), SO(5), ... , SO(N), ...

29 Always for a gauge group the number of force-carriers is equal to the dimension of the adjoint representation; for the case of SU(N), the dimension of this representation is $N^2 - 1$, therefore for SU(3) the dimension is 8.

30 The states are added in linear combinations according to the principle of superposition of QM. The numerical parameters are required for normalization.

2. Rotational symmetries of spheres that live in N complex coordinate dimensions: U(1), SU(2), SU(3), SU(4), ... , SU(N), ...
3. Symplectic groups, which are the symmetries of N harmonic oscillators: Sp(2), Sp(4), ..., Sp(2N), ...
4. The 'exceptional' groups: G_2, F_4, E_6, E_7 , and E_8

As it appears, the resulting landscape is undoubtedly vast; indeed, it is infinite, as we see in the infinite order of the Lie groups. Thus this classification allows us to realize the first dimension of the contingency of the symmetry groups chosen: SU(3) is just one of the infinite groups that can be used to describe the constitutents of an elementary interaction —not to say that this is already assuming that we must have a Lie group, which is hardly a priori [31].

FERMIONS Such is the classification of symmetry groups used to represent the properties of all elementary fields/particles of the Standard Model. Furthermore, there is another layer of multiple possibilities if one focuses the attention not to the bosons but to the fermions. As previously stated, for each group there are infinite possible representations. While for the bosons the representation chosen is always unique, namely the adjoint representation, for the fermions, the physically interesting representations are the so called 'irreducible representations'[32]. The states in the irreducible representation are those that possess the determinate properties measured in reality, like isospin and hypercharge for the case of SU(3)[33]. Thus, the connection of a symmetry group with physical reality —with empirical data— is made through the choice of an irreducible representation of the group [McKenzie, Forthcoming, 10]. It turns out, though, that there are infinite representations of this type for each group [*ibidem*]. So there is a connection mapping the irreducible representation with a physical interpretation of families of particles that would exist in the world. Therefore, there are *a priori* infinite possible classes of sets of particles allowed for *each* of the (in turn, infinite) symmetry groups. The moral I want to draw is that, in the end, the particular final choice is mostly made due to empirical adequacy among a vast space of possibilities.

31 It could be thought that, due to consistency with the other parts the whole theory, most possibilities are forbidden. Next subsection shows that this is not so.

32 Informally, the irreducible representations of a group are the representations of the smallest possible order, i.e. those that cannot be further reduced. More technically, they are said to have no nontrivial invariant subspaces.

33 More in detail, the force-carriers correspond, in general, to the eigenvectors of the generators while the eigenvalues of those eigenvectors are the physically measurable charges (color, in this case) [Robinson et al., 2008, 66].

2.3.4 *A ninth gluon*

To bolster this argument I put forward another detail regarding the choice of SU(3). We have seen before that the singlet element corresponds to one of the nine logical possibilities, but that it does not appear to exist. One can then wonder why eight gluons instead of nine [Griffiths, 2008, 285], [Bottomley and Baez, 1996]. In fact, such a ninth gluon could have existed; a nine-gluon theory is perfectly possible in principle, but it would describe a world (very) different from the actual [Griffiths, 2008, 285]. In that case, it would be as common and conspicuous as the photon[34]. The adjoint representation of SU(3), in this scenario, could have been properly used to describe the situation. The ninth gluon could have corresponded to the singlet state of SU(3). That is, I am remarking that the structural representation SU(3) (even after the representation is fixed) is not univocally correspondent with the actual world but is at least compatible with two different possible worlds.[35]

What about the singlet element?

Likewise, for the counterfactual scenario of nine gluons the symmetry group of QCD could have been other than SU(3). Specifically, a theory of nine gluons can be described by the group U(3) [Griffiths, 2008, 286]. The experimental results that discover eight gluons are what drives the choice of SU(3) rather than alternatives as U(3). This is a usual way scientific practice is carried out –and so it has to be–; to remark this practice in this specific field of physics is aimed to show that there is no known *a priori* or natural reason for the group SU(3) to be preferred over U(3).

U(3) or SU(3) for nine gluons

In sum, on the one hand even SU(3) with a fixed representation does not univocally correspond to one state of affairs and, on the other hand, there is not any *a priori* or natural reason over the rest of groups to choose this one to describe the actual world.

In sum

Summing up all the section so far, it turns out that the mathematical description :
1) is motivated by empirical adequacy among a large space of possi-

Recap of the section so far

34 [Griffiths, 2008, 303] entertains the possibility that the ninth gluon would have corresponded to the photon. This is now discarded but it was investigated in the eighties (Fischbach, E. et al., (1986) Physical Review Letters, S6, 3). In [Griffiths, 2008, 303, Problem 8.11] it is explained how the situation would look like: the gluon would couple to all baryons with the same strength, not, as the photon does, in proportion to their charge. In the end this would look like as an extra contribution to gravity, contrary to actual evidence.

35 The phenomenon of confinement states that all naturally occurring particles have to be color singlets. This "explains" why the gluons of the octet are not free particles. Appropriately, in [Griffiths, 2008, 286] the term 'explanation' is also quoted. $|9\rangle$ is indeed a color singlet, and if it exists as a mediator it should also exist as a free particle. Moreover, it could be exchanged between two color singlets (a proton and a neutron, say), bringing about a long range coupling of the color strength [Griffiths, 2008, 286]. Again, we do not know a priori that this is not the case, so empirical data makes us discard this option.

bilities (2.3.3);
2) it does not univocally correspond to one state of affairs (2.3.4);
3) there is no *a priori* or natural reason to prefer one description (SU(3) over U(3)) in the counterfactual scenario of 2.3.4; and finally, regarding what there appears to actually be in Nature,
4) there is no obvious reason to believe that the number of force carrier particles that constitute this fundamental interaction is natural (2.3.2).

In conclusion, the mathematical description of the strong force and the strong force itself appear to be non-natural and clearly not *a priori* nor metaphysically necessary, in spite of its beauty and elegance, of the unified account with the rest of interactions, and of the theoretical predictions much before any empirical evidence.

[Martin, 2003, 52] shares the same diagnosis against gauge invariance, "*often invoked as a supremely powerful, beautiful, deeply physical, even undeniably necessary feature of current fundamental physical theory*". He argues for the heuristic character in the discovery and postulation of gauge symmetries. Notably, he remarks other factors that have to be taken into account when a gauge-invariant term is added into the lagrangian. It is not only the mere demand of local gauge invariance because, as said above, infinite possibilities verify this condition. These other requirements are, he says, Lorentz invariance, simplicity, and renormalizability [Martin, 2003, 44]. His main upshot is to argue for the heuristic character of such symmetries and, as others before ([Brown, 1999], [Teller, 2000], [Lyre, 2000], [Healey, 1997]), he argues that *the gauge fields are put in by hand to large extent*, so that "*in contrast to how it is often portrayed, one does not strictly speaking 'generate' a new interaction field in running the gauge argument*" [Martin, 2003, 45].

2.3.5 *The whimsical big picture*

After the particular case of SU(3), the argument can be strengthened by looking at the big picture, at the Standard Model as a whole. Not being natural nor *a priori* nor necessary can be straightforwardly extended to the rest of local gauge invariances that describe all the interactions of the Standard Model. Crucially, all the candidate fundamental physical theories, even the most natural versions, share such local gauge symmetries as an essential constituent, thereby becoming prone to the present criticism (to this effect, recall the quote from Weinberg in p. 49).
The other two known elementary interactions are described by U(1) and SU(2). This leads to a resulting complex symmetry group which is the product of the three, the gauge group U(1)xSU(2)xSU(3). This well-known achievement, again, displays a *very specific form*[36]. It is

36 [Morrison, 2013, 383] raises a similar point when criticizing the lack of unification: *"the problem of free parameters and the fact that the theory is an amalgam of three different*

this form that asks for explanation, that should not be seen as natural. It can be immediately visualized in the schema that summarizes

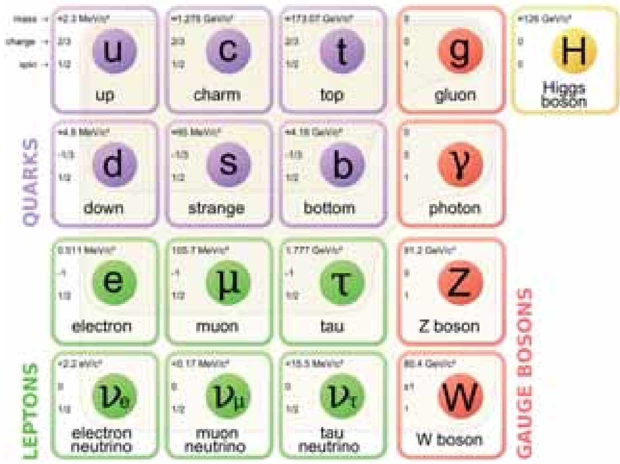

Figure 2: The Standard Model

the elementary particles of the Standard Model in figure 2. The "zoo" of particles is constituted by elementary matter particles and force carrier particles –the gauge bosons–; each type of gauge boson corresponds to one elementary interaction (with the infamous exception of gravity) and then there is the Higgs boson. The several considerations before regarding SU(3) have focused only in the 8 gluons; a glance at its place in the whole picture shows that the gluons are just one of the *five* types of bosons, in the middle of a total of *seventeen* types of fields/particles. This does not appear to be neither natural nor simple. The point is more evident when one recurs to one of the most investigated candidate fundamental theories (tens of thousands of papers devoted to it), supersymmetry (SUSY). While this theory allegedly achieves the unification of the four fundamental interactions in the scale of 10^{16}GeV, nonetheless it has to double the number of existing particles. This is seen in figure 3. We arrive thus to a quantity of total elementary particles of *thirty four*.

And being more precise, there are still more types of particles (as the eight different gluons, which occupy only one place in the chart). Fermions (i.e. quarks and leptons) have spin 1/2 which is possible only on two spin allignments: the 'left-handed' and the 'right-handed'. It turns out that only the left-handed fermions (labeled by

---

symmetry groups U(1)xSU(2)xSU(3) rather than a single group speaks against the idea that this is a truly unified theory". She also points out, against unification but then also against the metaphysical picture associated with (A2) –thus with naturalness and simplicity–, that *"a ToE presumes that gravity is a force like the others when according to General Relativity it is very unlikely the others in that there are no particles that couple to the gravitational field and act as force carriers; the effects of gravitation are ascribed to spacetime curvature instead of a force per se"* (the same observation was made in [Maudlin, 1996, 143]).

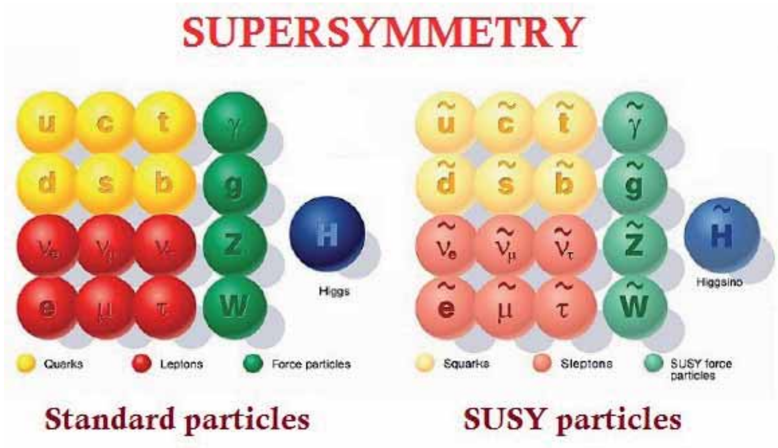

Figure 3: Supersymmetry MSSM model

subscript L in figure 4) participate in the weak interaction, the right handed fermions (with subscript R) do not. Moreover, there are five spinless higgs states that are not supersymmetric: neutral ones called 'h', 'A' and neutral, positive and negative H states. Counting 5 higgs bosons, and counting the $W^{\pm}$ as two types of bosons, this sums a total of 31·2= 62 types of different particles in MSSM.

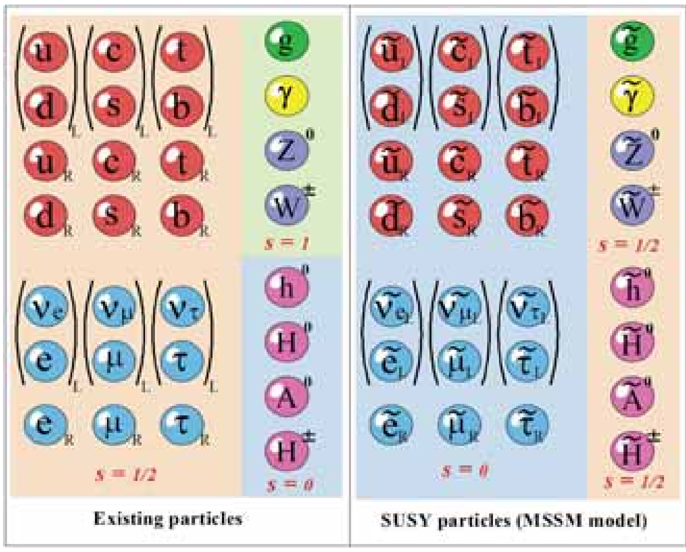

Figure 4: Supersymmetry MSSM model, here differentiating the different higgs states and the spin allignments of fermions (left with subscript L and right with subscript R).

LAST RESULTS FROM LHC If this still does not seem complex and unnatural enough, a last but not least addition is to note that the most recent experiments at the LHC are strongly suggesting the abandonment of supersymmetry in its most natural versions, since it is not finding the expected empirical support [Lykken and Spiropulu, 2014], [Evans et al., 2013], [Fowlie, 2014]. Therefore, the only way the

SUSY enterprise can go on is by switching to the officially more complex and unnatural versions of it (see also [Feng, 2013] for details on the naturalness of SUSY versions). And remember that SUSY represents the most researched of the improved versions of our empirically tested best physics (QFT) [37].

2.4 CONCLUSIONS

As it has been progressively advanced along the chapter, the specific unnaturalness of gauge symmetries described in the previous section aims to support the following conclusions:

1. *Current physical theories are in tension with any metaphysical picture related with criteria like (A1), (A2), or (B)* (p. 48), that is, criteria of naturalness, simplicity, aprioricity or necessity of any sort. Therefore, *if* our current physics is about the fundamental level or the fundamental theory turns to be structurally similar to current physics, then it is reasonable to *abandon criteria like (A1), (A2), or (B)*.

---

37 Let me conclude the section by stressing the main moral from the point of view of the lagrangian formalism. The lagrangian of the Standard Model is the best summarized description we have codified of our micro-physics. It is summarized thus [Cottingham and Greenwood, 2007]:

$$
\begin{array}{rll}
\mathcal{L} = & -\frac{1}{4}\mathbf{W}_{\mu\nu}\cdot\mathbf{W}^{\mu\nu} - \frac{1}{4}B_{\mu\nu}B^{\mu\nu} & \left\{\begin{array}{l}W^{\pm}, Z, \gamma \text{ kinetic}\\ \text{energies and}\\ \text{self-interactions}\end{array}\right. \\[2em]
& \begin{array}{l}+\bar{L}\gamma^{\mu}\left(i\partial_{\mu} - g\frac{1}{2}\mathbf{fi}\cdot\mathbf{W}_{\mu} - g'\frac{Y}{2}B_{\mu}\right)L\\ +\bar{R}\gamma^{\mu}\left(i\partial_{\mu} - g'\frac{Y}{2}B_{\mu}\right)R\end{array} & \left\{\begin{array}{l}\text{lepton and quark}\\ \text{kinetic energies}\\ \text{and their}\\ \text{interactions with}\\ W^{\pm}, Z, \gamma\end{array}\right. \\[2em]
& +\left|\left(i\partial_{\mu} - g\frac{1}{2}\mathbf{fi}\cdot\mathbf{W}_{\mu} - g'\frac{Y}{2}B_{\mu}\right)\phi\right|^{2} - V(\phi) & \left\{\begin{array}{l}W^{\pm}, Z, \gamma, \text{ and Higgs}\\ \text{masses and}\\ \text{couplings}\end{array}\right. \\[2em]
& -(G_1\bar{L}\phi R + G_2\bar{L}\phi_c R + \text{hermitian conjugate}). & \left\{\begin{array}{l}\text{lepton and quark}\\ \text{masses and}\\ \text{coupling to Higgs}\end{array}\right.
\end{array}
$$

Nonetheless, an explicit appearance of all the terms occupies *at least two pages*, cf. [Cottingham and Greenwood, 2007]. By looking carefully but in perspective at the whole formulation (at the full two-pages formulation that I cannot append here) it can be realized the high complexity as well as its whimsical contingent terms, both parameters as well as terms introduced to represent the interactions between the many particles exposed before. Thereby the same moral is glimpsed from this point of view.

As previously discussed (2.1.1 p.38), it is hard to maintain that our best theories are really fundamental. The denial of fundamentality can be justified by means of a scientific antirealist position, or else by arguments within the realist framework. Even if granting to science the authority as to what there is, the realist can have reasons to believe that what science says is not all there is and, especially, is not what fundamentally there is. The lack of commitment to fundamentality is what plausibly corresponds to the actual state of physics. That this is so at the present time can be realized by appealing to the existence of the big desert scale, and to the contemporary interpretation of current theories as effective theories of an undefined more fundamental theory. See next chapter for more details.
Taking current physical theories as *not* fundamental, and not assuming the other disjunct of the premises above (i.e. not assuming that the fundamental theory will turn to be structurally similar to current physics) leads us to the second conclusion elaborated in the next point.

2. So, conversely, *if* current physics is not fundamental, and is preferable to maintain criteria like (A1) or (A2) as far as possible —in spite of their tension with current physics— then *a qualitatively different new physics gains plausibility* at a fundamental scale. Furthermore, given the same premises, a *specific* proposal gains plausibility, namely that of a highly complex fundamental dynamics. More in detail:

    a) Assume that there are good reasons to preserve the two *desiderata* of naturalness and simplicity. We do not know how the fundamental theory is, but the *desiderata* suggest that it will not look like our current theories (QFT, SUSY, or any other resting on the gauge principle). So a fundamental physics structurally similar to how current theories are is at odds with the assumed criteria. Therefore such scenario should be discarded.

    b) An obvious continuation in the rationale is to expect a future unification which would restore the simplicity and naturalness. However, this scenario is not expected, according to what is thoroughly argued in [Morrison, 2013], [Morrison, 2000] and [Maudlin, 1996][38]). This, if correct, yields to the abandonment of the unified scenario '1' of p. 37.

    c) Therefore, after the abandonment of these two metaphysical scenarios, it is legitimated the possibility of a substantially different metaphysical scenario.

38 Though there are opposite opinions, e.g. [Wilczek, 2008] is optimistic (but it was written before the lack of success of MSSM in the LHC).

> And specifically, it gains plausibility the particular scenario of a highly complex dynamics. This is, arguably, the best candidate for a fundamental theory to preserve, within the possible, the naturalness and the simplicity. Let me elaborate below the last part of this conclusion.

As I have said previously, part of the physics community has endorsed, on other grounds (focused in the parameters), the idea that the current laws are unnatural. To solve this, some try to *restore* the naturalness. A famous move is the mentioned appeal to a multiverse, or to an (eternal) succession of big bangs and crunches. I argue that there is at least another option, whose virtues and defects I assess in ch. 3. One that does not attempt to restore naturalness, but takes at face value the non-naturalness and complexity, in that it accepts and actually enhances the complexity up to the fundamental level, hypothesizing that the true fundamental dynamics is indeed highly complex.

The actual unnaturalness can be taken to suggest an increasing complexity underneath[39], opening the door to a scenario seriously considered by some physicists —as next chapter shows— but neglected in the philosophical literature, the scenario of a highly complex (deterministic chaotic, or truly random) underlying dynamics.

Let's see in what respects this scenario should be preferred over one structurally similar to the actual. The idea is that this scenario is better because the present *intermediate degree* of complexity and unnaturalness is taken to be more unnatural and more complex (and, as argued in the next point 3 and in the next chapter, is more philosophically puzzling). Once accepting that the degree of complexity and unnaturalness presently acknowledged is something that is not going to leave (remind previous point 2.b), then, *instead of reluctantly acquiescing to this intermediate degree*, a better option is to consider that eventually the fundamental dynamics in higher energy scales is more complex, but complex enough so that any judgment of complexity and unnaturalness fades away. The complexity is so high that is not possible to state a finite set of dynamical principles; this results in a less contrived and puzzling situation than a highly contingent finite set of rules with a very specific form –i.e. the unnatural lagrangian made of a very specific list of interaction terms

39 There is both the intuitive and the technical understanding of 'naturalness' that support this point. As to the latter, it is usually considered that there is an unnatural parameter because there is some unknown mechanism underneath, whose effects can be quantified in the theory by introducing the parameter. The parameter thus codifies an unknown complexity underneath. This is evident with the corresponding technical definition of 't Hooft (cf. 2.1.2), according to which a parameter is natural as long as it can be reduced to 0 by the introduction of a new symmetry. The addition of a new symmetry clearly increases the dynamical complexity, as argued before.

and parameters–. Basically, it is not meaningful anymore to take it as composed of dynamical principles. Due to its chaotic or random nature, such metaphysics does not suffer either from being an *ad hoc* choice among a large space of possibilities. In sum, intuitively, it is so complex that it makes no sense to worry about whether it is composed of complex or unnatural principles. Basically, because it is not anymore composed of principles. As an illustrative analogy, the high complexity is judged to be less complex in the same vein as the liquid state of water is said to be more symmetric than the frozen snowflake, in spite of the chaos and lack of ordered organization of the molecules in the liquid state.
Thus, as long as we consider problematic the lack of naturalness and simplicity, it is preferrable the new proposal which, enhancing to the extreme these defects, can be considered less unnatural and less complex.

The rationale behind coincides with the line of thought of [Weinberg, 1981]: what is to be explained is not the symmetries but the lack of them, whereby it turns to be *more* natural, not a finite set of (governing) laws or symmetries, but an originally asymmetric, "chaotic", state —like [Mumford, 2004], I would talk about a 'lawless' state; [Wheeler, 1983a] says 'higgledy-piggledy'; and [Froggatt and Nielsen, 1991], as Peirce, talk about 'random dynamics'.

3. The intermediate degree of complexity previously described has been criticized on philosophical grounds in ch. 1; a metaphysical account of laws —a clearly unsolved issue in the philosophical literature— is not helped at all by a physics that postulates such a contrived finite set of unnatural and complex laws. That is, the metaphysical stance that postulates an ontologically primitive set of fundamental governing rules becomes *more* puzzling in light of the actual form of current physical theories. For, if *this* is the alleged set of fundamental laws —cf. the (not compressed version of the) Lagrangian of the Standard Model—, is it not more puzzling than the unified and simple first picture? While a necessitarian or an antireductionist would clearly have a hard time to deny that, it is disputable, though, for accounts that accept the contingency. Should the humean, which accepts the laws as an unquestionable brute fact, be *more* uncomfortable with such a contrived set of laws? In the first chapter I stressed that the overwhelmingly stable regularities (the highly ordered and stable "solutions of the equations") are an objectively philosophically puzzling issue, *also* for the humean. This chapter has analyzed some details of the actual laws of physics ("the equations") in order to flesh out the idea that such a specific form —such a "best induction", for the humean— displays also a very

specific, contrived, whimsical form. However, as I take that the humean does not endorse any criteria of naturalness or simplicity like (A1) or (A2), a structurally similar set of fundamental laws —thus unnatural and complex— will not really worry the humean conception of laws.

4. The specific sense of unnaturalness employed here is closely related to contingency, as evident in its definition. Hence, in what respects the modality of the laws, their description in 2.3 promotes their contingency, a conclusion opposite to what the previous chapter defended. To what extent this contingency can coexist or undermines the cited necessity is studied in next chapter (3.3).

3 FOR COMPLEXITY

Abstract

This chapter provides further arguments in support of the metaphysical scenario defended at the end of the previous chapter, the scenario of a highly complex underlying dynamics. I start by arguing how, even if endorsing scientific realism, the current interpretation of physical theories as effective theories allows for a variety of metaphysical images. Then, in 3.2 I present John A. Wheeler's project of 'law without law', which coincides with the alternative defended at the end of the previous chapter, and certain objections to such a project are assessed. In 3.3 I explore the same scenario from another framework, bringing forward the project developed in the last decades by a team of physicists led by H. B. Nielsen, which argues for a process of formation of symmetries. The project elaborates thoroughly Wheeler's idea of law without law from a specific perspective: they defend that the symmetry principles are not fundamental but derived, and that they are an inevitable consequence that arises from all possible complex lagrangians. I briefly reconstruct their major results and assess the rationale underneath, notably similar to that which constitutes part II.

Contents

3.1 INTRODUCTION: EFFECTIVE THEORIES AND FUNDAMENTALITY

The present chapter argues that a metaphysics whose fundamental level possesses substantially different characteristics (3.1) and, in particular, a metaphysics of a highly complex underlying dynamics (3.2, 3.3), is not an unreasonable nor ungrounded option.
As suggested at the end of the previous chapter, there is an association of a highly complex dynamics with a lack of guiding laws (and lack of stable patterns). Of course this need not be so; the relation is not exhaustive, in that the highly complex dynamics can be associated to a highly complex set of laws. But it is the former association that makes this picture in harmony with deflationist accounts of laws. As I argued in 1.4.2, these deflationist accounts, when denying a governing conception of laws, are indirectly facing this unexplored lawless scenario. They might say that this is not necessarily so, but I already argued how the lack of governing laws hardly explains the presence of stability and of non-accidental regularities (cf. 1.4.2). So it is in the interest of the antirealists that: 1) this scenario is at least a meaningful, consistent, possible scenario, and 2) that there is some explanation of how from this lawless level there appear to be stable, simple regularities —like those described by our current laws. The second point is studied in the whole second part of this dissertation. The first point is argued in the previous and the present chapter. It is obviously not the aim of the chapter to provide an exhaustive metaphysics (this would require at least another whole PhD dissertation), but just to provide a critical assessment of some specific arguments regarding such a scenario.

Outline of the chapter

The outline of the chapter is the following. First, I elaborate a previously mentioned argument related with how contemporary physics is thinking of physical theories; specifically, I draw a conclusion that follows from the treatment of scientific theories as effective theories. In section 3.2 I bring forward how this metaphysical picture has been advocated by John A. Wheeler. In section 3.3 I sketch a similar project —mostly unknown among philosophers— of a group of physicists which aim to provide an explanation of symmetry principles by considering that they derive from a highly complex underlying base.

A QUALITATIVELY DIFFERENT PHYSICS The thesis of a qualitatively and substantially different fundamental physics is not uncommon, as noted in [Weinberg, 1992] (cited by [Martin, 2003, 47]):

> "Though symmetry figures prominently into most attempts to go beyond the Standard Model, the search for a new fundamental theory unifying all forces is saddled with the task of

determining what, in all likelihood, are radically new ideas and fundamental physical principles governing the theory".

At the same time, the metaphysical picture assessed along all the chapters does not refute the truthfulness of what current physical theories state; it takes them not to be fundamental theories —something that, as argued in chapter 2, is not an implausible assumption.

A more conservative stance

Still, even if accepting that they are not fundamental, there is an option more conservative with the scientific image. It does not deny that the fundamental level *could* be substantially different, but it is not likely that it will *be* so. This is motivated by the virtues of current physical theories —in other words, it comes from the enthusiasts of current physical theories (foremost amongst them, the theoretical physicists working in them). In this respect, theoretical physicist Nima Arkani-Hamed, if faced with the lack of confirmation of supersymmetry in the LHC, suggested the postulation of other new supersymmetric models that put non-detected superpartners just beyond the reach of experiments [Lykken and Spiropulu, 2014, 38]. Among other reasons, the theory possesses strong virtues —in this thesis overlooked— that commit physicists to stick to essentially analogous and structurally similar theories. This move would be framed in the second metaphysical picture set up before, in 2.1 p. 37.

Effective (Field) Theories

Granting the plausibility of this scenario, let's focus on the arguments in support of another alternative. The possibility of a qualitatively different physics is bolstered by a change in attitude after the contemporary interpretation of our best empirically tested physical theory, QFT, as an *effective* field theory. While this new attitude clearly does not imply any specific thesis about the existence of a final theory —as underlined in the conclusions of [Castellani, 2002, 264]— it allows the possibility of a *"new physics"* [Castellani, 2002, 262]. Specifically, it is allowed that the renormalization up to higher energies can result in radically different theories from the current models of QFT.

Big desert scale

This possibility is bolstered by the fact that there are several orders of magnitude in the energy scale between the fundamental scale and the current experimentally tested scale, as previously explained. There is an order of magnitude between the Planck scale (arguably assumed to be the lowest possible scale, which is not even necessarily so) and the scales probed in high-energy particle physics. As advanced in 2.1.1 p. 41, from the electroweak $W^{\pm}$, Z^0 scale of 10^{-18}m to the presumably fundamental Planck scale of 10^{-35}m there are 17 orders of magnitude, a change in scale similar to that in passing from macroscopic physics, at the 1m scale, to the electroweak scale [Froggatt and Nielsen, 1991, 12]. Current fundamental theories are silent about what happens in such energy levels.

Formation of laws

On the other hand, since the universe in the initial instants after the Big Bang consisted of a regime of very high energies, the lower-energy renormalized dynamical laws that we are now discovering as QFT *did not exist*; therefore they have arisen while the expansion of the universe was taking place. That is, it is implicit in the standard scientific picture that there has been a temporal process of something like a formation of laws[1].

Putting together both claims, we can assert that current laws might have undergone a process of formation, from a possibly very different scenario.

Behind the method of renormalization

In fact, in the very definition of the notion of renormalization, the procedure at the base of effective theories, the possibility of allowing a substantially different scenario is evident; renormalization is defined as *"an expression of the **variation of the structure** of physical interactions with changes in the scale of the phenomena being probed"* [Gross, 1985, 153, bold added].
Castellani specifies how the description of the physics substantially changes as the scale varies:

> "as one scales down to lower energies, the solutions of the Renormalization Group equations approach a finite-dimensional submanifold in the space of possible Lagrangians thus defining an *effective low energy theory*, which is formulated in terms of a finite number of parameters and is largely independent of the high-energy starting situation" [Castellani, 2002, 262].

A certain energy threshold is established defining when a certain parameter (the parameter is usually a term in the lagrangian indicating, for instance, the mass of a heavy particle) can be ignored. This is to say that the effects of the nonrenormalizable interactions are small enough as to be negligible. When this energy threshold is approached the effects of, say, the heavy particle, cannot be ignored anymore, and a new theory becomes the most appropriate description. This can be *"a renormalizable QFT, another EFT, or something completely different"* [Castellani, 2002, 262].

This adds up to the specific conclusions of the previous chapter regarding the contrived form of the actual laws. There I argued that such form was in tension with the criteria of naturalness and simplicity, and the conclusion was supporting a specific alternative, that of a

1 This is not necessarily the case, though. One can assume that lower-energy renormalized dynamical laws did *"exist"* —whatever that means— before their regime of applicability existed —they exist somehow eternally, independently of their application to the actual world.

complex underlying dynamics, which I argued is not unnatural and, in some sense, even simpler than the actual laws.

3.2 WHEELER'S LAW WITHOUT LAW

I would like to bring to the fore one of the modern precursors of the line of thought defended in this thesis: John Archibald Wheeler. In [Wheeler, 1973] and [Wheeler, 1983b] he suggested the idea of "law without law", in concordance (if not almost identical) with what this thesis studies. His picture is in tune with any metaphysics skeptic of a realist conception of laws.
Wheeler does not develop his core idea so, in that sense, he does not provide new arguments beyond what I present throughout this dissertation; however, what he does is to provide detailed and useful examples. One of them is the case of a gas in a box, studied at length in chapter 5 from a specific contemporary approach. The other examples are the phenomenon of universality of exponents near thermodynamic critical points, a less known "semicircle law" for the distribution of characteristic frequencies of a randomly coupled system, and a new physicist's version of the so called problem of the traveling salesman. His proposal can be gathered thus:

LWL: *'Chaos plus unknown regulating principle(s) lead to approximate laws'*.

When he says 'chaos' he does not refer to deterministic chaos, as now usually understood, but to something closer to randomness; in his own words: *"everything is built higgledy-piggledy on the unpredictable outcomes of billions upon billions of elementary quantum phenomena"* [Wheeler, 1983b, 398]. I seek to evaluate how meaningful this chaotic scenario can be by assessing the diverse formalizations of the notion of randomness in annex A. In the remainder of the section I reflect upon some worries regarding the regulating principle.

Among the various examples, take what I will later study at length (ch. 5), the 2^{nd} law of thermodynamics. With respect to it Wheeler stresses the observation that motivates the idea of emergence of "order from chaos":

> "How can stupid molecules ever be conceived to obey a law so simple and so general? [...] What regulating principle accomplished this miracle?" [Wheeler, 1983b, 400].

The standard scientific stance was to assume that there is an ultimate set of fundamental laws of physics from which every other dynamical law follows — in the words of Wheeler, *"one or more beautiful* *equations, chiseled as it were on a tablet of granite and standing there from everlasting to everlasting"* [Wheeler, 1983b, 398]. Wheeler, instead, talks of *regulating principles*, so now the immediate question is: to what extent these principles differ from the standard laws?

The main objection

The same sort of suspicion has been raised to similar projects. For instance, Lee Smolin is attracted by the idea that laws did evolve through time and, inspired by Peirce's law of habit (cf. annex B) has started to elaborate these ideas (in [Smolin, 2014] and [Unger and Smolin]). In a recent talk he gave (at the conference "New Directions in the Foundations of Physics", 18 April 2014) he was justly asked this central objection. That is, whether the regulating principles/Peirce's law of habit are not themselves *laws*.

I take this as a main objection to the similar proposals presented throughout this dissertation. This is why in 1.1 one of the ways I phrased the goal of the thesis was as the study of how a smaller set of dynamical laws can lead to a bigger set of dynamical laws, where the smaller set is expected to be more plausible than the bigger set —the bigger set being the current physical laws. Likewise, this is why a central concern is to avoid, as much as possible, that the constraints sufficient for the emergence of stable behavior are anything like a dynamical law —so that is why the second part talks of *non-dynamical* constraints. I come back to this main objection at the end of the section.

THE IMPORTANCE OF THE QUESTION: "WHY THESE LAWS AND NOT OTHERS" Let's carry out an assessment of Wheeler's regulating principle. Physicist David Deutsch elaborated useful critical insights in [Deutsch, 1986]. He starts by defending Wheeler's motivation: he agrees that there is a threat of circularity in the search for an ultimate principle from which everything else follows if we take it to be itself a law of physics. For why *this* principle holds rather than some other would be *"an insoluble problem"*. Therefore, he contends, *the ultimate principle of physics cannot be a law*. As such, this Wheeler/Deutsch argument adds up to the series of arguments raised so far.

Notice that the question "Why these laws and not others?" has been introduced in the discussion, and its role turns out to be crucial: it is this question that motivates a critical stance against the standard scientific view on laws. This question is exactly what I stated in chapter 1 as the third philosophical question about laws (1.1 p. 6). Now it is presented pressing against the traditional scientific view on laws.

Let me connect this question '3' with the the long chapter 2, where I provided diverse arguments in support of an alternative to the traditional view. Amongst them, a central goal was an elaboration of question '3', for I spelled out how the laws actually look like, and I judged them as contrived (something that I analyzed mostly in terms

of unnaturalness): such a survey of the actual laws yielded a precise glimpse of what it means that they could have been otherwise, and a precise glimpse that they could have been otherwise indeed.
It is more plausible to postulate, at most, only very general principles, rather than dynamical laws of temporal evolution: the latter by definition inevitably display a contrived, contingent, unnatural form, be it in the form of a differential equation, or in the form of a vector field expressed as a matrix of a Lie algebra (cf. 2.3).
Those principles are then the only responsibles of constraining an *initially unconstrained space of possible time evolutions*.

TENSION WITH MODERN PHYSICS Deutsch admits that Wheeler's motivation is compelling, but describes a state of affairs in modern physics that is in tension with Wheeler's proposal:

Tension with how modern physics describes the world

> "a physical theory amounts to the specification of an action functional and the quantum field configurations that constitute its domain. Thus the expectation is that we shall soon discover the ultimate "fundamental fields", their invariance properties, and their action functional. The principle of the stationarity of this action functional, together with the principles of quantum theory, would be the ultimate dynamical "laws of nature" [Deutsch, 1986, 2].

Deutsch refers to the principle of stationary action, applicable in all the known domains of physics. Can Wheeler's proposal coexist with such a scientific image?
It can coexist only if those laws are not the most fundamental laws. In this respect, chapter 2 and the beginning of this chapter have been arguing for the plausibility of a substantially different scenario in the fundamental level. Then, a final theory along the lines of Deutsch's fragment should not signify a truly fundamental description of reality.

SYNTHETIC OR ANALYTIC PRINCIPLE? That being said, Deutsch analyzes the fundamental principle hypothesized by Wheeler in the following terms: should that principle be analytic or synthetic?
If it were analytic it would be a consequence of logic alone (such is Deutsch's understanding of analyticity). The question in this case becomes whether such type of analytic proposition is capable of making an assertion about the world or its factual content is empty.

How could be the fundamental principles?

If, on the other hand, we consider that the principle is synthetic, then it is not stating anymore truths about every possible world, the contingency I already highlighted (in ch. 2) shows up, and the question becomes: how can be justified that the synthetic principle is the true one *instead of some other*?
The former case is thus threatened by the vacuity of meaning of an

analytic principle —and this is so according to a widespread philosophical tradition (as e.g. [Wittgenstein, 1921]). If this is the case, it undermines the "rationalist" approach to laws of nature according to which laws follow from logical principles (a position presented in chapter 1 and pursued along a novel path in 1.5).
On the other hand, in the case of the principle being synthetic, the worry takes us back to the motivation for the search of an alternative proposal, that is: the assumption of the existence of a set of fundamental laws brings out the question of why these laws and not others. This question, now, is addressed at the undefined synthetic principle(s) postulated by Wheeler.

This second objection drives us back to what I named as the main objection, related with the fact that the principles postulated over and above the chaotic base do seem to be the same as what we wanted to avoid, i.e. dynamical *laws* of time evolution.

The main objection again: How might 'principles' differ from 'laws'?

Granting the resemblance with dynamical laws of time evolution, Deutsch himself evidences the real lack of lawlessness of Wheeler's schema. That is, the schema:

$$\text{Chaos} + \text{regulating principle} \Longrightarrow \text{approximate laws}$$

It is to be rewritten in the very uninspiring form:

'Stochastic laws lead to approximate deterministic laws'

Let's look at this main objection in our future field of study, statistical mechanics, a field also treated by Wheeler. In this field there is, according to Deutsch, an implicit lawlike element that he identifies in the probability distribution of the statistical postulate: *"the principle of equal a priori probability of statistical mechanics"* [Deutsch, 1986, 5].
This objection relies on the critique that stresses that equiprobability amounts already to a lawful stochastic distribution, not to randomness. I myself have indeed argued (1.3 p. 11) that a probability distribution contains an element of order. Deutsch thus describes the motion of molecules as not being lawless, and he thus interprets the uniform probability distribution function as the stochastic law we wanted to avoid.
The rationale of this line of thought goes like this: 'random' should not mean a specific distribution (even if it is the uniform distribution) for random means *any* possible distribution, not a fair visitation rate of all the outcomes as in the uniform distribution. In frequentist terms, the critique can be phrased thus: the equiprobabilities of the uniform distribution are stating that there will be an approximately equal frequency of all the outcomes, and this clearly need not be the case for a truly random distribution. Therefore, the uniform distribution should not be associated with the notion of randomness.
Parallel criticisms have been made against the association of randomness with equiprobability (e.g. [Gillies, 2000], [Myrvold]), oriented at

the classical interpretation of probability; see 4.3.2 for more detail.

A reply

However, I argue throughout this thesis against such view. To what extent does a uniform distribution have to be interpreted *only* as an *assignment of a fixed probabilistic form* and not as a description of a truly *random* or *very complex* situation?

I defend in annex A that there is a relation between the notion of randomness and that of equiprobability, as intuitively thought. I do so following the (equivalent) formal definitions of randomness as *unpredictability*, *typicality*, and *incompressibility* (A.3). In a nutshell: given such definition(s), what should be expected is that a random sequence will not display any biased distribution among the outcomes *in the long run*, but a uniform distribution.
Even if a uniform distribution is indeed a probability function, it does not follow that a uniform distribution is *exclusively* the result of a stochatic law. It can also be the most likely result of a random generation — I argue this in A.4.

Further, that randomization implies an equal visitation rate of all the outcomes when certain symmetries are present in the system under study is argued in [Strevens, 2013], and I analyze it in chapter 4. The procedure developed in chapter 4 helps to explain equiprobability not in the line of the criticized classical interpretation, but from other ingredients —namely, from a (genuine or not) random walk plus certain symmetries of the system. This specific argument is in 4.3.2 p. 108, based on [Strevens, 2005].
Independently, I study in chapters 4 and 5 whether the statistical postulate of SM can be justified without involving lawlike elements in the explanation. I argue that this postulate can be accepted for the actual laws as well as for most possible laws, given that they will exhibit certain properties, once we assume some spatial symmetries of the system. In this sense, the statistical postulate is explained from non-dynamical elements, therefore itself is not a lawlike factor that could harm Wheeler's idea of 'law without law'.

In sum, the elaboration of Wheeler's proposal, and an answer to what I labeled as the 'main objection', continues with the arguments of the next chapters. In a generic framework in chapter 4, in the particular field of statistical mechanics in 5, and in annex A.

Before this, it is worth mentioning the developments carried out by a project, mostly unknown among philosophers, that elaborates at length a process of formation of symmetries.

3.3 FORMATION OF SYMMETRIES

To frame this section, let me recapitulate what has been argued so far. I presented in 1.5 a central aspect of contemporary physical theories, namely the important role of local gauge symmetries, to the extent that I highlighted a quantum field theory whose laws are *univocally* determined by gauge symmetry principles. This is a novel result in the history of physics, and I argued that it is worth pursuing in that (perhaps) improves our understanding of the notion of law. Then, a thorough study followed in chapter 2, pointing out that the status of symmetry principles was far from necessary or a priori "superprinciples", and as such in need of explanation. This conclusion was taken as a premise for a further conclusion, namely, the plausibility of a substantially different physics in the fundamental level.
Now, a natural continuation of the dialectics is: if laws of nature stated in the form of symmetry principles are in need of explanation —just as laws of nature in general— what explanation could be provided? And two answers, at least, gain prominence: 1) symmetry principles are explained as consequences of the matter-content of the universe, the "initial conditions", or 2) symmetry principles are not fundamental but derived from a non-symmetric lower level. In what follows I reflect about what is meant by the two options —the latter being the main object of study of the coming chapters.

3.3.1 *Consequences of the initial conditions*

The first option is to recur to the matter-content of the universe, its elementary particles or fields —the elementary constitution of the space content, whatever that be— in order to explain the symmetry principles. The current symmetries are then explained as a consequence of the types of entities that populate the universe.
Remember that this claim has been already grounded in what I argued in 1.5: I presented the existence of renormalizable local gauge theories of QFT, where the fields univocally specify the laws (they specify the gauge symmetries, corresponding to the elementary interactions of such fermionic fields) except for the values of the constants appearing in them.

This means that now the burden of proof is translated to the matter-content of the universe. And an answer to this is to take those particular types of entities just as a primitive brute fact.
Brute facts are, perhaps, one of the worst types of explanation; still, if somewhere could be acceptable, here could be, where we face such basic unavoidable constituents of reality as its very *content*. Thus, take the initial conditions as a primitive brute fact, and this, following the well defined mathematical relations of QFT, implies its own dynam-

ics. The symmetries would be thus explained as a mathematical consequence of the types of particles that constitute the universe.

An objection, though, is just to consider another equally intuitive option which turns the direction of the postulation the other way around: the types of particles are the ones they are in virtue of the underlying symmetry principles. So symmetries are no more explained but assumed —and this is more in tune with the usual way physics understood the relation of the content and the laws. Many other worries could be brought up now (no more, though, than those that could be brought up against the coming alternative).

Be that as it may, a virtue stemming from this strategy is that the tight relation between laws and matter-content seems to be more adequate, in light of current physics and also of philosophical considerations. That is, both elements, laws and matter-content, are not independent of each other. They are not two parallel ontological realms, where one —laws— applies to the other —the initial conditions. One might supervene over the other, or they might be on an equal ontological footing (the latter is the conclusion defended in [McKenzie, Forthcoming]); but either way, they are not two orthogonal subjects of inquiry, as was traditionally conceived.

Though the path presented is promising and more can be said in its support, in the remainder of the chapter I want to put forward another option, less known in the existing literature, which shares evident similarities with the main rationale of this dissertation (especially with what is argued in part II).

3.3.2 *Consequence of a non-symmetric complex lower level*

I want to bring up and sketch the key features of the proposal that hypothesizes the existence of a process of *formation of symmetries*.
Symmetries are not to be considered neither fundamental nor exact, but derived and approximate. Whereas [Jacobson and Wall, 2010] has studied this possibility for the particular case of Lorentz symmetry, H.B. Nielsen and his team have explored the more radical idea that *all* symmetry principles are derived and approximate. They have published a large quantity of papers (e.g. [Froggatt and Nielsen, 2002], [Froggatt and Nielsen, 2005], or a book with a recopilation of the main ideas of their project and a selection of papers [Froggatt and Nielsen, 1991]).

It is not the standard viewpoint in physics, but many recognized physicists have been sympathetic to this strategy, a partial list of which I am citing along the chapter. Those that I know are: Carl von Weizsäker, George Gamow, Werner Heisenberg, John Archibald Wheeler, Eugene Wigner, and Steven Weinberg. More recently, Lee Smolin and all the team led by Colin Froggatt and H.B. Nielsen. Likewise, there is a profound similarity with the project called 'entropic gravity' (or more in general 'entropic forces'), investigated e.g. by Ted Jacobson [Jacobson, 1995], Ariel Caticha [Caticha, 2011], [Nawaz and Caticha, 2012], Erik Verlinde [Verlinde, 2011], Lee Smolin [Smolin, 2010], Shan Gao [Gao, 2011], [Gao, 2013].

For the sake of concreteness I only sketch the specific project of Nielsen's team, but undoubtedly a future line of research should be to elaborate a better analysis of the parallel proposals (not to say, a proper philosophical analysis of Nielsen's project, here only summarized).
The central point of Nielsen's project can be stated thus:

SYMM: *All possible complex lagrangians lead in the low-energy limit to the symmetries of current physics*.

That is, they are considering a fundamental level ruled by an undetermined complex behavior. This level is obviously below the current quantum level, for quantum mechanics does not describe a complex dynamics like the one they suppose. This complex behavior, labeled by them as "*random dynamics*", is thought to inevitably yield the emergence, in some limit, of all the current symmetries. The limit is taken to be the low energy domain, which corresponds to the experimentally accessible energies below 1TeV.
Notably, this approach is in the opposite spirit of that of assuming hidden symmetries that have been spontaneously broken (explained in [Brading and Castellani, 2013, sec. 4.2], and see my discussion in 1.3 p. 11).

The underlying idea is illustrated by the historical example of Heisenberg dealing with isospin symmetry in the thirties [Froggatt and Nielsen, 1991, p.188]. Heisenberg took that symmetry to be a fundamental principle of his unified field theory of elementary particles. However, later on it was shown that isospin symmetry is really an "accidental" consequence of QCD plus the smallness of the up and down quark masses. It is thus not fundamental but derived.
The authors take this idea and extend it to all symmetries:

> "we entertain the hypothesis that all known symmetries of the empirically discovered laws of nature —and the laws of nature themselves— should be derivable in some limit, essentially independent of what the physics might be at very short distances" [Froggatt and Nielsen, 1991, 12].

Gauge symmetries

Take as an example local gauge symmetries. What they have presumably shown is that, if the fundamental Lagrangian from which physical laws are derived is chosen at random, then the existence of local gauge invariance at low energy can be a stable phenomenon in the space of all Lagrangian theories. This is argued in [Chkareuli

et al., 2009], [Forster et al., 1980], [Antoniadis et al., 1983], [Froggatt and Nielsen, 1991, ch. 6].
A specific result is that the presence of a massless photon emerges from an open set of Lagrangians picked from the space of almost all possible functional forms. In sum, at low energy is obtained the appearance of a local U(1) gauge symmetry and the existence of what corresponds to a massless photon.

They have worked likewise in the derivation of all the other symmetries. The process of derivation of symmetries, as explained in [Froggatt and Nielsen, 1991, ch.7], follows different strategies. One is to derive them from other more fundamental symmetries. This is done for the derivation of parity, charge, conjugation, time reversal and strangeness (or more generally, flavour) conservation: if correct, these can be interpreted to be no more fundamental symmetries of the Standard Model but natural consequences of a gauge field theory (see below for more details, and cf. [Froggatt and Nielsen, 1991, ch.3] and the references therein).

The 3 methods of derivation

The other two types of derivation of symmetries are more relevant for our interest in metaphysical issues. One is the renormalization group method. According to this strategy, the symmetry becomes a better approximation as a certain domain of energy is approached. This method thus coincides with the observations laid out at the beginning of the chapter, regarding the interpretation of effective theories based on renormalization techniques. In the third place, another strategy is dubbed "formal appearance". A derivation of this type reveals the symmetry to be a purely formal one, without real physical significance, which *"nevertheless attains physical significance in some phase of the vacuum due to quantum fluctuations"* [Froggatt and Nielsen, 1991, 92]. Let's see what they argue more in detail.

THE SYMMETRIES OF THE STANDARD MODEL In order to provide a minimally wide picture of how this project has been conducted, I am going to summarize some crucial stages in their derivation of all the symmetry principles. Let's highlight how they assess the status of the symmetries of the Standard Model. Following [Froggatt and Nielsen, 1991, ch.3], they describe all the interactions of the Standard Model with a Lagrangian $\mathcal{L}$ such that $\mathcal{L}$ is *the most general* one consistent with the following four assumptions:

The derivation of the Standard Model

1) renormalisability, so that it forbids the coefficients of interaction terms having dimension of mass to a negative power ;
2) Poincaré invariance, which is to say that the Lagrangian has to be Lorentz invariant and translationally invariant ;
3) gauge invariance under the gauge group S(U(2)xU(3)) ;
4) a given matter content corresponding to the fermions of the Standard Model, so this includes neutrinos, and left and right-handed

quarks and leptons [Froggatt and Nielsen, 1991, 20].
Now, it turns out that these assumptions already constrain the Lagrangian $\mathcal{L}$ to exhibit all the other symmetries of the Standard Model. This result is illustrated in figure 5.

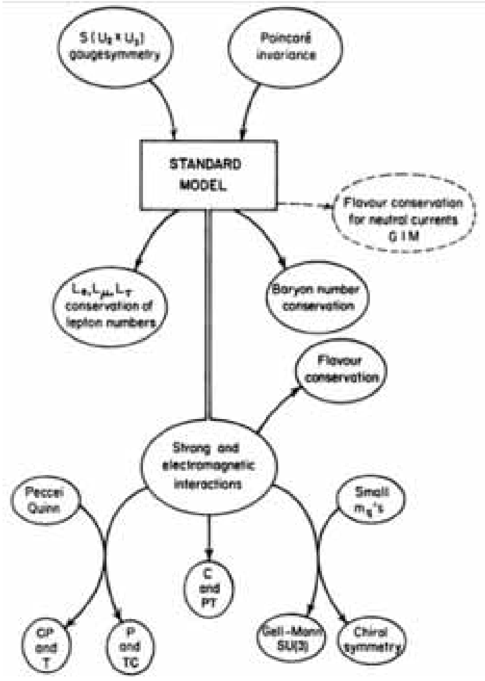

Figure 5: The symmetries of the Standard Model. Poincaré and S(U(2)xU(3)) gauge symmetries are assumed, the rest are derived in the 'Random dynamics' project. From [Froggatt and Nielsen, 1991, 42].

Thus, regarding the Standard Model, five out of eight of the symmetries have been derived. The 8 symmetries are: 1) space-time translational invariance, 2) Lorentz invariance, 3) gauge invariance, 4) charge conjugation invariance, 5) parity invariance, 6) time reversal invariance, 7) flavour conservation, 8) chiral symmetry (fig. 5). The last five are those that have been allegedly derived within the Standard Model. The first three instead require an explanation beyond it. The derivation of the five, though, requires some assumptions: the existence of small quark masses and the absence of the QCD topological θ-term. As the authors say, "it is reasonable to suppose that these auxiliary assumptions will be understood in terms of physics beyond the Standard Model" [Froggatt and Nielsen, 1991, 130].

Granting these results, we see that Yang-Mills gauge and Poincaré invariance are assumed. What can be done in their regard? The three diverse strategies are independently applied for the derivation of both symmetries (cf. [Froggatt and Nielsen, 1991, ch. VI] and references therein).

Poincaré invariance

For instance, regarding the Poincaré symmetry, consisting in rotational and space-time translation invariance, they have to recur to a pre-geometric theory, given that they have to go beyond general relativity: the reason is that the Poincaré invariance is just part of the diffeomorphism symmetry of general relativity, so it is built into it [Froggatt and Nielsen, 1991, 129]. Thus, a pre-geometric theory is sought such that it introduces a purely formal diffeomorphism symmetry (this when they opt for the strategy dubbed before as 'formal appearance'). Their proposal, then, is that quantum fluctuations can be the mechanism capable of stabilizing the Poincaré invariance, for the key point is to show that the corresponding formal Poincaré symmetry is not spontaneously broken [Froggatt and Nielsen, 1991, 6.2.1].

THE EMERGENCE OF LORENTZ INVARIANCE Let me enter a bit more in detail, in order give a more specific glimpse of their procedures, by outlining the rationale in the specific derivation of Lorentz invariance, found in [Chadha and Nielsen, 1983] (the papers [Antoniadis et al., 1983], [Forster et al., 1980] argue in the same direction). They start by considering a not covariant model of electrodynamics, which is to say that they assume a Lagrange function which is gauge invariant and renormalizable, but not Lorentz invariant[2]. They spell out the model and study the behaviour of the various couplings in function of the energy scale. Remember that the objective is to show that, as the energy scale lowers, the model progressively displays more Lorentz invariance. Therefore, such invariance is to be considered only a property in the infrared domain of a large class of Lagrange functions.

They start by writing down the most general form of the action, having something that looks like this:

$$W_{non-int}(J,\eta) = \int dx(J^{\mu}A_{\mu} + \eta\gamma^{0}\psi + \mathcal{L}_{non-int}(A_{\mu},\psi)) \qquad (1)$$

where $J^{\mu}A_{\mu}$ corresponds to the photon source and photon field respectively, $\eta\psi$ to the electron source and electron field respectively, and:

$$\mathcal{L}_{non-int}(A_{\mu},\psi) = -\frac{1}{4}\eta^{\mu\nu\rho\sigma}F_{\mu\nu}F_{\rho\sigma} - \frac{1}{2}\psi\gamma^{0}\gamma^{\alpha}\left(e^{\mu}_{+\alpha}\frac{1+iq\gamma_{5}}{2} + e^{\mu}_{-\alpha}\frac{1-iq\gamma_{5}}{2}\right)(1/i)\delta_{\mu}\psi$$

where worth remarking is $\eta^{\mu\nu\rho\sigma}$ which are the effective coupling constants[3].

After certain deductions that follow from such non-covariant model, [Chadha and Nielsen, 1983, sec.5] shows how the smaller is the scale

2 By gauge invariant they specifically refer to the fact that the primitive interaction term is obtained from the free Lagrange function by replacing every derivative by a covariant derivative. By renormalizability they refer to the fact that the Lagrange function only contains terms whose mass dimension is $\leqslant 4$.

3 The four indices of $\eta^{\mu\nu\rho\sigma}$, after some restrictions due to the strength field tensors $F_{\mu\nu}$ and $F_{\rho\sigma}$, represent a total of 20 independent coupling constants.

the more the property of Lorentz covariance manifests. The energy scale is λ, and the equation that specifies the behaviour of the model in function of the change of scale λ is[4]:

$$\frac{\delta\eta^{\mu\nu\rho\sigma}(\lambda)}{\delta\log\lambda} = -\frac{\alpha}{6\pi}(g_+^{\mu\rho}g_+^{\nu\sigma} - g_+^{\mu\sigma}g_+^{\nu\rho}) - \frac{\alpha}{6\pi}(g_-^{\mu\rho}g_-^{\nu\sigma} - g_-^{\mu\sigma}g_-^{\nu\rho}) \quad (2)$$

This can be written, after many steps and making a slight notational change (I am avoiding all the possible technicalities, but this is necessary to show the coming conclusion) as:

$$\frac{1}{e^2(\lambda)}\eta^{\mu\nu\rho\sigma}(\lambda) = \frac{1}{2e^2(\lambda)}\left[g_{(\gamma)}^{\mu\rho}(\lambda)g_{(\gamma)}^{\nu\sigma}(\lambda) - g_{(\gamma)}^{\mu\sigma}(\lambda)g_{(\gamma)}^{\nu\rho}(\lambda)\right] + \frac{1}{e^2(\lambda)}\delta\eta^{\mu\nu\rho\sigma}(\lambda) \quad (3)$$

where the first term corresponds to the covariant term and the second to the non-covariant[5]. Thus, this equation describes the behaviour of the model (more specifically, of the effective coupling $\eta^{\mu\nu\rho\sigma}$) under change of scale λ.
Now, by another way they arrive to the result that ([Chadha and Nielsen, 1983, 142]):

$$\frac{\delta}{\delta\log\lambda}\left(\frac{1}{e^2(\lambda)}\delta\eta^{\mu\nu\rho\sigma}(\lambda)\right) = 0 \quad (4)$$

With these two results we are now ready to verify the emergence of the Lorentz invariance. Keeping in mind the result of equation 4, look at the evolution of equation 3 as λ decreases: the first covariant term will increase, given that $e(\lambda)$ in the denominator will decrease. Then, equation 4 is basically stating that the second noncovariant term will not change its value as λ changes. Therefore, the more λ decreases, the more predominant will be the effects of the covariant term!
Thus, in this massless non-covariant electrodynamic model, Lorentz covariance emerges in the infrared limit[6].

3.4 CONCLUSION

If we grant the success of this whole project of derivation of symmetries, given the minimum number of assumptions and the subsequent most generic form of the lagrangian employed, it means that the symmetries will hold in the low energy limit regardless of the details of the fundamental theory. This crucial feature is made explicit here:

4 Where the $g_\pm^{ij}$ are diverse metrics, and α the fine-structure constant.
5 And more in detail, $e(\lambda)$ is a coupling constant and $g_{(\gamma)}^{ij}$ is the photon metric, onto which the previous metrics have been contracted to it.
6 Needless to say, see [Chadha and Nielsen, 1983] for further details. There are a lot of subtleties that I neglected for sake of exposition.

> ”it would then hardly matter what the fundamental ’theory’ is; it would anyway give rise to the observed regularities. This would mean that almost any theory could explain ’everything’ known and thus be good enough as a theory of everything (ToE). There would then be no reason to accept any spatial model or ToE as the truth; it would be better to imagine that the most fundamental physics is chaotic —a random model— or not to assume anything about it. This is really the content of the random dynamics hypothesis: the fundamental physics, or ToE, does not matter, since almost all models at the fundamental level will have sufficient structure that they agree with the phenomenologically observed regularities” [Froggatt and Nielsen, 1991, 133].

It is of course disputable whether the assumptions are sufficiently minimal. An interesting future line of research would be to carry out this study. The project is vast, given the quantity of work that this line of research has produced. For each paper different assumptions and derivation procedures can be employed —a brief example of both assumptions and derivation procedures has been shown for the case Lorentz invariance in the previous section.

Without the cosmological principle

Lastly, the meaningfulness or plausibility of this project is bolstered if we do not assume the cosmological principle, the principle that postulates that the distribution of matter in the universe is homogeneous and isotropic when viewed on a large enough scale[7]. This point of view is clearly illustrated by Barrow in [Barrow, 1988, p.299], where he connects the emergence of symmetries with the falsehood of the cosmological principle, and with the chaotic cosmologies (as those developed by [von Weizsäcker, 1951], [Gamow, 1952]), in turn supported by the theory of inflation. Let me quote him, to conclude with the words of an authority (the fallacy of appeal to authority notwithstanding):

> "Suppose that the Universe did indeed begin in a state of chaotic anarchy. If the evolutionary emergence of particular symmetries depends upon the local temperature and density, then they will emerge at different rates in different parts of a chaotic universe. (...) If the phenomenon of inflation occurred in such a heterogeneous Universe then one could find that our entire Visible Universe evolved from the accelerated inflation of a single microscopic domain possessing similar laws and symmetries. Outside this domain things might, literally, be unimaginably different".

7 The principle is a basic assumption of modern cosmology, whose motivation comes from assuming that the forces are expected to act uniformly throughout the universe, so they should not produce observable irregularities in the large scale structure over the evolution of the initial matter field.

There is no space here to assess the success of the 'random dynamics' project; this would require at least another PhD dissertation. In the coming part II a similar project is carried out from a different perspective. Nevertheless, I wanted to bring forward this parallel approach, largely unknown among the philosophical literature, that Wheeler began to work out (and probably others before) and that these physicists have been thoroughly developing. If the project is successful, then it is able to explain some of the most evasive philosophical notions: laws of nature and symmetry principles.

Part II

NON-DYNAMICAL CONSTRAINTS SUFFICIENT FOR REDUCTION OF COMPLEXITY

4 ARBITRARY DYNAMICS

Abstract

This chapter argues that, if a system displays a chaotic trajectory in phase space and certain non-dynamical conditions hold, stable regularities arise without needing a specific dynamics guiding the evolution of the system. I start by explaining how stable regularities can be obtained from a lower level in which the actual dynamics is chaotic, an explanation based on the so called 'method of arbitrary functions'. I then argue how in this method the particular form of the assumed laws is irrelevant. If this is the case, regular behavior would emerge not only from the presupposed actual dynamics but from a larger set of possible laws. More specifically, regular behavior would emerge from the set of possible laws that display a chaotic trajectory. Finally, I discuss how this extension to all chaotic trajectories can be significant to the case of a scenario without fundamental guiding laws. To this end I appeal to the recent insights regarding the shared properties of the notions of randomness and chaos.

Contents

4.1 STABILITY FROM COMPLEX UNDERLYING DYNAMICS

The aim of the thesis can be restated in this chapter in a specific way. *The goal* In order to propose an account of stable regularities without appealing to fundamental laws, this chapter studies whether it is possible to explain the formation of stable probabilistic regularities through the so called 'method of arbitrary functions'. First, I remark that the method provides an explanation of probabilistic patterns in actual deterministic newtonian systems —I explain how this is so in 4.2—. Then I bring up the development, carried out by Michael Strevens, that generalizes the conditions of application of the method and extends its applicability in the domain of complex sciences. Then, from these results stems the claim, I argue, that the same probabilistic patterns can be obtained *without requiring a particular form of the dynamics involved*. The results are thus expected to hold for a wider class of dynamical laws and, as such, for a wider set of possible trajectories in phase space. Specifically, any trajectory that displays a random-looking path —no matter if fruit of deterministic laws or not— will yield, when plugged into the method of arbitrary functions, a higher-level stable probability distribution. I argue that the random-looking path can be accomplished by any chaotic dynamics and also by, typically, a truly (process-)random trajectory. This result will be used to defend the main claim, that is, the possibility of stable regularities without fundamental guiding laws. I spell out the detailed form of the whole argument at the end of this section. A precise clarification of what is meant by 'chaos' and 'randomness' is respectively dealt in a coming subsection (p. 90) and in the annex A.

4.1.1 *From the humean point of view*

Though not necessary, it is going to be convenient to keep in mind the point of view of the humean about laws and express the situation at stake in his terms: consider all the possible trajectories a system can take through phase space —that is, without committing to specific laws—[1]; allegedly each of them has a best induction corresponding to a set of statements —the humean laws— that best describe such a trajectory. What I argue is that, for the set of those humean laws that display a random-looking trajectory —irrespective of whether the "best system account" induces in some cases deterministic laws or not— and in certain conditions, higher-level stable probabilities will

1 Even so, the set of possible trajectories will be restricted according to some principles that we would like to assume. One of the most obvious examples is the conservation of energy —as e.g. Maxwell also assumed—. This is elaborated in 4.4.2.

arise in the long term. The method of arbitrary functions grounds the explanation of this phenomenon. The conditions mentioned are non-dynamical constraints to the setup of the system under study, constraints that the method was already assuming in standard examples.

This humean point of view is not necessary but optional; however, it will be useful to adopt it, especially when the results defended are interpreted as the ground for the main goal of this thesis. The main goal is, I repeat, an account of stable behavior, of the stability of the at least apparent non-accidental regularities. Notably, the regularities described by the current best laws of physics. Likewise, and even more interesting, the regularities described by the ideal true final laws (cf. ch. 1 and 2). The account aims to explain the stability or robustness of the regularities without assuming fundamental pre-existing governing laws. Thus, it is within a framework like the humean, that lacks fundamental governing laws, that we can better understand this idea[2].

An objection

If we do not switch to this type of framework an objection can be raised, as we are showing something —the emergence of stable regularities, i.e. of lawful behavior— by always previously assuming some fundamental laws. This is a natural objection that could be raised if the reader does not switch to the scenario without guiding laws; in fact, my argument can be interpreted from both points of view —assuming laws and not assuming them— and thus can be misinterpreted, so let me clarify this now.

Reply

In order to consider a lawless scenario my strategy is indirect, I take into account not only one dynamics but all the possible dynamics and then I arrive to a certain conclusion; thus one can *both* affirm that:

1) what has been done is to show that the conclusion holds for all dynamics, in the sense that they are always assumed in some or other form —and this is a correct interpretation but not the interesting one—, or

2) by showing that the conclusion holds for all dynamics it follows that it will also hold for any lawless unguided behavior whose trajectory could have been described by any of those dynamics[3]. Therefore the conclusion holds also in a lawless scenario.

A lawless unguided behavior can exhibit many different trajectories in phase space; obviously it does not correspond to only one. That is a reason behind taking this approach that considers a whole set of trajectories. Hopefully, it considers at least the overwhelming majority of possible trajectories that an unguided random behavior could

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2 I say 'like the humean' because I am not committing to a humean framework but to any framework that dispenses of guiding fundamental laws.

3 Later I spell out this argument and I label it as 'B'. Its justification is dealt in 4.4.

take, as I argue in 4.4[4].

Compare again this proposal with the deflationist physical necessity as understood by a humean. He is also reluctant to bestow necessity to the humean mosaic, and so he is to the trajectories in phase space. There is no further explanation, according to him, as to why the whole humean mosaic is as it is, and even why it displays some extremely frequent —perhaps even exceptionless— regularities. The humean does not appeal to the notion of randomness when describing the humean mosaic, he talks of a 'brute fact': the mosaic is a primitive fact without a reason (cf. 1.4.2). This thesis shares the deflationist spirit in that it does not commit to a genuine physical necessity. However, unlike the humean literature, this thesis has argued that the "cosmic coincidence" of extremely stable regularities *requires* explanation (cf. 1.1 p.4 and 1.4.2 p.26). To provide a possible explanation I postulate some non-dynamical conditions, meaning by 'non-dynamical' that do not directly specify the form of the dynamics. I advance that these conditions are related with an assumed stable symmetrical configuration of the physical setup of the system under study.
In sum, from the humean point of view this chapter can be phrased in the following way: any random-looking trajectory in a subregion of the energy hypersurface that satisfies certain non-dynamical conditions will display, with probability 1, in the long run and in higher levels, stable regular behavior.

4.1.2 *What is meant by 'chaos'*

What is done in this chapter is not a modification of the method of arbitrary functions, it is an exploration of its sufficient conditions, arguing that the domain of applicability can be extended to a wide set of possible (fundamental) laws. More specifically, to any possible chaotic dynamics. This is the first claim that later I label as (A). Further, the method will also work for any lawless random behavior whose trajectory could be identified with any of those trajectories fruit of those chaotic dynamics. This is the conclusion of the chapter, which I later label as (C).
What is required of the chaotic trajectory is that it displays a random-looking "wandering" behavior through phase space. Thus, the step to a random dynamics seems, *prima facie*, rather straightforward, as a truly random dynamics will also typically possess such a property. However, it could be objected that true process-randomness is not always random-looking. But this will not represent a strong threat. It is true that it can be that a coin launched a million of times lands heads all the times. But as [Frigg, 2004] shows, the proportion of cases

4 For sake of clarity: when I talk of 'lawless unguided behavior' is the same as when, for instance, I employ the similar terms 'random wandering trajectory'.

in which process randomness does not display a product random sequence is small enough to be neglected for our purposes. The detailed argumentation of this is in 4.4 (and A.4). In brief, I claim that if the results hold for any chaotic trajectory they also hold for a random trajectory (what I later label as (B)).

Regarding the term 'chaotic', I use it in the technical sense though the colloquial sense also transmits the key characteristic pertinent here, that is, the random-looking aspect of the trajectory. What is the technical sense? And also: What exactly means random-looking? Regarding the former question there is not a shared answer. Traditionally (e.g. in the textbooks [Strogatz, 1994], [Hasselblatt and Katok, 2003]) 'chaos' has been defined in terms of, first of all, sensitive dependence on initial conditions, i.e.: in a dynamical system that is chaotic arbitrarily small variations in initial conditions become magnified over time —what is also known as exponential divergence of nearby trajectories. Regardless of the discussion about the proper definition, all the characteristics I mention —especially the coming ones— support my purpose (of sec. 4.4) of highlighting shared properties between chaotic and random trajectories.[Strogatz, 1994, 323] includes in the definition the requirement of *aperiodic long-term behavior*. It means that there are trajectories which do not settle down to fixed points, periodic orbits, or quasiperiodic orbits as $t \to \infty$. In [Hasselblatt and Katok, 2003], for a dynamical system to be chaotic it is also required that it is *topologically mixing* and that its *periodic orbits are dense*. The former means that the system evolves over time so that any given region or open set of its phase space eventually will overlap with any other given region. The latter that every point in the space is approached arbitrarily closely by periodic orbits. These three characteristics are especially pertinent because they remind and resemble the idea of ergodicity. As I clarify later, the random-looking aspect I have been appealing to is aimed to yield, together with other conditions, an approximate resemblance with an ideal ergodic trajectory (i.e. a trajectory that visits with same frequency regions of phase space of same volume). Finally, in addition to these definitions, let me mention the proposal by Charlotte Werndl [Werndl, 2009a] who defends a definition of chaos in terms of *mixing* [5]. This is also especially appropriate, as mixing implies ergodicity.

The random-looking aspect of the trajectory in phase space is achieved when the input is not the same, due to the exponential divergence of the chaotic dynamics. The resulting random walk is the first ingredient for having the necessary condition such that the method of arbitrary functions can be applied. As I explain later in 4.2, this condition is that, in the long term, all the phase space is visited by the system in approximately the same proportion. The reader familiar

5 She thus qualifies a previous definition of chaos in terms of the Kolmogorov property [Belot and Earman, 1997]. This is further elaborated in 4.4 (and A.4.2).

with the foundations of statistical mechanics can recognize that this requirement resembles the ergodic hypothesis.

4.1.3 *The argument*

Let me state explicitly what is the final conclusion of this chapter, call it (C):

(C): *'There are systems in certain conditions that exhibit stable simple regularities without being guided by any dynamics'*

The departure point is the formation of stable 'simple' regularities from an underlying complex dynamics[6]. This is basically the main upshot of [Strevens, 2003], which I explain in 4.2. This introduces an important general remark: an essential characteristic of my proposal is that it is based in the phenomenon of reduction of complexity. A phenomenon that occurs in the long term and in higher-levels according to the mathematical theorems stated in nex section.

Likewise, this scenario is the same as that considered by Nielsen's team, whose abundant work has been summarized in 3.3. Strevens' approach is abstract and suggests its application in different domains of high-level sciences, whereas these physicists address the origin of the fundamental symmetries. They have in mind not (only) a complex lower-level deterministic dynamics but a truly random dynamics.

Conclusion (C) aims to support a deflationist metaphysics of laws, the main subject of this dissertation, according to which non-accidental regularities are only strongly stable regularities, arisen not because some rules dictated it but because of the processes here explained, grounded in probabilistic theorems.

To arrive to conclusion (C) I divide the argument in two steps, whose truth together entails (C). The first is premise (A):

(A): *'There are systems in certain conditions that exhibit stable simple regularities for all lower-level chaotic dynamics'*

I show this via the method of arbitrary functions. The second step is premise (B):

(B): *'If an event X holds for all chaotic dynamics, then it holds in most cases with no dynamics at all'*

From these two premises, (A) and (B), it follows the conclusion (C). At the end of this chapter, p. 119, there is a recapitulation of the argument showing explicitly its logical form.

If (B) is true and there are (hopefully plausible) non-dynamical con-

6 'Simple' as opposed to 'complex'. Still, a more precise definition is in next section.

ditions that render (A) true, I have argued that this can be taken to be the central part of an explanation of the existence of non-accidental regularities in a scenario without governing fundamental laws. As such, this is a possible account of the notion of physical necessity and of the nature of laws.

In section 4.3 I argue for (A). In section 4.4 I argue for (B). The philosophical significance of (C) is stated in 5.5. Next section 4.2 paves the way by introducing the key points of the method of arbitrary functions.

Chapter 5 centers around the field of classical statistical mechanics, where it argues for the same conclusion (C) with a different strategy, namely with the approach called 'typicality'. An extension of the conclusion (C) in the domain of quantum mechanics is claimed in 5.3.1.

4.2 THE METHOD OF ARBITRARY FUNCTIONS

INTRODUCTION I expose in this section what has come to be known as 'method of arbitrary functions', which dates back to Von Kries [Kries, 1886] and Poincaré [Poincaré, 1896], including the recent insights developed by Michael Strevens[Strevens, 2003][7]. There is a lack of non-technical literature explaining the method and its possible significance, so a secondary aim of this section is to fill this gap. A primary aim is that, by spelling out the details of the method, the reader starts to see the main point of this chapter, i.e. the irrelevance of the details of the dynamics for obtaining the expected results of the method.

The way I present it is starting from the questions raised by Strevens' book "Bigger than chaos. Understanding complexity through probability" [Strevens, 2003]. This book and the paper "How are the sciences of complex systems possible?" [Strevens, 2005] seek to explain the fact that higher level laws are simple, whereas they are assumed to be reducible to lower level laws that are instead complex. By 'simple' is meant that they can be described by equations with few variables[8]. Clear examples of simple laws are the laws of thermodynamics or the laws of the increase rate of rabbits' population in a certain ecosystem. In each case there is an assumed complex microlevel dynamics to which the macrolevel laws reduce. The complexity in the microlevel is due to the very large number of degrees of freedom (e.g. the large number of particles) as well as the nonlinear interaction among them. In the example of thermodynamics, its microlevel is modeled by kinetic theory (or statistical mechanics), which presupposes a large number of particles ruled by chaotic newtonian deter-

The target: explaining the reduction of complexity

7 Other philosophers that have explored this technique are [Hopf, 1934], [Engel, 1992], [Plato, 1983] and [Myrvold].

8 Usually linear equations, but not necessarily, as nonlinear equations —like the logistic equation shown later— can produce simple behaviour too.

ministic dynamics. This microlevel model aims at a reductive explanation of the macrolevel laws of thermodynamics, as the tendency to equilibrium (the "minus 1st" law), the nondecrease of entropy (the 2^{nd} law), or the ideal gas law[9] $pV = kNT$. Strevens explains not only how this difference in the type of laws is possible within a reductionist framework, but also how, in certain cases, the simplicity of the higher-level laws is (surprisingly) due to the complexity of the lower-level laws. More specifically, it is the microlevel chaotic behavior, together with some other properties of the systems, that leads to the macrolevel simple laws[10].
It is puzzling indeed, let me remark, how there could be some simple regularities in an ecosystem, as for instance the increase rate of rabbits population. This regularity depends on an innumerable quantity of variables interacting among them in a complex way: the rabbit population depends on the quantity of predators, the rate of reproduction, the health status of each rabbit and each predator, the availability of resources, their individual geographical position, etc. Still, these sort of simple laws do exist in population ecology, as e.g. the Malthus equation, the Lotka-Volterra equation or the logistic equation. Then, as said, Strevens focuses the attention on the existence of simple dynamics in many macrolevel sciences in spite of the supposed microlevel complex dynamics that underlies them. The scope of his explanation spans many of the high-level sciences, from the behavior of gases to ecosystems, economy, meteorology, chemical reactions, linguistics or sociological statistics.

It is my aim to provide a further development of his insights in order to argue for a philosophical reinterpretation of the results that can be achieved with this technique, namely, an explanation of the formation of stable regularities. This is studied in 1) an abstract setting potentially applicable in those systems that verify the proper conditions (this chapter) and 2) in the case of statistical mechanics (ch. 5).

As highlighted before, there is something about low-level complexity and chaos itself that is directly responsible of high-level simplicity. This is just what in the approach of typicality (ch. 5) I also underline: in certain contexts, the fact that a dynamics is chaotic involves, justly because of this property, a simple higher-level behavior. In the case of typicality, it yields a typical behavior that is the tendency to equilibrium of the "minus first" law.

AN EXAMPLE Let's advance now in this gradual approach to the method. The best way to understand it is with an example. Later I will add a formal definition and further references. Among the many

9 Where p is pressure of the gas, V its volume, k is the Boltzmann's constant, N the number of particles of the gas and T the absolute temperature.

10 This does not mean that such type of explanation holds for *all* high-level simple behavior. Thanks to Michael Strevens for pointing me this and many other valuable observations.

possible examples, I choose the roulette wheel used by Strevens and originally used by Poincaré. Still, other gambling games could have been chosen, as well as more "exotic" examples like the interesting application of the method that Poincaré himself carried out for proving the equidistribution of planets in the celestial sky [Poincaré, 1896, 129], [Engel, 1992, 68]. I have schematized the mathematical formulation of this example later in p. 104.

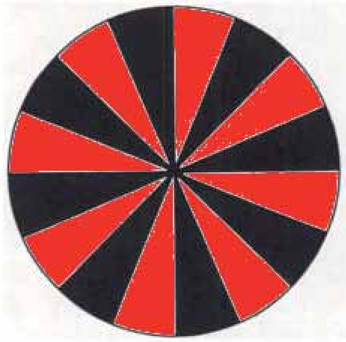


Figure 6: A wheel of fortune

The roulette wheel

Consider a roulette wheel. [Strevens, 2003, p.48] says:

> "The complex probability of the ball's ending up in a red section is determined, like all complex probabilities, by two things: the physics of the wheel, represented by an evolution function, and the distribution of the initial conditions, represented by an ic-density. The initial condition distribution will be determined by facts about the croupier who is spinning the wheel. Because the croupier changes from time to time, the relevant ic-density presumably changes from time to time as well. But, as everyone knows, the probability of obtaining red remains the same".

This fragment states a key idea that leads to the conclusion that almost any probabilistic distribution of the initial conditions will determine approximately the same probability at the outcome. Let me note that in this quote what he calls the "physics of the system" is fixed; later I will investigate the possibility of variation of it.
Now let's analyze how it is that the roulette wheel tends to exhibit, in the long run, a stable 50/50 frequency of red and black outcomes, and does so irrespective of the croupier (irrespective of the way the croupier uses to launch the ball). The outcome of a trial on the wheel is determined by the initial speed with which the wheel is spun, which can be modeled by a random variable ζ. The contribution of the croupier to the probabilities of red and black, then, takes the form of a probability distribution over the initial spin speed ζ. While this ic-variable ζ can take different distributions, the probability at the outcome, as we know, is always the same, 0.5 for red and 0.5 for black. We then have a constancy of the probability at the outcome,

independently of the initial probability distribution. A way of understanding this (remarkable) fact is the following. The outcome red, equidistributed between the black slots, is represented in function of the variable 'speed of the wheel' ζ as seen in figure 7. Now take

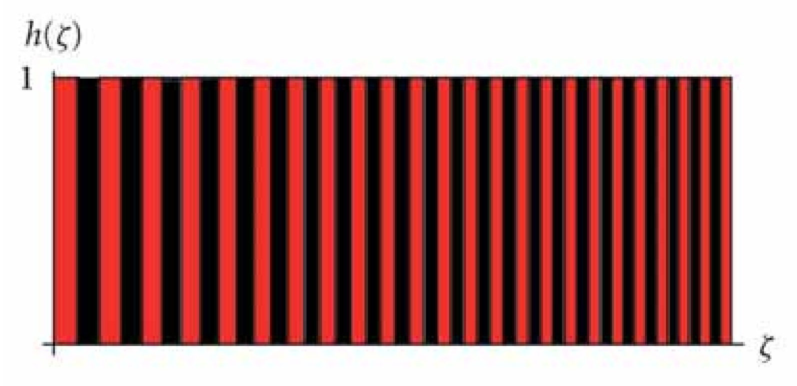


Figure 7: Red and black outcomes of a roulette in function of the initial speed

two different initial probability distributions, one corresponding to an enthusiastic croupier that uses to launch harder than a second more mellow croupier; both are represented in figure 8. We already have the intuition —and the casino already knows— that changing the croupier does not affect the final probability at the outcome. The reason is that the contributions of each slice of red over the entire graphic will be approximately the same proportion as those of the black slices and, crucially, it will be so also if we fix the attention to a small enough region of possible values of the speed (i.e. it will be so in any small region of the domain of the function of fig. 7). Thus, for each croupier the proportion of outcomes will approximate to 0.5.

The verification of 'micro-constancy'

For this to occur there must be an alternation between red and black *quick* (i.e. the alternation must be highly sensitive to the variation of the variable —speed in this case—) and *constant* (i.e. its pattern must be constantly repeated)[11]. Strevens labeled in a single term these two crucial properties as the property of '*microconstancy*'. This property will be necessary for the obtaining of a stable frequency at the outcome. It is easy to visualize graphically (see figure 7).

Plus the property of smoothness

Together with this property, the other necessary condition is that the probability distribution over the ic-values has to be *smooth*. That is, the croupier can be stronger or more mellow, but it cannot have a highly peaked distribution that does not cover a whole pattern (that is, the variance of the distribution cannot be too small). But not covering the whole pattern will be difficult when the patterns are constantly repeated, i.e. when the aforementioned 'microconstancy' obtains. A visualization of this requirement is obtained when we see how a non-smooth distribution as the one of figure 9 would clearly

11 The relativity of terms as 'quick' or 'small' will be soon considered.

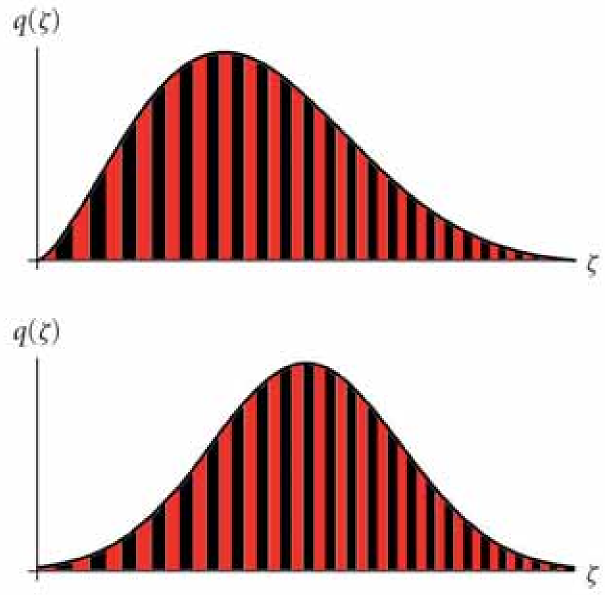


Figure 8: The frequency distribution of speeds for the throws of two different croupiers, superimposed over the outcome in function of the initial speed

not display the output distribution equal to the 50/50 strike ratio. Thus,

> "a smooth density will be approximately flat over any neighboring pair of red and black areas in the evolution function, for which reason the contribution made by that part of the ic-density to the probability of red will be approximately equal to the contribution made to the probability of black" [Strevens, 2003, p.50].

Strevens generalizes the case presented showing that, in general, if the evolution function for an outcome *e* is microconstant, any smooth ic-density determines the same probability for *e*, equal to the ratio of red to black. This is the explanation of 'microlevel insensitivity', that is, the "washing out" of the microlevel details.

4.2.1 *Smooth arbitrary functions*

Let's elucidate better what there is behind the method of arbitrary functions. Consider a system ruled by deterministic nonlinear dynamics and whose initial condition values are variable according to some arbitrary probabilistic function[12]. If we assume microconstancy there is a constant ratio of possible outcomes when the value of a variable

12 These are the 'arbitrary functions' that name the method. It says 'arbitrary' because this probability distribution can be arbitrary, that is, can take any form —as long as it is smooth. Notably, [Strevens, 2013, 12.5] argues that also the lack of a probabilistic distribution will work.

changes, and this occurs within any small region. (In the case of the wheel: when the value of the spin speed changes, the proportion of the outcomes in a given small region is constant and is the same proportion for different regions).
Optionally, we can take the variation of the input variable ('ic-variables' in Strevens' terminology) as representing an ensemble of systems, each of which possesses one value of that variable. (In the case of the wheel: we have a set of wheels each with a croupier, each croupier launches with a specific spin speed, then the global ensemble will be represented by a probability distribution). Later will be useful to take this point of view.
The constant ratio visualized in the evolution function (fig. 7) of the ensemble turns to be the final proportion of outcomes. Why this is so and why the functions at the input can be arbitrary?
Curiously, one of the keys is that what you have as an input is not a determined unique value but a probability distribution; and the more distributed —so the more smooth— the better. Because then you will have different values that will be distributed over the constant evolution function (as in figure 8). If it were just a unique input (i.e. the spin speed would be exactly the same in many trials) the final outcome would always be the same (assuming a deterministic dynamics), always red or always black.The surprising fact of the method is that we obtain a stable outcome when the input is not unique but runs across different possible values smoothly distributed, for any possible form of this distribution —i.e. for any "arbitrary function". Therefore, it is to be remarked that for the method these details are irrelevant —the particular form of the (smooth) input probabilistic distribution.

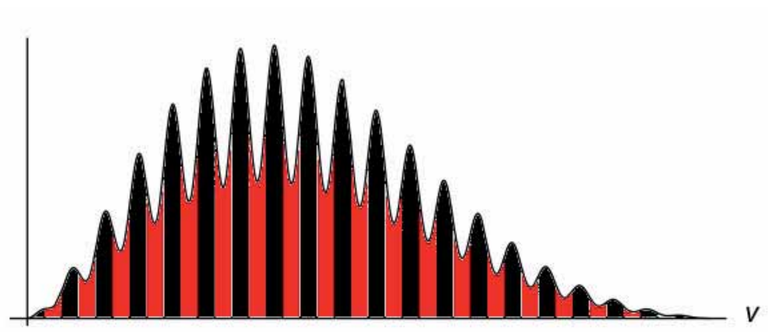


Figure 9: This is not a smooth distribution

With smoothness, a bit more specifically, it is meant that the function is absolutely continuous with respect to the variable. The motivation for requiring smoothness is that it will make possible to approximate the slow variation of the function in a given small domain of the function as if it were a flat distribution. This will be combined with the microconstant property, which will mean that in this same small domain the pattern of outcomes will be exhibited[13].

ON ASSUMING SMOOTHNESS Poincaré himself put forward the question as to why to accept the continuity requirement. He answered by appealing to the unreasonableness that would mean its denial in the physical systems he was considering —in the roulette and in a system of planets equidistributed around a star. He argues this in his famous "Science and Hypothesis" [Poincaré, 1905, p.222]. The choice of a discontinuous function would have been unreasonable, he says. Let's accept by now this answer, which at least seems intuitive.

Poincaré's 1st reply

But still accepting continuity there is another possible pitfall. There are some continuous cases that would not lead to the desired results. In the mentioned example of the equidistribution of planets around the Sun, Poincaré remarks one possible initial configuration of values of the variables that would entail the alignment of all the planets, that is, the opposite of the expected equidistribution. This corresponds to the case in which the two random variables X and Y, ignoring for the moment what they refer to (see p.104 for the answer), are related such that $Y = \frac{\pi}{2} - Xt$. It amounts to a case in which the planets would have *"all been lying on a kind of spiral of peculiar form, with its spires very close together"* [Poincaré, 1905, p.223].

I am bringing forward this objection mostly for the interest in the type of answer he gives. Poincaré says that "*all will admit that such an initial distribution is extremely improbable*". Here he is giving his opinion about a crucial point that shows up along the whole dissertation, as I point out right away. It is a point about the meaningfulness of (or lack of) assigning probabilities to events as the initial configuration of a system whose manifestation is unique, e.g. the universe. His answer appeals to the principle of sufficient reason in a, perhaps, disputable idiosyncratic way: we might admit, he says, that there could be a reason for the initial configuration of planets were distributed, for instance, in a straight line, or either in any irregular way. However, it is difficult to conceive a reason such that the initial distribution is the highly regular and complicated one described before. In his words: *"there is no sufficient reason for the unknown cause that gave them birth to have acted along a curve so regular and yet so complicated"*, adding *"which would have been expressly chosen so that the distribution at the present day would not be uniform"* [Poincaré, 1905].

Poincaré's 2nd reply: Such counterexample is highly unlikely, ...

... for it is difficult to conceive a reason behind.

Poincaré's two answers, with his own phrasing —relying on the hard to conceive reason behind—, shares the same moral of what I defend differently throughout the whole dissertation. Namely, that these exceptional cases need not be treated as unsurmountable objec-

A type of argument common throughout this thesis

13 The 'constant' ratio of the term 'microconstancy' is the pattern exhibited, and the 'micro' is due to its application in said small domain.

tions as long as we just relax our demands of what is going to be proved and with which certainty. Like the case of $\epsilon-$ergodicity instead of ergodicity, or the case of typicality (which neglects cases of Lebesgue measure 0), here the level of confirmation must be relaxed to a minor degree: in this and the rest of chapters I appeal to probabilistic theorems of convergence[14], so in the ideal circumstances in which the sufficient conditions are satisfied the results obtained will not hold with certainty; what can be mantained instead is that there will be a convergence in probability. And (more disturbing, though) the rates of convergence might be far from acceptable depending on the case[15].

How can this be explicitly defended? To do so, I contend the intuitive but criticized relation between randomness and an approximately uniform probability distribution. This relation is a necessary implicit premise such that both Poincaré's and the arguments here follow, for it is implicit in both that any prior option (e.g. any specific spatial distribution of planets in Poincaré's case, or any different humean mosaic in one of my cases) is not more likely than any other. In a nutshell, this premise is what grounds the step that goes from 'X is much more numerous in the space of possibilities' to 'X much more probable' —or, as I prefer to put it, from (a): 'X is much more numerous in the space of possibilities' to 'Statement (a) is an explanation of the occurrence of X'.
This step is present in:

- 1.4.2, where I criticize the postulation of the actual humean mosaic as a brute fact, stressing that such a whimsical set of laws ("so regular and yet so complicated") is in need of explanation (something elaborated at length in ch. 2);
- at the end of 3.2 defending Wheeler's approach from an objection, i.e. against the argument that a uniform distribution is (exclusively) the result of a stochastic law;
- chapter 5, implicit in the very idea of typicality;
- A.4, where I defend a definition of process-randomness in terms of its outcomes;
- here in 4.4 where, in the same line of A.4, I argue that from a proportion of cases of measure 1 verifying property X it follows that the probability of X is close to 1 —or, as I prefer, what I argue is that there is a proportion of cases of measure 1 verifying property X, and this explains the occurrence of X.

14 Basically the law of large numbers in its weak version (cf. A.3.1), and the analogous version in ergodic theory that is the ergodic theorem for markov chains (cf. [Strevens, 2003, pp. 10, 376]).
15 But see later how theorem 2 p. 104 guarantees good rates of convergence when smoothness is assumed.

Other hints on the justification of smoothness

After this long interlude, let me recapitulate: a smooth ic-distribution has been assumed as one of the necessary conditions. How plausible is such an assumption? Are there any arguments in its support?
In addition to Poincarés observations above, [Strevens, 2013, 12.3] elaborates[16] an argument based on the idea that enviromental noise, or surrogates in its place, provoke a general tendency in the ic-distributions to smooth out or, even, to uniformity. Likewise, [North, 2010] defends something stronger, namely a uniform distribution, but restricted to hold for a narrower set of variables, i.e. the "canonical variables" of fundamental physics. From now on I won't question the smoothness assumption.

INTUITIVE EXPLANATION OF THE RESULTS OF THE METHOD After these observations regarding smoothness, let's continue with the elucidation of the method. We have a method that tells us that a probability distribution at the input will spread its values over the evolution function in a way that will inevitably equilibrate and converge in the long run to the constant proportion manifested in the evolution function. I provide now a non-technical explanation of why this is so, followed by a formal statement of the method and references for further details.
Keep in mind figure 8. Both the values around the mean as well as those in the tails of the arbitrary ic-function will fall under some region or other of the evolution function; to visualize it, take for instance a region of the right side of the distribution of figure 8, that is, a part of the decreasing slope. As long as the input distribution is smooth enough, some values will fall in one microregion corresponding to red and some others in the contiguous region corresponding to black. The values of the black region will be less, but: 1) the smaller are the divisions in the evolution function the less accentuated will be the difference (this is the 'micro' of the 'microconstancy'), 2) the more smooth is the input distribution the less accentuated will be the difference (this is the smoothness condition) and 3) in global, it can be seen how this difference when comparing two contiguous regions is compensated by comparing one of the regions with the other contiguous at the other side of the curve. In sum, given that the ratio is micro-constant, when a high number of trials is made, the final distribution will correspond to the constant ratio of the evolution function irrespective of the form of the smooth input probability distribution. The resulting proportion of outcomes coincides with the constant ratio because for each small region (but not too small as to not contain at least one pattern) of the curve we get approximately the constant ratio —in a way that the error produced by the slope gets compensated in the total addition of regions.

16 Actually he reelaborates more clearly what was spelled out in [Strevens, 2003, 2.53]

4.2.2 *When does microconstancy obtain?*

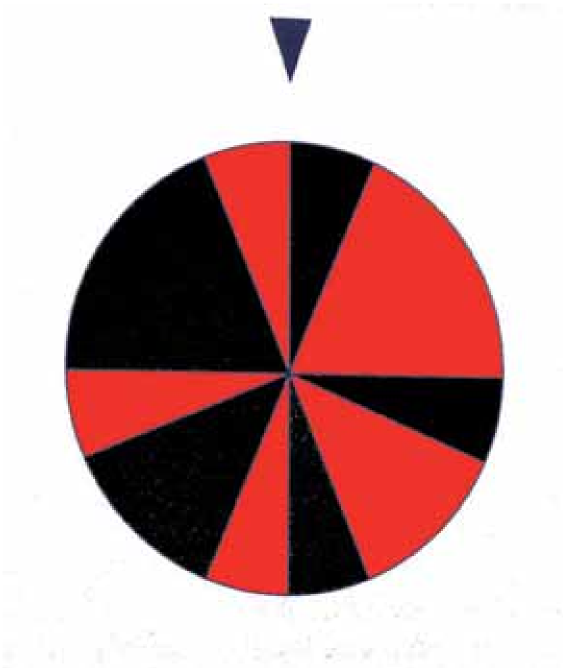


Figure 10: A motley wheel

A variation of the example of the roulette is ideal to show a way to get microconstancy, i.e. to get a pattern frequently repeated in the evolution function.

In the previous case of the roulette, the many cycles around the wheel imply that the pattern reflecting the paint scheme of the wheel appears many times. So we find it in the evolution function of fig. 7. A whole cycle of the wheel yields a repetition of the whole pattern, which in fig. 7 consists of a pattern of 8 reds plus 8 black outcomes (according to how I draw it at the beginning of the chapter, in fig. 6). Now, Consider an asymmetrically painted roulette as the one of figure 10[17]. Here the pattern repeated would be like the one of fig. 11 for one cycle. As such, it would *not* be microconstant, because an ic-distribution superimposed would favor more one zone of the whole pattern than other zones. However, there are several ways according to which a variation in the setup can be made such that the evolution function becomes microconstant. One labeled 'embedding' is discussed in [Strevens, 2013, 78-82]. Just let me mention a simpler one: extend the range of the variable spin speed much beyond the value 'M' of fig. 11. 'M' coincided with the wheel making just one cycle. But allowing for the wheel to spin and spin for many cycles, we can start to obtain an evolution function every time more "com-

17 This explanation is based in [Strevens, 2013, p. 78]. In that book he treats with many other examples, so I recommend it to the interested reader as well as to the skeptic.

pressed", as in fig. 12. Thus the evolution function becomes more and more microconstant.

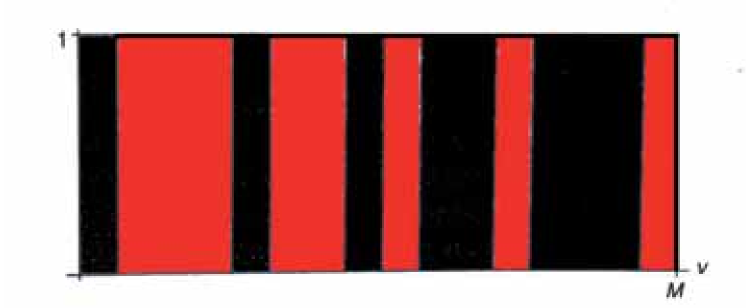


Figure 11: A non microconstant evolution function

Figure 12: The setup allows for many cycles. The evolution function is microconstant.

Let me now recapitulate by highlighting why we needed the two conditions. If we had a uniform distribution of the possible initial conditions, in the long term all the values would have been equidistributed among all the possible outcomes corresponding to each initial condition. Each initial condition would have been approximately equally frequent (because the distribution is uniform), so the final probabilistic outcome would mirror the pattern drawn in the evolution function. In this case it would not have been necessary that the evolution function is microconstant; it can just display an asymmetric pattern like in fig. 11. However, if we have microconstancy we can dispense of the uniform distribution and allow any smooth arbitrary function. In this case we also get an outcome that is proportional to the patterns manifested in the evolution function. Given the smallness and repetition of the patterns, we can balance and compensate the unequal distribution of a non-uniform probabilistic distribution. In this way we achieve a proportional visitation rate of the outcomes for any IC-distribution.

All this has been an outline of how and why the method works. A rigorous proof and generalization can be found in [Engel, 1992]. Yet, some of the most representative theorems are presented right away.

4.2.3 *Theorems*

I want to end the section by stating the theorems in which the method is based.I state the formal definition in general terms for a physical system with one degree of freedom. The generalization to higher dimensions can be found in [Engel, 1992, ch. 4] and is analogous to the one-dimensional case (compare the necessary and sufficient conditions for the two cases as summarized in [Engel, 1992, p.35 and p.72]).
It has been proved that, being X a random variable and $t \in \mathbb{R}$ large, the random variable (tX)(mod1) converges in the variation distance to a uniform distribution on the unit interval if and only if X has a density. This is proved and succintly stated in theorem 5.3 of [Engel, 1992][18]:

Theorem 1. *A necessary and sufficient condition for convergence in the variation distance is that X have a density*

Though, additional smoothness assumptions are needed to have good rates of convergence, and so is stated in theorem 3.1 of [Engel, 1992]:

Theorem 2. *The random variable (nX)(mod1), which is the fractional part of the product of a large real number* n *and a random variable X, converges in the weak-star topology to a uniform distribution in [0,1] when n tends to infinity if and only if the characteristic function of X vanishes at infinity.*

I include here the formulation by Poincaré of the case of the roulette wheel. Following [Engel, 1992, 3.1], the problem considered is finding conditions on the random variable X under which, being n a positive integer

$$\lim_{n\to\infty} P\{(nX)(\mathrm{mod}1) \leqslant 1/2\} = \frac{1}{2} \tag{5}$$

Equation 5 has been proved to hold for any random variable X with a density[19].

The law of small planets

Let me state here also the case of the law of small planets that Poincaré elaborated. It helps in the comprehension of the method, is related to physical macroscopic regularities and I think it is beautiful. [Poincaré, 1896] considered a large number of planets orbiting circularly around a star. His aim was to prove that after enough time the planets are distributed uniformly among all the signs of

18 Some technical clarifications: The weak star topology on a set X, with respect to a family of functions on X, is defined as the coarsest topology on X which makes these functions continuous. The variation distance between two n-dimensional random vectors X and Y, $d\nu(X,Y)$, is defined as $\sup_A \mid P(X \in A) - P(Y \in A) \mid$. A sequence X_k converges in variation distance to X if $d\nu(X_k, X)$ tends to zero as k tends to infinity.

19 An extension by Fréchet(1921) upon [Poincaré, 1896].

the zodiac. Denote by X the angular velocity of one of these planets and by Y its distance to the star at a fixed time t_x. Assume that both are unknown and described by a continuous joint density. The planet's distance at a time t, $L(t)$, is equal to the random variable $L(t) = (tX + Y)(mod 2\pi)$. The proof assumes the joint density of X and Y to be sufficiently smooth (absolutely continuous with respect to x) and then uses Fourier methods to prove that $(tX + Y)(mod 2\pi)$ converges in the weak-star topology to a random variable U uniform on $[0, 2\pi]$ as t tends to infinity.

Stronger smoothness for realistic (finite) cases

Additionally, [Strevens, 2003] argues that the analysis presented, as such, is philosophically flawed for finite, if large, values of the variable at stake: it needs a further constraint stronger than the smoothness described above (as absolute continuity). The sort of smoothness that it has to be demanded is what he dubs 'macroperiodicity', which roughly demands that the initial probability distribution has to be approximately uniform over almost all micro-sized regions (cf. [Strevens, 2003, 2.23, 2.5]).

Resemblance with other fields

Before concluding this exposition, an important note is that the results of this method resemble other approaches found in statistical mechanics, namely 1) Ergodic theory, 2) what Jos Uffink calls 'coarse-graining stochastic dynamics' [Uffink, 2006, 135] as well as 3) 'typicality' approaches to equilibrium, especially when it is stressed the "typicality of dynamics", as I do in chapter 5. These approaches share central points with the method of arbitrary functions. They share a) the appeal to probabilistic convergence, b) the employment of an apparently random initial element —random variables in the MAF, a random-looking trajectory in the others— and c) the aim of explaining regular macroscopic behavior: what in statistical mechanics is the tendency to the equilibrium macrostate, in [Strevens, 2003] is generalized, with the subsequent addition of further constraints, to an open variety of situations.

4.3 THE ROLE OF THE DYNAMICS

In the previous section I have summarized central aspects of the method of arbitrary functions and of the generalization elaborated in [Strevens, 2003]. Two conditions, the so called microconstancy and smoothness, must be fulfilled by the system so that simple stable probabilistic behavior arises. This holds in cases in which "*the physics of the wheel*" is ruled by the actual microlevel deterministic dynamics. This section aims at showing that the role of the actual microlevel dynamical laws is dispensable. If this is the case, simple behavior can emerge not only from the presupposed actual chaotic deterministic laws but from a larger set of (metaphysically) possible chaotic laws. This is what I labeled before (p. 92) as '(A)'. *Prima facie*, this extension to a wider set of laws should not be very controversial given that the original analysis in [Strevens, 2003] (granting that is correct) is generic and oriented to be applied in different cases. In fact, Strevens made a tangential observation that here instead becomes the central concern:

> "The value of a microconstant probability may come out the same *on many different, competing stories about fundamental physics*. The probability of heads on a tossed coin, for example, is one half in Newtonian physics, quantum physics, and the physics of medieval impetus theory" [Strevens, 2003, p.62 (italics added)][20].

This section aims to show explicitly that this statement is true.
Then, together with (B), I advocate for the possibility of stable regularities without being ruled by any particular dynamics, that is, being the system genuinely randomly wandering through phase space — what I labeled as '(C)'. As previously stated, these stable regularities can correspond, in any non-governing view of laws, to the only apparent non-accidental regularities that we acknowledge in the world. Or better, these stable regularities can correspond to *the regularities that are summarized by our best laws of physics*.

After the previous section I am in a position where I can specify the non-dynamical conditions that (A) was appealing to. (A) is restated here:

(A): *'There are systems in certain conditions that exhibit stable simple regularities for all lower-level chaotic dynamics'*

Fortunately, no new constraints need to be added to the generic analysis presented so far. The two conditions cited —microconstancy and smoothness— suffice. I briefly cited before (4.2.1) what has been argued for the justification of the smoothness condition —citing Poincaré, Strevens, and Jill North. Then, for a system to verify the first prop-

20 And the same is said in [Strevens, 1998, p.19]

erty, microconstancy, I propose (following [Strevens, 2013]) that some particular physical non-dynamical constraints suffice and, crucially, do so for any possible chaotic dynamics. I propose that these non-dynamical constraints are certain spatial symmetries of the setup of the system under consideration. This is a "non-dynamical" proposal based in Strevens' suggestions, though he opts for another "dynamical" option. He does so for weak and somehow irrelevant reasons, so we can still mantain the "non-dynamical" proposal.

4.3.1 *Random looking and proportional visitation rate*

It is important to understand what do we achieve when we have these two conditions. The idea is that the conditions simulate what would be achieved by a truly ergodic trajectory. They guarantee, together with a chaotic trajectory, a stable proportional visitation rate among all the outcomes. As analyzed in [Strevens, 2013], the proportional — in most examples equidistributed— visitation rate of the possible values is accomplished in the long random wandering across the space of possibilities and is what we intuitively believe that effectively happens in plenty of cases[21]. This happens (or we think this happens) when throwing dice, including in the process many variations involving shaking, bouncing and rolling the dice, drawing balls from urns, roulette wheels of different sorts, in less artificial setups as in certain ecosystems, certain arguments present in Darwin's hypothesis of natural selection, the model of statistical mechanics of hard spheres bouncing in a container and probably many other cases within complex sciences. In all these examples, the real reason of the correct belief of equiprobability (of equal —or proportional— visitation rate of every possibility) is, arguably, related with microconstancy. This will be more clear when I justify the non-dynamical account of microconstancy summarized in (S):

(S): *'There are setups with certain spatial symmetries such that, if there is sensitive dependence on probabilistic initial conditions, then the evolution function is microconstant'*

The explanation of (S) I am offering in the next subsection paves the way to my claim (A). If (S) is correct, I can reformulate (A) (p. 92) making explicit the 'certain conditions' referred and, in turn, getting rid of the neologism 'microconstancy' by substituting it according to (S). Then we arrive to something like (A*):

21 We believe this because we believe that a random walk takes place, though this not necessarily implies equiprobability. A speculation of the psychology behind our ascriptions of equiprobability is found in [Strevens, 2013, ch.4].

(A*): *'A stable probabilistic pattern at the outcome holds, in the long term, for any chaotic dynamics constrained by some facts about physical symmetries and with a smooth input probabilistic distribution'*.

4.3.2 *(Equi)probability from physical symmetries and not from epistemic ignorance*

What has been said so far suggests a link from physical symmetries to objective probabilities (evident in my statement (A*))[22]. Let me elaborate this generalization. [North, 2010] and [Strevens, 2013] explain how the principle of indifference can be given a non-epistemic reading, thus obtaining an alternative to the classical interpretation of probability that avoids its main problems. The problems I refer to are basically the claim that you can extract knowledge from ignorance and the Bertrand paradoxes. The works cited remark that there is a process of extraction of probabilities from symmetry arguments that not necessarily must involve symmetries of the (lack of) knowledge; that is, you can infer probabilities from *physical* symmetries. The principle of indifference takes us from a symmetrical situation due to epistemic ignorance —so, symmetries in our knowledge— to probabilities. The suspicious and widely criticized a priori gain of knowledge from ignorance involved in the principle of indifference (see e.g. [Gillies, 2000] or [Myrvold]) does not appear in the new point of view[23]. Here the knowledge of the probabilities comes from the knowledge of physical symmetries. Then, the objective probabilities of some events are such in virtue of —at least in part— the physical symmetries of the world. This new point of view brings up a nice explanation of the failure of the classical interpretation. It explains why it seemed so reasonable and straightforward, in many cases, to think according to the traditional epistemic reading of the principle of indifference: it was a wrong way of thinking but it was bolstered by the coincidence, in paradigmatic cases, of the *correct* physical symmetries of the mechanism and the symmetries in our knowledge (i.e. our ignorance) about the outcome. Thus the former was the real responsible of the probabilities but it was the latter wrongly taken to be the responsible.

After this general observation, let's continue with the justification of (S) (and therefore of (A*) and (A)).

22 This link is what Strevens suggested initially in the paper [Strevens, 1998] that has now become the book 'Tychomancy' [Strevens, 2013].

23 Allegedly, the paradoxes related with the choice of a reference class, versions of the Bertrand paradox, are also solved.

4.3.3 *The dispensable role of the dynamics for microconstancy*

To depart from a settled base that fits my goals, I have been taking Strevens' claims as the departure point of my arguments (though as said before, Strevens' claims are not evident or uncontroversial, so I try to provide further justifications when needed). So let's continue from his approach. In later papers the ideas of [Strevens, 2003] have been developed towards different goals. All are different from mine but they describe features that indirectly support the aim of this section, i.e. that the particular details of the dynamics are dispensable. For instance, in [Strevens, 2011] his aim is that of a metaphysical reduction of a stable probability distribution from something non-probabilistic. My aim instead is that of a metaphysical reduction of a stable probability distribution from something not only non-probabilistic but also non-dynamical. With 'non-dynamical' I mean that there must not be any specific mention to any constraint that defines the form of the dynamics. I claim that this can be achieved following, with few qualifications, the theory stated in [Strevens, 2003] and [Strevens, 2013]. There it is concluded that, in order to apply the method in a generic situation, the two properties, microconstancy and smoothness, must be satisfied. This section focuses on the first property. In order to satisfy it, there must be a *constant* ratio of the outcomes[24] and that it alternates *frequently*. So the question that has to be formulated in order to understand and generalize microconstancy is: *on what depends the constant stable form of the evolution function and its frequent alternation?* This question can be answered, I argue, without appeal to the dynamics. If this is so, the answer represents a justification of (A) (p. 92), and as such it represents the core of this thesis. As previously stated, the non-dynamical condition proposed consists in the stable symmetrical structure that must be exhibited by the spatial configuration of the system. In some sense, then, the stability assumed in the space is "propagated" to the dynamics, whose stability I "refused" to assume *ab initio* for several reasons (ch. 1). The stability of the dynamics is, in fact, the *explanandum* of the whole dissertation. Whether is better or worse to have the stability postulated in the space / initial conditions than in the laws is a question I will not treat. Even so, it seems a philosophical question worthy of further research.

4.3.3.1 *Demystifying microconstancy*

Besides the non-dynamical account of microconstancy assessed here, there can be other proposals equally valid. In fact, [Strevens, 2013] opts for another candidate involving the dynamics after proposing the one here defended. Now let's deepen into the non-dynamical can-

24 In the case of the roulette of fig. 7 is 50:50, but any other ratio would hold too, as in fig. 10.

didate.
Microconstancy can be defined as the property that obtains if and only if, within any small neighborhood of the evolution function, the proportion of initial conditions producing a given outcome is the same (see fig. 7 p. 96). First of all, we must be aware that the fact that the wheel alternates sections of equal length is not necessary for microconstancy; had the wheel been painted so that one third of its slots were red and two thirds black, the evolution function would be different but it still would display a constant ratio of outcomes, now of 1/3 for red and 2/3 for black (figs. 10 and 12).

Now, would it still be microconstant if we modify the actual dynamics of the wheel, for instance by taking the case of a wheel constructed so that it wobbled as it rotated around its axis? *Prima facie* it is not at all clear that it would still be microconstant. Still, I want to show that it would be so for this case as well as all for the possible variations of this sort.
My answer is that the quick alternation (the 'micro') is achieved by the random-looking behavior of the chaotic trajectories and the constancy can be achieved by the symmetry of the physical configuration of the system.

Microconstancy through the symmetry of the physical configuration

We can start to see this in the simple case study of the roulette wheel: the microconstancy is given by the wheel's symmetric paint scheme and the rotational symmetry of the wheel's dynamics. The latter, in turn, is related with the circular shape of the wheel and with the cycles it makes. In fact, Strevens himself suggested this:

> "The physical details underlying these facts are unimportant in themselves. In a wheel that comes slowly to a halt, for example, the precise facts about the frictional forces that slow the wheel do not matter. Only one fact about these forces matters, the rather abstract fact of the rotational symmetry of their combined effect." [Strevens, 2003, p. 62].

He adds that the same is true for the case of a tossed coin: here only the symmetrical distribution of mass in the coin matters. As I am arguing in the next paragraphs, it turns out that we can infer the value of a probability "*from few facts about physical symmetries, even if one knows very little about physics*" [*ibidem*].

The spatial symmetry is a feature that the setup of the roulette wheel exhibits as well as some other closed systems in science (a gas in a box, ecosystems, etc.). Perhaps also the whole universe in some initial configuration. This last claim, considered also in p.138 (5.3), 5.5 and explored from a different approach in 3.3.2, is not strictly necessary given the abstract analysis I carry out along this chapter, leaving open the applicability to each particular system. However, as the thesis is, in all, about the metaphysics of fundamental laws, the applicability of this theory in the cosmological domain is of interest. More

exactly, its applicability would support a universal unified version of laws. In that case, the allegedly universal non-accidental regularities that the laws of physics state —the Schrödinger equation, the Einstein Field Equations and so on, or better, the regularities that the true final laws would describe— are a result of the process described here. This would involve a fundamental level of either high complexity or randomness. As such, regularities would not be the result of a "pre-existing" governing set of rules.
Otherwise, the lack of application in a cosmological scale would be more in tune with an antirealist account of laws, in the sense of something like oases of order emerging among the "chaos", *a la* Cartwright [Cartwright, 1999]. I.e.: the convergence theorem applying not globally but only in a finite set of closed local setups that verify the proper conditions; a sort of "nomological machines"[25]. More on this in the referred sections.

Coming back to what makes a dynamics microconstant, what is needed is whatever guarantees the existence of relevant symmetries in the operation of the mechanism, for example, "*whatever entails that a spinning coin takes about the same time for each half-revolution, or that a spinning roulette wheel takes about the same time for each 1/36th of a revolution*" [Strevens, 1998, p. 19]. In the case of the wheel, the equal constant ratio is a consequence of the fact that, at any point in any spin, the wheel takes equal time to rotate through a red segment as it does to rotate through a black segment. Further, another contribution to the microconstancy of the evolution function in the wheel example comes from the fact that the repetition of the whole pattern in the evolution function occurs each time the wheel performs a whole cycle. In this case, it has been thanks to the rotational symmetry of the wheel that allowed the constant pattern.
In the case of the coin, the symmetry is due to the fact that the coin has two equal sides. The time one side takes to flip has to be the same time the other side takes. Crucially, there *cannot be* a preference for any side. This observation links the role of the spatial symmetries with the dispensable role of the dynamics. There is no physical difference between the sides of the coin and as such they cannot be differentiated. There cannot be a preference for any side whatever the laws are: it is not logically possible that any law whatsoever differentiates one side from the other. Thus, necessarily, the symmetry will hold in all the possible laws.
The same occurs when the spherical shape of the gas particles is involved: the symmetry of the shape makes (metaphysically) impossible to have a dynamics that discriminates a certain side of the particle having a different reaction to a collision.

25 The laws just mentioned (Schrödinger equation, EFE, etc.) could be cited here too, as the antirealist does not consider them as universal.

In sum, in the three cases there is a spatial symmetry responsible of the microconstancy. Furthermore, the spatial symmetries involved are features treated necessarily in the same way by any dynamics. For, how could any law distinguish what is indistinguishable in virtue of the spatial symmetry? A spatial symmetry is a mathematical property of objects. Two paradigmatic cases are the homogeneity of the whole space or a geometrical symmetry. The latter is an invariance under a specified group of transformations, which can be rotations, reflections or translations, among others. What can happen is that a law ignores the symmetry while other laws do not. This is, for the law it just does not matter the symmetric features of certain objects because, say, the law is about certain interactions in other ontological level. But no law whatsoever can distinguish —in the sense of treat differently— the invariant aspects that the symmetry is gathering. For instance, the lack of a preferred region of a spherical shape. It just does not matter how the law is because the properties referred as spatial symmetries are independent of any dynamical consideration.

Clarifications Two important remarks to avoid confusion. First, do not confuse these symmetries with the symmetries of laws. The latter are essentially related with the dynamics[26]. The former are by definition independent of the time evolution of the system they belong to. Second, what of course can happen is that the spatial symmetry is broken because one dynamics dictated so (while not all). In this case, it cancels the application of the method and therefore our desired results. But the stability of the symmetries is just something that is postulated in the premises of (S) and (A*); if the assumption does not hold then the conclusion will obviously not follow. In other words, among all the possible dynamics, those that break the symmetries just make the antecedent of the conditional claims (S) and (A*) false, so both claims are still true. This, still, allows me to remark an underlying moral behind this chapter: the chapter talks of the propagation of the property of stability in the world itself to the level of its own behavior; in other terms, from the symmetries of things to the symmetries of laws. This, if correct, is an example of what is called a 'symmetric argument' (following the terminology of [Brading and Castellani, 2003]), not a symmetric principle.

Finally, to complement this point, return to what (S) was stating:

(S): *'There are setups with certain spatial symmetries such that, if there is sensitive dependence on probabilistic initial conditions, then the evolution function is microconstant'*

Notice that the account of microconstancy based on (S) does not appeal to the details of the actual dynamics or to any specification of the form of the dynamics in general; for instance, it does not appeal

26 1.5, chapter 2, and 3 have been studying this other type of symmetries.

to how the collisions between entities have to be (something explicitly faced in 5.4) or, say, what rate of decrease some repulsive force has to obey. Besides the sensitive dependence, the statement (S) itself does not appeal to how the dynamics have to be. Therefore, I maintain that (S) holds not only in our world with our actual laws but in any other metaphysically possible world[27]. A justification of microconstancy via (S) shows that the form of the dynamics is irrelevant, thus justifying (A).

As to the symmetries, I have been focusing on *spatial* symmetries for sake of exposition, but the analysis holds for any symmetry existent *with respect to any basic property*[28]. For instance, the difference in the property of mass between two objects is prone to be treated differently by the laws.
It is left open to each particular case which are the relevant symmetries; unfortunately, I do not think that a general account can be more concrete about it. A case by case analysis should be made, in spite of the attempt to provide the underlying reasons behind. Thus, the extent of the claims is not the strongest, but it need not be so. It has not been argued that wherever there is microconstancy there is some symmetry underlying it. Nor that wherever there is a symmetry this will yield microconstancy. But it need not be stronger inasmuch as (A) is stated consequently as a weak claim (p. 92), sufficient to serve as a premise for (C).

27 The quantification 'There are' of (A) combined with the modal scope has to be understood such that the same setups with same symmetries will display microconstancy in every metaphysically possible world.
28 Thanks again to Michael Strevens for this important point.

4.4 COINCIDENCE OF A RANDOM TRAJECTORY WITH A CHAOTIC TRAJECTORY

After the defense of (A) in the previous section, this section argues for (B), which was:

(B): *'If an event X holds for all chaotic dynamics, then X holds in most of the cases with no dynamics at all'*

In different terms we could also say: if all chaotic trajectories have property X then a random trajectory will typically have X. The intuitive underlying idea for supporting (B) is the likely coincidence of a generic random trajectory with a generic chaotic trajectory. But, is this justified?

Prima facie seems plausible to believe that a random trajectory will look like a chaotic trajectory —the latter, as said in 4.1, looks like random. But the intuition is, of course, insufficient. Fortunately, the idea that any truly random trajectory can typically be interchanged with some chaotic trajectory can be discussed with precision.

The immediate objection with such an identification is that, even if granting the random-looking character of all chaotic trajectories, process-randomness is not always random-looking, and it is in this 'process' sense that we are interested at[29]. As said before, it is perfectly possible that a coin launched a million of times lands heads all the times. This is stressed with insistence in Antony Eagle's entry [Eagle, 2012]. So what can be said? Let me start gradually by an outline of how to understand the two notions, chaos and randomness, to get a grip of the similarities, thus defending the existence of relevant shared properties. More specifically, that a sequence generated randomly shares some pertinent properties, most of the times, with a sequence generated by a chaotic dynamics.

I sketched at the beginning of the chapter (p. 90) the characteristics that define chaos, suggesting its resemblance with the notion of randomness. First of all there was sensitive dependence on initial conditions. This is something that a random trajectory would intuitively share: a random dynamics would display, very probably, an exponential divergence for close initial conditions (indeed, it would even show divergence for the same initial conditions). Even if one can be reluctant to appeal to probabilistic reasonings in such scenarios, the very high number of possible paths to take in the next state by a random dynamics would provide the reason for believing in an exponential divergence for small variations of initial conditions. Because there is

29 See the annex A (esp. A.3) for definitions of the terms employed. In a nutshell: 'process-randomness' means that the trajectory is generated randomly, by a "random process"; not that the resulting sequence looks actually random (this is the "product sense"). In (B), the sense has to be the process sense, both when talking of "no dynamics" (i.e. process-randomness) as when talking of "chaotic dynamics".

just no reason at all to follow the same path.
Then I mentioned aperiodic long-term behavior [Strogatz, 1994, 323]. That trajectories do not settle down to fixed points, periodic orbits, or quasiperiodic orbits as $t \to \infty$ seems obviously also a feature of a truly random trajectory. A product-random trajectory verifies this by definition; however, as I explain in annex A, we have to consider a process-random trajectory. And a trajectory fruit of a random generation does not necessarily verifies aperiodic long-term behavior. However, as before, it is possible but clearly unlikely that, as $t \to \infty$, a fixed point or a periodic orbit will be maintained by a random process[30].
A more precise argument can be made when chaos is defined in terms of mixing [Werndl, 2009a], not in the topological sense but in the measure-theoretic one. This is presented in next subsection. After this, another argument from a more generic point of view is presented in 4.4.2. So let's see now how chaos fits in a hierarchy in which randomness is also present.

4.4.1 *'Randomness' and 'chaos' within the ergodic hierarchy*

There is a straightforward way to show the relevant shared features of chaos and process-randomness when we take into account the ergodic hierarchy as a reference. This hierarchy consists of the following classification of dynamical properties (cf. annex A.4.2):

Bernoulli $\subset$ Kolmogorov $\subset$ Strong Mixing $\subset$ Weak Mixing $\subset$ Ergodic

Now consider the following two accounts of chaos and randomness. First, following [Werndl, 2009a], define chaos in terms of strong mixing[31]. Second, identify, as I argue in annex A following [Berkovitz et al., 2006] and [Eagle, 2005], process-randomness as unpredictability, whose degree is quantified by the ergodic hierarchy. Randomness in its highest degree is then associated with the Bernoulli level[32].
It follows that the highest degree of randomness, the Bernoulli level, includes the properties of mixing; hence, it includes the properties of a chaotic system. Therefore, if we get some results in virtue of the properties of chaotic systems, we can get the same results with a truly random system, which will also possess those properties. Thus:

30 In next subsection 4.4.2 I make a precise elaboration of this type of argument. Now we are still at the beginning of the gradual presentation.
31 I have been oversimplifying for sake of clarity but let me be more detailed here. Regarding characterizations of chaos within the ergodic hierarchy, others proposed [Belot and Earman, 1997], before Werndl, that strong mixing was only a necessary condition for a system to be chaotic; a sufficient condition was to be in the higher level of Kolmogorov (also called a 'K-system'). Regardless of this discussion, my results hold.
32 It remains open whether I could relax the demands and have a weaker definition of randomness still appropriate.

if an event X holds for all chaotic systems, then it holds for a random system. Therefore, (B) is true.

4.4.2 *Coincidence of trajectories*

Let me insist on the validity of (B) from a more general perspective that does not appeal to the ergodic hierarchy. Even so, I do not think that the argument of previous section rests on controversial premises. The argument applies whether one identifies chaos with strong mixing or with a K-system. Likewise, the current best account of randomness is in terms of the ergodic hierarchy, as I elaborated independently in annex ch. A.

Now I want to show that a trajectory in phase space generated by a random dynamics (which is to say: it has not been generated by any dynamics) will typically coincide with a trajectory generated by a chaotic dynamics. If that were the case, (B) would be true. However, as pointed before, a random trajectory —in the process sense— can perfectly be a trajectory that does not possess any of the properties of chaos. Therefore, at most I will be able to defend the coincidence of a random trajectory with a chaotic one *in most cases*. I will argue that the rest of cases are neglectable. There are mathematical tools, namely 'measure theory', to verify this. Thus, in my defense of (B) I must justify that the measure of chaotic trajectories is much bigger than the measure of non-chaotic ones. Or else, justify the (less demanding) claim that the measure of the intersection of chaotic and random trajectories is much bigger than the measure of the intersection of non-chaotic and random.

Kinematical possibility

I put the argument more explicitly. Consider a class T of all the kinematically possible trajectories in phase space. T is a subclass of the whole space of metaphysical possibilities, delimited by prior ontological assumptions. For the set of metaphysical possibilities is plausibly too wide: metaphysical possibility can arguably tolerate, for instance, that any function from points in space to points in space qualifies as a possible trajectory. And this includes, for instance, objects jumping all over the place with no continuity to their motion, something perhaps "too wild" to be willing to admit. So the class T includes some assumptions aimed to exclude certain behaviors, conceptual possibilities not ruled out by logic. Now, what are these prior ontological assumptions? This is hard to say, and I am deliberately not committing to a definite answer —as much analytic philosophy does, for better or worse. Even so, I mentioned already the assumption of continuity (i.e. as the continuity equations that pervade physics) and a principle of conservation of energy[33]. Yet things become more concrete when

33 But admittedly, these assumptions are disputable too. It is just in this dissertation that I am entertaining a concept of 'law of nature' that might have evolved through

looking at how physical theories treat the kinematically possible. But even so, 1) there are plenty of different physical theories (CM, GR, QM, ...), and 2) it is not straightforward to know what is the space of kinematical models for a given physical theory.
Consider by way of illustration the Newtonian theory, with a model of gravitating point particles with distinct masses, as in [Belot, 2011, 7]. A point in the space of kinematically possible models of the theory assigns to each of the particles a worldline in spacetime, without worrying about whether the worldlines of each particle jointly satisfy the Newtonian laws of motion.
There is also a subset of the kinematically possible models, the so called set of dynamically possible models. This is a space of solutions which is a 6N-dimensional submanifold whose points correspond to particle motions obeying Newton's laws. Here such submanifold will be ignored for it constrains too much, to a single set of laws, the actual.
This is precisely formulated e.g. in [Pooley, 2013, 12, italics added]:

> In a coordinate-dependent formulation of Newtonian theory like that so far considered, the KPMs [kinematically possible models] might be sets of *inextendible smooth* curves in $\mathbb{R}^4$ which are nowhere tangent to surfaces of constant t (where $(t, \vec{x}) \in \mathbb{R}^4$). The models assign to the curves various parameters (m, ...). Under the intended interpretation, the curves represent possible trajectories of material particles, described with respect to a canonical coordinate system, and the parameters represent various dynamically relevant particle properties, such as mass. The space of DPMs [dynamically possible models] consists of those sets of curves that satisfy the standard form of Newton's equations.

The picture can be completed with the corresponding formulation in the lagrangian formalism, as in [Pooley, 2013, 40]. Here, the kinematically possible models are monotonically rising curves in the product space formed from the configuration space (whose points represent possible instantaneous states of the system), and a one-dimensional space representing time. Then, the dynamically possible models are those curves that extremize a particular functional of such histories: the action.

Still restrained to classical mechanics, a brief guide to realize what can be said about the most fundamental structural assumptions can be found e.g.: "The structure of physics" [North, 2009], "On the Necessary Truth of the Laws of Classical Mechanics" [Darrigol, 2007], or

time. Thus, this seems, at least *prima facie*, at odds with the time translation symmetry entailed by the conservation of energy. This shows that this is muddled ground, and it is in some sense not necessary to commit to an defined answer —at the obvious prize of leaving more abstract and unsettled the discussion.

"Symmetries and the Explanation of Conservation Laws in the Light of the Inverse Problem in Lagrangian Mechanics" [Smith, 2008].

Now, let's continue with the argument. Among this space T of all the kinematically possible trajectories, consider the following. Call C the set of those trajectories that display the property of chaos. Define a generic random trajectory a such that $a \in T$. There are two ways to continue from here. One is to continue without further assumptions. That is, a random trajectory is *any* generic trajectory belonging to T. In this stronger case, to justify (B) I must defend that the (standard Lebesgue) measure of $\overline{C}$, the complementary set of C, is much smaller than the measure of C, i.e. $\mu(\overline{C}) << \mu(C)$. Or alternatively, that its measure is 0, i.e.: $\mu(\overline{C}) = 0$. If true, it follows that in most of the cases a random trajectory will coincide with a chaotic trajectory. We can write this formally as something like:
$\mu(\overline{C}) = 0 \Longrightarrow P(Ca) \sim 1$
However, I do not know if this is the case and, to the best of my knowledge, there is not literature proving this. So the second way to go is by constraining the set to which a belongs in virtue of having, by definition, the properties of being process-random, thus belonging to a set $R \subset T$, the set of random trajectories. Then, what has to be proved is that $\mu(\overline{C} \cap R) << \mu(C \cap R)$ or that: $\mu(\overline{C} \cap R) = 0$. As before, this would entail that $P(Ca) \sim 1$. And the latter can be seen as a schematic way of phrasing (B).

So, how to defend the neglectability of the size of $\{\overline{C} \cap R\}$?
[Frigg, 2004] shows that the proportion of cases in which process randomness does not display a product random sequence is small enough to be neglected. I quote him: *"whenever a dynamical system behaves randomly in a process sense [...], almost all of its trajectories exhibit product randomness (in the sense of algorithmic complexity), and vice versa"* [p.21]. In short, he says: *"product and process randomness are extensionally equivalent"* [34].

Then, this allows to follow the strategy of identifying product-randomness in chaotic models and then substitute the product sense by the process sense. More formally, the argument can be stated like that. Being (R*) the set of product-random trajectories,

34 A synthesis of the elements grounding his claim is the following: *"Brudno's theorem states that the Kolmogorov-Sinai Entropy is equivalent to the Algorithmic Complexity for almost all trajectories of a system. The above theorem states that the communication-theoretic entropy is equivalent to the Kolmogorov-Sinai Entropy. Hence, Algorithmic Complexity is equivalent to the communication-theoretic entropy for almost all trajectories. The punch line of this is that the last equivalence equates notions of process and product randomness"[ibidem].*

$$\begin{array}{l} 1.\ \mu(R \cap R^*) = 1 \\ 2.\ \mu(R^* \cap C) = 1 \\ \hline 3.\ \mu(R \cap C) = 1 \end{array}$$

Premise '1' is what I stated above appealing to [Frigg, 2004]. Then, that a chaotic model typically generates a random-looking sequence —understood in the product sense— is what premise 2 says, and is an uncontroversial statement. At the beginning of this section I pointed out the similarities in the definitions of chaos and randomness in the process sense. In the product sense it is even less remarkable as it is in the very definition of chaos [Bishop, 2008, Sec. 1] that is contemplated, among other properties, the exhibition of *"seemingly random and unpredictable behavior that nevertheless follows precise rules"*. Again: *"Phenomenologically, the kinds of chaotic behavior we see in real-world systems exhibit features such as SDIC, aperiodicity, unpredictability, instability under small perturbations and apparent randomness"* [Bishop, 2008, sec. 1.2.7], where 'apparent randomness' is clearly meaning product-randomness.

Therefore, accepting the two premises, '3' follows. And given that: $\mu(R \cap C) = 1 \text{ iff } \mu(R \cap \overline{C}) = 0$, this conclusion is equivalent to what I wanted to prove, i.e. that the size of $R \cap \overline{C}$ is neglectable. Therefore $P(Ca) \sim 1$. Therefore (B) is true.

4.4.2.1 *Recapitulation*

Now I would like to schematize all the results of the chapter in the terms of the logical structure used in this section. Claim (A) was stating that for all the members of C high-level patterns arise (call this the property 'L'). This is the first premise. The conclusion of the chapter was that systems whose dynamics is random can exhibit in certain circumstances stable regularities. Or as I put it before: (C): 'There are systems in certain conditions that exhibit stable simple regularities without being guided by any dynamics'. Summarizing the premises and the conclusion together we can write that, for a system exhibiting certain spatial symmetries:[35]

$$\begin{array}{l} (A):\ \forall x(Cx \to Lx) \\ (B):\ P(Ca) \sim 1 \\ \hline (C):\ P(La) \sim 1 \end{array}$$

35 This nice very highly condensed summary is a valid argument if and only if rules like the following hold:
$\forall x \alpha \Rightarrow P(\alpha(^{x}_{a})) \sim 1$; and
$(P(\alpha \to \beta) \sim 1) \wedge (P(\alpha) \sim 1) \Rightarrow P(\beta) \sim 1$

5 TYPICALITY OF DYNAMICS

Abstract

This chapter studies the same subject of the previous from a different point of view. The subject, the irrelevance of the form of the dynamics for the rise of a specific stable behavior, is now studied in the field of statistical mechanics. Specifically, through an approach called 'typicality'. This approach argues that the tendency to the equilibrium macrostate occurs for initial conditions that are typical, where 'typical' is spelled out in measure-theoretical terms. This chapter reconstructs the arguments underpinning this approach and the objections that stress that the independence from the underlying dynamics cannot be correct. A way to avoid the objections is to reformulate the approach such that the typicality has to be not only of the initial conditions, but also of the dynamics. That is, the tendency to equilibrium occurs for all dynamics that are typical, which roughly means that it occurs for the overwhelming majority of them. After defending that this approach represents a case in support of the emergence of lawful behavior, I claim that, unfortunately, the extent to which the typicality of dynamics has been proven is too narrow for this purpose. Finally, nuancing this negative conclusion, I end the chapter by showing supportive results from numerical simulations that I have programmed in MATLAB. Specifically, the simulations aim to show that the tendency to equilibrium, in the hard-sphere model of classical statistical mechanics, holds for a wide set of possible dynamics.

Contents

5.1 INTRODUCTION

This last chapter 5 tackles the same subject of chapter 4 from a similar point of view. Specifically, it aims to explain the occurrence of a certain stable behavior, namely the tendency to the equilibrium macrostate in classical statistical mechanics, *without needing in the explanation any detail about the particular form of the underlying dynamics*. In the literature on the foundations of statistical mechanics there is a recent account that *arguably* does so, the approach called 'typicality'. This approach is a contemporary sophisticated version of the boltzmannian approach, which I will outline below. It is originated in the work of [Lebowitz, 1993a] and [Lebowitz, 1993b]. Roughly, it states that as long as the initial conditions of the system are typical —where the meaning of 'typical' is cashed out with precision in measure-theoretical terms—, then the system will exhibit thermodynamic-like behavior —that is, a tendency to the equilibrium macrostate, a non-decrease of entropy.

I shall follow [Goldstein, 2001] and [Frigg, 2009] and explain how they argue that typicality makes no reference to the dynamics —in an analogous way as to how, in chapter 4, the method of arbitrary functions did not mention how the dynamics had to be, besides being chaotic[1]. If the details of the dynamics are irrelevant, this opens the door to consider that the conclusion —i.e. the tendency to equilibrium in the coarse-grained level— obtains for almost any dynamics; therefore probably also for a random dynamics. The heedful reader will have realized that the logical form of the argument is the same as that of the previous chapter (compare with 4.1.3 and 4.4.2.1).

I shall assess to what extent the claims of the typicality approach are proven, and whether in the end the results can be useful for a metaphysics of laws. While the goal is to assert that they can be useful, this claim will be strongly nuanced.

The section 5.4 aims to support the claims of the independence of the dynamics with computer simulations that I have programmed. Specifically, the simulations show that the tendency to equilibrium, in the hard-sphere model of classical statistical mechanics, holds for a wide range of different possible dynamics —arguably, almost any dynamics in which energy and linear momentum are conserved. The simulations model a group of hard spherical particles that move according to a certain set of laws in a container. I have programmed the simulation in a way that is easy to change the laws that rule their behavior. Thus, for each different set of laws it can be verified whether the tendency to equilibrium obtains. The results are shown in 5.4, and the algorithm is included in the annex C. A brief animated illustration of the simulations can be found online in the links:

1 I must thank Tim Maudlin for orienting me towards the idea of the typicality of dynamics, so I got confident that the project was worthwhile.

https://vimeo.com/90044328
https://vimeo.com/90863487
The global conclusions of the whole thesis are found in the last section 5.5.

This chapter is obviously in line with the previous chapters. Chapter 3 was concerned with the formation of symmetries employing reasonings that resemble informal accounts of typicality: the results allegedly held for all complex lagrangians. Chapter 4 was arguing for the irrelevance of the details of the dynamics in the method of arbitrary functions, to conclude that its results would hold independently of the underlying dynamics.
Similarly, this chapter is a discussion of some mathematical properties that can be interpreted to be sufficient conditions for the physical world to exhibit certain regular behavior. Subsequently, the chapter defends the import of these insights in a possible understanding of the notion of physical necessity.

[Strevens, 2003, 4.8] and [Strevens, 2013, esp. ch. 2 and 9] analyze the case of statistical mechanics from his specific perspective (based on the method of arbitrary functions). I am going to pursue an alternative similar approach, that of typicality. It remains a future interesting question to what extent these two perspectives overlap.

5.1.1 *Boltzmann's statistical mechanics*

The point of departure is Ludwig Boltzmann's project of understanding the macroscopic properties and laws of thermodynamics in terms of their microconstituents and their laws. This is, in brief, the main mission of kinetic theory and statistical mechanics. The latter can be said to be the continuation of the former, after the introduction of irreducible probabilistic distributions not to the microconstituents but to the states of macroscopic entities —to the state of the whole gas— [Uffink, 2006, 9]. After Boltzmann, plenty of different paths have been pursued in order to obtain a reductive explanation of the laws of thermodynamics. Nowadays, the recent approach called 'typicality' is a sophisticated version of Boltzmann's insights. More precisely, Boltzmann along his life endorsed different views [Uffink, 2014]; typicality resembles his most famous contribution, the permutational argument —what corresponds to his first and second period, following [Uffink, 2014, 1.4][2].

In the classical case study of an ideal gas in an isolated container, the macrostate towards which all systems tend is the macrostate in which the gas has spread out all over the box filling its volume, the 'equilibrium macrostate'. Boltzmann showed, recurring to combinatoric mathematics, that *the equilibrium macrostate is compatible with an overwhelmingly higher number of microstates*. An idiosyncratic presentation of this key fact of statistical mechanics can be found in [Albert, 2000, ch.3], where the figure 13 comes from. Now let me formulate more concisely Boltzmann's conclusion, following [Frigg, 2008].
Consider a gas composed of n particles with two degrees of freedom each (usually three, but I adapt it to my 2-dimensional simulation). The state of this system is specified in a 4n-dimensional phase space Γ by a point x. This point is the microstate, with the information of position q and momentum p of every particle:
$x = (p_{x1}, p_{y1}, p_{x2}, p_{y2}, ...p_{xn}, p_{yn}, q_{x1}, q_{y1}, q_{x2}, q_{y2}...q_{xn}, q_{yn})$
The phase space comes endowed with the standard Lebesgue measure μ. The particles obey the laws of classical hamiltonian mechanics; they define a phase flow ϕ_t that is measure preserving, which means that for all regions $R \subseteq \Gamma, \mu(R) = \mu(\phi_t(R))$ (this result is known as Liouville's theorem). The system is perfectly isolated from the environment, therefore the energy is conserved. This restricts the motion of the microstate x over a region of Γ that is the energy hypersurface Γ_E, of 4n-1 dimensions. The Lebesgue measure μ restricted to Γ_E, μ_E, is also invariant.
From the macro point of view, the gas is characterised by its macrostates, where the equilibrium macrostate is labeled as M_{eq}. $\Gamma_{M_{eq}}$ is the corresponding macroregion in phase space which contains all the $x \in \Gamma_E$ for which the system is in M_{eq}. Macrostates supervene on microstates; a macrostate is compatible with many different microstates.

Now, the main conclusion of Boltzmann's combinatorial argument is that the measure of Γ_{Meq} with respect to μ_E is overwhelmingly larger than any other macroregion. For the details of the proof, see e.g. [Uffink, 2006, 4.4], or the primary source [Boltzmann, 1877]. In fact, this region occupies almost all the energy hypersurface, as the figure 13 conveys.

Given this radical difference in the sizes of the different macroregions, it was intuitive to think that the non-decrease of entropy

2 Extended presentations of Boltzmann's thermodynamical theory abound. Specific of Boltzmann, see the entry [Uffink, 2014]; a canonical treatment is [Sklar, 1993, II.3] as well as [Uffink, 2006, ch.4]. As to general works on the foundations of statistical mechanics, in addition to those just cited, very clear and concise are [Frigg] and, more extended, [Frigg, 2008]. Cf. also the idiosyncratic [Albert, 2000], and [Sklar, 2009], [North, 2011], [North, 2002], [Lebowitz, 2008], [Atkins, 2007], and [Zanghì, 2005, ch.2]. Further, David Wallace has prepared a lengthy bibliography on the foundations of statistical mechanics that can be found in his webpage. For a less academic and more poetic introductory work, see [Schneider and Sagan, 2005].

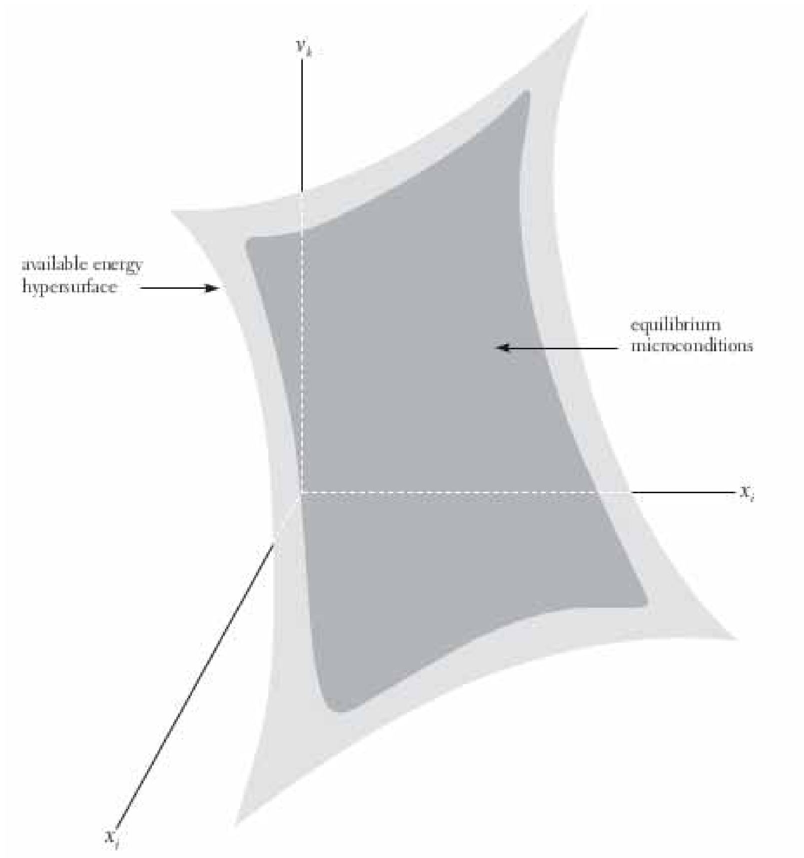


Figure 13: An illustration of an energy hypersurface, displaying the predominant size of $\Gamma_{M\,eq}$, the region of all the microstates correspondent to the equilibrium macrostate. From [Albert, 2000]

stated by the 2nd law and, more in general, the tendency to equilibrium stated in the 'minus first law' [Brown and Uffink, 2001], will be overwhelmingly more likely to occur. These insights would preserve the time-symmetric newtonian picture of the world while explaining the time-asymmetric behavior stated by the 2nd law. Further, the 2nd law and the tendency to equilibrium in general would not be strict laws but approximate, holding only because of the overwhelming likeliness of such behavior. Hence, there seemed to be no real conflict between reversible microscopic laws and irreversible macroscopic behavior.

The success of his project, however, was undermined from many grounds. In general, an innumerable quantity of obstacles and riddles have been showing up ever since: the reversibility objection, the recurrence objection, the implausibility of the independence assumptions, the unsettled intepretation of the various probabilities, the status of the past hypothesis, the validity of the results outside the simplified models studied[3], the ensemble approach versus the standard, the plausibility of the ergodic hypothesis, etc.

This brief fragment of the beginnings of kinetic theory/statistical mechanics represents the seed of an abundant ulterior research. It is widely recognized that it is difficult to recognize the forest for the trees among the vast extant literature; to this end see the references recommended in footnote 2.

It is not a goal of the chapter to elaborate more thoroughly the compelling riddles around the foundations of statistical mechanics; from next section onwards I shall focus on the particular elaboration of Boltzmann's combinatoric argument in the typicality approach. In so doing, I will come back to the leitmotif of this dissertation, the irrelevant role of the underlying dynamics in the explanation of stable behavior; in this case, thermodynamic-like behavior.

5.1.2 *On the treatment of the actual newtonian dynamics in statistical mechanics*

ASSUMPTIONS OF THE MODEL Several assumptions are made in kinetic theory / statistical mechanics, varying depending on the philosophical approach and the system under study. I will reproduce the model of hard-spheres, which models molecules of a gas closed in a perfectly isolated container. The gas is either ideal or a diluted gas, neglecting long-range forces, with a fixed kinetic energy $T = \frac{p^2}{2m}$.

To study thermodynamic-like behavior, it is usually assumed that the initial conditions are in a state of low entropy: in the numerical sim-

3 In principle, statistical physics is more generic than just studying the behavior of gases in a laboratory; the study of macroscopic properties in terms of their microconstituents is something that can be studied also of galaxies, where the stars are the microscopic constituents [Uffink, 2006, p.4]. But not everybody agrees on that; more on this in 5.3 p.138.

ulations at the end of the chapter, I place all the molecules in the "famous" left down corner of the container.
The molecules of the gas, then, are the micro-constituents, and they are modeled not as point-particles but as hard spheres, so they have a certain small radius. In my computer simulation the scenario is in 2 dimensions, so the spheres are actually circles. The gas molecules interact like billiard balls, so they have no effect on one another except when they collide. 'Hard' entails that the collisions are elastic (i.e. no kinetic energy is transformed in other forms, for instance is not lost in the form of heat). It is also assumed a large number of microscopic constituents, typically of the order of Avogadro's number $N = 10^{23}$ or more. Due to technological limitations —the limited performance of my computer— I have modeled a much smaller number of particles; even so, the expected results obtain.

Regarding the laws, the *deterministic* laws of classical mechanics are assumed.
As it belongs to classical mechanics, the model ignores the allegedly negligible corrections of relativity theory as well as quantum mechanical effects[4].

an erratic wandering dynamics There is a further assumption that I especially want to highlight. It is again about the presence of randomness in physical models, and it is manifested in assumptions of most of the approaches of statistical mechanics. Remember how I raised this point in 1.3 p. 11, where I put forward the ontological commitment to the randomness implicit in indeterministic laws, as well as in the '*spontaneous*' processess found in contemporary physics (spontaneous symmetry breaking, spontaneous fluctuations, ...).
Here, in statistical mechanics, in spite of the guiding laws being classical deterministic newtonian laws, there is, *also*, "*an assumption about the erratic nature of the dynamics*" [Uffink, 2006, 5].

The requirement of randomness or independence...

Quoting what [Sklar, 1993] states, one of the chief assumptions of the Maxwell–Boltzmann program, and it is an assumption that runs right through to the ergodic theory of modern physics, is that at some level of description a condition of *independence* must be met "*for the theory to properly explain why the correct values of state parameters of gas systems can be obtained from taking the average values of mechanical properties of the individual particles of the system*".
There have been various strategies for underpinning this requirement of independence: the assumption of equal initial probabilities, the ergodic hypothesis, the 'rerandomization posit', also known as the Stoßzahlansatz in the initial theory of Maxwell (i.e. the assumption that there are no correlations in the collisions of molecules of dif-

4 Yet, for arguments against the irrelevance of quantum mechanics, see [Albert, 1994] or [Wallace, 2001].

ferent velocities), or the hypothesis of molecular chaos, also known as 'stochastic postulate'. Each of these introduces *a condition of randomness or independence* at subtly different locations in either the gas model employed or in the method of calculation of the state properties of the gas (e.g. temperature, pressure, or entropy).
How can the posits about the erratic wandering nature of the dynamics coexist with the deterministic guiding laws? [Sklar, 1993, p.215] says "*that further justification is needed is clear for, as usual, it is a deep question remaining open as to whether any rerandomizing assumption is even consistent with the underlying deterministic evolution of the phase points*".

...achieved through chaos,

All these sort of requirements, I would like to suggest, can be naturally justified by appealing to the well-known compatibility between determinism and chaos, and then realizing the relation between chaos and randomness —that is, what I have argued in the previous chapter in 4.4 (and A.4).
To see this, let's start by reading how Lebowitz talks about this essential assumption of statistical mechanics: *"interactions in the domain $\Gamma_{M_{ab}}$ will be **so convoluted as to appear uniformly smeared out** in Γ_{M_b}. It is therefore reasonable that the future behavior of the system, as far as macrostates go, will be **unaffected by their past history**"* [Lebowitz, 1999, (bold is mine)] [5].
This is an assumption about the independence of one state with its past states. In sections 4.4 and A.4.2 I showed the proposal based on the Ergodic Hierarchy according to which randomness is defined in terms of *the lack of correlations of one state with its past states*. These independence/randomness requirements can be obtained through the chaotic properties of the deterministic newtonian mechanics, which wash out the correlations with past states, yielding the needed random-looking, erratic nature —see 4.4 for more details.
For sake of illustration, in the particular case of the hard-sphere model I came to know the reasons of the high sensitivity to initial conditions of classical deterministic mechanics when reading [Strevens, 2003, 4.8]: in such model, the outcome direction after the collision of one sphere with the convex spherical surface of another sphere is highly sensitive, as seen in figure 14.

...or achieved through genuine randomness

That being said, the justification of the randomness and independence premises can be also undertaken from another interesting path. Namely, rather than by appealing to the chaotic properties of the deterministic dynamics —as just defended—, by entertaining the scenario of a genuine random dynamics.
A substitution of the chaotic laws by a genuinely random dynamics

5 Here, M refers to the system's macrostate, Γ the phase space, Γ_M the phase space region corresponding to M, M_a the system's initial macrostate, M_b its later macrostate. $\Gamma_{M_{ab}}$ is the region of Γ_{M_b} that came via Γ_{M_a} (the set of microstates within Γ_{M_b} that are on trajectories that come from Γ_{M_a}).

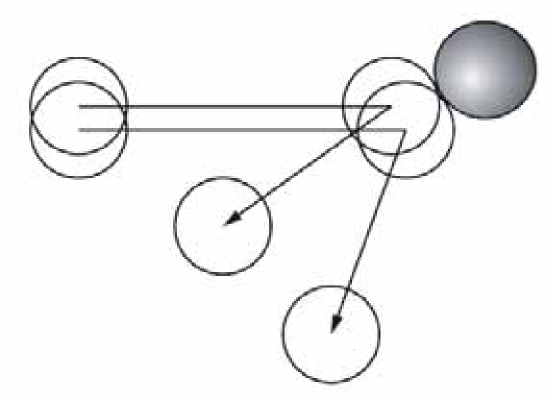


Figure 14: The collision of two classical circular particles is highly sensitive to initial conditions, as seen in this figure. From [Strevens, 2003]

would immediately justify the diverse randomness and independence requirements. Remember that Lebowitz's quote above was stating the requirements in the same terms as how randomness was characterized in [Frigg et al., 2011] (and [Berkovitz et al., 2006]), as explained in ch. 4 and A. Thus, the rerandomization posit, the independence assumption(s), and any other surrogates would be met by a genuinely random dynamics —in the long run, almost always.

After these reflections about statistical mechanics in general, let's analyze the specific variation of the boltzmannian approach called typicality.

5.2 typicality of initial conditions

The leading idea of the typicality approach is that a system exhibits entropic behavior because it is typical for the system to behave in this way. 'Typical' is defined as a property assigned to a behavior of a system: intuitively, a behavior is typical if it is displayed by the evolution of *most of the possible initial states* of the system. A precise meaning for 'most' is achieved through measure theory, as I spell out later. Thus, in contrast to Boltzmann's approach, typicality-based explanations eschew commitment to probabilities.

Galton board example

In [Maudlin, 2011] diverse examples illustrate the general idea: the toss of a coin, of a die, or the case of a Galton board (also known as 'quincunx'; a board with pins through which balls fall down from a hopper to a set of baskets). As we already know, the galton board displays, in the long run, a normal distribution centered in the middle basket. How to understand the nature of this probability distribution, that we take to be neither subjective nor epistemic? The limiting frequency in the middle basket can be explained in terms of typicality, that is: because most of all the possible initial distributions end in that result. In other words, the typical behavior for a ball falling in

the Galton board is to fall in the center. Why this already known fact is actually the case can be understood focusing on the typical behavior in the deflection of a ball hitting a pin. The typical behavior of a ball hitting a pin is that it gets deflected to one of the sides half of the times (without needing to know why this is so, we can venture that it is due to the symmetry of the pins and the chaotic actual dynamics). This explains the previously stated typical behavior of falling in the center; it explains so through the law of large numbers (defined in A.3.1): if the typical behavior is to get deflected half of the times to the left and half to the right, in the long run (i.e. when the balls will hit a high number of pins), it is expected that the number of turns to the left and to the right will be approximately the same, yielding the ball to land approximately in the center.
'Typical', then, is understood as follows:

> "when some specified dynamical behavior (like passing a single pin to the right, or passing successive pins first to the right and then to the left) has the same limiting frequency in a set of initial states that has measure one, that frequency for the dynamical behavior is typical". [Maudlin, 2011]

In this quote measure theory is introduced, and there is no appeal to probabilities or likeliness. If the set of states that leads to some outcome has measure one, then it can be defined as typical (the measure is calculated with a flat Lebesgue measures over the appropriate interval).

According to this approach, a certain probability distribution of the outcomes is guaranteed, irrespective of how the assignation of a distribution over the initial conditions is made[6].

Other examples

Similarly we can treat the case of a coin toss. The fair coins have as typical a frequency of 50% of landing heads, because most of the sequences of fair coin tosses, whatever the initial state, lead to that result in the long run.

In the case of a gas condensed in a corner of a box, the gas will fill the box because this is the typical behavior, that is, because most of the initial states lead to that result.

Definition of typicality

After these examples, let's formulate typicality with more generality, to come back later to the gas in a box. Following [Frigg, 2009], an element e of a set Σ is typical if most members of Σ have property P and e is one of them.
In the case of statistical mechanics, the element corresponds to a micro-state x, the measure employed is the Lebesgue measure μ[7], the set Σ amounts to the set of all microstates, and the property P is the

6 Notice the high similarity with what has been defended in the previous chapter.

7 The standard Lebesgue measure over volumes is the usual tool employed, but the choice of the type of measure is not uncontroversial; see e.g. [Sklar, 2009, sec. 4].

property of being such as to evolve to equilibrium.

Notably, it turns out that one gets a set of frequencies that are typical as an *analytical consequence* of the deterministic dynamics together with a measure over initial states. And, *"what we in fact believe as a purely mathematical fact is that the set of initial states with 50% limiting frequency for deflections to either side is a set of measure one"* [Maudlin, 2011, 286].

Now, let's deepen more in the role that the dynamics play in this approach.

5.3 TYPICALITY OF DYNAMICS

CHARACTERIZATIONS OF TYPICALITY [Frigg, 2009] spells out a distinction between three different characterizations of typicality in the literature. In the end, he concludes that only the one who takes into account the dynamics is really promising. Frigg criticizes this first statement of typicality from [Zanghì, 2005]:

> "reaching the equilibrium distribution in the course of the temporal evolution of a system is inevitable due to the fact that the overwhelming majority of microstates in the phase space have this distribution".

It seems that no appeal to the dynamics has to be made in order to explain the tendency to equilibrium, and this is taken to be an objection. It is comprehensible that it raises suspicions, especially after reviewing the history of the foundations of statistical mechanics and realizing that one or another dynamical assumption was always introduced.

The role of the dynamics in Botzmann

Always, except in the case of Boltzmann's combinatorial argument [Uffink, 2006, 4.4.2]. There it was irrelevant to the argument how the particles collide. Following [Uffink, 2006, 57], if this irrelevance were correct, Boltzmann's argument would be general enough as to be applicable not only to ideal gases, but also to dense gases and even to solids, as Boltzmann indeed suggested [Boltzmann, 1909, 223].

However, there were non trivial assumptions in Boltzmann's argument. [Uffink, 2006, 58] cites (among other objections not related with the dynamics) the assumption that the total energy can be expressed in the form $E = \sum n_i \epsilon_i$, where the energy ϵ_i of each of the n_i particles i depends only on the cell in which it is located, and not on the state of other particles. This can only be maintained if there is no interaction at all between the particles, so the argument remained restricted to ideal gases.

Later on, ulterior approaches invoked all sort of dynamical assumptions; for instance, mixing, or ergodicity[8].

8 Mixing and ergodicity are two properties defined in ergodic theory, the former being stronger than the latter: intuitively, they are properties that hold in certain stages of

Coming back to the typicality approach, is it able to really dispense with dynamical assumptions? If this were so, it would be a success for our aims, for stable behavior would be obtained independently of how the dynamics is described; therefore, it would be obtained for all dynamics; therefore, both the tendency to equilibrium as well as the stability of the equilibrium macrostate itself could be explained without a specific set of pre-existent fundamental laws.

Physical necessity as statistical necessity

A mathematical necessity or, better said, a *statistical necessity*, would ground the stable behavior achieved in the equilibrium macrostate. In other words: without mentioning the hamiltonian of the system, a statistical necessity would entail at least *some* physically necessary behavior.

The success of this project is critically assessed in this chapter. I am going to lay out the attempts that dispense with the dynamics, but also underline that they have been proved for a too narrow domain. Those attempts, as advanced before, are those that weaken the typicality approach by explicitly introducing a typicality of dynamics. Then, the numerical simulations of the final section aim to reinforce the plausibility of a positive answer. Finally, 5.5 reflects upon the philosophical significance of the results of this chapter and of the whole dissertation.

Let's reconstruct the dialectics following [Frigg, 2009]. Regarding the statement above from [Zanghì, 2005], which was roughly characterizing the typicality approach, Frigg remarks the objection that *typical states do not automatically attract trajectories* [Frigg, 2009, 5]. He says so in the sense that the approach to equilibrium will not occur only because the corresponding microstates are more numerous. Hence, this first characterization is insufficient.

There is a second way of characterizing the typicality approach. Leaving aside the details, the argument states that the microstates leading to a decrease in entropy will tend to zero (will be atypical) as the number of particles in the system increases (in [Frigg, 2009, 6], who quotes [Goldstein and Lebowitz, 2004, 57]). Frigg argues that this formulation is justified essentially by the fact that the *macro*-region of equilibrium is overwhelmingly larger than the other macrostates (for further details see [Frigg, 2009, 8]).

This justification, though, is unsatisfactory too. The disparity of sizes of macroregions does not imply, alone, that an entropy decrease will be atypical. It could be perfectly the case that a phase flow Φ_t is such that it violates such conditions —that is, Φ_t leads to anti-thermodynamic-like behaviour. In sum, the correct account of typicality must take into account the dynamics. This is the third approach.

Take Γ_E to be the hypersurface of 6n-1 dimensions of the phase space

the process of mixing and stirring a shot of scotch in a glass of water, as Gibbs originally explained. A definition of ergodicity is formulated in the coming subsections. For more details see [Frigg et al., 2011], [Berkovitz et al., 2006] and 4.4.

Γ in which the energy is conserved. Being M_{eq} the equilibrium macrostate, take $\Gamma_{M_{eq}}$ to be the macroregion consisting of all $x \in \Gamma_E$ for which the macroscopic variables assume the values characteristic for M_E. Then, quoting Sheldon Goldstein , who first explicitly mentioned the dynamics into the definition[9]:

The proper account of typicality

> "[Γ_E] consists almost entirely of phase points in the equilibrium macrostate [$\Gamma_{M_{eq}}$], with ridiculously few exceptions whose totality has volume of order $10^{-10^{20}}$ relative to that of Γ_E. For a non-equilibrium phase point [x] of energy E, the Hamiltonian dynamics governing the motion [x(t)] would have to be ridiculously special to avoid reasonably quickly carrying [x(t)] into [$\Gamma_{M_{eq}}$] and keeping it there for an extremely long time—unless, of course, [x] itself were ridiculously special" [Goldstein, 2001, p.43]

What is said here is that, together with the already known statement that equilibrium states are typical in Γ_E —something already contained in the quote from Zanghí—, it adds that the Hamiltonian must be typical. The key point then is that the dynamics have to be *"ridiculously special"* for not tending to the equilibrium macrostate. While typicality was initially oriented to the measure of initial conditions, now it regards also the dynamics-space.

Let me put it emphatically, for this account purportedly reacts in front of a rationale that pervades the whole history of the foundations of statistical mechanics. Yes, the tendency to equilibrium is not logically *proved* by Boltzmann's combinatorial argument nor by typicality, for one can find specific counterexamples; nevertheless, the counterexamples do not refute that (a refined version of) Boltzmann's or the typicality approach can *actually be* the explanation of the thermodynamic-like behavior manifested around us —in gases of all sorts, in coffee cups, in the whole universe. Relaxing the requirements to count as the proper explanation is what the typicality approach does. From expecting a mathematical proof holding in all possible cases, now we can be satisfied with an explanation for most possible cases[10].

The typicality of chaos

Furthermore, let me underline how Frigg (in turn following Dürr and Maudlin) points out the fact that a typical dynamics will be

9 The brackets in the fragments indicate that the notation has been changed from the original paper for the notation used here.

10 This move is analogous as to what is made in two other moments cited in this thesis. One is when instead of proving ergodicity, one proves epsilon-ergodicity, a less demanding version of the former (see [Frigg and Werndl, 2011], and later in this same section). The other is when the extensional identity between process and product randomness is defended for most of the times, in spite of counterexamples, which are said to be neglectable given their small quantity (see A.4, 4.4, and [Eagle, 2012]).

random-looking, and will be so in virtue of the chaotic properties of the actual dynamics, something that should sound familiar to us, as I will remind after the fragment:

> "the Galton Board seems to exhibit random behavior. Why is this? Dürr's and Maudlin's answer is that the board appears random because random-looking trajectories are typical in the sense that the set of those initial conditions that give rise to nonrandom-looking trajectories has measure zero in the set of all possible initial conditions, and this is so because the board's dynamics is chaotic [Dürr, 1998, sec.2]. Translating this idea into the context of SM suggests that the relevant property P is being chaotic." [Frigg, 2009, p.8]

So, in the context of statistical mechanics (and in that of the Galton board), hamiltonians with a chaotic dynamics are typical. This is, exactly, a claim that I argued for in 4.4.2 (p. 116). Specifically, that $\mu(C) = 1$ (where C denoted the set of all possible chaotic trajectories in phase space). This claim was important because it was justifying one of the two premises of the main argument of chapter 4, premise (B)[11]. This claim of 4.4.2 amounts to say, in the terminology of this chapter, no more and no less than the chaotic trajectories are typical[12].

In what follows, I am going to summarize Frigg and Werndl's way of spelling out the typicality of dynamics, plus the numerical simulations they cite in their support; then I will highlight how these results are far from sufficient for the purpose of this thesis. Finally, the next section presents my numerical simulations, still clearly insufficient to prove the typicality of dynamics, but nonetheless illustrative and supportive of the purpose of the chapter.

SPELLING OUT THE TYPICALITY OF THE HAMILTONIAN The research carried out in another paper by Frigg and Werndl [Frigg and Werndl, 2012] represents a step forward towards our purpose. The authors formalize which conditions a dynamics has to meet to be typical.
A first claim of [Frigg and Werndl, 2012, 8] is to restrict the discussion to the obtaining of *epsilon-ergodicity*, because such property is a

11 I remind that (B) was stating: 'If an event X holds for all chaotic dynamics, then X holds in most of the cases with no dynamics at all'. This was loosely formulated as (B) : P(Ca) ~ 1; where 'a' was a generic trajectory. To prove (B) it sufficed to prove that $\mu(C) = 1$.

12 However, the comparison between these two moments contains an important distinction. The fragment here cited is not really supporting my claim (B), for the former is weaker than the latter. Frigg's fragment is implicitly sticking only to the actual laws, while my claims have to commit to a (much?) wider scope. Across all the fragments that I quote along the dissertation, never other laws than the actual are assumed —in the fragments quoted, that is. Instead, in 4.4.2, my claim was committing to all metaphysically possible chaotic trajectories; as such it needed further justification, that I tried to provide.

sufficient condition for thermodynamic-like behavior. They had previously proved this claim in [Frigg and Werndl, 2011]; cf. also [Vranas, 1998].

A solution is said to be ergodic with respect to a measurable set A if and only if the proportion of time it spends in A equals the measure of A (i.e. the time average is equal to the phase average). A dynamical system is ergodic iff for any any set A any solution $x \in \Gamma_E$ is ergodic. We can define B as the set of points which lie on non-ergodic solutions (with respect to M_{eq}). It follows that a system is ergodic iff $\mu_E(B) = 0$ (this is a complementary definition that will help when defining ϵ-ergodicity).

Definition of ergodicity and epsilon-ergodicity

It is proven that for ergodic systems initial conditions that lie on thermodynamic-like solutions are typical [Frigg and Werndl, 2012, 9].

ϵ-ergodicity is meant to be a more relaxed version of strict ergodicity. It does not require that $\mu_E(B) = 0$, allowing sets of initial conditions to be on non-ergodic solutions with respect to M_{eq}, only if the sets are of small size; that is: $\mu_E(B) \leqslant \epsilon$ (with ϵ a very small number).

Notably, it is proved in [Frigg and Werndl, 2012, 9] that, also for epsilon-ergodic systems, initial conditions that lie on thermodynamic-like solutions are typical. This is what they label as 'm-typical' ('m' standing for 'measure').

The typicality of initial conditions, though, is not sufficient. As stressed along the chapter, an explicit typicality measure of the dynamics must be included. This is what Frigg and Werndl seek to do; specifically, it has to be shown that the Hamiltonian is typical. Thus, the complete argument has this form [Frigg and Werndl, 2012, p.6]:

The structure of the argument

Premise 1: The macrostate structure of the gas is such that equilibrium states are typical in Γ_E.
Premise 2: The Hamiltonian of the gas is typical in the class of all relevant Hamiltonians.
Conclusion: Typical initial conditions lie on solutions exhibiting thermodynamic-like behaviour.

The task, then, is to ascertain the truth of premise 2. The hamiltonian has to be typical with respect to a certain measure. The measure, for a dynamical trajectory, has to be a topological measure; thus, it can be said that it has to be 'topology-typical', 't-typical'. I advance that the final result of [Frigg and Werndl, 2012] is the following: the Hamiltonians are t-typical with respect to the so called 'Whitney topology', and this is proved for the Lennard-Jones potentials, a subset of all the possible potentials —yet, this subset should suffice, they say, for a wide class of realistic gases.

TOPOLOGICAL TYPICALITY OF THE HAMILTONIANS To measure the typicality of the hamiltonians in its dynamics space, instead of the Lebesgue measure we must employ a topological notion, namely that of 'comeagre' or 'generic'. A set A is 'comeagre' (or 'generic') if and only if its complement is a countable union of nowhere-dense sets. Intuitively, the idea is to provide a measure able to inform whether the other sets are scarce. As we are searching for the typicality of the hamiltonians, we seek to claim something like the following statement, which I label as 'TYPDYN':

TYPDYN: ϵ-ergodic Hamiltonians are comeagre/generic in the entire class of gas Hamiltonians 'G'.

TYPDYN is a refined version of premise 2 above. Unfortunately, the class 'G' is too big. 'G' is not easily definable, so Frigg and Werndl restrict their claim to something narrower, namely, the sub-class 'L' of smooth Hamiltonians that are small perturbations of the so called Lennard-Jones potential. This constraint, the authors argue, is very acceptable given the aim of explaining thermodynamic-like behavior of gases. Thus, first we restrict the Hamiltonians to the smooth ones, that is, those with a fixed kinetic energy $T(p,q) = \frac{p^2}{2m}$. Thus, the variation of Hamiltonians amounts to the sweep over all the possible potential energies $V(p,q)$ of a generic $H(p,q) = T(p,q) + V(p,q)$. Second, we restrict the potential $V(p,q)$ to the so called Lennard-Jones potential, the potential most notorious and most used to describe intermolecular interactions[13].
In [Frigg and Werndl, 2012, p.11] we read about it:

> "The Lennard-Jones potential is important because there is good evidence that the interaction between many real gas molecules is accurately described by that potential at least to a good degree of approximation. Hence, whatever potentials G comprises, many real gases cluster in a subclass of G, namely L, and so knowing how the members of L behave tells us a lot about how real gases behave".

After that, the choice of the Whitney topology comes from a physically natural way of saying that two Hamiltonians are close; namely, when the difference between the Hamiltonians themselves as well as all their derivatives is small [Frigg and Werndl, 2012, p.10].

13 Specifically, this potential has the form:

$$V = 4\alpha\left(\left(\frac{\rho}{r}\right)^{12} - \left(\frac{\rho}{r}\right)^{6}\right)$$

where α describes the depth of the potential well, r the distance between two particles, and ρ is the distance at which the inter-particle potential is 0. The potential of the entire system is obtained by summing over all two-particle interactions. The r^{-12} term describes the repulsion forces at short ranges, The r^{-6} term describes the attraction forces at long ranges (van der Waals force or dispersion force).

Full formulation

Summing up, the goal has become to confirm what I gather in TYPDYN2:

TYPDYN2: ϵ-ergodic Hamiltonians are comeagre/generic in the sub-class 'L' of smooth gas Hamiltonians with a Lennard-Jones potential with respect to the Whitney topology.

Now, to prove that TYPDYN2 is actually the case, they show that the sub-class 'L' is comeagre by referring to empirical evidence —i.e. referring to numerical simulations cited below— according to which Hamiltonians that are ϵ-ergodic for the energy values defined —i.e. the set of all perturbed Lennard-Jones Hamiltonians— are typical in L. Thus the end of the paper [Frigg and Werndl, 2012] makes reference to numerical simulations that provide evidence that this is the case. The departure point is [Sinai, 1970], that proved the assumption implicit in Boltzmann and Gibbs that, in a hard-sphere model, the trajectories of the particles are erratic paths wandering freely over the energy surface and spending equal times in equal hyperareas of this surface; more specifically, Sinai proved that the hard-sphere gas is unstable and that this suffices to guarantee ergodicity (and mixing). Then, the main reference they cite is [Ford, 1973], which improves these results. There the authors start by pointing out that: *"the extension of Sinai's results to systems having purely repulsive interparticle forces is expected to be straightforward"*. In addition to that, we have the progress of [Ford, 1973] itself: there the proof is extended to systems having *attractive as well as repulsive interparticle forces*. Contrary to some mathematical calculations that might suggest pessimistic results (namely, nonergodic regions of KAM stability[14]), the simulations of [Ford, 1973] provide evidence that, variating the energies and densities of particles (so even in the allegedly problematic cases of low energies or high densities), the expected entropic behavior obtains without evidence of KAM regions. The KAM regions might be perfectly negligible, given that none has been found in the simulations and *"nothing known precludes their being so small as to be physically irrelevant"* [Ford, 1973, 1].
At the end of the day, the numerical simulations reflect that the Lennard-Jones gas, which includes repulsive as well as attractive forces, exhibits thermodinamic-like behavior. It is an interesting investigation given that is closer to actual circumstances and, especially, because it extends the range of cases under investigation. Its results confirm that this type of gases conform to the properties of ergodicity demanded originally by Boltzmann (and Gibbs), and that we demand on behalf of a typicality account that seems to hold for most of the dynamics.

14 These are finite regions in phase space in which there are trajectories confined displaying perpetually quasiperiodic motion. These regions are predicted by the Kolmogorov–Arnold–Moser (KAM) theorem.

Extend the results?

DISCUSSION OF THE RESULTS So far, then, certain results in what respects typical behavior have been highlighted, with a set of constraints specific of the model under study. To what extent can these conclusions be applied beyond these constraints? Some argue that the scope of classical statistical mechanics is highly restricted to certain experimental setups in which it has been tested [Myrvold], [Hoefer]. Others, like [Uffink, 2006, 2] or [Goldstein, 2001] do not find problematic to take such model as representative of a much wider portion of reality. Let's take a look at the constraints involved. The main constraint is that we are dealing with classical (not relativistic, not quantum) isolated and mechanical systems, namely gases in a closed container. This is based on the model of hard spheres bouncing.

Classical mechanics is false

Regarding the dynamics of the model, remember that we know that the classical newtonian laws are actually false in microscopic quantum domains, as well as where relativistic effects are not negligible. Yet, see the coming subsection 5.3.1 for some hints about how the same idea of typicality has been traslated to quantum systems.

A limited context

Does this high simplification rule out the philosophical interest of the results attained? I think that some arguments attenuate such a worry. One could be satisfied that just in certain limited contexts it has been proved the emergence of stable regular behavior for almost all possible dynamics. We could be satisfied with the typicality of dynamics proved only for gases: we have found a restricted domain in which it is shown the emergence of macroscopic stable behavior, irrespective of the microscopic dynamics; I take it as an interesting result that might (or might not) bear an import beyond its domain. Compare this conclusion with the similar conclusions for similar results in 4.3.3.1. There I was entertaining that the needed property of microconstancy (actually, the symmetry with respect to some basic property) was something that might verify, not only the roulette wheel and many other examples, but perhaps also the whole universe in some initial configuration. Then I raised the question whether this should be necessary for my purposes. I claimed it is not necessary, and that whether it holds or not, in the end it would support a different metaphysics of laws. Namely, if the results would hold for the whole universe, then this would support a universal unified version of laws. In that case, the allegedly universal non-accidental regularities that the fundamental laws of physics describe would be a result of the procedures described here. Otherwise, the lack of application in a cosmological scale would be more in tune with an antirealist account of laws, in the sense of something like oases of order emerging among the "chaos", a la Cartwright [Cartwright, 1999] —the context of the gas in a box would be a nomological machine, whose stability and lawful behavior, now, I would say, is explained.

A positive take on the limited context

There is a further observation against those who argue that the scope of classical statistical mechanics is restricted only to certain experimental setups in which it has been tested [Myrvold], [Hoefer]. I think it is fair to appreciate the significance of classical statistical mechanics far beyond experiments of certain classes of gases in a laboratory —as other philosophers do, e.g. David Albert, Jos Uffink, or Sheldon Goldstein. My argument relies on the fact that the highly-idealized model of classical statistical mechanics is, in its generality, grasping the relevant causal structure that really in fact explains thermodynamic-like behavior. This claim, more in general, is linked with the habitual and successful modeling which is characteristic of physical science itself. Modeling reality always neglects lots of features, but nonetheless it is very successful. In our case, it neglects, among others, the time-variable curvature variations of the container fruit of General Relativity (GR), as noted in [Hoefer, fn 7]. Yet, to neglect these effects is justified because this variation does affect *infinitesimally* the motion of molecules; crucially, it affects so infinitesimally that in the end it is not going to play any causal role in the final explanation of thermodynamic-like behavior. Ultimately, this is the general idea of modeling in physics, and is what I think justifies that the insights of classical gases in a container can shed light on the behavior of, say, the long-term behavior of clusters of galaxies [Uffink, 2006, 2].

Another defense of the limitations of the context

However, even if appreciating a contextualized solution, or granting that the results hold beyond the delimited context —e.g. the universe might have shared in some moment the relevant properties of this (over)-simplified model—, other constraints potentially undermine the interest and philosophical significance of the results.

Another objection

The class of Hamiltonians under consideration is clearly not all the possible set G aforementioned, but a substantial, though important, subset, i.e. the subset L of Lennard-Jones potentials. Frigg and Werndl defend that is not necessary to consider G because L is the most relevant subset and suffices as an approximate proof of their objective. However, for the purpose of this dissertation, this observation misses the point, for we are interested in extending the range of possible dynamics as far as possible.

The range of the dynamics

I recall that the goal of the chapter is to argue that the results and theories presented can be interpreted to support a certain metaphysics of laws; namely, support an account of emergence of stable behavior from a lawless level. Thus, it is not sufficient that the typicality is proven for a wide class of *realistic* dynamics of gases; we absolutely need the typicality to be proven also for the *unrealistic* dynamics. In the end, this means that we need it for the whole class of hamiltonians G, not only L.

Furthermore, the kinetic energy T(p,q) has been fixed for delimiting the subset of smooth Hamiltonians. So, which would be the result if we vary the possible values of T(p,q)? Again: Can we account for all the class of Hamiltonians G, instead of the restricted subset L?

Thus, these limitations imply that the very idea of the typicality of dynamics has not been strictly proven —not even for the delimited context. On the positive side, at least the range already covered is significant, as the potentials under consideration include those with attractive forces, those just with repulsive forces, and those with both (the subset L)[15].

5.3.1 *Outline of the typicality of dynamics in the quantum domain*

As advanced before, the typicality approach has been also pursued in the quantum domain, where the typicality of Hamiltonians is explicitly stressed. Those who have researched this path are the already mentioned J. Lebowitz, S. Goldstein, and N. Zanghí, together with Roderick Tumulka and Christian Mastrodonato, in the papers [Goldstein et al., 2009], [Goldstein and Tumulka, 2010], [Goldstein et al., 2010a], [Goldstein et al., 2010b] and, in parallel, the authors of the paper [Linden et al., 2009]. This subsection summarizes their results.

It is considered an isolated, macroscopic quantum system. This system is described by a wave function ψ evolving according to the Schrödinger equation:

$$i\hbar\frac{\delta\psi_t}{\delta t} = H\psi_t$$

[Goldstein and Tumulka, 2010] argues that, for every initial state ψ_0 of a *typical* macroscopic quantum system, the system will spend most of its time in thermal equilibrium.

More specifically, they argue so by purportedly showing that *for typical Hamiltonians* with given eigenvalues, all initial state vectors ψ_0 evolve in such a way that ψ_t is in thermal equilibrium for most times t.

Now we have moved from phase space to Hilbert space. Following [Goldstein et al., 2009, 1], let $\mathcal{H}$ be a micro-canonical energy shell, that is, a subspace of the system's Hilbert space spanned by the (finitely) many energy eigenstates with energies between E and $E + \delta E$. The thermal equilibrium macro-state at energy E corresponds to a subspace labeled '$\mathcal{H}_{eq}$' of $\mathcal{H}$, such that $\dim(\mathcal{H}_{eq})/\dim(\mathcal{H})$ is close to 1. They say that a system with state vector $\psi \in \mathcal{H}$ is in thermal equilibrium if ψ is "close" to $\mathcal{H}_{eq}$. Then, as said, they claim that for typical Hamiltonians all initial state vectors ψ_0 evolve in such a way that ψ_t

15 Furthermore, it is worth admitting that perhaps I am missing further results for (or against) the typicality of dynamics, besides the supportive *old* numerical simulations mentioned by [Frigg and Werndl, 2012] (basically [Ford, 1973]). A future task can be to continue pursuing the goal of the chapter by looking for further supportive results in the ocean of literature around statistical physics.

is close to $\mathcal{H}_{eq}$ for most times t. Hence, ψ_t is in thermal equilibrium for most times t.

This is inspired and closely related to von Neumann's quantum ergodic theorem of [von Neumann, 1929], which they investigated and elaborated in this modern version. Such theorem is thoroughly analyzed in [Goldstein et al., 2010b].

The macroscopic appearance is expressed in terms of von Neumann's concept of macroscopic observables, developed for the first time in [von Neumann, 1929]. Then, they argue that for a typical finite family of commuting macroscopic observables, every initial wave function ψ_0 from a micro-canonical energy shell evolves such that for most times t in the long run, the joint probability distribution of these observables obtained from ψ_t is close to their micro-canonical distribution.

Likewise, according to [Linden et al., 2009, 8], reaching equilibrium is a universal property of quantum systems: almost any subsystem in interaction with a large enough thermal bath will reach an equilibrium state and remain close to it for almost all times[16].
They claim that:

> "With almost full generality all interacting large quantum systems evolve in such a way that any small subsystem equilibrates, that is, spends almost all time extremely close to a particular state. The only conditions we require are that the Hamiltonian has no degenerate energy gaps (which rules out non-interacting Hamiltonians) and that the state of the whole system contains sufficiently many energy eigenstates.
> Virtually all physical situations satisfy these requirements. Firstly, all but a measure zero set of Hamiltonians have non-degenerate energy gaps".

They have proved that for every state of the subsystem, almost every state of the bath, and almost every hamiltonian, the subsystem equilibrates.

I only very briefly have cited these works and their results. Yet, it is desirable a proper critical assessment of these results in the quantum domain, which I cannot elaborate here. Future research should critically assess the insights presented.
Before the global conclusions of the whole dissertation, let's overview the particular numerical simulations that I have elaborated.

16 The bath refers to the fact that the quantum system is supposed to be surrounded by an external system at a constant temperature; the whole system is thus composed of the quantum subsystem and the bath.

5.4 NUMERICAL SIMULATIONS

This section aims to support the claim of the independence from the dynamics with computer simulations that I have programmed. Specifically, the simulations aim to show that the tendency to equilibrium, in the hard-sphere model of classical statistical mechanics, holds for a large class of possible dynamics; arguably, for almost any dynamics in which energy is conserved.

5.4.1 *Description of the model and the algorithm*

The simulation models a hard-sphere group of particles of an ideal gas in a container that move according to a certain set of laws. I have programmed the simulation in a way that is very easy to change the laws that rule the behavior of the particles. Thus, for each different set of laws it can be verified whether the tendency to equilibrium obtains. This can be verified by merely looking at the graphical simulation of the particles bouncing in the container (see the figures 15, 16, and 17 and the links on vimeo.com cited below), or by verifying the results displayed in the histograms (figures from 15 onwards), which quantify the spatial distribution of the particles at a certain time .

The source code is in the annex C. A detailed explanation can be obtained by reading the several comments included in the source code —and of course, by reading the code itself. It is based on a code that I found in Mathworks' web[17], simulating a standard case of billiard balls colliding —I must thank the author Peter van Alem for making publicly available his code. A general explanation of my code comes in what follows.

The aim of the algorithm is to simulate different dynamics in different runs of the simulation. It does so in function of a variable labeled 'θ' (because it represents an angle, as we will see), whose value can be changed in each execution of the algorithm (or, of course, it can be programmed an automatic change of the parameter in a multiple execution of consecutive trials). The variation of this parameter changes the way collisions occur between particles. Specifically, it changes the rebound angle after the collision. I made it such that it only changes the angle, hence the direction, not the intensity of the velocity vector. Therefore, the linear momentum and the energy are conserved. I made (incidentally, before arriving at the definitive version) versions in which linear momentum or energy were not conserved (but they can be interesting too).

How to manage "all possible laws"

17 http://www.mathworks.com/matlabcentral/fileexchange/20759-billiards/content/Billiards2D.m

Figure 15: The Initial Conditions set the particles randomly distributed but within the left down corner. The picture is after a time $t \geqslant 3sec$. For the sake of illustration, the number of particles in this figure is only N=80, but in the results it was used a higher N.

Notably, the method employed allows a relatively clear-cut way of covering a full set of different laws. This permits a way of tackling the elusive task of managing a wide set of possible laws in a concise way (what I commented in p. 1.3).
More specifically, the value θ represents the value of an angle that is added to the standard angle post-collision (where the 'standard' angle means that it is the angle calculated in the standard case of actual newtonian laws). As seen better in the details of the code, in that way its variation will be meaningful within the range of values from 0 to 360; further values would only redound in already tested cases. Hence, sweeping from 0 to 360 degrees, with the accuracy provided by a variable of type 'double', we obtain the range of *almost all possible post-collision directions*[18].

First, it has been reproduced the standard case, following newtonian laws, corresponding to $\theta = 0$. This allows to verify that the simulation works properly, by visualizing the behavior as well as checking the final results.

In order to verify the proper behavior of the actual dynamics and of the variations, simulations in very clear conditions —with few particles, moving slowly— have been carried out. A simulation modeled the case of only two particles, of much bigger size, symmetrically placed, and colliding. This allowed to easily verify the proper working both of the standard actual laws as well as the sensitivity to the variations of the variable θ.

18 The accuracy of a variable 'double' is limited, but is it sufficient to our purposes? Given that there is high sensitivity to initial conditions, the use of a too coarse grained variable might miss possible outcomes. The range of the variable of type 'double', though, is sufficiently fine-grained, I would say; specifically, it ranges from the minimum value $-1,79 \cdot 10^{308}$ to the maximum $+1,79 \cdot 10^{308}$.

The key moment in which the different post-collision angle is set is here:

```
        alfa=abs(asin(V(1,2)/sqrt(V(1,1)^2+V(1,2)^2))) ; % In radians
        alfa=rad2deg(alfa); % Change to degrees
        beta=alfa-theta;
        V(1,1)=sign(V(1,1))*abs(cosd(beta));
        V(1,2)=sign(V(1,2))*abs(sind(beta));
        % IDEM FOR PARTICLE 2
        alfa=abs(asin(V(2,2)/sqrt(V(2,1)^2 + V(2,2)^2))); %In radians.
        alfa=rad2deg(alfa);
        beta=alfa-theta;
        V(2,1)=sign(V(2,1))*abs(cosd(beta));
        V(2,2)=sign(V(2,2))*abs(sind(beta));
```

As previously said, I recommend to see the source code for all the details. Especially, the variable 'CollisionEffectMatrix' and the various interesting changes that are commented in the code (this was a different way to change the post-collision effects).

```

       % The original matrix that calculates how the velocity vector of the two particles will change after collision
        CollisionEffectMatrix=[0,0,1,0; 0,1,0,0; 1,0,0,0; 0,0,0,1;];
        % VARIATIONS THAT ALLOW FOR DIFFERENT COLLISIONS
        %(though the variation is finally introduced after this matrix, so I maintain the original matrix)
        %CollisionEffectMatrix=CollisionEffectMatrix*0.6 ; % Reduction of speed intensity after collision
        %CollisionEffectMatrix=-CollisionEffectMatrix; % negative sign
        %CollisionEffectMatrix=[0,0,1.3,0; 0,1.1,0,0; 0.2,0,0,0; 0,0,0,1;];
        %CollisionEffectMatrix=[0,0.7,sind(theta),0; 0,sind(theta),0,0; sind(theta),0,0.7,0; 0,0,0,sind(theta);]; % VALUES WITH DIFFERENT COLLISIONS!  (Good but diverges to infinite velocities, because momentum is not conserved!)
```

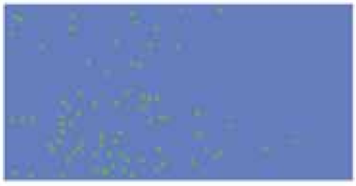


Figure 16: After 5 seconds, the particles are spreading out, still more concentrated in one region

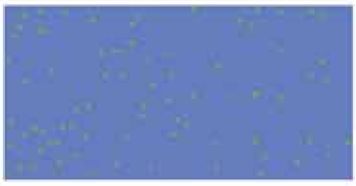


Figure 17: After $t > 20$ seconds, the particles have spread out almost uniformly around the whole container

At the end of the day, a sweep of all the values of θ from 0 to 360 illustrates that the tendency to equilibrium is a property not only for the actual newtonian laws but of a wider set. It is shown how the tendency to equilibrium in the macroscale occurs in all the counterfactual scenarios tested.

Assumptions of the simulation

Of course, a lot of assumptions are made in this model. Besides the assumptions of the hard-sphere model (see the details at the beginning of the chapter), there are the limitations of the computer simulation. Worth mentioning is the fact that the random element introduced in the second series of simulations is not truly random, but pseudo-random. Even so, this limitation is not a problem, I think. The reason has been argued justly in the previous chapter: the chaotic behavior involved in the collisions produces a random-looking evolution indistinguishable from a truly random evolution. A pseudo-random value will be generated by a deterministic algorithm, but will be anyway totally unpredictable, satisfying the definitory condition of randomness[19].

In addition to these assumptions, the most important constraint is

19 See MATLAB documentation for a clear survey on the sophisticated pseudo-randomness that this software possesses:
http://www.mathworks.es/es/help/matlab/random-number-generation.html.

that the variations of the dynamical behavior are always made such that the conservation of energy and total linear momentum are preserved.

5.4.2 *Results*

A brief illustration of the simulations can be found online in the links:
https://vimeo.com/90044328
https://vimeo.com/90863487
What I attach here is a partial summary of the results.
For the sake of clarity, the results are presented in a way that it is easy for the reader to glimpse the overall results and acknowledge the key conclusion. To this end, I am presenting the results for each of the different runs of the simulation in the form of a 3-dimensional histogram. The histogram represents the whole container in the two horizontal axis 'x' and 'y'. This region is divided in different coarse-grained squares. Then, the third axis, 'z', represents the number of particles in each coarse-grained region.
These graphics allow to easily glimpse the approximate equidistribution of particles over the whole region. Obviously, here I cannot include the thousands (millions) of graphics for each variation of the parameter θ. This is only a partial selection of the results obtained, sufficiently illustrative of the underlying moral.

5.4.2.1 *The tendency to equilibrium for almost all possible elastic collisions*

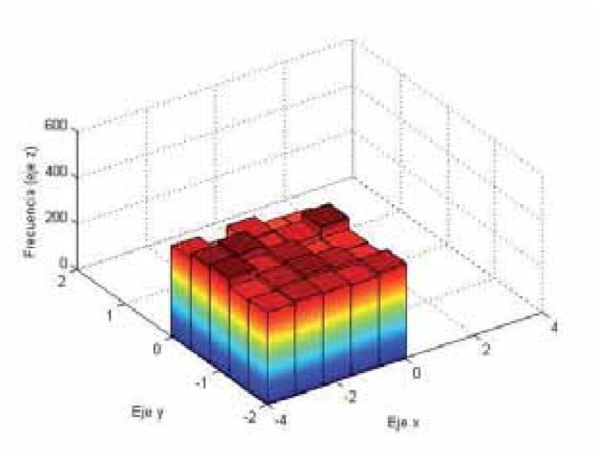
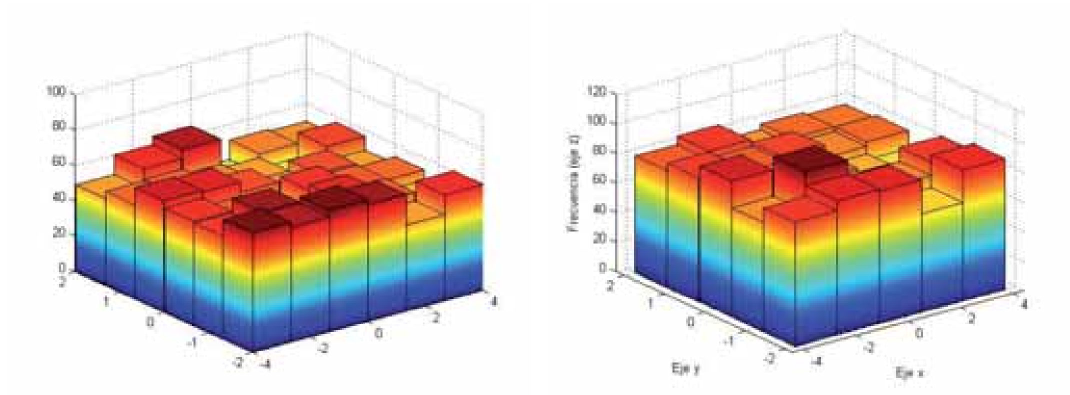


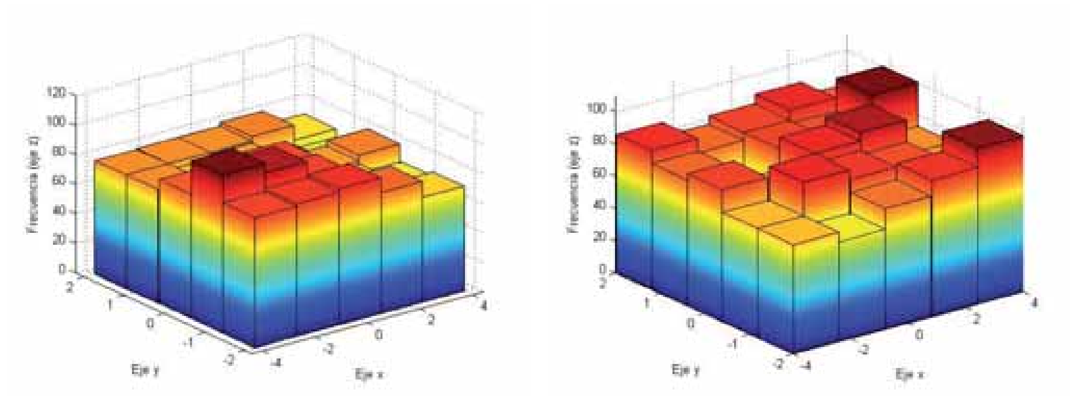
Figure 18: Histogram of the initial distribution, with number of particles $N = 10^4$

Due to the limited throughput of my computer, the maximum number of particles simulated is $N = 2 \cdot 10^3$. Even if the number is extremely small compared with what is usually assumed (of the order of Avogadro's number), nevertheless the results hold, as it is shown.



(a) Run #1; area coarse-grained in 36 macro-regions(b) Run #2; area coarse-grained in 25 macro-regions

Figure 19: Histograms of two simulations with parameter θ=0, after a relaxation time t (t ⩾ 20sec).



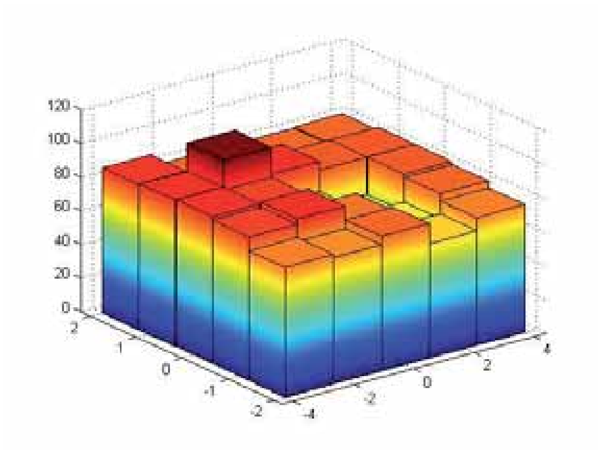
(a) Run # 1 (b) Run # 2

Figure 20: Histograms of two simulations with parameter θ=200, after a relaxation time t (t ⩾ 20sec).

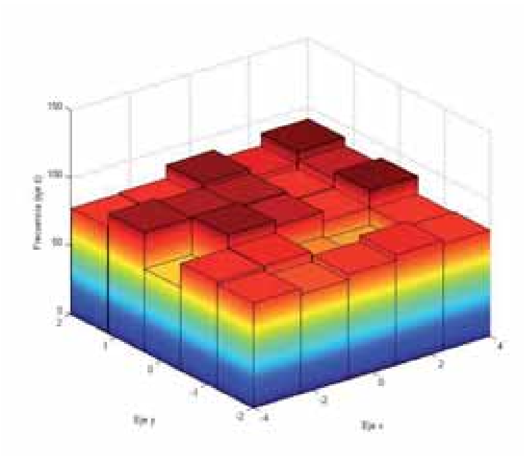
5.4.2.2 *Long term statistics of a pseudo-random dynamics*

In addition to the simulations varying the underlying laws —i.e. for almost all possible elastic collisions—, another set of simulations has been carried out with a significant change in the source code. Now, in *each* collision the post-collision angle takes a different value, which is decided randomly. The outcoming value is decided by the MATLAB function $rand()$. Therefore, the outcoming value is random —or more exactly, pseudo-random. The parameter θ is set to the value dictated by rand(), for each collision of each pair of particles. This can be obtained by adding the line:

```
theta = rand()*360 ;
```

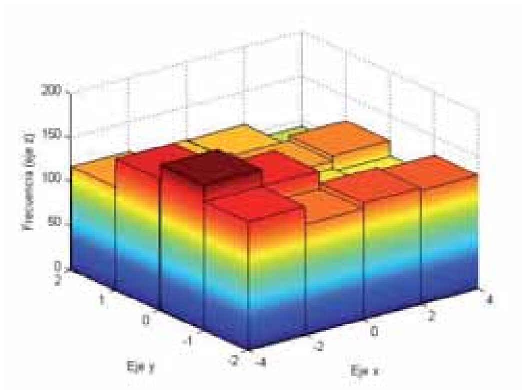


(a) Run # 1, with low-entropy I.C.

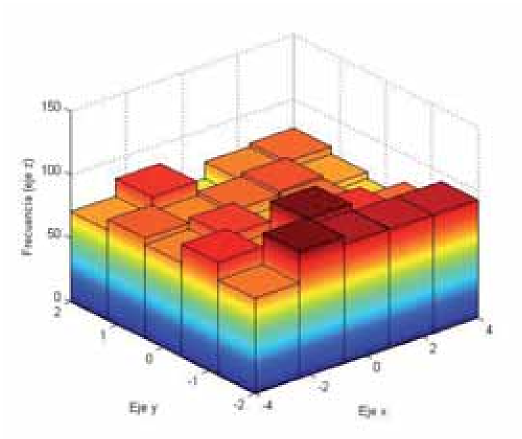


(b) Run # 2, with random I.C.

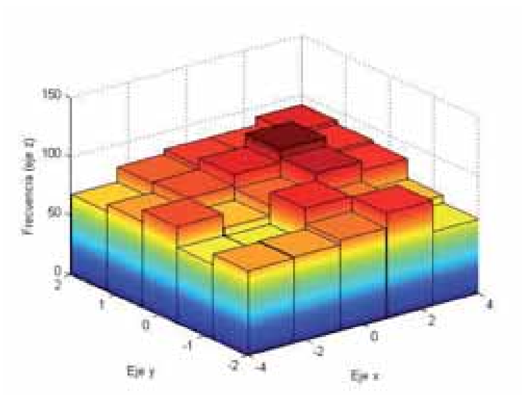
Figure 21: Histogram of the distribution at time t=20, with parameter θ=208.



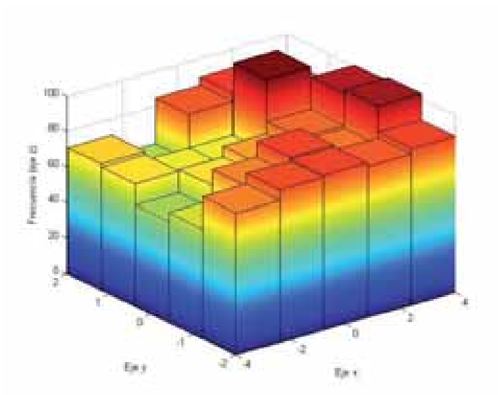
(a) Run # 1



(b) Run # 2

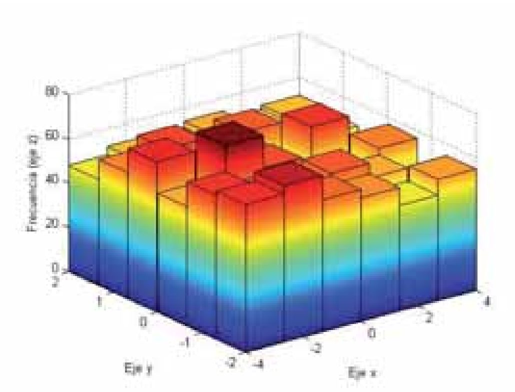


(c) Run # 3

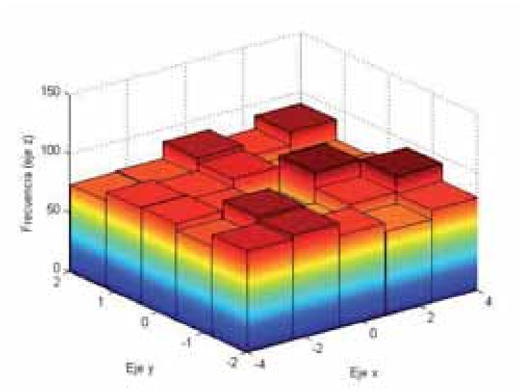


(d) Run # 4

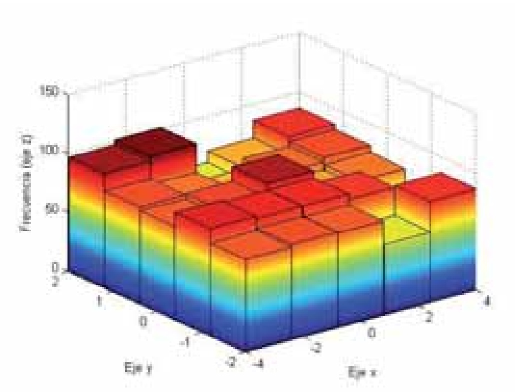
Figure 22: Histogram of the distribution at time t=20, with parameter θ=72.



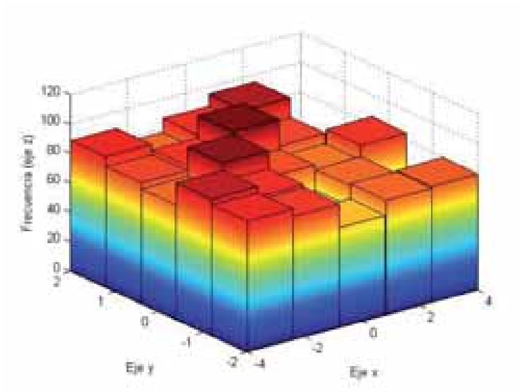
(a) Run # 1



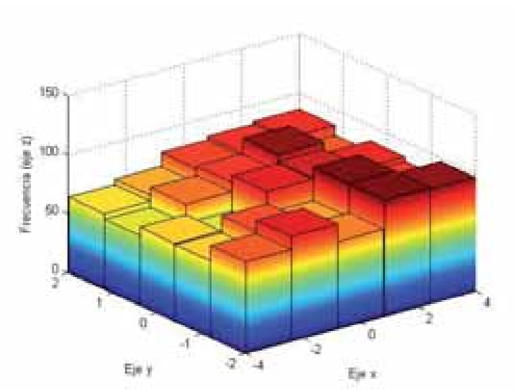
(b) Run # 2



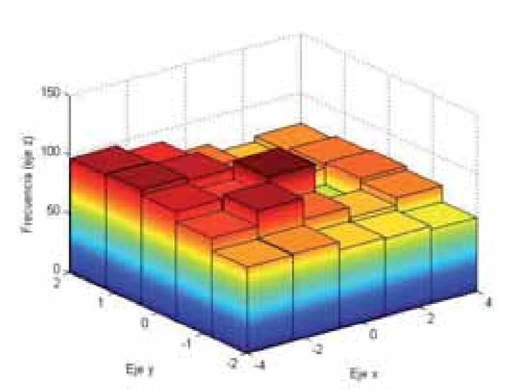
(c) Run # 3



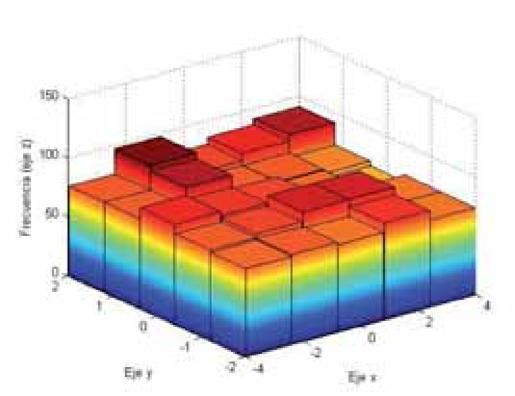
(d) Run # 4



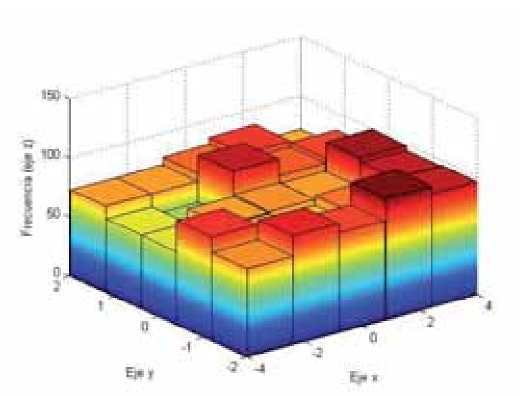
(e) Run # 5



(f) Run # 6



(g) Run # 7

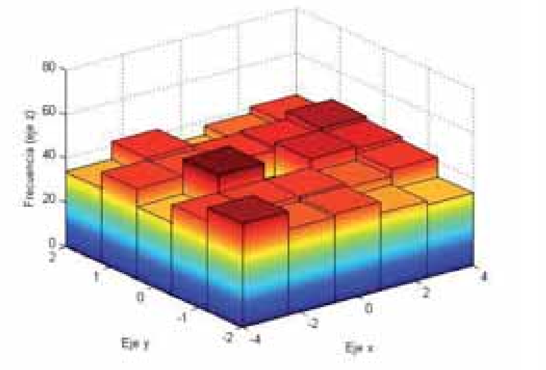


(h) Run # 8

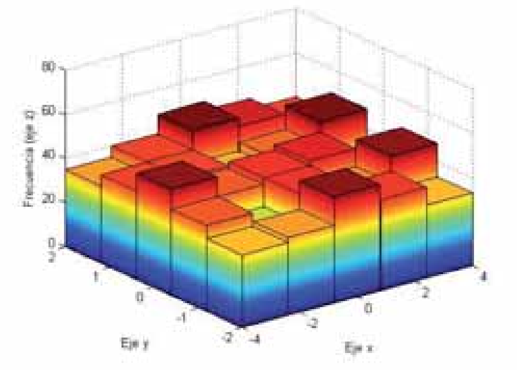
Figure 23: Histogram of the distribution at time t=20, with parameter θ=117.

before the moment of the calculation of the collision angles.
The results are the same, as shown in figures 24.

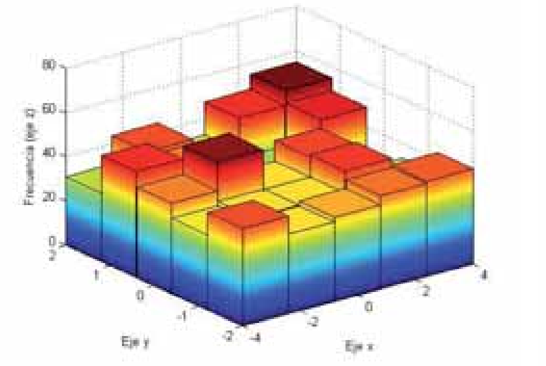
In conclusion: the results are the same for any of all the variations of dynamics tested. That is, the tendency to the equilibrium macrostate holds 1) for the actual laws, 2) for any laws corresponding to any variation of the angle θ, and 3) for any iteration executed with the (pseudo)-random dynamics.



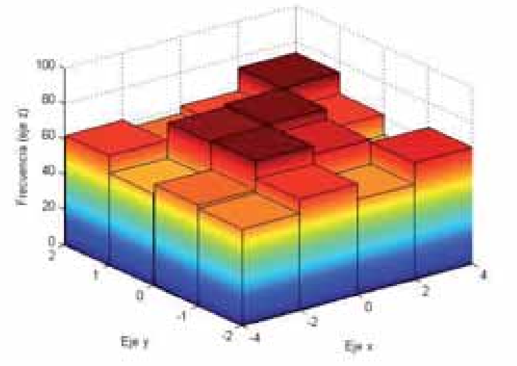
(a) Run # 1



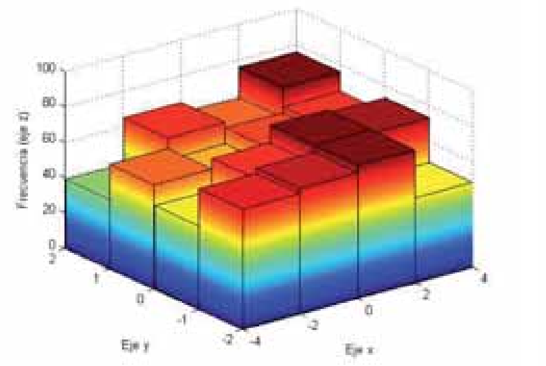
(b) Run # 2



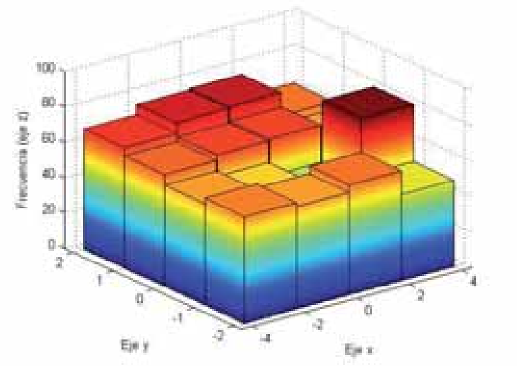
(c) Run # 3



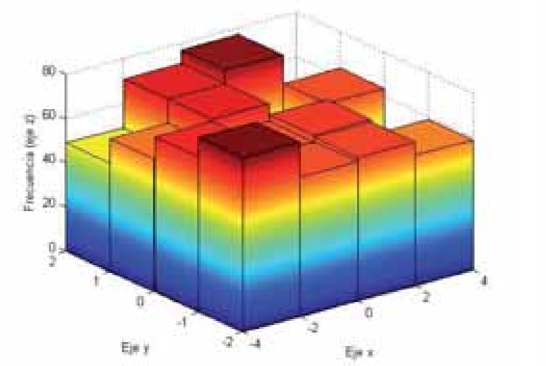
(d) Run # 4



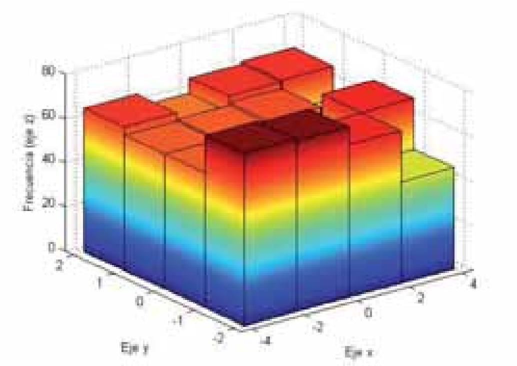
(e) Run # 5



(f) Run # 6



(g) Run # 7



(h) Run # 8

Figure 24: Histogram of the distribution at time t=20, low-entropy I.C., and parameter θ=rand()*360 at each collision.

5.5 CONCLUDING REMARKS

I have assessed specific results and objections of this chapter in 5.3 p. 138. Now I move away from the specialization of the chapter and take a global perspective. So this section recapitulates in a concise manner the results and objections of all the chapters and states the future lines of research.

Part II

To begin with, the present chapter has shown continuity with the previous while offering a different approach, as it has carried out a delimited study into the well defined field of classical statistical mechanics (with a further extension to the quantum domain).
The results of the present chapter, focusing on a simplified model and on a single regular behavior (the tendency to the equilibrium macrostate), show how difficult is to obtain something close to a proof of the main claim, which is wider in scope —the claim that lawful behavior can emerge from a lawless level.
On the other hand, chapter 4 was more generic, following the abstract study of the necessary properties for the method of arbitrary functions to lead to stable behavior. In doing so the chapter is making clear the directives for the future analysis of each particular case of interest. This means that one could study certain systems and assess whether the necessary conditions (microconstancy and random-looking motion) obtain.
Both chapters constitute part II, which has focused on studying the feasibility of the process of emergence of stable behavior / reduction of complexity.

It would be interesting, for instance, to ascertain the feasibility of such projects in areas of theoretical "fundamental" physics, as those treated in Nielsen's project, or the areas of those who research about entropic forces. This comprises diverse fields, some of which appeared in chapter 3, especially 3.3.2. Thus the continuity between part I and II would be enhanced (even more).
Furthermore, even if not so linked to a metaphysics of laws, a development of the applications in different complex sciences (as meteorology or economics) suggested in [Strevens, 2003] would be worth pursuing.

Part I

Part I has mostly focused on studying the plausibility of a bottom level which is lawless. It started in chapter 1 motivating the interest of the whole project, reviewing the existent literature on the metaphysics of laws, and underlining the lack of satisfactory answers. I continued in chapter 2 with a reflection on how our most fundamental laws look like. I argued why such laws, formulated in the form of gauge symmetry principles, are neither fundamental nor a priori reasonable according to certain criteria. This excluded the otherwise interesting scenario of renormalizable local gauge theories of QFT, in which laws appeared to be exclusively determined by the postulated matter-content (treated in 1.5 and 3.3.1). Further, in chapter 2 I argued for the unnaturalness of current physical laws, in support of an antirealist metaphysics of laws. More in detail, I proposed a landscape of plausible metaphysical scenarios, arguing for the plausibility of a highly-complex or lawless one. This option is supported if one is willing to find unsatisfactory the high contingency displayed by any physical law —more specifically, if one finds puzzling the 'unnaturalness', which I defined as the high contingency of the mathematical expressions in which laws are formulated, chosen only because of high empirical adequacy.

After defending the possibility and plausibility of such a metaphysical image, in chapter 3 I reflected upon the meaningfulness of such a lawless scenario. To this end I set forth some arguments around David Deutsch's discussion of John Wheeler's proposal. Then chapter 3 concluded with a summary of the central points of the project that attempts to explain a process of formation of symmetries.

WHAT PHYSICAL NECESSITY? As I have stated throughout the thesis, the interest is to apply the results brought to the fore as the central part of an explanation of the notion of physical necessity.

It has been proposed that this kind of modality is grounded in mathematical, probabilistic facts. That is, the occurrence of apparent physically necessary behavior is due to the fact that the context displays conditions such that it is overwhelmingly more likely that a specific stable behavior occurs. This is assumed to hold of an unknown, undefined, fundamental level of reality. The fundamental level is assumed to consist of a highly complex or random dynamics (part I), which leads in a higher level to simpler and stable regularities (part II).

This *stable* behavior is the result of what can be called a *statistical necessity*. Thus, the stability has been explained in terms of probabilistic reasoning plus further non-dynamical conditions. This is all the "physical necessity" that there is.

Statistical Necessity

Is this a sufficient explanation of what we used to describe as physical necessities and non-accidental regularities in 1.1 p. 6? It is obviously not the explanation of *those* specific high-level non-accidental regularities gathered in the sentences (i) to (vi) of p. 6. But it is uncontroversial to assume the reductionist doctrine according to which higher-level laws supervene on lower-level laws. This supervenience presumably holds up to the actual laws of physics, and they in turn supervene on others below, up to the bottom fundamental level. So in this way the conclusions of this dissertation can be understood as a possible explanation of how some regularities, no matter in which level of reality they hold, appear to be non-accidental.

Does this work for 'All emeralds are green' or 'electrons repel each other'?

I have been elaborating this claim in an abstract context in chapter

4, studying the generic conditions of applicability; in a more delimited domain (a classical gas in a container) and for a specific behavior (entropic behaviour) in chapter 5; likewise I have brought up similar insights, in quantum physics (in 5.3.1) and in all the current Standard Model physics (in 3.3.2).

The assumption of stability

It has been assumed that nothing can emerge from nothing —*ex nihilo nihil*—, so a stable factor has to appear in the explanans in order to account for the stability of the explanandum. The stability here is postulated not in the dynamics but in the stable spatial symmetries required for microconstancy[20] and in certain basic constraining principles (like principles of conservation).
Given this and other assumptions, we arguably obtain a *stable* probabilistic outcome, which can be described by a *stochastic dynamics*. Remember the explanation of a coin toss. In the long run the sequence exhibits a stable output frequency —the frequency of the strike ratio, 1/6 for a die, 1/2 for a coin, etc.— and, surprisingly, for this to happen a necessary ingredient is just the sensitivity to initial conditions. As [Strevens, 2003, 61] explains, the generator of chaos creates not just short-term disorder but also long-term statistical order. Thus, macroscopic order is explained in part by microscopic chaos. This seems magic, as Maudlin and Strevens literally say each on his own. But it is explained by our current knowledge of complex systems.

This project leads to a weak notion of physical necessity, weaker at least than what is granted in some contexts when speaking of the immutability and universality of the fundamental laws of nature. From a certain perspective, this weakness is a virtue, as it rules out any mysterious metaphysical status to the laws. This is so because their type of modality gets demystified when elucidating on what it is based. As said in chapter 1, in this picture there is only the category of logical necessity and all the rest is contingency. Then, what we call physical necessities are "just" *robustly stable contingencies*.

Physical necessity as robustly stable contingencies

Thus, physical necessity is to be understood as a matter of degree, and in that sense it can be linked with the works of Marc Lange's "Laws and Lawmakers: Science, Metaphysics, and the Laws of Nature" [Lange, 2009] (cf. [Lange, 2005] and [Lange, 2008]) and James Woodward's "Making Things Happen: A Theory of Causal Explanation" [Woodward, 2003]: the first spells out an account of laws in which there are different layers of invariance under counterfactual perturbation, thus establishing a hierarchy of degrees of necessity. A similar idea is found in Woodward but from a different approach. Woodward appeals to generalizations with different degree of invariance. He also faces the dichotomy law vs accident in the same line

Similarity with other philosophy about laws

20 More generally, the symmetry need not be only spatial, but any basic property (thanks again to Michael Strevens for this observation).

of eliminating any abrupt threshold between them[21]. The approach of this dissertation, though addressed from a different viewpoint, fits well with these versions. An interesting future line of research would be to analyze to what extent can be merged and complemented all these approaches.

A rationale of the whole project has been that an explanation of physical necessity can be achieved by focusing on understanding *why it is that there are certain stable symmetries* (analogous to question '2' of 1.1 p. 6) and *why it is that there are these symmetries rather than others* (analogous to question '3' of 1.1 p. 6). To this end, the existence of two opposite approaches has been envisaged: *The dialectics*
1) understanding the actual presence of symmetries by considering that they are a result of a process of *[spontaneous] symmetry breaking*, coming from an initial situation of absolute symmetry. In this option, absolute symmetry is assumed from the beginning, as the initial state of the universe. The breaking of symmetries is thus the process responsible of arriving to the present situation in which some symmetries remain —being the ground of the current fundamental laws.
2) The other option assumes the lack of any particular symmetry principle for a generic initial state, thus without any defined dynamics. That is, the initial state will exhibit a random walk. Thus it argues for the likeliness of the *formation* of symmetries in the long term, for most of the possible evolutions that can be taken from that initial non-symmetric situation.
This second option is the one explored and defended in this chapter.

This fragment from [Barrow and Tipler, 1986, 256-257] illustrates the plausibility that the second option may have:

> "matter and radiation have a purely random origin, and even gauge invariance may be an 'illusion': a selection effect of the low-energy world we necessarily inhabit. Some preliminary attempts to flesh out this idea have shown that even if the underlying symmetry principles of Nature are random —a sort of chaotic combination of all possible symmetries— then it is possible that at low energies (10^{-32} ºK) the appearance of local gauge invariance is inevitable under certain circumstances. A form of 'natural' selection may occur wherein, as the temperature of the Universe falls, fewer and fewer of the entire gamut of 'almost symmetries' have a significant impact upon the behaviour of elementary particles, and orderliness arises. Conversely, as the Planck energy (which corresponds to a temperature of 10^{-32} ºK) is approached, this picture would predict chaos".

21 As indicated in [Samaniego, 2011], other approaches press in the same direction, namely Michael Redhead [Redhead, 1987] who appeals to the notion of robustness, and Sandra Mitchell [Mitchell, 1997], who appeals to stability.

Refine the expectations

FURTHER OBJECTIONS Depending on the expectations, it could be thought that the results are too programmatic and incomplete. For instance, this is clearly so if the reader expects a sort of full proof of the metaphysical account of laws of nature here proposed. This disappointment, though, can be fairly resolved just by properly readjusting the scope: this dissertation analyzes certain arguments for a metaphysical account of laws of nature, highlights certain virtues and defects of the account, studies specific applications through which its feasibility could be defended, and summarizes the key arguments of the diverse authors that have pursued the same goal (philosophers and physicists, like Peirce, Wheeler, Nielsen's team, and a long etc.).

The separation of laws and I.C.

Furthermore, there is a general objection to the whole project regarding the separated treatment of laws and initial conditions (of matter content), which I am somehow uncritically inheriting. I myself reflected in [Filomeno, Forthcoming] about the separated ontological nature between the two notions, where laws are supposed to apply onto the matter content[22]. Many others have raised the doubt on this apparent independence of laws and initial or boundary conditions, e.g. [Barrow, 1988] or [Frisch, 2004].
What has been said in this respect, at least, is the observation of the link of symmetries and matter content as developed in 1.5, plus the further reflections in 3.3.1.

FUTURE LINES OF RESEARCH Let me summarize concisely other open branches of research. Given that the quantity of different approaches that I wanted to mention in the dissertation prevented to treat all of them thoroughly, an interesting future research is to continue critically assessing all of them:

- For instance, I only gave a summarized presentation of the key points of the typicality approach in the quantum domain.
- Likewise, probably further experimental results can be found in support (or against) the conclusions of the section on the typicality of dynamics in the classical gas in a box. In such a way the results can extend, or restrict, the scope of the claim of the typicality of dynamics.
- Another project would be to critically assess the "random dynamics" project of Nielsen *et al*. There is an interesting variety of dialectics present in their work. It is obvious that this was too much —it has already been a long task to discover, realize that it was worthwhile, and summarize their key features.

22 More in detail, in the paper I interpreted the mathematical inconsistency (infinities) of some physical equations with some initial conditions as suggesting a case against their separated nature; specifically, I cited the cases of Maxwell's equations, following Mathias Frisch' investigations.

- The similarities between ch. 4 and 5 should be evidentiated so that it becomes clear the role that perhaps the method of arbitrary functions plays in the claims of typicality. My intuition is that the former is a more precise explanation of why the more generic claims of the latter hold. Explanations in classical statistical mechanics with the method of arbitrary functions (and with Strevens' insights in particular) have been already carried out —in [Engel, 1992], [Strevens, 2003, 4.8], [Strevens, 2013].

- As to the applicability of the results in a metaphysical theory of laws, is it meaningful to consider a certain epoch of the universe as a certain closed system which possessed the property of microconstancy, given the postulation of spatial symmetries? Perhaps good or bad arguments can be found in this respect focusing on our most fundamental physical theories.

- How reliable are the parallel lines of research cited before? In particular, 1) that of entropic gravity, or 2) the recent project of Lee Smolin (see references for both at the beginning of 3.3.2). As far as I know after reviewing their papers, the projects are not developed to the point that a proper philosophical assessment of them can be carried out. Perhaps further research improves the situation.

Part III

APPENDIX

A

ANNEX. NOTIONS OF RANDOMNESS

Contents

A.1 INTRODUCTION

This annex surveys how the sometimes overlapping notions of randomness and objective chance have been fleshed out. Among the different senses in the literature I discern the most relevant for our philosophical purposes. More in detail: in A.1 and A.2 I introduce the discussion exposing some claims found in the philosophical literature about the notion of objective chance, in relation with notions like probability, indeterminism or subjective chance. I make some remarks and then I differentiate the relevant sense to our purpose.
In A.3 I carry out a survey of the mathematical formalizations of the sense of 'randomness'. A formal definition of the 'law of large numbers' is also included, plus a theorem regarding the inevitability of the presence of patterns in random sequences —what I found as 'Van der Waerden theorem'.
Finally, in A.4, I reflect upon the notion of more interest here, the so called 'process-randomness'. In A.3 I was referring to the so called 'product-randomness', and there I summarized how it has been achieved a precise formulation. In A.4 I outline an elucidation of 'process-randomness' in terms of the previously well defined 'product-randomness'.

Roughly, this is a way of fleshing out such notion in terms of its outcomes.

Three roads to objective chance

To frame the exposition, a source to begin with is [Maudlin, 2011], which expounds three main approaches to the notion of objective chance. He calls them 'stochastic dynamics', 'humeanism' and 'deterministic chances'. Of the three, the one that most concerns us is the first, for the others share a sort of deflationist or antirealist spirit, contrary to the chance that we want to entertain[1].

Stochastic dynamics

The first type, 'stochastic dynamics', is the objective probability that is present in indeterministic systems, in which the state of a system at one time and the laws are jointly compatible with different states at later times. The paradigmatic case of this type of chance is to be found in some interpretations of quantum mechanics —in which the assignation is not of equiprobability, but of a Normal (or Poisson or Binomial) distribution—, where these probabilities are interpreted as reflecting a genuine indeterminism of Nature. It is worth noting that these are dynamics *governed by a law*, though in this case a probabilistic law. That is, as I remarked at the very beginning, in 1.3 p. 11, this is an intermediate degree between the two notions of deterministic laws and pure randomness, where both of them are present —and both demanding philosophical elucidation.

There is, though, no further elucidation of the nature of these probabilities, where do they come from and why. For Maudlin, there is nothing in need of explanation, literally: *"the notion of irreducibly stochastic dynamical laws is perfectly clear and requires no philosophical elucidation at all"* [Maudlin, 2011, p.5]. He adopted the same strategy in what respects the traditional (i.e. non-probabilistic) notion of laws of nature [Maudlin, 2007, ch.2]. I am personally sympathetic to the attitude of "dissolving" problems that are not really such, as sometimes has been proposed (Wittgenstein being a famous example): perhaps the correct attitude towards some current philosophical disputes has to be this[2]. However, it does not seem a legitimate move to resolve the difficult issue of the nature of laws by taking them as a brute fact: even granting the advantages that this strategy yields —as Maudlin argues in [Maudlin, 2007]—, I have devoted 1 (especially 1.3 and 1.4.1.2) in justifying the pertinence of this subject. As we can explicitly realize in

1 About the humean view on chance, see e.g. [Lewis, 1980] or [Hoefer, 2007]. About deterministic chances, see our well-known [Strevens, 2011].

2 Let me speculate with some possible candidates for a "dissolution": the metaphysical debates about personal identity; those about the persistence of objects —in fact, a metaontological view is that philosophers talk past each other—; or the failure of the truthmaking principle —that is, it is not a problem that such "principle" fails. Perhaps, even for more settled debates the answer is to dissolve the problem, e.g.: the disputes between normative ethics —because relativism is the correct position—; the definition of 'art' in aesthetics —for the same reason—; as well as ancient issues as the meaning of life, or the existence of God —because there is not a clear-cut meaning nor reference of the terms involved.

some of the fragments I quote in annex B, C. S. Peirce also manifested a clear opposition with respect to Maudlin's attitude. As a representative point, in what respects the case of stochastic laws Maudlin's move is at odds with the discussion that arose at the beginnings of quantum theory among their founders —Bohr, Pauli, Schrödinger, Einstein, et al[3]. Borrowing Einstein's quote, if God does play dice, then this fact does require philosophical elucidation. See further arguments in 1.4.1.2 p. 20.

A.2 A MAP OF THE SENSES OF 'CHANCE'

A certain sort of randomness has popped out implicit in the definition of stochastic dynamics. This section seeks to distinguish the diverse ways in which 'randomness' or 'chance' has been fleshed out in the literature[4].

Process and product senses

The first thing to remark is that two senses of randomness are to be distinguished. One is the so called *process* (or genesis) sense, the other the *product* (or performance) sense. The product sense remits to a feature of an already occurred sequence; it regards whether the resulting outcomes, as they appear, present certain characteristics so that the sequence merits the qualification of random. As we will see, this sense is accurately defined in terms of *complexity*, *disorder* and *typicality*. The process sense, instead, is in principle orthogonal with the appearance of the outcomes of a sequence. It is related with the process that generates an outcome, with its generation. John Earman [Earman, 1986, p. 137] describes the process sense as that of a process that is *without principle, without guidance of laws* (whether deterministic or not), whereas the product sense is referred to the output of a process that is *disordered or lacking in pattern*.

It is obvious that 1) we have to be interested in the process sense, and that 2) there is some sort of stronger or weaker link between both senses. It is rather plausible to bestow process-randomness to a process that presents a product-random outcome, and, conversely, to predict a product-random output for a process we know to be process-random. At the same time, it is also very straightforward to realize that this does not need to be the case necessarily, having one without the other. Antony Eagle devotes the Stanford's Encyclopedia entry 'Chance versus randomness' [Eagle, 2012] to analyze this link. There the author (idiosyncratically perhaps) associates the term

3 An extended recopilation of part of these discussions can be found in the book "Quantum Theory at the Crossroads: Reconsidering the 1927 Solvay Conference" [Bacciagaluppi and Valentini, 2009].

4 The terminology can be confusing, so let me make an initial clarification. At the moment, as some literature does, 'chance' refers to stochastic dynamics, which amounts to a probabilistic distribution function. However, as we will see, other bibliographical references use 'chance' in a different way, i.e. to refer to a specific sense of 'randomness' ('process-randomness'), which is something "stronger" than stochastic dynamics. As to the term 'randomness', it is going to be defined in what follows.

'chance' with the process sense, and 'randomness' with the product sense.

Further, Earman talks about the possibility, at least conceptual, of an intermediate option, which he labels as 'stochastic', meaning not wholly random, not wholly haphazard, but in accord with probabilistic laws. The reader will realize that this coincides with the stochastic dynamics referred by Maudlin before.

Mathematical literature on product randomness

With respect to the investigations about the notion of chance and randomness in the history of philosophy and mathematics, the results are basically recent. There has been since the 70's strong and successful research regarding product-randomness, spelled out in a precise mathematical language with the aid of computational theory. Likewise, the subject of the philosophy of probability has also grown in recent decades. However, regarding the point as to the precise meaning of process-randomness, the literature is more scarce, perhaps because in the sciences (both in special sciences as in physics) the presence of stochastic laws is just presupposed (or ignored in deterministic frameworks), while in the philosophy of physics is not faced directly, as the main focus has been the related topic of chance —understood as linked with probability and with stochastic dynamics. Some approaches to chance are far from what we are interested. For instance, the survey of philosophy of probability found in the book [Gillies, 2000] never engages with the question about the definition and meaningfulness of process-randomness. Likewise, nothing in the vicinity of process-randomness is ever faced in the (also good) book [Handfield, 2012].

Philosophical literature on objective chance

A.2.1 *Chance*

CHANCE AND PROBABILITY Chance is usually associated with the notion of probability. Chance has been defined (cf. [Eagle, 2012, secs. 1 and 1.1]) as the "objective kind of probability", and also the "single-case objective probability". Nevertheless, there is a more generic way of understanding chance which does not commit to probability: quoting Leibniz, Eagle suggests the definition of chance as *graded possibility*.

It should be noted that the notion of probability stems as a subfield of mathematics, and as such it is axiomatized in Kolmogorov's "Foundations of the theory of probability" [Kolmogorov, 1933]. Accordingly, it is agreed that chance has to obey these axioms. It is this requirement that I see of dubious necessity, and it is one of the reasons why I would like not to tie a priori 'chance' with 'probability'. Alan Hájek's fragment in the Stanford's Encyclopedia entry on interpretations of probability [Hájek, 2012] recognizes the problems in this association:

> "Kolmogorov's axiomatization [...] has achieved the status of orthodoxy, and it is typically what philosophers have in mind when they think of 'probability theory'. Nevertheless, several of the leading 'interpretations of probability' fail to satisfy all of Kolmogorov's axioms, yet they have not lost their title for that."

CHANCE AND INDETERMINISM Now, having suggested that chance needs not be associated with probability, let's face the following question to improve the comprehension of the notion: Does chance have to be necessarily associated with indeterminism?

Indeterminism without chance?

[Eagle, 2012, sec. 5] assesses whether there can be cases of indeterminism without chance, to verify whether both concepts can exist dissociated.

He does so by citing John Norton's Dome example[5]. This example has given rise to critical literature of all sorts, and indeed it is not the best example of indeterminism, but anyway set aside the critics for the sake of the argument (actually Eagle's reception is the opposite, as he dubs it as a "very elegant" example of classical indeterminism). Thus, grant that we are in front of a case of classical indeterminism[6]. The alleged particular problem that concerns us appears when trying to assign probabilities for the time of the spontaneous excitation that will cause the ball to fall. As Norton says:

> "The symmetry of the surface about the apex makes it quite natural for us to add a probability distribution that assigns equal probability to all directions. The complication is that there is no comparable way for us to assign probabilities for the time of the spontaneous excitation that respect the physical symmetries of solutions. Those solutions treat all candidate excitation times T equally. A probability distribution that tries to make each candidate time equally likely cannot be proper -that is, it cannot assign unit probability to the union of all disjoint outcomes[7]. Or one that is proper can only be defined by inventing extra physical properties, not given by the physical description of the dome and mass, Newton's laws and the laws of gravitation, and grafting them unnaturally onto the physical system." [Norton, 2008, p. 9,10].

5 For further information of this alleged violation of determinism in classical physics, see [Norton, 2008] or the explanation in the author's webpage: http://www.pitt.edu/ jdnorton/Goodies/Dome/index.html

6 Other example cited is that of space invaders, presented by [Earman, 1986, ch. 2]. This is even more disputable and "exotic". See [Hoefer, 2010] for a concise explanation of both examples.

7 Norton's footnote: "Since all excitation times T would have to be equally probable, the probability that the time is in each of the infinitely many time intervals, (0, 1), (1, 2), (2, 3), (3, 4),... would have to be the same, so that zero probability must be assigned to each of these intervals. Summing over all intervals, this distribution entails a zero probability of excitation ever happening".

The example is aimed to represent a case of indeterminism without chance. But the answer against this reasoning can be found just in the previous paragraph of Norton, where he refers to the assignation of probabilities to each direction (not to the infinite times as above), conveying what I previously said:

> "One might think that (...) we can assign probabilities to the various possible outcomes. *Nothing* in the Newtonian physics *requires us to assign the probabilities*, but we might choose to try to add them *for our own conceptual comfort*. It can be done as far as the direction of the spontaneous motion is concerned." [Norton, 2008, p. 9, italics added].

Here Norton does not refer to the assignation of probabilities to infinite possible times at which the ball could fall. Still, the important upshot is the idea of not being obliged to assign a probability, something that holds *also* in the case of infinite possibilities —that is, of infinite times.
So, against [Eagle, 2012, 5.3], given this dissociation of chance with probabilities which I previously emphasized, Norton's dome must not be seen as a case of indeterminism without chance —at most, a case of indeterminism without probability.

In sum, I have not found cases of genuine indeterminism without chance. Not even in disputable cases within classical mechanics like Norton's Dome (let alone the space invaders), whose physical significance is far from granted.

CHANCE IN THE REAL WORLD Antony Eagle talks about the main candidate in physics for being the bearer of chance. As can be guessed, this candidate is the presence of apparently irreducible probabilities that come out in our current best theories, but especially in the theory at the most fundamental level, i.e. quantum mechanics. Quoting Eagle, *"if our best physical theories did not feature probabilities, we should have little reason to postulate them, and little reason to take chances to exist"* [Eagle, 2012, section 1.2] . This sentence can be nuanced, though. Recall that there are some approaches to the concept of chance that do not confer a special status to the probabilities of QM more than to the probabilities of other areas of physics *and* to other daily situations in which we talk about chances, namely the humean chance or deterministic chance that I mentioned in A.1. So, even from a naturalist and reductionist position, if physical theories did not feature probabilities, but did not refute them either, it is still a conceptual possibility worth exploring.

A.3 DEFINITIONS OF PRODUCT RANDOMNESS

Now let's see how product-randomness is formally defined. Product-randomness is a well defined notion that can help us in our research for a good elucidation of process-randomness, or so I argue. It is the latter that is interesting for our purposes: what Peirce, Wheeler, Nielsen, or chapters 4 or 5 refer to, is something like product-randomness.

Following Ahijip Dasgupta's exposition [Dasgupta, 2011], we can identify three approaches for defining product-randomness: *unpredictability*, *typicality*, and *incompressibility*.

Unpredictability

Unpredictability corresponds to the impossibility of there being any successful gambling strategies for predicting future outcomes. Von Mises has been one of the main responsibles of this approach [von Mises, 1957].

Typicality

Typicality is what a property of an infinite binary sequence is *"if the probability that the property holds is one"* [Dasgupta, 2011] while, on the contrary, a property is called *special* if the probability that the property holds is zero. As Dasgupta explains:

> "Consider the property of having no run of zeros of length seven. It can be shown that the probability is zero that an infinite binary sequence has no run of zeros of length seven, thus this property is special. The intuition here is that if a sequence has a special property or attribute, then it cannot be random, and randomness is equivalent to the complete lack of special attributes." [Dasgupta, 2011]

The precise mathematical formalization was essentially developed by Martin-Löf [Martin-Löf, 1966].

Incompressibility

Incompressibility is the third way for characterizing product-randomness. Some finite and infinite strings can be effectively specified by descriptions much shorter than the string itself. The idea then is that random sequences cannot be compressed at all. As Dasgupta states, *"A string is random if its shortest description has length equal to the length of the string itself"*. This approach is known as Kolmogorov Complexity [Kolmogorov, 1933] .

Additionally, it has been mathematically proved that these three approaches, that seem to be independent of each other, turn out to be equivalent! This is formally stated right away in theorem 3 p. 168. This surprising equivalence between randomness as unpredictability, randomness as typicality, and randomness as incompressibility, strongly suggests that this common equivalent definition is satisfactory.

Let's elaborate a bit more these definitions. It has been said that there must not be any predictable pattern in the sequence. This can be stated, in the case of infinite or finite binary sequences, as that *"for any string σ appearing in a sequence, it will as commonly be followed by a 1 as by a 0. This lack of predictability based on the previous elements of the sequence is necessary for genuine randomness."* [Eagle, 2012]. The same key point can be understood appealing to the impossibility of random systems being gambled (the von Mises approach [von Mises, 1957]); as Eagle comments:

> "If the sequence were really random, then [...] the values of any previous members of the sequence, and the place of the desired outcome in the sequence should be of no use to you in this task. To suppose otherwise is to suppose that there is an exploitable regularity in the random sequence; a gambler could, for example, bet reliably on their preferred outcome and be assured of a positive expected gain if they were in possession of this information. [...] A random sequence should not be such that any initial subsequence x_1, x_2, x_{k-1} provides information about the content of outcome x_k" [Eagle, 2012].

The approach of typicality, due mainly to Martin-Löf, takes as a central feature that random sequences are those which are not special in any effectively determinable way. And having something 'special' amounts to having any sort of "special hallmark", as for example, having a '1' every 13 places.

The third approach, of Kolmogorov complexity, focuses on the incompressibility of the sequences. Random sequences result to be impossible of being generated by a compact algorithm. Following [Eagle, 2012], a random sequence is one such that the shortest algorithm which produces it is approximately the same length as the sequence itself. Thus, randomness characterises the algorithmic or informational complexity of a sequence.

We can realize that Kolmogorov complexity fits well with the intuitions about disorderliness of the Martin-Löf account, as well as with the impossibility of gambling systems. It is the so called Schnorr's theorem which proves the striking result:

Theorem 3. ***Schnorr's Theorem.***
A sequence is Martin-Löf random if and only if it is prefix-free Kolmogorov random.

PRODUCT-ORDER With all these definitions in place, a comprehension of *order* of sequences can be directly deduced as the opposite of the definition of product-randomness. Therefore, an ordered sequence has to be *predictable*, *special*, or *compressible*. More specifically, only a "minimal" degree of predictability, specialness or compressibility is required, while the opposite concept requires total accomplishment of the opposite properties (i.e. absolute unpredictability, typicality and incompressibility).

A.3.1 *The Law of Large Numbers*

One of the main results of probability theory is the theorem called 'Law of large numbers' (LLN). It can be stated as (from [Dasgupta, 2011, section 3.2]):

Theorem 4. ***The Strong Law of Large Numbers, or Borel Strong Law.*** *For independent infinite sequences of flips of a fair coin, let B denote the event that the proportion of successes* S_n *among the first* n *flips,* $\frac{1}{n}S_n$ *, approaches the limit 1/2 as* $n \to \infty$.
Formally stated:

$$B := \left\{ x \in \{0,1\}^N : \lim_{n \to \infty} \frac{S_n}{n} = \frac{1}{2} \right\}$$

The probability of the event B is 1, i.e. the set B has Lebesgue measure 1.

This is the *strong* version of the law, but a *weak* version also exists, more adequate for real physical finite events. The latter permits a small difference between the expected mean value and the effective outcome. In fact, the weak version states that the sample average converges in probability towards the expected value. Following [Loeve, 1977], being each outcome X_i, the number of trials n, the sample average $\overline{X_n} = (X_1 + X_2 + ... + X_n)/n$, μ the expected value, and ε any positive number:

$$\lim_{n \to \infty} P\left(|\overline{X_n} - \mu| > \varepsilon\right) = 0$$

The weak version allows for a bit of the "freedom" that a finite random sequence is intuitively thought it will have. The freedom is quantified by the small quantity tolerated, ε. This version leaves open the possibility that $|\overline{X_n} - \mu| > \varepsilon$ happens an infinite number of times, although at infrequent intervals. Instead, in the strong version, for any $\varepsilon > 0$ the inequality holds for all large enough n [Sheldon, 2009]. In any case, $X_1, X_2, ...X_n$ are assumed to be an infinite sequence of independent and identically distributed integrable random variables with expected value $E(X1) = E(X2) = ... = \mu$.

A.3.2 *The order from randomness of the LLN*

An upshot that can be drawn from the LLN is that, in the long run, order arises. The expected value of a six faced die will converge towards an average value. No matter if the die is not fair and there is bias, the converging value will just be some other. The LLN can be seen as the mathematical, logical source of order arisen from process-randomness.
Perhaps, the *order* that we are recognizing around us and willing to explain might correspond to the *existence of an average whose value inevitably converges*. But how could this be translated to the physical realm?

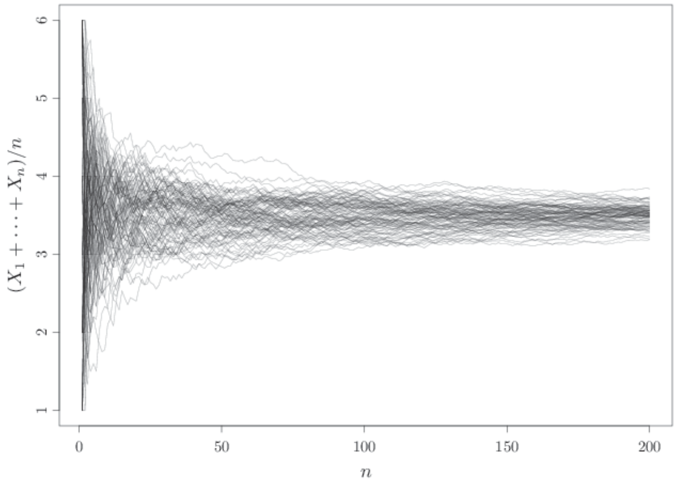


Figure 25: Average value of a six faced dice over an increasing number of tosses. The average converges to the expected value for all the trials depicted, as the LLN states.

The convergence of the LLN arises because what it is doing is the mean of all the accumulated values. Therefore, as more time passes, more difficult is to get away from the mean value [8].
The converging expected value is like an attractor in dynamical systems' theory (see e.g. [Pollicott and Weiss, 2004]). You will sooner or later approach it; for instance, in a die it is more probable to get values near the 3 than near the 6. Then, when you approach, it will *store this information*, as it takes into account each value occurred in the past and recollects the new one. Thus, you know that slowly it will store more and more information about having been near that point. And the average will approach more and more that expected value, *with probability one*. In the very definition of a mean it is implicit the existence of some sort of *memory*. Thus, every time it will be more difficult to escape from that value. In other words, in order to escape at a certain moment from approaching the expected value,

8 There are, however, sequences without convergence in frequency. E.g. a binary sequence whose outcome consists every time in a string of outputs with the inverse value as the string before, with the length of each new string increasing in proportion to the length of the already occurred global sequence. This creates a graphic for the expected value that oscillates regularly from one of the values to the other. But these counterexamples can be replied in many ways. The more straightforward is to say that these are just pathological cases that represent an extreme minority of all the possible random sequences, being then very unlikely. It can be also remarked the important fact that for not converging they must follow this precise pattern *always*, otherwise, if they fail, the convergence will be each time more difficult to avoid. And we must not forget that we are dealing with random sequences, not ordered ones, so these ordered sequences should appear (and hold forever) by mere hap. This is extremely unlikely. In any case, they are not a threat to our matters given that the output of the expected value, even if it does not converge, it presents a cyclic pattern, and this is even more in tune with what we are seeking, which is regular behavior.

the inputs ought to insistingly be far from the center to counterbalance the tendency imposed by the already occurred events; moreover these outcomes must all be "pressing" in the same direction (e.g. in the die all should be above the mean or all below) because otherwise they can balance one each other thus again converging to the mean (a 6 and a 1, though far from the mean, together converge to it).
In sum, the crucial factor for the system is to have something that amounts to the storage of past and present information, i.e. a mean. If the system has this memory, then it allows the Law of large numbers to be applied, so that the mean value will converge towards a fixed point.

A.3.3 *Impossibility of product-randomness?*

That being said, there are interesting theorems constraining the space of possibilities of a product-random sequence [Dasgupta, 2011, p. 15]:

> "randomness cannot be identified with complete and absolute lawlessness. We may try to think of a sequence to be random if it satisfies no law whatsoever ("absolutely lawless"). But, as pointed out by Calude in [Calude, 2000] (see also [Volchan, 2002]), no such sequence can exist, since every digit-sequence satisfies the following Ramsey-type combinatorial law first proved by [van der Waerden, 1927]: The positions (indices) for at least one digit-value will contain arbitrarily long arithmetic progressions. Thus we have to abandon such ideas of 'complete lawlessness'."

Some theorems of probability deny the possibility of absolute product-randomness. Do these results help our inquiry of understanding the emergence of order? The upshot is that they do *not* serve for the sought explanation of the onset of physical necessity. Basically because they refer to the product sense, not the process. On the contrary, these theorems could be taken as an objection, given that I will lay out, in the next section, an attempt to understand process-randomness in terms of product-randomness, and those theorems attack the possibility of the latter.
Specifically, I am going to present Van de Waerden's theorem. Confining again to binary sequences, the theorem indicates a nontrivial regularity shared by all sequences:

Theorem 5. ***Van der Waerden's Theorem.***
In every binary sequence at least one of the two symbols must occur in arithmetical progressions of every length.

As explained in [van der Waerden, 1927], the theorem states that for any given positive integers r and k, there is some number N such that if the integers [1, 2, ... , N] are colored, each with one of r different colors, then there are at least k integers in arithmetic progression all of the same color. The least such N is the Van der Waerden number W(r, k).

A clear explanation from [Graham, 2007] is the following. When r=2, you have two colors, say red and blue. W(2, 3) is bigger than 8, because you can color the integers from 1 to 8 like this:

1	2	3	4	5	6	7	8
B	R	R	B	B	R	R	B

Here, no three integers of the same color form an arithmetic progression. But now you cannot add a ninth integer to the end without creating such a progression. If you add a red 9, then the red 3, 6, and 9 are in arithmetic progression. Alternatively, if you add a blue 9, then the blue 1, 5, and 9 are in arithmetic progression. In fact, there is no way of coloring 1 through 9 without creating such a progression. Therefore, W(2, 3) is 9.

The proof of the theorem provides only an upper bound for the value of W(r,k). For the case of r = 2 and k = 3 the argument given below shows that it is sufficient to color the integers [1 ... 325] with two colors to guarantee there will be a single-colored arithmetic progression of length 3. But in fact, the bound of 325 is very loose; the minimum required number of integers is only 9. Any coloring of the integers [1, ..., 9] will have three evenly spaced integers of one color.

A.4 PROCESS-RANDOMNESS FROM ITS OUTCOMES, I.E. FROM PRODUCT-RANDOMNESS

With the knowledge attained about product-randomness, could we gain insight on the notion of process-randomness ? The first intuitive answer is 'yes', for a common thought is to identify both concepts: something is random (i.e. product-random) if and only if happens by chance (i.e. process-random). The identity can be more precisely stated, as in [Eagle, 2012, sec.3]:

> **Randomness Chance Thesis (RCT)**[9]:
>
> "An outcome happens by chance if and only if, were the trial which generated that outcome repeated often enough under the same conditions, we would obtain a random sequence including the outcome (or of which the outcome is a subsequence)".

That would be great for our research of process randomness, because in function of a good survey of the outputs we could conclude

9 'Revised Commonplace Thesis' is the meaning actually ascribed in [Eagle, 2012], but I change it for sake of clarity.

about the nature of the generator process. We would know what is empirically produced by the process-randomness that has been entertained and assumed along the whole dissertation. I am going to argue in favour of this thesis, that is, I am going to argue (only) for the left to right direction of the RCT biconditional.

However, it is rather straightforward to find apparent counterexamples for both directions of the biconditional, i.e. cases of product-randomness without chance involved and chancy outcomes without them being product-random.

Process-randomness leads to process-random outputs; yet, one of those possible outputs can be an ordered sequence, so it is not random in the product sense, but it would still be a random output as what regards its origin, its genesis.

I advance that my answer (as already commented along the thesis, in 3.2 and 4.4) is based on the fact that most of the trials of a process-random process will indeed produce a product-random sequence. Crucially, when appealing to the long run, the unlikely cases fade away, increasing the likeliness of the product-random outputs. Being the long run sufficiently long, the product-random outputs become overwhelmingly likely, so the "spurious" cases can be neglected.

In this respect, Laplace made a similar observation, relying on something that has been so important along the previous chapters, i.e. the proportion (measure) of ordered versus non-ordered sequences:

> " [...] the almost infinite number of combinations that can arise in a hundred throws are divided in *regular sequences*, or those in which we observe a *rule* that is easy to grasp, and in irregular sequences, that are incomparably more numerous." (italics are mine) [Laplace, 1826]

In the coming subsection the left to right direction of the RCT biconditional is defended. Independently, the final subsection briefly defines process-randomness from a different framework.

A.4.1 *Deviating from absolute randomness: bias*

I am going to dispute the failure of such conditional, that is, I am going to dispute that there are cases of a chancy (i.e. randomly generated) outcome but which, being the trial repeated often enough under the same conditions, we would *not* obtain a product-random sequence (including the chancy outcome).
More exactly, I am not going to deny that there are counterexamples, but that they are a negligible proportion, i.e. they form a set of measure 0.

To begin with, I am going to dispute that there are general types of cases in which there is chance without randomness. According to

[Eagle, 2012], there are compelling cases of chance without randomness; namely, those in which there is a biased chance process.

The counterexample of bias

Consider a sequence of unfair coin tosses; it will have an unbalanced number of heads and tails, so such a sequence is not random. However, such a sequence, and any particular outcome in that sequence, happens by chance [Eagle, 2012, sec. 4.3]. So it seems we are facing a problem for the RCT thesis.

A *biased* coin toss will have an *expected mean* that will converge to a number different that 1/2. The counterexample shows how product-randomness is incompatible with any bias. This is correct, for bias involves the denial of absolute typicality (since there will be special privileged outcomes in the sequence) and of absolute incompressibility (as compression algorithms exploit such biases[10]). As Earman stresses in [Earman, 1986, chap. VIII sec. 3], Kolmogorov randomness is conceptually linked to *disorderliness*, while on the other hand Eagle cites Dasgupta highlighting a link of biasedness with an increase in *orderliness* of a sequence. Therefore, it can hardly be the case that we have a biased product-randomness.
Likewise, it is incompatible with unpredictability, as it will be possible to gamble betting in favour of the biased zone.

Reply

However, this example of bias should not be taken as a counterexample to the RCT thesis. The point is to refuse the assumption that the process is chancy. In such case the antecedent of the conditional is not true, so the conditional is not false. Take the unfair coin toss. Take, for instance, that the coin used is biased because gamblers fabricated it with some particular asymmetric weight. We can grant that, paying attention to this process that generates the outcome —not in the resulting sequence— we can hardly admit that the process is *absolutely* chancy. Thus it disappears as a counterexample of a chancy but not product-random case.

Other threats of chance without product-randomness

There are other possible threats to the left to right direction of RCT; let me make a brief survey of them. Following [Eagle, 2012], they are:
1) unrepresentative outcome sequences [*ibidem* 4.1],
2) the reference class problem [*ibidem* 4.2],
3) the dependence on history [*ibidem* 4.4], and
4) pseudorandom sequences [*ibidem* 4.5].

All are rather innocuous. Unrepresentative outcome sequences are convincingly solved in [*ibidem* 4.1]. The reference class is indeed a problem, but a problem of chance in general, that should not be seen as a specific threat to RCT.
The objection of pseudorandom sequences is just an error in the se-

10 The well-known softwares 'Zip' and 'Rar', as well as jpeg and mpeg image and video compression formats, do exactly this: they exploit the biased resulting outcome in the same way that they exploit the regions with patterns in ordered sequences.

quences chosen —so that they are not really random.
Finally, dependence on history is a contextualization to the surrounding setup purported to hold for chancy processes (and not to product-random sequences). But this is just a way of interpreting what is part of the process and what is not, so it is only relative to intuitions that this can be harmful, so the few examples presented are utterly missing the point. Indeed, all the setup could be included in what the random process is, thus avoiding any alleged problem —this is the same sort of move as that made in the discussion of deterministic causation, specifically in Mackie's account of INUS conditions, when one solves some counterexamples just by taking into consideration the whole universe (or the past light cone).

inferring process-randomness from the outputs After the defense of the left to right conditional of the RCT thesis from specific counterexamples, let me now spell out the association of the generation of a process-random process with a product-random sequence. Because, even if the several objections of [Eagle, 2012, sec. 4] have been answered, there still remains the fact that a random process should, by definition, be able to produce any resulting sequence, product-random or not. So, as said at the beginning of the section, let me lay out why, even if this is true, it is nonetheless much more likely that the resulting sequence will be product-random.

It has been never doubted that the right to left direction of the biconditional fails. This means that just by looking at a single resulting sequence we cannot conclude that the generator process is process-random. It just cannot be identified like that. Yet, we can do better in comprehending process-randomness. Starting by the correct intuition of this fragment I shall elaborate an elucidation of such notion:

> "the main feature of randomness is some degree of independence from the initial conditions. (...) Better still, if one performs the same experiment twice with the same initial conditions, one *may* get two different outcomes" [Ekeland, 1988, 49, italics added].

Following this quote, a random process can be defined as that process that *can* have *any* possible outcome at any trial.
So the first feature of a random process is the unrestricted theoretical space of potential outcomes. It is *not* sufficient to look at the actual outcomes, because *anything* can be the result of a random process, so it cannot be empirically verified by looking at a single outcome. This absolute lack of constraint, this "total freedom" *of the outcomes* of a random process, is its defining characteristic. I have italicized the 'may' in Ekeland's quote because the modal content of the claim is crucial in the definition of process-randomness.

That being said, we could pay attention to various sequences ("products") generated by the same process: which relations they show may suggest that the process is process-random. We can often have sequences that are not product-random, that display some pattern — they are not typical, not incompressible, not unpredictable. As said, a random process can perfectly give rise to a very biased sequence. But the crucial point is to be somehow able to infer that they are an exception, a minority in the space of possible sequences. To this end, remember that I said in 4.4 that the random-looking trajectories are the overwhelming majority of the dynamics-space, i.e. their measure is 1. This is what I wrote there as $\mu(\overline{C}) << \mu(C)$ or, equivalently: $\mu(C) = 1$, where C was the set of random-looking (actually the 'chaotic') trajectories.
So we would need to be able to repeat the process of generation of sequences in a fixed setup. The underlying idea is to exploit the convergence theorems that hold for random sequences. So, we should try to apply the same idea not to a single randomly generated sequence, but to a sequence of sequences, in a sort of "second order" LLN. Thus we should be able to infer that the process that generated such sequences was probably process-random.

The LLN demands i.i.d. variables, but what here I propose does not require a prior identical distribution among the variables. The outputs of the random process are not fruit of a stochastic dynamics, so they do not have any probabilistic distribution endowed —not a uniform distribution either.

The proposal consists in summing the occurrences of each of the possible results[11] of all the sequences $S_i[1..l]$ of size $l \to \infty$ (infinite, or very long) generated by a generative process $\mathcal{X}$. In the long run, i.e. after an infinite (or very high) number of trials $n \to \infty$, we can check the global distribution of the results of all the sequences, gathered in a l-dimensional array $\mathcal{S}[1..l] = \sum_{i=1}^{n} S_i[1..l]$, and infer that:

PROC-FROM-PROD:
'It is overwhelmingly likely that if $\mathcal{S}$ is not approximately equidistributed among all the possible outcomes, then the generator $\mathcal{X}$ is not a process-random generator'.

Intuitively, this strategy of employing this sort of second order LLN (as it averages various sequences together) is a way for washing out the disturbing counterexamples. *If* it is true that the not product-random sequences/trajectories in the dynamics-space are of a neglectable size, then the statement PROC-FROM-PROD should be true. How could PROC-FROM-PROD be false? If no option has any preference in being chosen —which does not mean that I am setting an identical distribution to each possibility, but that I am not setting a

11 Like a histogram, as those of 5.4.

preferred *fixed* expectation to any specific possibility—, then there is no reason to think that *in the long run* there will be a substantive preference for certain possibilities after averaging a high enough number of trials.

Grant that the measuring step is not controversial[12]. The measure says that the product random generated sequences are much more numerous. Then, from 'more numerous' we step to 'more likely'. This intuitive move, though, is not necessarily true (and this is a problem that pervades most of the philosophical problems discussed along this thesis). Here it could be justified thus: without involving objective probabilities, one can say that the subjective probability of having an approximately uniform distribution must be much higher. That is, if you do not bet much more money on that type of outcome you would be behaving irrationally. Therefore, it must be expected that they will appear more often. This argument is relying on an attachment to the ideal credences of an agent, thus to something in principle with all the earmarks of being subjective. Even so, it infers something about the objective world. The conclusion is stating something objective about the world in a similar way as an account of randomness does in terms of an ideal unpredictability, in spite of the subjective flavor of the concept of unpredictability.

Summing up, realize that PROC-FROM-PROD has the logical form of $(\neg\alpha \to \neg\beta)$, where α is '$\mathcal{S}$ is approximately distributed', and β is '$\mathcal{X}$ is a process-random generator'. More accurately, the claim PROC-FROM-PROD refers to most of the cases, which I have phrased in terms of likeliness. So the claim has the logical form:

PROC-FROM-PROD: $P(\neg\alpha \to \neg\beta) > 1 - \epsilon$

where ϵ is a very small positive real number. This is logically equiva-

12 But it is controversial. To what extent is difficult to assess. The problem comes in the choice of the type of measure. As it has been remarked elsewhere ([Handfield, 2012, 91], or [Sklar, 1993, sec. 4]), there are infinite measures to choose. One might hope to choose the most "natural" measure, and argue for it. Such candidate is the Lebesgue measure, for both its empirical success as well as its theoretical virtues. But some do not find it fully justified. See [Sklar, 1993, sec. 4] for a more fair explanation of the problems around it. See also this concise fragment of [Handfield, 2012, 94]:

> "There is one measure, known as the Lebesgue measure, which does not flout any of the physical constraints we might like to impose on the choice of measure, and which is in some sense 'uniform'. It treats all dimensions of phase space equally, in a manner of speaking. But even so, this is scant reason to be confident that it is the correct measure. Can we really be so confident that the correct measure is uniform? Recall that the different dimensions of phase space track completely different properties. Half of the dimensions relate to velocity, and half relate to position. Why should we expect these dimensions to contribute to the correct measure in the same way? These questions should make us very uncomfortable about boldly picking a measure without any independent basis to verify that choice".

lent to:

PROC-FROM-PROD: $P(\beta \to \alpha) > 1 - \epsilon$

Which is to say that: for the vast majority of cases, if $\mathcal{X}$ is a truly process-random generator, then the resulting sum of an infinite (or very high) number of sequences $\mathcal{S}$ will be approximately equidistributed among all the possible outcomes —i.e. it will be approximately product-random.

A.4.2 *Process-randomness from Ergodicity*

Having defended the relation between process and product-randomness in the previous subsections, now I lay down an approach to the notion of process-randomness with the same conclusion but from a different framework: from the so called Ergodic Hierarchy. This subsection is aimed to complement the outline carried out in section 4.4.

The Ergodic Hierarchy allows to understand randomness in deterministic dynamical systems. What is the notion of randomness in such context is something that seek to clarify [Berkovitz et al., 2006], [Frigg et al., 2011], and [Eagle, 2012].

The Ergodic Hierarchy (EH) consists of the following classification of dynamical properties of a deterministic dynamical system:

Bernoulli $\subset$ Kolmogorov $\subset$ Strong Mixing $\subset$ Weak Mixing $\subset$ Ergodic

A central claim defended in these references is that the different levels of the hierarchy correspond to *different degrees of unpredictability*, which in turn correspond to different patterns of *decay of correlations between their past states and present states*. This is how randomness is identified. Process-randomness is to be understood as unpredictability, coinciding with what has been defended by [Eagle, 2005] and with Von Mises account, as explained before (A.3). Unpredictability should not be understood in the sense of an epistemic limitation, but in the sense of the ideal impossibility of prediction.

Within this framework, randomness is a matter of degree, whose degree is quantified by the hierarchy. In its highest degree randomness is thus associated with the Bernoulli level, in which there is a total lack of correlation between the present state with the past states. The lower the degree, the more the correlations arise, yielding a weaker notion of randomness.

As I argue in 4.4, p. 114, this definition of randomness easily conveys the intuitive thought that chaotic trajectories, even if deterministic, share some relevant properties with randomly generated trajectories. As said, the highest degree of randomness, the Bernoulli level, includes the properties of what is usually meant to be a chaotic system, identified by displaying the degree of mixing. Hence, a random system includes the properties of a chaotic system. Therefore, if we get some results in virtue of the properties of chaotic systems, we can get the same results with a truly random system, which will also possess those properties.

It is objected that the Ergodic Hierarchy is irrelevant because real physical systems are not ergodic. But [Frigg et al., 2011] replies against this charge, in the same vein that I have been defending many of my claims throughout the dissertation, so I think it is worth reading:

> "Almost all Hamiltonian systems are non-integrable (in the sense that non-integrable systems are of second Baire category in the class of all normalised and infinitely differentiable Hamiltonians) and therefore in large regions of their phase space the motion is random in various ways well captured by EH. Further, as [Werndl, 2009b] argues, EH could also be used to characterize randomness and chaos in dissipative systems. So EH is a useful tool to study the dynamical properties of various ergodic and non-ergodic systems".

B

ANNEX. HISTORICAL ANTECEDENTS: PEIRCE'S EVOLUTIONARY METAPHYSICS

To provide a wider perspective of the proposal I have been investigating, this section sketches some similarities of the metaphysics of the American pragmatist Charles Sander Peirce (1839–1914).

Precursors in ancient Greece

First of all, as in almost all other branches of philosophy, some ancient greek philosophers deserve their recognition as the pioneers. The first philosopher to propose an emergence of order from a chancy world is, as far as is known, Leucippus (V century B.C.), within his also pioneering theory of atomism. Leucippus' guiding chance was later recovered by Epicurus and the epicurean tradition, in what they called 'fortuna gubernans'. Fragments of this philosophy can be found in Lucretius' "De Rerum Natura" [Lucrezio, Ist c. BC, 5.107].
It is interesting to note to what extent this hypothesis endured: just after Leucippus, his pupil Democritus embraced his master's atomism, but he displaced the presence of chance by that of necessity.
The proposal of a fundamental underlying order rather than chance is prior to Democritus and Leucippus; indeed, it is what some establish to represent the very beginning of Philosophy after the theistic cosmogonies. The postulation of a principle of order, *Arché* (Ἀρχή), is a distinctive sign of all presocratic philosophy, from Thales of Miletus onwards.

Be it as it may, here I center on Charles Sanders Peirce's elaborated metaphysics, dubbed as 'evolutionary cosmology'. I am deliberately going to select only the parts more in tune with my account, leaving aside the more idealistic elements of his thought. The bibliography upon which I base my survey is mainly: the article 'The Complementary Roles of Chance and Lawlike Elements in Peirce's Evolutionary Cosmology' by Fred Kronz and Amy McLaughlin [Kronz and McLaughlin, 2002] (from [Atmanspacher and Bishop, 2002]), the last chapter of Ian Hacking's 'The taming of chance' Hacking [1990] which briefly surveys Peirce's metaphysics, and the more extensive study of the book 'Peirce's Scientific Metaphysics' [Reynolds, 2002]. The relevant works of Peirce can be found in [Peirce, 1867–1893].

Fundamental randomness

Peirce's metaphysics is clearly reminiscent of the account proposed in this thesis, with some substantial differences. According to Peirce, the world is fundamentally random. Regarding the laws of nature, none of them is absolutely exact, and *they have evolved from a random*

original primeval state.
He was strongly inspired by the then recent evolutionary theories in biology. With the passing of time, Peirce believes, there is an increased tendency from less to more order and more complexity.
In his evolutionary cosmology, a crucial element is the role of probability; specifically, the law of large numbers. [Reynolds, 2002, 3] defends that the law of large numbers is the architectonic principle that ties together the phenomena of cosmic evolution and indeterminism. Another central methodological belief in agreement with this thesis is what Peirce called the 'first rule of inquiry'. As Reynolds states:

> "The first rule of inquiry requires that no fact be accepted as brute or inexplicable. This, as we saw, led Peirce to require explanations of laws themselves"
>
> "In Peirce's opinion, it is regularity above all else that requires an explanation. Using a series of coin tosses to make his point, he notes that if we observe no regularity in the outcomes, we feel no need for an explanation. If, however, we observe all heads, we do find this peculiar and seek an explanation. "Law is par excellence the thing that wants a reason" (Peirce 6.12). Conversely, irregularity does not require an explanation, and because Peirce identifies irregularity with chance, the starting point of his explanation of the order and regularity in the universe is chance". [Reynolds, 2002, p.16].

The reader will remind that the same sort of methodological consideration has been remarked at length previously, in chapter 1, esp. 1.3, about what is to be explained —laws of the lack of them, symmetry or asymmetry. Further:

> "because regularity is the thing par excellence that requires an explanation, Peirce maintained that it was legitimate for him to suppose in the beginning a chaos. Irregular chance is the one thing that does not require an explanation, neither does this primordial state of unorganized and nonpersonalized feeling". [Reynolds, 2002, p.116]

A primitive direction of the cosmos towards higher order

A controversial trait of Peirce's metaphysics is the existence of *a direction* of the cosmos' evolution towards an increasing degree of order —thus, a primitive temporal asymmetry. There is an evolving and end-directed universal trend toward what Peirce named the cosmological "growth of reasonableness".

More specifically, [Kronz and McLaughlin, 2002] concisely summarizes Peirce's evolutionary cosmology, making clear that the evolution of the universe is due to a *tendency* of entities to take habits. This tendency is the responsible of the formation of order from lawlessness.

This is the key underlying concept. But it is immediate to question what is supposed to be this tendency and where it comes from — especially: is it not a law?
Furthermore, it seems, *prima facie*, that recurring to a 'tendency' is in harmony with the parlance of dispositions and propensities, discussed in chapter 1.4.
Even so, this sort of approach, as I argued in 1.4 regarding dispositions, is at least an incomplete explanans. So why there is this tendency would still remain a mystery.
Peirce, however, proposed some insights for justifying this tendency, closer to a process metaphysics than to a traditional substance metaphysics:

> "Once you have embraced the principle of continuity, no kind of explanation of things will satisfy you except that they grew. The infallibilist naturally thinks that everything always was substantially as it is now. Laws at any rate being absolute could not grow. They either always were, or they sprang instantaneously into being by a sudden fiat like the drill of a company of soldiers. This makes the laws of nature absolutely blind and inexplicable. Their why and wherefore can't be asked. This absolutely blocks the road of inquiry. The fallibilist won't do this". [Peirce, 1867–1893, 1.175]

Likewise:

> "from mere non-law nothing necessarily follows, and therefore nothing can be explained; for to explain a fact is to show that it is a necessary or, at least, a probable result from another fact, known or supposed" [Peirce, 1867–1893, 6.606]

What is to be explained? Order or chaos?

As the fragments above show, I want to underwrite that Peirce's approach reverses the usual direction of explanation: traditionally it is from symmetry (of laws, and maybe also of I.C.) to asymmetry, whereas now it is proposed a process that goes from asymmetry to symmetry. This reverse approach, the reader will remind, underpins the whole dissertation, and in fact it has been explicitly defended in chapters 1 and 2.

There is a tendency toward increasingly more regular behavior, which Peirce compares to the formation of habits in an organism — inspired as he was by the evolution of species in biology.

The only law, the 'law of habit'

There is an argument in support of the tendency of taking habits, and about how the increase of this tendency is produced by the tendency itself. It can be shown in this illustrative fragment:

> "The state of things in the infinite past is chaos, tohu bohu, the nothingness of which consists in the total absence of regularity. The state of things in the infinite future is death, the nothingness of which consists in the complete triumph of law and

> absence of all spontaneity. Between these, we have on our side a state of things in which there is some absolute spontaneity counter to all law, and some degree of conformity to law, which is constantly on the increase owing to the growth of habit. The tendency to form habits or tendency to generalize, is something which **grows by its own action, by the habit of taking habits itself growing**. Its first germs arose from pure chance." (Peirce 8.317; bold added)

Peirce speaks of a first *seed* of an habit-forming tendency. This tendency toward regularity or habit could arise, Peirce proposes, by chance alone.

The law of habit is a self-organizing tendency, destined to be reinforced by acting on itself in an autocatalytic fashion —like autocatalytic systems of chemical reactions create a self-sustaining feedback loop (on the notion of auto-catalysis in complex systems and in biology, see respectively [Haken, 2008], [Dawkins and Wong, 2005]).
Here, an immediate worry is about how a tendency can affect itself. But the *actual* cases of auto-catalysis are the best answer. As well as autocatalytic chemical reactions do occur and persist —and they are surely surprising phenomena, but they exist and are well-known indeed—, and as well as certain well studied phenomena emerge, likewise some minimal constraints can be proposed to suit for the emergence and persistence of regularities from a process-random dynamics.
It might be illuminating to recur to the analogy with biology, specifically with the persistence of life through natural selection, and the importance therein of *heredity*. Richard Dawkins stressed this point:

> "Theories of the origin of life need to account for both heredity and metabolism, but some writers have mistaken the priority. They have sought a theory of metabolism's spontaneous origin, and somehow hoped that heredity would follow, like other useful devices. But heredity, as we shall see, is not to be thought of as a useful device. Heredity has to be first on the scene because, before heredity, usefulness itself had no meaning. Without heredity, and hence natural selection, there would have been nothing to be useful for. The very idea of usefulness cannot begin until the natural selection of hereditary information does." [Dawkins and Wong, 2005, p.468]

Then, further metaphysical assumptions should be sought, in order to justify how a process of "heredity" —i.e. a memory— might occur and persist (cf. A.3.1 for reflections on the need of a memory for the obtaining of the mean in the Law of Large Numbers).

Then, following Peirce's metaphysics (now different from the dialectics that pervaded the chapters of this thesis), these *chancy regularities will become self-reinforced*, ultimately achieving what we nowadays take to be 'necessary' behavior. That is, it won't be strictly necessary behavior, it is just that from a chancy origin, some phenomena bolstered their own repetition, producing in the long run a stronger tendency to repeat themselves. This would then signify a progressive increase in their *degree of necessity*, in that each time will be more difficult to run against their occurrence.

Such a tendency is just one of many that had arisen from the original chaos by sheer chance. The habit-taking tendency, once arisen by chance, is destined to become ever stronger and to result in the kind of systematic world in which we find ourselves.

C

ANNEX. MATLAB ALGORITHM OF CHAPTER 5

The following algorithm does not include the creation of graphics and statistics. The comments within the code self-explain the most complex parts.

```
%% Based on 'Billards2d.m' from Mathworks.com
% IMPORTANT VARIABLES:
% X(i,j) = matrix with the coordinates x and y of the particles
% V(i,j) = matrix with the info of velocities x and y of all the particles
% Bound=[-4 4 -2 2];  %% The limits of the container, from -4 to +4 in horizontal, and -2 to +2 in the vertical axis.
% THE VARIABLE THAT ALLOWS TO VARIATE THE EFFECT OF THE COLLISIONS:
theta=0; % The range of the value is [0..360]. Each increment has the precision of a double. Each variation of theta yields a different post-collision direction. It could be changed automatically, but for ease of simplicity here it's changed manually each turn. So each run of the simulation with a different theta amounts to a variation of the actual newtonian collisions.
%There is also the interesting case of theta=rand() in which it changes randomly for *each* collision each turn, resembling more a random dynamics.

% GOOD VALUES FOR THE FIRST TRIALS IN MY LAPTOP: With graphical simulations: 80 balls of r=0.03. O 20/30 balls de r=0.1 O 5 bolas de r=0.3
% Without graphical simulation: 2000 balls, r=0.0001
NumberOfBalls=2000;

%%%%%%%%%%%% PRELIMINARY SETUP %%%%%%%%%%%%%%%%
close all;
%hold on;
drawflag=1;
factor=82; %Scaling factor; for fullscreen should be adjusted.
DT=2e-2;  % time differential
Bound=[-4 4 -2 2];
Color=[0 1 0];  % Green color for the particles
TableColour=[.4 .5 .8];
%Plothandle
axis(Bound);
set(gcf,'color',[1 1 1]);
set(gca,'Color',TableColour,'xcolor',TableColour,'ycolor',...
TableColour,'PlotBoxAspectRatio',[1 abs((Bound(3)-Bound(4))/(Bound(2)-Bound(1))) 1],'xtick',[],'ytick',[])
```

```
%Set the radii, hence the dimensions of the molecules
%r=0.25+0.25*rand(NumberOfBalls,1); % Random value of the particles, but between [0.25..0.5]
r(1:NumberOfBalls)=0.03; % Radius of molecules. CHANGE FOR BETTER PERFORMANCE
r=r'; % Traspon
% Storage of the sum of radii of each pair of particles (fix for my simulations)
for j=1:NumberOfBalls;
   for i=1:j,
       rmatrix(i,j)=r(j)+r(i);
   end;
end;
rmatrix=rmatrix.*triu(abs(-1+eye(size(rmatrix))));
%Mass
mass=pi*r.^2;
%OffsetIC=1:NumberOfBalls; % Offset for allocating the particles wherever
%OffsetICCol=OffsetIC'; % column format

%%% Initial position of particles: Random
X=[(Bound(2)-Bound(1)-2*max(r))*rand(NumberOfBalls,1) + Bound(1)+max(r),...
   (Bound(4)-Bound(3)-2*max(r))*rand(NumberOfBalls,1) + Bound(3)+max(r)];    % posicion inicial 'x' e 'y'
% Move all the particles into the left down corner
X=-abs(X);
%%% Other options for checking it works: Initial position symmetric (for two particles): e.g. for X1=(-1,-1) y X2=(1,-1). Would be: %X=[[-1,-1] ; [1,-1]];
%X=[[-1,0] ; [1,1]];

% Calculate distance among particles
for j=1:NumberOfBalls;
   for i=1:j;
       % This matrix stores the distance between i and j particles
      distmatrix(i,j)=sqrt((X(j,1)-X(i,1))^2+(X(j,2)-X(i,2))^2);
   end;
end;
%Initial Collisiondetectionmatrix
% The matrix 'CollisionMatrix' sets the initial distances between borders of particles
CollisionMatrix=(distmatrix-rmatrix)+tril(abs(-1+eye(size(distmatrix))))+eye(size(distmatrix));
while find(CollisionMatrix<=0);
    X=[(Bound(2)-Bound(1)-2*max(r))*rand(NumberOfBalls,1) + Bound(1)+max(r),...
   (Bound(4)-Bound(3)-2*max(r))*rand(NumberOfBalls,1) + Bound(3)+max(r)];    % X stores the coordinates 'x' and 'y' of each particle
    X=-abs(X);
   for j=1:NumberOfBalls;
      for i=1:j;
         distmatrix(i,j)=sqrt((X(j,1)-X(i,1))^2+(X(j,2)-X(i,2))^2);
      end;
```

```
    end;
    CollisionMatrix=(distmatrix-rmatrix)+tril(abs(-1+eye(size(distmatrix))))+eye(size(
        distmatrix));
end

%Initial velocities
V=2*(-1+2*rand(NumberOfBalls,2));  %% V stores v_x e v_y of each particle
%V=ones(NumberOfBalls,2); %% Set all at the same velocity
% Other values tried:
%V=[[0.6,0.6];[-0.8,0.9]];
%V=[[16,16];[-16,16]];  % For symmetric collisions of 2 particles.

%Plot starting positions:
for k=1:NumberOfBalls;
    plot(X(k,1),X(k,2),'o','MarkerEdgeColor',Color,...
    'MarkerFaceColor',Color,'MarkerSize',factor*r(k));
end
drawnow;

%%%%%%%%%%% HERE STARTS THE MAIN LOOP: %%%%%%%%%%%%%%

while drawflag==1;
    cla; % Clear screen
    %Case Edgedetecton positive
    % 'd' is a matrix with the info of distance (just adds the radius):
    d=X+repmat(r,1,2)-repmat([Bound(2),Bound(4)],NumberOfBalls,1);
    dt=(d>=0).*d./V;  % t=x/v
    X=X-V.*dt; % Calculate new position
    V=V.*(2*(d>=0==0)-1);
    %Case Edgedetecton negative: IDEM. Only the limits of the axis change, now the
        negatives and d<=0
    d=X-repmat(r,1,2)-repmat([Bound(1),Bound(3)],NumberOfBalls,1);
    dt=(d<=0).*d./V;
    X=X-V.*dt;
    V=V.*(2*(d<=0==0)-1);
    %Calculate new distances:
    for j=1:NumberOfBalls;
        for i=1:j;
            distmatrix(i,j)=sqrt((X(j,1)-X(i,1))^2+(X(j,2)-X(i,2))^2);
        end;
    end;
    %Correction with the radii:
    CollisionMatrix=(distmatrix-rmatrix)+tril(abs(-1+eye(size(distmatrix))))+eye(size(
        distmatrix));
    %=======================================
    % HERE CALCULATE WHAT IS GONNA HAPPEN FOR A PARTICULAR COLLISION
```

```
if find(CollisionMatrix<0); % If there's a pair of particles with distance 0, then (in
     rounds when there are no collisions, it does not enter here)
   [I,J]=find(CollisionMatrix<0); % I & J have the index of the particles A and B in
       collision (p. ex. '2' and '5')
   for i=1:length(I)
      % Normalized distance to 1 between particles I and J. normdist(1) corresponds to
           'x' component, normdist(2) to 'y' component.
      normdist=normr([X(I(i),1)-X(J(i),1) X(I(i),2)-X(J(i),2)]);
      %Velocity component along the line connecting the centres of ball A and ball B:
      vaA=(V(I(i),1)*normdist(1)+V(I(i),2)*normdist(2)); % Suma de vectores
          componentes x e y
      vaB=(V(J(i),1)*normdist(1)+V(J(i),2)*normdist(2));
      dt=abs(r(I(i))+r(J(i))-distmatrix(I(i),J(i)))/(abs(vaA)+abs(vaB)); % t=x/v
      %Set back the positions of the colliding balls====================
      %X(I(i):J(i),:)=X(I(i):J(i),:)-V(I(i):J(i),:)*dt;
   end
   for i=1:length(I) % This second bucle calculates the velocities post-collision
      normdist=normr([X(I(i),1)-X(J(i),1) X(I(i),2)-X(J(i),2)]);
      %Coordinate transformation matrix
      % M is based on normdist
      M=[normdist(1) -normdist(2);
      normdist(2)  normdist(1)];
      v_old=[V(I(i),:)';V(J(i),:)'];

      %Calculate velocity in the new coordinate system
      v_new=[M' zeros(2);zeros(2) M']*v_old;

      f=(1+mass(I(i))/mass(J(i)));  % I make mass fixed, so always f=2
      g=(1+mass(J(i))/mass(I(i)));
      %f=(1+mass(I(i))/mass(J(i)))*60*rand;  % RANDOM VALUE EACH TIME. THUS THE
          INTENSITY OF POST-COLLISION VELOCITY IS DIFFERENT
      %g=(1+mass(J(i))/mass(I(i)))*60*rand; % *30*rand; works well

      %%%%%%%  THE COLLISION IS DETERMINED HERE : %%%%%%%%%
      CollisionEffectMatrix=[0,0,1,0; 0,1,0,0; 1,0,0,0; 0,0,0,1;]; % THE  ORIGINAL
          MATRIX.
      % VARIATIONS THAT ALLOW FOR DIFFERENT COLLISIONS (though the variation is
          finally introduced after this matrix, so I maintain the original matrix)
      %CollisionEffectMatrix=CollisionEffectMatrix*0.6 ; % Reduction (or increase if I
           will) of speed intensity after collision
      %CollisionEffectMatrix=-CollisionEffectMatrix; % negative sign (interesting
          result)
      %CollisionEffectMatrix=[0,0,1.3,0; 0,1.1,0,0; 0.2,0,0,0; 0,0,0,1;];
      %CollisionEffectMatrix=[0,0.7,sind(theta),0; 0,sind(theta),0,0; sind(theta)
          ,0,0.7,0; 0,0,0,sind(theta);]; % VALUES WITH DIFFERENT COLLISIONS!  (Good
          but diverges to infinite velocities, because momentum is not conserved!)
```

```
      %CollisionEffectMatrix=[0,sind(theta),1,0; 0,1,0,0; 1,0,sind(theta),0; 0,0,0,1;]; %% This only changes Vx between [-2Vx..+2Vx].
      %% Then change Vy with a cos(theta) so velocity does not diverge:
      %CollisionEffectMatrix=[0,sind(theta),1,0; 0,1,cosd(theta),0; 1,0,sind(theta),0; 0,0,cosd(theta),1;];
      %%CollisionEffectMatrix=[0,sind(theta),0,0; 0,0,cosd(theta),0; 0,0,sind(theta),0; 0,0,cosd(theta),0;];
      %%CollisionEffectMatrix=[0,0,sind(theta),0; 0,cosd(theta),0,0; 1,0,0,0; 0,0,0,1;] ;
      %%CollisionEffectMatrix=[0,cosd(theta),sind(theta),0; 0,sind(theta),0,0; sind(theta),0,cosd(theta),0; 0,0,0,sind(theta);];

      v_new_col=CollisionEffectMatrix*v_new ; % final velocity post-collision

      %Put the velocities in the old coordinate system
      V(I(i),:)=M*v_new_col(1:2); % Put it back in variable V
      V(J(i),:)=M*v_new_col(3:end);

              %%%%% CHANGE IN THE POST-COLLISION, IN FUNCTION OF ARBITRARY VALUE THETA (not changing intensity):
              % THETA ranges between [0..360]
      %% The value of theta is added to a new angle beta=alfa+theta (where alfa is the old angle).
      alfa=abs(asin(V(1,2)/sqrt(V(1,1)^2+V(1,2)^2))) ; % In radians. v_new_col(2) corresponds to v_y
      alfa=rad2deg(alfa); % Change to degrees
      beta=alfa-theta;
      %%% Uncomment this for the version that assigns a random value to theta at each iteration
      % theta=rand()*360; % Random value constrained between 0 and 360
      V(1,1)=sign(V(1,1))*abs(cosd(beta));
      V(1,2)=sign(V(1,2))*abs(sind(beta));
      % IDEM FOR PARTICLE 2
      alfa=abs(asin(V(2,2)/sqrt(V(2,1)^2 + V(2,2)^2))); %In radians. v_new_col(4) corresponds to v_y of particle 2
      alfa=rad2deg(alfa);
      beta=alfa-theta;
      V(2,1)=sign(V(2,1))*abs(cosd(beta));
      V(2,2)=sign(V(2,2))*abs(sind(beta));
      %V

      %Update the positions after collision
      X(I(i),:)=X(I(i),:)+V(I(i),:)*dt;
      X(J(i),:)=X(J(i),:)+V(J(i),:)*dt;
   end
end
%Propagation. Free motion of particles when there are no collisions
```

```
    X=X+V*DT;
    % Plot the new turn
    for k=1:NumberOfBalls;
        plot(X(k,1),X(k,2),'o','MarkerEdgeColor',Color,...
        'MarkerFaceColor',Color,'MarkerSize',factor*r(k));
    end
    drawnow;
    %drawflag=0;
end
    close all;
```

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