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On Separating the Wheat from the Chaff: Surplus Structure and Artifacts in Scientific Theories

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Abstract

Although logical empiricism is now mostly decried, their naturalist claim that a theory's content can be read off from its structure, with no philosophical considerations needed, still supports many strategies to escape cases of underdetermination. The appeal to theoretical equivalence or to theoretical virtues, for instance, assume that there is a neutral standpoint from which the structure of the theories can be analyzed, the physically relevant separated from the superfluous, and a comparison made between their theoretical content and virtues. In my dissertation, I argue that the methodological principle underlying these strategies, according to which theories with no superfluous structure should be preferred, is unpractical, for what constitutes relevant structure is determined by epistemic considerations about the aim of scientific theories.

In chapter 2, I analyze the claim that theories with ordinary bosons and fermions are theoretically equivalent to theories with exotic 'paraparticles'. I argue that this claim does not do justice to the latter, as the proof is formulated in a vocabulary parochial to the former and thus favors it while giving an impoverished version of the second.

In chapter 3, I assess the argument that any interpretation of Quantum Mechanics offering a no-go theorem against paraparticles possesses an explanatory advantage over other interpretations and should, as such, be favored over others. Given that most physicists consider paraparticles as surplus structure whose non-observation does not require an explanation, I evaluate arguments of both sides and suggest a third way to approach the question.

Chapter 4 focuses on methods for excluding another kind of unphysical structure, numerical artifacts. Simulations are the only window into what rival dark matter models predict about the universe's structure. But for them to play a useful role in generating knowledge, we need to distinguish reliably between real predictions and artifacts. I argue that robustness analysis fails to fulfill this task and propose in its place another methodology, that of crucial simulations.

Keywords

Surplus Structure, Artifacts, Theoretical Equivalence, Theoretical Virtues, Robustness Analysis, Paraparticles, Simulations, Crucial Experiments, Dark Matter

Summary for Lay Audience

The problem of underdetermination, i.e., the problem of choosing between scientific theories that differ in the picture of the world they provide but make the same predictions, is especially salient nowadays. As no new physics has been discovered in the most recent runs of the Large Hadron Collider, there is no hint to where physics should go next. New data that could hint at new phenomena and guide the development of new theories are either absent or extremely difficult to collect. As a result, rival theories or models have been developed that fit the known data equally well, and without any clear sense of whether empirical evidence could be found in a few years to discriminate among them. Hence, many physicists have turned to strategies previously discussed by philosophers to privilege their theory over another, such as the appeal to theoretical virtues simplicity or explanatory power for instance.Yet, many traditional strategies to escape cases of underdetermination rely on the unscrutinized claim that there is a neutral standpoint from which the structure of rival theories can be analyzed, the relevant separated from the superfluous, and a comparison made between their theoretical content and virtues. In my dissertation, I examine the presuppositions upon which such strategies depend. I argue that the methodological principle underlying them, according to which theories with no superfluous structure should be preferred, is unpractical, for what constitutes relevant structure is determined by epistemic considerations about the aim of scientific theories.

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Ne pas rester un peu, pas même auprès du mieux connu,
voici notre partage; des images remplies,
l'esprit se jette en d'autres qu'il faut emplir soudain; les lacs
ne sont qu'en l'éternel. Ici, la chute
est la plus valeureuse conduite. Du sentiment qu'on sut
avoir; tomber, tomber encore, dans celui qu'on devine.

Rilke, À Holderlin.

Contents

List of Figures

Chapter 1

Introduction

1.1 Preliminary remarks

> "Consider those utility cabinets where tools for the same purpose lie side by side, and where partitions logically separate instruments not designed for the same task: the worker's hand quickly grasps, without fumbling or mistake, the tool needed. Thanks to theory, the physicist finds with certitude, and without omitting anything useful or using anything superfluous, the laws which may help him solve a problem" (1991, 24).

These words of Duhem describe the ideal physical theory and how it serves the physicist by providing her the set of tools she needs to operate on the world. Like the surgical tray provides the surgeon all the instruments and only those instruments which are needed to perform her surgery, the theory makes sure that the physicist has at her disposal the set of well-ordered tools she needs to 'carve nature at its joints'. For the theory to accomplish this goal, nothing superfluous must be included within it, to make sure that the physicist grasps the required utensils as efficiently as possible.

How do we know however, what is superfluous and what could turn out to be useful on this tray of principles, laws and physical quantities that the physical theory makes available to us? Going back to our surgical comparison can be useful here. The composition of a surgical tray is determined by the nature of the surgery performed–no need for a sternal saw or a dentist drill for an appendectomy, and the physician could be distracted by the presence of these tools, waste precious time because of them or, even worse, grab one of them by mistake. One might think that the same applies to a physical theory: it should include everything necessary to achieve its role and exclude anything that does not contribute to do so. Duhem's famous book, *The Aim and Structure of Physical Theories*, from which the quote above is extracted, seems to develop such a line of reasoning. As the title indicates, according to Duhem, the aim and structure of a theory must be thought together, and the structure of physical theories determined by the aim assigned to them. For Duhem, the main role of a physical theory is to turn a chaos of unorganized empirical laws into a perfectly ordered classification, which allows the physicist to access all the knowledge gathered about this domain of phenomena conveniently. In other words, a physical theory must efficiently organize the knowledge that would otherwise remain the forgotten prisoner of a multitude of empirical laws lumped together with no order, as would be lost the historical knowledge contained in old documents, piling up on the shelves without any proper archiving system. What the theory does *not* aim at, for him, is to supply metaphysical explanation going beyond the phenomena, i.e., to "put us in relation with the reality hidden under the sensible appearances" (1906, 7). Because a theory aims at classifying, i.e., at providing an economical representation of a set of empirical laws, it should not include anything that is dispensable to its goal and predictive success. Therefore, it should not include any such attempts at grounding physics in metaphysics; not only because metaphysical claims do not contribute to fulfilling its task and are therefore superfluous, "attached to [...] (the theory) like a parasite" (1906, 33), but also because the explanatory part of a theory is misleading, restricting, and contains "whatever is false in the theory and contradicted by the facts" (1991, 33).

What if, however, one disagrees with the aim assigned to physical theories by Duhem? Does it mean that one would also disagree on what constitutes relevant and superfluous structure within the theory? A good example of such a case is provided by Duhem himself, in his criticism of Huygens' mechanistic philosophy. To Duhem's eyes, Huygens' wave hypothesis about the nature of light did not play any role in his successful extension of Descartes' theory of refraction to phenomena of double refraction. A comparison between the propagation of sound and the propagation of light, the experimental fact that one of the two refracted rays followed Descartes law while the other did not obey it, a felicitous and bold hypothesis about the form of the surface of the optical wave in media of crystals (1991, 35), this was all what was needed for Huygens to successfully extend Descartes' theory according to Duhem, who justifies the dispensability of metaphysical hypotheses based on this reconstruction of Huygens' achievement. Yet, this is not how Huygens himself tells the story. Huygens suggested an ellipsoidal model of light propagation based on the idea that waves propagate in two distinct ways, one corresponding to light propagation in the Iceland Spar he was using to study double refraction; the other to propagation in the aether contained in this crystal. He hypothesized that the latter mode of propagation would be spherical, while the former would follow an ellipsoidal model.[1] Even though the wave hypothesis may not have been needed, properly speaking, to account for double refraction, it was still integral to the thought process that led Huygens to his success, and was certainly not superfluous from his point of view. And this is precisely the problem arising if one takes seriously the Duhemian claim that the questions of identifying the aim of a theory and determining what its structure should be are inseparable. A sewing machine[2] has an uncontroversial function, that of sewing, and any features or elements of the machine that can be removed without jeopardizing its capacity to sew constitute superfluous structure. But there is no consensus on what a theory should aim at, and even less so on what a good theory is. On the contrary, during the second half of the XXth century, many rival interpretations of Quantum Mechanics or approaches to Quantum Field Theory have been developed precisely because of how controversial these two questions are. If these questions are as intricate as Duhem seems to think, can two physicists who disagree on what is a (good) physical theory ever agree on

[1]See Huygens (1690/1920), 73).

[2]This example is used in Ismael and Van Fraassen (2003), 371.

what constitutes relevant, physically significant content for a given theory and what is mere superfluous structure?

Consider for instance what led to the development of multiple interpretations of Quantum Mechanics. The postulates of Quantum Mechanics include the Schrödinger equation, a law that describes the dynamical evolution of a wavefunction representing the quantum state of a system. When applied to a typical quantum system prepared in a superposition of states and interacting with a measuring device, the linear Schrödinger equation does not yield a unique, definite experimental outcome, but instead a superposition of different experimental outcomes. There are three main ways to address this difficulty:[3] one can deny that the wavefunction provides a complete description of the system. Bohmian Mechanics, for instance, supplement the theory with hidden variables which specify the position of particles at every moment, and the particles' velocities are expressed in terms of the wavefunction in a 'guiding equation'. Another strategy consists in modifying the dynamics, so that one can account for our observations that the quantum system seems to be in a definite eigenstate of the measured observable whenever a measurement is performed. Dynamical collapse theories, for instance, propose a collapse mechanism that breaks the linearity of the Schrödinger equation. Finally, one can consider that the theory provides a complete description as it stands and that the superposition of states must be taken seriously, so seriously that it should lead us to revise our ontology. According to the Many-Worlds Interpretation for instance, a superposition of two states (say 'up' and 'down' for the measurement of the spin of two electrons in a Stern-Gerlach experiment) is actually the description of a universe containing two worlds, with one world recording 'up' and the other recording 'down'. This means that a measurement-like interaction has caused the universe to split into two worlds or branches, one for each element of the superposition. Now consider the structure of these interpretations, each adopting a distinct epistemological stance on what a theory should be like. From the point of view of the Copenhagen or collapse interpretation of Quantum Mechanics, the hidden variables added by the Bohmian constitute dispensable

[3] See Myrvold (2018), section 4.

structure. After all, the two theories are observationally equivalent, which is enough to show that one can obtain from the theory all the desired observational consequences or explanatory relevant elements without appealing to this extra structure:

> Bohm's theory adds a definite, hidden position for the particle, always possessed by it at every moment, and our ignorance of its true value is expressed in a probability distribution. A Bohm theorist can insist that this is a physically real addition to the ontology, so that the Bohm theory is physically distinct from traditional quantum mechanics. A traditionalist can reply, however, that the particle position only becomes manifest at the moment of measurement, so that standard quantum mechanics can assert that the position and its probability distribution came to be at the moment of measurement. All a Bohm theorist has done is to project the position and associated probability distribution back in time to the initial set up– a superfluous addition since all the theoretical information needed to specify the actual measurement outcome is already fully encoded in the wave function (Norton (2008), 37).

For a Bohmian, the hidden positions may constitute *additional* structure compared to standard Quantum Mechanics, but certainly not *superfluous* structure: without them, Bohmian Mechanics would not be the complete, fully deterministic theory, with no references to an observer needed, that Bohm strived to achieve. Of course, these features come with a cost, that of a specific kind of non-locality that puts Bohmian Mechanics at odds with relativity.[4] However, the availability of Bohmian Mechanics shows that the non-determinism of Quantum Mechanics

[4] Bell's theorem shows that any quantum theory that is predictively equivalent to Quantum Mechanics must violate Bell's inequalities. These inequalities rely on a locality condition that has been analyzed by Abner Shimony as a conjunction of two distinct locality assumptions, that of Parameter Independence, according to which the outcome of an experiment performed on particle 1 is independent of the analyzer parameters of a spatially separated apparatus for particle 2; and of Outcome Independence, which states that the outcome of an experiment performed on particle 1 is independent of the outcome of the experiment performed on 2. Any theory equivalent to Quantum Mechanics must be non-local in the sense of violating either Parameter Independence or Outcome Independence. Bohmian Mechanics violates the former, which puts it in tension with relativity, inasmuch as the parameters of the apparatus could be adjusted such as to allow the sending of superluminal signals. Quantum Mechanics violates Outcome Independence, which makes it a non-deterministic theory.

and its vagueness in deciding which dynamical rule applies to which systems is a *theoretical choice*, based on a specific stance adopted about what a good physical theory should be:

> Why is the pilot wave picture ignored in text books? Should it not be taught, not as the only way, but as an antidote to the prevailing complacency? To show us that vagueness, subjectivity, and indeterminism, are not forced on us by experimental facts, but by deliberate theoretical choice? (Bell et al. (2001), 990)

In other words, the very possibility of a non-local hidden variables theory, as pointed out already by Bohm (1952, 166), shows that the assumption that the wavefunction provides a complete description of a quantum system, determining only the probability of experimental outcomes is not forced upon us, as was long thought given the impossibility proof famously developed by von Neumann. On the contrary, it is a choice, based on an epistemological take on what a theory should be like. A similar analysis could be made about the Many-Worlds interpretation. Although I will not get into details in this case, one of the main arguments developed by De Witt (DeWitt 1970, 33) to support a Many-World Interpretation is its simplicity–i.e, this interpretation considers that the formalism offered by the axioms of Quantum Mechanics without random collapse is sufficient and that nothing is gained by the addition of a random collapse rule but unnecessary complexity or even inconsistency.[5] From De Witt's point of view, this random collapse rule is superfluous in the same sense as Duhem considers metaphysical hypotheses superfluous: not only it is not needed, but it is also dangerous and misleading, for it leads the standard interpretation into inconsistencies. A defender of the standard interpretation would however probably gasp at the claim that the Many-Worlds Interpretation is simpler, when it requires an exponential inflation of our ontology and the belief into the reality of all the simultaneous worlds corresponding to the terms of the superposition obtained when applying Schrödinger's equation, and the constant splitting of these worlds into branches at each new measurement. This ontological extravagance is certainly superfluous from the point of view of

[5] See for instance how the Wigner's friend thought experiment is presented by De Witt in the aforementioned paper.

standard Quantum Mechanics too.

A similar debate arise with distinct approaches to Quantum Field Theory, especially between 'conventional' quantum field theories and the algebraic approach of Haag and Kaastler. The development of axiomatic approaches to Quantum Field Theory in general was motivated by the problem of infinities, i.e., of calculations showing integrals diverging to infinity and leading to nonsensical results. But the algebraic approach is also based on two other motivations, which both had a direct impact on the analysis of the structure of the theory:

- Locality: One of the core principles of Algebraic Quantum Field Theory is the claim that the entire content of the theory is contained in the net of observables, i.e., the collection of local algebras of observables assigned to every region of Minkowski spacetime. Given that all possible experiments involve measurements performed in finite regions of spacetime, the relevant physical quantities should be the ones accessible to such measurements, i.e., local quantities defined over finite regions of spacetime– as opposed to global quantities such as total charge, energy or mass defined over infinitely extended spacetime regions. Unobservable quantities such as the total momentum of a field should be dismissed as physically insignificant, inasmuch as they cannot be reduced to the only physical notions deemed relevant by Haag and Kaastler: that of a state, referring to a statistical ensemble of physical systems, and that of an operation, i.e., of a physical apparatus acting on the ensemble (1964, 850). So, the theory is meant to be local in a very precise sense: not only all observations must be local observations, but they must be local in the sense of localized detections of particles by detection devices.

- Unitarily Inequivalent Representations: Another problem stressed by Haag and Kaastler (1964) is the existence of unitarily inequivalent representations in quantum field theories. The Stone-von Neumann theorem that guarantees the uniqueness, up to unitary equivalence, of the representations of the canonical commutation relations[6] does not apply to

[6]One can think of a representation of the canonical commutations relations as a collection of operators that satisfy the commutation rules through which classical observables are 'quantized' or new quantum observables

systems with infinitely many degrees of freedom, as is the case for systems described by QFT. Hence, QFT exhibits infinitely many unitarily inequivalent representations, which creates both an ambiguity–which representation should be chosen?– and an (at least apparent) inconsistency. Indeed, given that there is no one-to-one mapping between the expectations values assigned to the operators in the first representation and to the corresponding observables in the second representation, unitarily inequivalent representations are strictly speaking physically inequivalent. According to most authors, this means that a representation *must* be chosen over the others, without there being any clear grounds for such a choice however. However, according to Haag and Kaastler, unitarily inequivalent representations only differ about these global aspects defined over infinitely extended regions of spacetime that I mentioned above: "they differ in the global aspects of their states but this difference is irrelevant as long as we are interested only in experiments in finite regions" (1964, 853). If one readjusts their notion of equivalence based on what is possibly measurable, or, more precisely, on the fact that no actual measurement can be performed with absolute precision, then a realistic notion of equivalence is not that of unitary equivalence, but that of weak equivalence. One of the pioneer of Algebraic Quantum Field Theory, Segal, had pointed as a way out of the unitary inequivalent representations the fact that most of the physically interesting questions could be answered without any reference to a concrete representation in a Hilbert space. Haag and Kaastler's adoption of weak equivalence as physical equivalence leads to the same result, the vanishing of the problem of unitarily inequivalent representations: if one admits that there is no absolutely precise state of a system, but only a state determined to within a weak* neighborhood–i.e., that one can only measure the expectations of a finite number of observables to a finite degree of accuracy–, and given that the set of relevant states of every representation lie in this weak* neighborhood, then the choice of representations will not make any measurable difference, and this equivalence should be considered as providing

defined. This collection is 'concrete' in the sense of being defined on a Hilbert space, as opposed to the abstract algebra of observables.

> the grounds for the relevant notion of physical equivalence.

The motivations underlying the development of AQFT clearly finds their roots in Haag and Kaastler's operationalist take on the aim of a quantum field theory, that they see as a statistical theory about local detection events happening in finite spacetime regions. This operationalism in turn contributes to shaping the resources that AQFT has at its disposal. A defender of AQFT who shares the strong operationalism of Haag and Kaastler has no difficulties to escape the problem arising with unitarily inequivalent representations and will deem the extra structure obtained with a concrete representation of this net of observables as superfluous. However, if one thinks that the aim of a physical theory is to describe what the world is really like, and not only how it appears to our measurements; then the status of global properties becomes more controversial. Likewise, if one thinks that restricting the meaning of 'observable' to 'detectable' is too much of an impoverishment, or if one attributes to local another meaning– that of coincidence at spacetime points for instance, instead of detections within regions–, then the structure of the theory would not be analyzed in a similar manner in terms of what is physically significant and what is not. But it would also not achieve the task that Haag and Kaastler had in mind, that of avoiding mathematical difficulties and undesirable features of the previous formulations of the theory. Again, quantum field theories present cases where disagreements on the role of a physical theory carry over deciding the minimal structure it should include.

Sklar, in his 2000, has criticized the strand of naturalism that claims that there is no need to engage with scientific theories from a philosophical perspective, as theories can be understood independently of any epistemic considerations:

> On this suggestion, the scientific theories are complete and sufficient unto themselves, and they reveal to us on their face all we need to know about their "meaning" or their "interpretation". From this perspective, the perennial philosophical desire for an analysis of a theory's meaning, and the desire to reformulate or reconstruct the theory based upon considerations arising out of philosophical cri-

> tique, are the pointless pursuits of will-o'-the-wisps. But, as we have seen, there is something misguided about the suggestion that one can deal with the fundamental theories of physics in a manner that is independent of the sort of critical arguments, based on epistemic considerations, that are so familiar from empiricist philosophy. For the very construction, justification, and reconstruction of theories within the progress of science itself is replete with just that kind of reasoning we took as paradigmatically philosophical (Sklar 2000, 32).

Sklar focuses on cases where a theory is reconstructed based on the ontological elimination of structure deemed otiose in the previous formulation of the theory. Such projects of reconstruction can be found everywhere in the history of science, motivated by a diversity of reasons: the desire to formulate a new theory that is not only compatible with novel data, but compatible in a simple, non-arbitrary way; the desire to reformulate an old theory so as to make it compatible with a newly established background theory; the desire to retroactively reformulate and rehabilitate old and discarded theories; the desire to get rid of artifacts generating conceptual or mathematical difficulties; the desire to clarify concepts whose role may have shifted due to a change in the assumed background theory; the desire to explain away cases of underdetermination, and finally the desire to provide a clear metaphysical interpretation of a theory whose physical meaning may be problematic.[7] In each of these cases, the reconstruction of the theory is based on eliminating superfluous structure, based on considerations that are typically philosophical: a debate about whether unobservable theoretical terms should be excised from scientific theories, a disagreement on how to demarcate the observable content of a theory, or on whether a theory should restrain itself to local, observable aspects of spacetime as opposed to global, unobservable, ones.[8] Hence, the construction and reconstruction of theories always rely on epistemic considerations that need to be made explicit, acknowledged and taken into account when analyzing the structure of a theory in terms of its physically relevant and irrelevant

[7] See Sklar (2000), pp. 15-19 for a detailed discussion of all these motivations and examples.

[8] This list is not meant to be exhaustive. Its role is merely to emphasize that the debate between different forms of empiricism–constructive empiricism, instrumentalism, positivism, operationalism– and realism is not the only source of disagreements when analyzing the structure of scientific theories.

structure.

Why should the absence of a neutral point of view to diagnose superfluous structure worry us? Because the question of determining what constitutes additional, dispensable structure or mathematical surplus structure with no physical counterpart is not only important for theory reconstruction, but also plays a central role in theory choice, especially when facing cases of seemingly equivalent theories targeting the same domain of phenomena. Indeed, two of the most used strategies to handle these cases of underdetermination, the appeal to theoretical equivalence and to theoretical virtues, presupposes at least implicitly that one can with no difficulty separate the essential from the superfluous within a given scientific theory. The strategy of theoretical equivalence, for instance, assumes that there exists a neutral standpoint from which a criterion of physical significance can be formulated that applies to both theories indifferently; and that such a criterion captures all that and only that which is physically relevant in a theory, allowing for a comparison between the theoretical structures thusly analyzed. Likewise, the use of theoretical virtues to privilege a theory over another presupposes that a neutral comparison can be done between their simplicity or their explanatory power. Putting aside the vagueness of a notion such as that of simplicity and the subsequent difficulty to apply it, all the interpretations of Quantum Mechanics we have already met could call themselves 'simpler' than the other, without any chance to convince its rivals, privileging other theoretical features. Bohmian Mechanics is simpler in the sense of more intelligible, as more directly related to our classical concepts to describe the world; the Many-Worlds interpretation only needs one dynamical rule and dispense with the random collapse rule; the standard interpretation is ontologically simpler, given that it does not introduce an infinity of unobservable worlds or unobservable variables assigning positions to every particle.[9] Less often discussed, the strategy consisting of privileging a theory over another because of its greater explanatory power seems to presuppose that the structure of both theories can be fully elucidated in a way that satisfies

[9]Only the last two examples make a use of simplicity that requires to determine what is superfluous and what is not in a theory.The first example illustrates what I meant by the vagueness, or ambiguity, of the concept of simplicity.

both parties. Suppose that you have a body of evidence e_i(i=1,...,N) and two theories T and T'. Suppose now that the entire set e_i can be deduced from the set of axioms–and the subsequent laws derived from them–that T postulates, while in T' a subset (e_3, e_4) requires the addition of an extra-postulate. This extra assumption is introduced as an *ad hoc* patch, whose only purpose and justification are to recover e_3, e_4. Although T and T' both account for e_i, T seems to be in a stronger epistemic position than T': it gets for free what T' only obtains by hand, by adding an extra-postulate not required in the other.

The Cold Dark Matter model, for instance, is often defended against its rival the Modified Newtonian Dynamics (hereafter MOND) because of its explanatory power. Through the addition of non-ordinary matter, only interacting gravitationally, this model provides a unique explanation for a multitude of anomalies–at large scales, the abundance of deuterium, the CMB anisotropies and the study of large structure formation; at meso-scale, Zwickys study of the Coma cluster (1933, 1937), gravitational lensing results (Gavazzi, 2002; Pointecouteau and Silk, 2005), and the Bullet Cluster are all anomalies that find a common answer with dark matter, at small scales, the anormal rotational velocities of galaxies is also explained if more 'dark', non-visible matter is added. The explanatory power of the dark matter model is really impressive, given the diversity of scales and the number of problems that this unique hypothesis can address. But can it really convince the other side, when defenders of MOND consider dark matter either as an artifact of applying Newtonian Dynamics at scales where it has not been tested, or as a superfluous addition, as the modern 'aether'[10] that not only is not needed to account for galaxy phenomenology but hides the fact that a revision of our gravitational law is needed? Can the appeal to the explanatory power of the Cold Dark Matter Model really settle the debate between the two theories?

As one can see from the list of examples studied so far, one of the main difficulty in this debate is how broadly this idea of superfluous structure is conceived, and how related, sometimes overlapping, but distinct concepts are subsumed under this idea. There are at least three closely

[10]See for instance McGaugh (2014).

related concepts that can fall under this notion of superfluous structure: structure that is purely mathematical in the sense that it fails to capture something in the world (i.e., it posits something that is not there to begin with); structure that is mathematical surplus in the sense that too much has been imported from a mathematical model within a physical theory; finally mathematical artifacts that result from surplus structure, or from idealizations or simplifications made to represent the target system. In what follows, I detail these three kinds of unphysical structure and evaluate the effectiveness of philosophical tools that have been suggested to eliminate them.

1.2 Three Kinds of Unphysical Structure

1.2.1 Additional structure

The idea of additional and dispensable structure is best understood in two different contexts: when rival theories propose to resolve a problem, one by adding some properties or some entity to their initial content, the other by revising at least one of their laws; and when examining the history of the spacetime structure that was deemed necessary to explain the dynamical behavior of objects. The second scenario has been discussed by many authors, notably in Ismael (forthcoming), 6-10, with a clarity I can certainly not rival with. The history of physics is full of examples of the first kind, where the alternative approach that consists in revising laws turns out to be successful and thus blames its rival for having posited superfluous structure. Compare, for instance, Einstein's with Lorentz's theory of special relativity. Both accept notions such as time-dilation or length-contraction. In Lorentz's, light is carried by a medium, the ether, which defines what rest is; length-contraction and time-dilation are real effects with respect to the ether. In Einstein's, length-contraction and time-dilation are not real, but frame-dependent appearances: the denial that the distinction between rest and inertial motions is a meaningful one together with the postulate that light propagates at a constant speed are enough to provide an empirically adequate account of the electrodynamics of moving bodies, thus making the ether a superfluous notion:

> These two postulates suffice for the attainment of a simple and consistent theory of the electrodynamics of moving bodies based on Maxwells theory for stationary bodies. The introduction of a luminiferous ether will prove to be superfluous inasmuch as the view here to be developed will not require an absolutely stationary space provided with special properties, nor assign a velocity-vector to a point of the empty space in which electromagnetic processes take place (Einstein 1905, 1-2).

So, one could argue that Einstein's theory can be preferred for not positing superfluous structure, as 'simpler'. However, from a Lorentzian point of view, the ether has a physical significance with no counterpart in Einsteins theory, which seemed too poor to explain the motion of light. Sound waves are vibrations in the air; light waves are vibrations in the ether-the wave motion of light made no sense without a material medium. Somebody already in board with Einstein's special relativity could easily buy this kind of argument, but could it possibly convince a defender of Lorentz's theory? More importantly, could somebody try to establish the equivalence of these two theories without discarding the physical importance of ether from the point of view of Lorentz? In this case, the 'superfluity' of the entity or property postulated can only be called so because an alternative has been developed, and because this alternative is considered successful. But there is no available criterion to diagnose what is superfluous structure other than a retroactive judgment informed by the success of the alternative.

Cases of additional entities vs. revised laws are even trickier to address when the extra entity or property is by principle, or at least in the context of its introduction, unobservable. This was the case for instance for the rivalry between the quark and the paraquark model in the 1960's. In 1964, a particle was observed at the Brookhaven National Laboratory that violated Pauli's exclusion principle, according to which no two fermions can occupy the same state. This particle, the Ω^-, is a baryon, and is therefore made of three quarks. But quarks have half-integer spin and should therefore behave as fermions, satisfying Pauli's principle. Yet, the three quarks seemed to be in the same state. Likewise, the baryon spectra observed for this particle seemed to require a symmetric wavefunction under the permutation of two of these quarks, which is the normal permutation behavior of bosons but not of fermions. Two different answers were suggested to address this 'statistics problem'. One was to abandon Pauli's principle and replace it with a generalized principle, taking into account the non-fully symmetric behaviors characteristic of paraparticles, i.e., to particles not obeying the perfect permutation symmetry that bosons and fermions do. For parafermions of order 3 for instance, the wavefunction can be symmetric up to 3 fermions. The other suggested answer was to add a hidden degree of freedom to quarks, in order to antisymmetrize the wavefunction with respect to this new degree of freedom. This is the choice made by the 'colour' quark model. If paraquarks were the correct answer to this violation of Pauli's principle, the exact same phenomena known at that time and accounted for by the colour quark model would have been observed: the baryon spectra would be the same, as would be the neutral pion decay rate, or the ratio of the cross-section of electron-positron annihilation to hadrons to the cross-section for annihilations to muons pair (see Greenberg 1993, 11-12). Thus, there were no known observations that could support the idea that the world is not made of only bosons and fermions, but also of parabosons and parafermions. On the other hand, from the point of view of the colour model, parastatistics are mere superfluous structure: there is no need to add a new kind of particles to account for the exact same phenomena. Yet, the colour hidden degree of freedom is by principle unobservable, given that bounded systems of quarks are colourless and no free quarks can be observed. In such a context, how do we tell which model should be privileged over the others? Historically, the paraquark has been given up because it could not be gauged and thus unified with the strong force, and because it was deemed 'obscure', with 'disagreeable' properties. Later however, attempts to justify this abandonment have been developed that were all based on the theoretical equivalence strategy–according to this line of thought, parastatistics are 'superfluous' in the sense that they are a mere notational variant of the quark model with an extra degree of freedom, and that both descriptions can be interchangeably used–a surprising claim given that one theory can be unified with the strong force and not the other, but the proof was developed between a parafield theory and a quark theory equipped with a global gauge group, as opposed to the locally gauged theory that was unified with the strong force. As one can see from the discussion above however, such a proof can only be provided under the agreement that parastatistics have no physical significance and do not define the permutation behavior of a new kind of particles that could be observed in the world, as permutation symmetry usually do. In other words, such a proof must rest on the dismissal of parastatistics as physically irrelevant. So, again, the idea that parastatistics are superfluous is formulated from the point of view of the 'winner', i.e., the colour quark model. Is there any way, however, to diagnose superfluous structure from a neutral point of view, to avoid re-writing the history from the point of view of the winner theory? In other words, can one determine what constitutes superfluous structure from an external, neutral point of view, free of any philosophical stance, and formulate a criterion of physical significance that would not be formulated in the vocabulary that favour one theory over the other, that would not impose one's philosophical beliefs on the structure of the other? Is there any way for MOND, for instance–granted that dark matter particles have yet to be detected–to provide an objective criterion according to which dark matter is otiose structure? I explore this set of questions in chapter 1, based on the case study of paraparticles and the proof of equivalence recently provided by Baker et al. (2014). I argue for a negative answer, based on this case study. I also contend that theoretical equivalence still can be used to clarify the structure of rival theories and understanding which parts of the theory really contribute to its success without pretending to pair these theories as two versions of one and the same theory.

1.2.2 Surplus Structure

Another kind of unphysical structure discussed in the literature is the concept of 'surplus structure' developed in Redhead (1975). Redhead analyzes the mathematical formalization of a physical theory as an operation of embedding a physical theory into a mathematical model: a theory T can be embedded in a mathematical structure M if and only if there exists an isomorphism (a one-to-one structure preserving correspondence) between T and a substructure M of

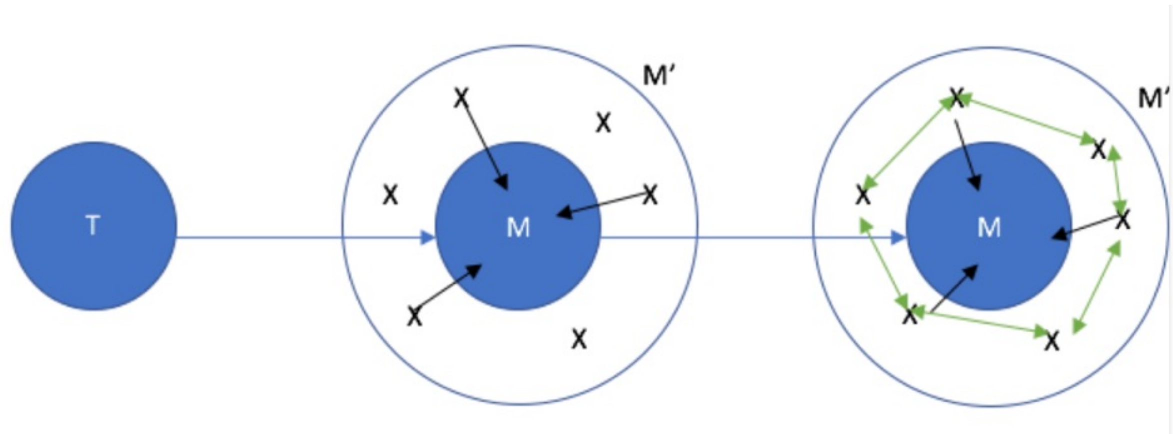

Figure 1.1: Redhead's account of surplus structure. The set of green arrows correspond to relations between surplus elements that do not have any initial relation with the set of elements in T as embedded in M.

M'. Thus, when embedded, the physical theory inherits a surplus from the complement of M in M', which allows many-to-one relations between the elements of the model in M and the states described by T. From this many-to-one relations has been derived the idea that surplus structure manifests itself by the emergence of multiple representations available for a unique physical state. The surplus structure involves both relations among the surplus elements and relations between these elements and elements of M (Redhead 2003, 128). In other words, the physical theory not only inherits superfluous mathematical states, but the latter opens the door to a whole ensemble of new elements, *qua* related to the surplus elements. The figure below shows how more and more elements can be imported from the complement in M'.

Like in the first case, the 'surplus' is superfluous in the sense that it is dispensable, inasmuch as the same phenomena can be accounted for without having to posit this extra-structure. But unlike the first kind of unphysical structure review, this structure is not added to the theory as obviously physical, as an answer to a physical problem but may have been unwillingly imported within the physical theory and interpreted as physical by mistake. Although there are, to my knowledge, no uncontroversial example of surplus structure, one that often comes back when discussing Redhead's account is, maybe surprisingly, that of paraparticles -again! One can find such a description of paraparticles, i.e., of particles obeying parastatistics, in Massimi (2005), French and Krause (2006), in French and Rickles (2003) or in Bueno and French (2018). Read for instance the following words by Massimi (2005, 178) :

> This is an example of 'surplus structure, to use Redhead's terminology: a physical structure P (e.g. the quantum statistical behaviour of an ensemble of indistinguishable particles) is not represented by the mathematical structure M (e.g. Fermi-Dirac an Bose-Einstein statistics) with a one-one structure-preserving map between P and M. Rather, P is represented by a larger mathematical structure M (e.g. permutation invariance), hence a surplus structure M-m (e.g. generalized rays) in the representation of P by means of M.

As interesting as this account can be in helping us to understand how surplus structure appears in a physical theories, it is not clear how it can actually provide a criterion for determining what constitutes surplus. Yet, it comes according to Redhead with a methodological principle, according to which a theory that does not exhibit such surplus structure should be preferred. Such a principle is crucial for Redhead and Teller's criticism of the view according to which quantum particles have 'transcendental individuality', i.e., quantum particles of the same family are individual despite having the exact same properties (1991). According to Redhead and Teller, the use of labels to refer to quantum particles that nothing yet distinguishes rests on an unacknowledged metaphysical stance, according to which quantum particles are individuals. To construct the configuration space of a system of identical particles, one starts by building a Hilbert space for one particle, another one for the second particle despite their indiscernibility, and then take the tensor product of both individual Hilbert space. In the configuration space thus obtained, each ray correspond to a state and distinct rays to distinct states. Thus, it is assumed, if one measures the observable A whose eigenvalues can be a or b, that the ray that describes a situation in which the eigenvalue a is assigned to particle 1 and b to particle 2 represents a physical state distinct from the situation in which the eigenvalue b is assigned to particle 1 and a to particle 2. According to Redhead and Teller, transcendental individuality is built-in in such a formalism. But this metaphysical assumption leads to unwanted consequences, in that the Hilbert space of the total system contains non-symmetric and partially symmetric vectors corresponding to states that never occur in nature. Thus, because of this built-in transcendental individuality, Quantum Mechanics inherits surplus structure that must be removed if possible. Redhead and Teller argues that an alternative formalism such as the Fock space formalism dispenses with this extra-structure and is therefore preferable.

But again, how do we know when structure is surplus in this sense? This idea that the availability of many mathematical representations for describing one and the same physical system not only is not a sufficient condition, for it captures many cases that we would not call surplus structure, but it is also not necessary–as many cases historically accepted as surplus structure do not really fall under this description. Let us start with the many unintented states of affairs that are wrongly captured by this definition. A same system can be described using polar coordinates or Cartesian coordinates, depending on which representation is more convenient for the calculus. The physical situation described by both systems of coordinates is clearly the same, yet two mathematical descriptions are available. Likewise, one quantity can be measured using different standard of measurements. Mineral's 'scratchability' is described using a scale of 1 to 10, which means that "the physical structure involved in ordering the hardness of minerals is mapped isomorphically onto the finite segments of the arithmetical ordinals running from 1 to 10" (Redhead (2003), 126), but any other segment, from to 2 to 11, or from 21 to 30 could have been used instead. The current criterion for surplus structure would consider the simple difference in measurement standards as surplus structure. But would we consider the availability of two standards of measurements as surplus structure in the same sense as parastatistics? Would we say that, in the same sense that parastatistics fails to have any physical counterpart, rival systems of coordinates do not capture anything in the world? More importantly maybe, this attempt at a definition does not explain what constitutes one and the same physical situation. At the time of the initial rivalry between quark and paraquark for instance, it was thought that the paraquark model and the quark model could be used interchangeably, as describing the same known phenomenology. However, those who took seriously the possibility of the existence of paraparticles and further developed this model succeeded to show that parafermions would violate Pauli's exclusion in a detectable way, for instance if one were to observe the spectrum lines of atoms in which electrons cascade down to already occupied states. In this specific scenario, fermions and parafermions obviously do not describe one and the same specific state of affairs. So, how do we know what constitutes surplus structure unless we have already decided that one of the rival representations is superfluous? How do we apply this methodological principle of Redhead and Teller to ground our choice of a theory or of a metaphysical stance?

1.2.3 Artifacts and Robustness

The third kind of unphysical structure I would like to investigate is that of artifacts. Like surplus structure, artifacts are not explicitly, willingly posited in response to a given problem but are imported within a theory given some other choices whose consequences are not always fully understood. They usually are the result of idealizations or simplifications made in building a model or a theory. Artifacts are surprisingly rarely discussed in the context of theoretical physics, despite some fairly recent examples of artifacts in General Relativity and their use to constrain choice between rival models. One such example can be found in the discussion of singularities in the early days of General Relativity. As reported by Earmann (1999, 241) many cosmologists in the 1930's were considering space-time singularities as pathological and dismissed them as resulting from the highly unrealistic symmetry assumptions upon which Einstein's Field Equations and Friedman-Lemaître-Roberts-Walker cosmological models were based. The Penrose-Hawking singularity theorems, however, have shown that geodesic incompleteness[11] is unavoidable for a more general class of spacetime, based on much weaker assumptions: a generic solution to Einstein Field Equations that only assumes some energy condition, some causal properties of spacetime and a closed trapped surface is not compati-

[11] An easy, intuitive way to conceive a spacetime singularity is to see it as a place where the curvature of spacetime becomes infinite. However, in General Relativity, such a definition is slightly misleading, for Einstein field equations define spacetime and do not apply anymore for infinite curvature. Thus, one can think about singularities as missing points in spacetime, that can be found by finding particle paths that end as they run into the singularity–the incomplete geodesics.

ble with geodesic completeness. Nonetheless, one can still see in the literature the idea that a singularity-free quantum gravity theory is preferable, and as such should be chosen over its rivals.

The Penrose-Hawking singularity theorems can be seen as establishing the robustness of singularities, as geodesic incompleteness hold for a much more general class of spacetime than was originally thought, and does not depend on specific symmetry assumptions. Robustness analysis has indeed been used in many areas as a methodology allowing to determine when the predictions extracted from a model are genuine predictions, as opposed to an artifact of the idealizations upon which this model depends. Roughly speaking, it consists in examining a variety of models each relying on independent simplifications to look for robust properties, properties that would hold in all models despite the fact that they rely on different simplifications or idealizations.[12] Yet, this methodology does not seem to be a good candidate to identify artifacts, for at least two reasons. First, robustness analysis cannot be of any help when the suspect idealizations generating artifacts are inevitable. This happens more often that one would believe. Consider for instance the case of referential ambiguity that is created by the indiscernibility of quantum particles in Quantum Mechanics. The fact that quantum particles are individually labelled does not create any problem when describing systems of particles belonging to distinct families. However, when applied to systems of indiscernible particles, it generates cases of referential ambiguity: labels are creating distinctions between particles that are not grounded in any property or justified by any resources that standard Quantum Mechanics has at its disposal. Using labels anyway has important consequences: given the linear nature of the laws that quantum states obey, it is really easy to construct from the availability of two seemingly distinct states an infinity of mathematical states. Among these states can be found those describing paraparticles, which I argue in chapter 2 constitute an example of artifacts of such referential ambiguity. One could argue, as Redhead and Teller did in their 1991, that switching from the Hilbert formalism to the Fock formalism is enough to solve this difficulty.

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[12] See Levins (1966).

Yet, although particles are not directly labelled in the Fock representation, the Fock space is constructed from the tensor product of individual Hilbert spaces, which already contains the paraparticles they were hoping to avoid. How can robustness analysis help us to eliminate artifacts in such cases? More importantly, should we try to avoid them if they are based on ineliminable idealizations, or rather to precisely pin down which assumptions are responsible for their introduction and temporarily neutralizing them? In sum, robustness analysis does not always constitute a possible answer, and in cases it does, does not always constitute an effective one. In particular, in chapter 3, I will examine the failure of robustness analysis to identify artifacts in cosmological simulations, and argue that the agreement over distinct models taken to be the symptom of the 'physicality' of a prediction is sometimes the result of artifacts, which are responsible for the convergence of different models towards a similar solution. Philosophers must develop new tools to identify artifacts, especially if the absence of artifacts is taken to be a reason to prefer a theory or model over another.

Beyond the 'unphysicality' of these three kinds of structure, their common features that greatly contributed to motivate this dissertation are: 1) the fact that they all come with a methodological injunction according to which a theory is preferable if it does not present any extra, dispensable structure/surplus structure/artifacts; 2) the lack of a criterion or successful methodology in all these cases that would unambiguously identify what constitutes superfluous, surplus or artificial structure. My reader will not be surprised to learn that, given the above, I do not think that such a criterion can be delivered at least in the first two cases. If the epistemological stance that one adopts on what constitutes a good physical theory is the ultimate judge of what is relevant structure and what is not, then it comes with no surprise that no unique and neutral criterion can be delivered. Yet, as we have seen earlier, strategies to choose over rival theories seems to presuppose that such an unequivocal criterion not only can be found, but be applied to decide which theory should be privileged over another. My first two chapters explore the consequences of appealing to such strategies, that of theoretical equivalence and of theoretical virtues, without making explicit why the criterion of physical significance adopted should also be considered as relevant by the rival side. This does not mean, however, that these strategies should be abandoned altogether. On the contrary, they can still be of great help to elucidate the structure of scientific theories, if one puts aside the question of theory choice. I am more optimistic that a methodology can be developed to diagnose artifacts within scientific theories, mainly because of successful examples of such methodologies in the context of cosmological simulations. However, the development of such methodologies require first to provide solid grounds for dismissing the overwhelmingly used methodology of robustness analysis; second, to formalize and generalize such methods; and thirdly, in order to think about other applications for such a method, to list the conditions under which it can succeed in separating the reliable from the unreliable. This is the task undertaken by my third chapter.

1.3 Outline of the thesis

1.3.1 Theoretical equivalence, Realism and the Structure of Scientific Theories

Chapter 1 exploits the case study of paraparticles to analyze the presuppositions upon which the theoretical equivalence strategy depends. In this chapter, I argue that using theoretical equivalence as a way to escape the underdetermination problem, i.e., as a way to pair theory as notational variants of one and the same theory, presupposes that we have a self-standing vocabulary to elucidate the structure of one of the theories that can also be applied to the other without distorting or miscontructing its content. In other words, it presupposes that everything that the theory says and assumes about the world can be read off its formalism, without having to consider any of the philosophical beliefs that contributed to shape its development. In many cases however, such an assumption is not practical: we do not have in practice a way to formulate claims of equivalence that are not parochial and thus do not favor one of the theory while giving an impoverished version of the second. As a result, proofs of equivalence that rely on the assumption that a criterion of physical significance can be formulated in a neutral way may be established at the cost of dismissing as physically insignificant fruitful parts of the rival theory. One of the most recent examples of the theoretical equivalence strategy is the alleged proof of theoretical equivalence between a parafield theory and a theory with ordinary bosons and fermions. Such a proof, however, is formulated in a theoretical context, that of Algebraic Quantum Field Theory, that is already based on the dismissal of large parts of the structure of conventional Quantum Field Theories as superfluous. Moreover, it is motivated by a strong operationalism that greatly contributes to shaping its own structure and vocabulary, especially when it comes to the notion of equivalence upon which it is based, and does not appear as compatible with the use of theoretical equivalence as a realist strategy. Eventually, Algebraic Quantum Field Theory does not seem to have the resources to model precisely what was interesting about parafield theories, i.e., what allowed to turn them into experimental programs playing a crucial role in confirming Pauli's exclusion principle. This case study raises interesting questions tackled in this chapter, notably the following ones:

- Can we impose a realist grid on a theory explicitly grounded in operationalism? Can we simply ignore the philosophical beliefs that motivated the construction of a given theory, and analyze its structure from another viewpoint? Does it even make sense to interpret the structure of such a theory realistically?

- Is there a vocabulary that could be used to compare the experimental parafield program as developed in the 1980's with AQFT formulations of ordinary and parafield theories that would be neutral, not already tainted with one's epistemological stance on what is the aim of a theory and the determination of its corresponding resources?

- How can a proof of equivalence that convinces both sides be formulated if framed in a vocabulary that favor one of the opponents, in the sense that it includes all the content of one while dismissing significant parts of the other theory as dispensable? How does one reach an agreement on the equivalence of two theories when one of the theory includes additional structure that is not superfluous from their point of view but might be according to the other?

Finally, I conclude the chapter by suggesting another use for theoretical equivalence, still related though to elucidating the structure of the theories under comparison. I suggest to redefine the theoretical equivalence strategy as a strategy consisting of first identifying the maximal degree of equivalence that can be established between rival theories in a given vocabulary, then determining under which conditions this maximal result holds, and finally assessing the extent to which these conditions can be stretched. By analyzing where exactly the equivalence breaks down, i.e., which loosening of the equivalence conditions is crucial in breaking the maximal degree of equivalence, one can hope to define where the interesting differences between the competitors lie, and which parts of the theory are really responsible for its observational consequences and their differences.

1.3.2 Easier said than unsaid: Artifacts in Quantum Theories

Chapter 2 focuses on the use of theoretical virtues to decide between rival theories, especially in these cases where it is claimed that a theory has greater explanatory power than its rivals because it demonstrates the impossibility of some physical states that the other dismisses as surplus structure. Again, I use paraparticles as a case study. More specifically, I evaluate the recent claim made by defenders of Bohmian Mechanics that their interpretation should be preferred over rival interpretations because they can successfully exclude states corresponding to paraparticles. On one hand, the lack of clear criterion for identifying paraparticles as surplus structure should undermine the idea that the non-observation of paraparticles has never been a problem to begin with. On the other hand, a careful examination of the assumptions needed to establish the explanatory power of Bohmian Mechanics shows that their topological approach of the configuration space can eliminate states corresponding to paraparticles only under the same assumptions that they had deemed *ad hoc* in standard Quantum Mechanics. Thus, not only their explanatory power strategy seems doomed to fail to convince the rival sides, but it raises an important question: could the apparent inevitability of parastastistics in both frameworks indicates that they are artifacts of some ineliminable assumptions made by both theories?

The second part of the chapter explores this hypothesis, and how it relates to the idea that the epistemological standpoint one adopts on what a good theory is when developing a new interpretation of Quantum Mechanics determines its resources. I make the hypothesis that paraparticles are artifacts of the implementation of quantum indiscernibility through permutation invariance. The concept of permutation invariance is both the assertion that there are two distinct states, and that these states can be considered one and the same inasmuch as permuting them makes no difference. Standard Quantum Mechanics, however, has no resources to ground such a distinction: a distinction between these two states cannot be justified by any of the properties of quantum particles that this interpretation accepts as relevant. It seems thus natural that standard Quantum Mechanics would need an extra postulate to neutralize the consequences of such an assumption, whereas other interpretations accepting more structure and thus in a position to ground this distinction would be able to dispense with such an extra postulate. I test this hypothesis on Bohmian Mechanics and Quantum Field Theory and conclude that the Symmetrization Postulate should not be considered as an extra, dispensable postulate that justifies privileging a theory over another but as a mere acknowledgement of a case of referential ambiguity. When it comes to systems of identical particles, the reference of labels to states describing indiscernible particles is no longer uniquely defined and the consequences of these labels must therefore be neutralized.

1.3.3 On Robustness in Cosmological Simulations

In the last chapter, I focus on whether artifacts can be eliminated by robustness analysis. I argue that robustness analysis fails to distinguish physical predictions from numerical artifacts in cosmological simulations. This criticism is supported by the following two arguments: first, robustness analysis in the form of convergence studies does not deliver a sufficient criterion for identifying trustworthy predictions; and second, artifacts can sometimes produce the convergence supposed to exclude them. Furthermore, the success of other methodologies, like code comparisons, that pretend to identify robust predictions is undermined by a tension inherent to these methods. Indeed, code comparisons require a common, generalized infrastructure for comparing different codes to ensure that apples are compared to apples on one hand, but this common infrastructure contradicts the diversity needed for robustness analysis on the other hand. If the diversity of models is favoured, then one cannot guarantee that the simulated systems under comparison are actually the same. But if the emphasis is put on the common infrastucture that will provide such a guarantee, then the diversity of models, based on different assumptions, that constitute the essential tenet of robustness analysis is lost. The last part of this chapter suggest a new methodology for replacing robustness analysis in the diagnosis of artifacts, based on the work of van den Bosch and Ogiya (2018) and Baushev et al. (2017). I refer to this methodology as that of crucial simulations, meant to put the numerical or physical origin of a prediction under a crucial test. I define a 'crucial simulation' as a kind of simulation which proposes an idealized, simplified scenario where a physical hypothesis can be tested against a numerical one, by allowing the observation of a prediction P drawn from one of the hypotheses and absent from its rivals. The observation of the phenomena P in the outcome of the simulation then disproves one of the alternatives, thereby confirming the other. Finally, I assess how this methodology fares against Duhem's famous criticisms of crucial experiments. I suggest to re-direct the use of code comparisons such as to supply crucial simulations with relevant alternative to test, in order to counter traditional worries about the possibility of non-conceived alternatives.

Chapter 2

Theoretical equivalence, Realism, and the Structure of our Scientific Theories

2.1 Introduction

Imagine a cosmological theory where our universe rests on the back of four elephants, themselves standing on the back of a giant turtle, swimming in the void. This theory is constructed in such a way that it makes exactly the same predictions as the standard cosmological model, about all the observable phenomena. They are distinct theories, in that they deliver different pictures of the world, but there is no observation that would allow to discriminate between these two theories. Consider now that you are a scientific realist, i.e., that you think that a theory aims at describing how the world truly is, including that which we cannot observe. How can you reconcile your realism with the existence of two theories that cannot both be true yet say the same things about what can be observed? One way to face this challenge is to deny that these theories describe the observable world in the same way; they look equivalent only because of limitations of the technology available to us. Another way is to show that they are variants of one and the same theory, hidden underneath different clothing. The elephants and turtle are not physically relevant parts of the theory, but simply an evocative image meant to help scientists represent the Universe. If you choose this identical rivals strategy[1] –as many philosophers have since the empirical equivalence argument was fully fleshed out by Van Fraassen (1980), then you have to provide a criterion to determine what is physically relevant and what is not, apply it to your theory, and proceed to pair together formulations sharing the same theoretical content.

The proliferation of allegedly rival theories has been mostly addressed *via* the latter strategy, i.e., *via* the development of formal tools to prove their theoretical equivalence. Many challenges arise, however, for such a strategy. As shown by Coffey (2014), there is no agreement on an unambiguous criterion for theory equivalence, and more broadly on which theories to pair as theoretically equivalent. Indeed, finding such a criterion requires that the two theories under comparison agree on what counts as physically significant, be it a directly observable posit or an unobservable entity whose explanatory power warrants its inclusion in the theoretical content of the theory from a realist point of view. What appears as superfluous structure from the point of view of a theory–e.g., the extravagant fauna that carries the universe in our example from the point of view of our current cosmological model– might play a decisive role within this theoretical framework. In the Huron cosmogony, for instance[2], the earth rests on the shell of the turtle. According to a story related in Thwaites (1898, 73), solar eclipses were explained by the Hurons by the fact that the movements of the turtle brought its shell in front of the sun. Removing the turtles in this context would leave some natural phenomena unexplained within their cosmogony. Given the argument developed by Magnus and Frost-Arnold suggesting that the identical rivals strategy is only appropriate in cases where a parsimonious ontology is tenable; that is, when the structure on which the disagreement bears is superfluous and can therefore be 'occamized', one can already see the difficulty in applying this strategy when rival theories do not agree on which parts of these theories constitute superfluous structure. This, however, does not constitute the only difficulty that the theoretical equivalence strategy faces in its traditional use as a solution to underdetermination. In this paper, I argue that using theoreti-

[1]This is the term used by Magnus and Frost-Arnold 2010 in their recent paper. I will refer to this strategy as the "identical rivals" strategy or the "theoretical equivalence" strategy.

[2]See the Wyandot Huron legend.

cal equivalence in such a way presupposes that we have a self-standing vocabulary to elucidate the structure of one of the theories that can also be applied to the other without distorting or misconstruing its content. In many cases however, such an assumption is not practical: we do not have in practice a way to formulate claims of equivalence that is not parochial and thus does not favor one of the theory while giving an impoverished version of the second. Moreover, if this is true, then claims of equivalence will often be formulated in the language of the theory which has the thinner notion of content. Indeed, whereas one can conceive to map each physically significant quantity and theoretical terms of the thinner theory into the richer one, the same could not be done the other way around. Such a claim of theoretical equivalence would be asymmetric, since the bijective mapping would no longer hold if starting with the theory with the richest content. As a result, proofs of equivalence that rely on the assumption that a criterion of physical significance can be formulated in a neutral way may be established at the cost of neglecting precisely what is interesting in the other, i.e., what cannot be modelled in the rival framework and could lead to fruitful results when bringing this theory to maturity.

I will ground my criticism of the traditional use of theoretical equivalence in one of the most recent examples of the identical rivals strategy–the alleged proof of theoretical equivalence between a parafield theory and a theory with ordinary bosons and fermions–as an illustration of how attempts to provide proofs of equivalence to solve an alleged underdetermination problem tend to be based on an impoverished reconstruction of one of the theory. I start this paper by reconstructing the history that led to the development of a paraparticle program in Quantum Field Theory. I insist on the late developments of the program, and on what sense it has been turned into an experimental program after the 1980's. Section 2.3 presents the proof of theoretical equivalence between a paraparticle theory and a theory with ordinary bosons and fermions obtained by Baker et al. (2014)–the so-called "equivalence thesis". Section 2.4 examines the motivations underlying the development of Algebraic Quantum Field Theory (henceforth AQFT) and the extent to which these motivations shape its resources, its vocabulary and the scope of the equivalence thesis. In section 2.5, I investigate what status should be attributed to parafield operators such that a case of underdetermination arises and whether AQFT has the resources to describe such a case, before hinting at another way of thinking about theoretical equivalence that leaves aside problems of underdetermination. Thinking of equivalence, as I suggest, as the search for a restricted domain where the rival theories are maximally equivalent is a more fruitful way to explore the structure of the theories, in that successful and failed attempts to stretch the conditions under which this degree of equivalence holds will provide insights into the role played by each part of the theories.

2.2 The Coloured Quark Model and the Paraquark Model: how a mathematical construction found a physical application

In this section, I reconstruct the history behind the parastastistics[3] model from the 1950's to the 1990's. There are really two distinct debates about paraparticles, one focusing on the equivalence of the quark and the paraquark model; the other focusing on how a parastatistics program can be used to test the Pauli's Exclusion Principle. The history of the rivalry between the quarks and parastatistics models has been previously detailed in French (1989), French and Krause (2006), Pickering and Cushing (1986), but their analysis is limited to the time period between the 1960's and the early 1970's–i.e., to the time when the coloured quark model was definitely accepted. It is certainly true that the parastatistics model was, by the beginning of the 1970's, not a genuine rival for the colour quark model. While the latter received more and more empirical support from experimental data[4], the former could not be made gauge invariant, and failed to propose experimental tests that could possibly confirm it. Whether

[3]Particles are defined by the kind of statistics they obey. Paraparticles are those particles that obey parastatistics, as opposed to Bose-Einstein statistics (for bosons) and Fermi-Dirac statistics (for fermions).

[4]See Massimi (2005), section 5.3.2 for a detailed presentation of the experimental successes of the colour model.

the reasons for choosing one over the others were sociological, as argued by Pickering[5], or epistemic –such as the fruitfulness of the former–, at that time the parastatistics program could not really compete. However, in the 1980's, several major steps were undertaken that put this program in a much stronger position often ignored in the literature, given that the coloured quark model had already been adopted by a vast majority of physicists. After providing a brief overview of the early stages of the program (section 2.2.1), I focus on the development of the program after the 1980's and on the efforts to indirectly observe paraparticles (section 2.2.2). In 2.2.3, I describe three recent developments of parafield programs and the sense in which these programs are *experimental* programs.

2.2.1 From the statistics problem to parafermions

In February 1964, at the Brookhaven National Laboratory (Barnes et al., 1964), the Ω^- particle was observed for the first time. This observation became quickly a remarkable challenge for the newborn quark model, as devised by Ne'eman (1961) and Gell-Mann (2015). The Ω^-, as a baryon,[6] is constituted of three quarks, each of which with a strangeness $S = -1$, a fractional charge $z = -1/3$, and a spin $-1/2$. According to the spin-statistics theorem, which connects the spin of particles to the statistics they obey, particles with half-integer spin are fermions, obeying Fermi-Dirac statistics; while particles with integer spin are bosons, subject to Bose-Einstein statistics. Ω^- should therefore satisfy Pauli's exclusion principle (hereafter PEP), according to which no two fermions can occupy the same quantum state. This baryon however is made of three equivalent quarks, three fermions in the same state. This was the first violation of the PEP since its formulation by Pauli in 1925 [7] and became quickly known as the "statistics

[5]Pickering explained the appraisal of the quark model over the parastatistical one based on how"obscure" and how "unfamiliar" the latter was considered (Pickering and Cushing 1986, 218-220; French and Krause 2006, 136). In other words, according to Pickering, only conservatism drove this choice. For a criticism of this thesis, see French (1995).

[6]Baryons are those composite particles composed of a triplet of (anti)quarks; and quarks themselves come in three different "flavours"–*up*(u), *down*(d) and *strange*(s).

[7]For a detailed history of how Pauli's initial *Ausschliessungregel* became the exclusion principle, see Massimi (2005), chapters 2-4.

problem".

Physicists explored two ways of meeting this challenge. One was to retain the exclusion principle and to develop a model that could reconcile this principle with the data–this was the road taken by the coloured quark model, whose strategy relied on the addition of a gauge degree of freedom –the "colour"– to quarks. The other way was to revoke the strict validity of the PEP and to generalize statistics to include intermediate statistics, between Bose-Einstein and Fermi-Dirac statistics. In this case, fermions would still obey the PEP, but particles obeying generalized statistics would not–they would satisfy instead a generalized version of it. Greenberg and Messiah were the major defenders of this so-called "parastatistics"[8] program, initiated by Green back in 1953. Green was focusing on generalized statistics as a way of relaxing what he described as the "rigid structure of field theories". The generalization of the methods for quantizing fields, departing from the Bose-Einstein and Fermi-Dirac quantizing scheme, was the path he chose to achieve this goal of 'loosening' the mathematical structure of Quantum Field Theory. In Quantum Field Theory, creation and annihilation operators have to satisfy the following bilinear commutation relations:

$$\begin{array}{ll} \{a(k), a(l)\} = 0 & [a(k), a(l)] = 0 \\ \{a(k)^\dagger, a(l)\} = \dfrac{1}{2}\delta_{kl} & \left[a(k)^\dagger, a(l)\right] = \dfrac{1}{2}\delta_{kl} \\ \text{for fermions,} & \text{for bosons.} \end{array} \tag{2.1}$$

Following-up on Wigner's remark that the equations of motion do not uniquely determine the commutation rules, Green noticed that trilinear commutation rules such as the following would

[8]The term "parastatistics" first appeared in Dell'Antonio et al. (1964).

also satisfy the equations of motion:

$$
\begin{aligned}
&[a(k),[a(l),a(m)]] = 0 && [a(k),[a(l),a(m)]] = 0 \\
&\left[a(k),\left[a(l)^\dagger,a(m)\right]\right] = \frac{1}{2}p\delta_{kl}a(m) && \left[a(k),\left\{a(l)^\dagger,a(m)\right\}\right] = \frac{1}{2}p\delta_{kl}a(m)
\end{aligned}
\tag{2.2}
$$

A representation of the creation and annihilation operators obeying these rules can be given through Green's *ansatz*:

$$a_k = \sum_{\alpha=1}^{p} a_k^{(a)} \tag{2.3}$$

where operators with equal values of α (or "Green index") obey the usual (anti)commutation rules and operators with different values of Green index obey abnormal (anti)commutation relations. This generalized quantization procedure, however, was then "a solution in search of a problem" (Pickering and Cushing, 1986), i.e., a mathematical possibility with no known application. No particles had ever been observed that satisfied these relations. It took actually an entire decade for somebody to follow up on Green's proposal and to apply these trilinear commutation relations to the statistics problem.

In 1964, Messiah and Greenberg published two decisive papers for the paraparticle model; one in non-relativistic Quantum Mechanics (Messiah and Greenberg, 1964), the other in Quantum Field Theory (Greenberg, 1964). In the first paper, they showed that Dirac's requirement that the wavefunction describing systems of indiscernible particles be either symmetric or antisymmetric is stronger than required by the mere indistinguishability of those particles. Indeed, they argued, indistinguishability only requires the observables to be permutation invariant. However, more particles than the ones accepted at that time potentially satisfy this weaker requirement. These particles includes bosons, obeying fully symmetric statistics; fermions, obeying fully antisymmetric statistics; but also paraparticles, obeying more complicated, partially symmetric, statistics. In the second paper, they extended this result to relativistic Quantum Mechanics, using the trilinear commutation rules suggested by Green to represent respectively para-Fermi particles (2.2, left side) and para-Bose particles (2.2, right side) of order p—"p" is the maximum number of particles possibly occupying an antisymmetric state in the case of para-bosons, the maximum number of particles occupying a symmetric state in the case of para-fermions. Paraparticles of order p=1 correspond to usual statistics, which turns bosons and fermions into special cases of the generalized paraparticle model. As soon as the Ω^- was discovered and the statistics problem emphasized, Greenberg connected this violation of PEP with the parastatistics program he had been developing, thus suggesting to solve the statistics problem by considering quarks as parafermions of order 3 (Greenberg 1964, 599-600).

2.2.2 Can a paraparticle be observed?

The parastatistics model was thus immediately associated with the search for violations of Pauli's principle. At first sight, such a violation should be easily observed, at least for para-fermions of order 2, such as, e.g., electrons: changing the electronic configuration of atoms by allowing double occupancy of a quantum state would change the chemical properties of atoms. No model for exploring such violations, however, were available, for a reason made explicit by Greenberg and Mohapatra in 1987. What considerably slowed down experimental research on PEP's violations was Messiah and Greenberg's demonstration that superselection rules forbid transitions from states with bosons or fermions and at most one non-bosonic or fermionic particles to states with more than one non-bosonic or fermionic particles. This superselection rule provides severe restrictions on the possibility of observing paraparticles and is pretty straightforward in the context of non-relativistic Quantum Mechanics. Remember that quantum particles are indistinguishable, i.e, that they share properties such as charge, mass, and spin. If two particles are strictly indiscernible, then it cannot make any difference whether they are permuted or not. Therefore, the observables for a system of N indistinguishable particles must be permutation invariant and so must commute with the permutation operator:

$$[\hat{A}, \hat{P}] = 0 \tag{2.4}$$

for all observables $\hat{A}$ and all permutation operators $\hat{P}$ belonging to the permutation group S_n. In particular, the Hamiltonian must be invariant under the permutation of two particles and thus cannot change the permutation symmetry of the wave-function: as stated in Amado and Primakoff (1980), once a boson (fermion), always a boson (fermion)![9] This argument rules out any possible transition from a normal state to an anomalous state violating the PEP, or from an anomalous state to a normal state. It constitutes a serious obstacle for turning the parastatistics program into an experimental program. It took actually almost two decades to overcome the no-mixing statistics argument. Only in 1987, Ignatiev and V.A.Kuzmin succeeded in constructing a single oscillator model allowing for the double occupancy of quantum states that Greenberg and Mohapatra immediately analyzed as consistent with parafermions of order 2. This model was the starting point of every experiment conducted after 1990 to test Pauli's exclusion principle (i.e., Ramberg and Snow 1990, Deilamian et al. 1995, VIolations of Pauli's principle experiments VIP1 and VIP2 still happening at the Laboratory Nationali del Gran Sasso, etc.).

In Ignatiev and Kuzmin's (hereafter IK) single oscillator model, a β parameter is introduced such that, when β=0, the PEP is satisfied and when $\beta \neq 0$, then violations of the PEP are possible but suppressed by some power of β.[10] Greenberg and Mohapatra's insight was to use Green's ansatz to turn the IK model into a local quantum field theory of parafermions, where a can be

[9]This means that violations of the PEP need to be introduced more subtly than by considering the Hamiltonian as the sum of a statistics-conserving term and a statistics-violating term as is the case for parity or charge violations.

[10]The model is defined as follows:

$$\begin{aligned} &\alpha^\dagger |0\rangle = |1\rangle, && \alpha|0\rangle = 0, \\ &\alpha^\dagger |1\rangle = \beta|2\rangle, && \alpha|1\rangle = |0\rangle, \\ &\alpha^\dagger |2\rangle = 0, && \alpha|2\rangle = \beta|1\rangle, \end{aligned} \tag{2.5}$$

which gives the following trilinear commutation relations:

$$\begin{aligned} &\alpha\alpha\alpha^\dagger + \beta^2\alpha^\dagger\alpha\alpha = \beta * a, \\ &\alpha\alpha\alpha^\dagger + \beta^4\alpha^\dagger\alpha\,\alpha = \beta^2\alpha\alpha^\dagger\alpha, \\ &\alpha\alpha\alpha = 0. \end{aligned} \tag{2.6}$$

read as a parafermi operator of order 2 modified by a factor $1-N_0$, with N the number operator, to prevent double occupancy; and then equipped with the term βN_0, allowing double occupancy proportional to β. In other words, from 2.2.6 it follows that $\beta = 1$ acts as a parafermi operator of order 2 would, while $\beta = 0$ acts like a normal Fermi operator. Given the $1 - N_0$ mechanism, Greenberg and Mohapatra call this construction a "hindered parafermion of order 2"–for short, a "paron". Such a construction allows for two possible kinds of experiments searching for anomalous to anomalous transitions, in order to put bounds on β :

- Exciting atoms and observing their spectra. The normal configuration of a helium atom in its ground state is $1s^2$. If such an atom exhibits paronic electrons, then it could be in the anomalous state $1s^3$. Thus, it would behave chemically like a $Z - 1$ atom–here, hydrogen–in that it would accept another electron in its K shell, but would exhibit spin 1, unlike hydrogen which has a spin $\frac{1}{2}$. Given the symmetric wavefunction of the anomalous helium, its energy level would be shifted[11], thus allowing to detect anomalous spectral lines by spectroscopy.

- The second kind of experiments involves bringing slow electrons in contact with an atom and look for photons emitted with probability β^2 from the transition of an electron in a high PEP-violating state to a low-lying such state.

The two kinds of experiments listed above identify possible observations of a parafermion of order 2. However, there is no corresponding model for parafermions theories applied to quarks, since the baryon spectroscopy predicted by this theory and predicted by Greenberg is observationally equivalent to a colour model–in both cases, the baryon states are symmetric with respect to their observable degrees of freedom (spin, flavor) but antisymmetric with re-

Finally, an analogue of the the no-particle and the single-particle states for Fock-representations are defined by

$$\alpha\,|0\rangle = 0 \tag{2.7}$$

and

$$\alpha\alpha^{\dagger}\,|0\rangle = |0\rangle \tag{2.8}$$

for IK oscillators.

[11]The amount by which it would be shifted as been calculated by Drake in 1989.

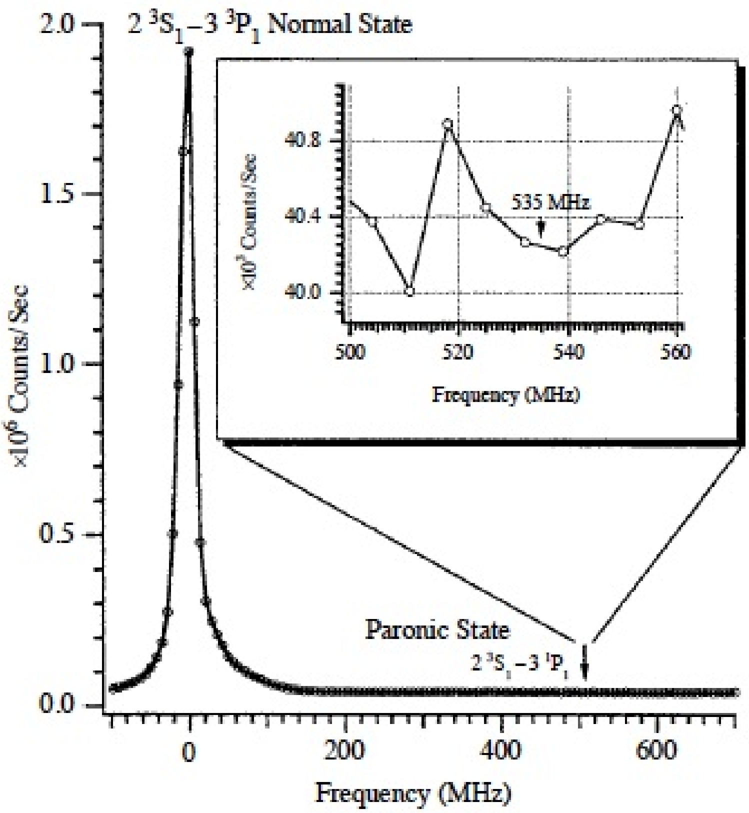

Figure 2.1: Deilamian et al.'s experiment: fluorescence spectrum of helium over the region of interest, where paronic spectral lines were predicted.

spect to an unobservable internal degree of freedom. It is only in their respective extensions, as we will see in 2.2.3, that these theories empirically differ.

2.2.3 Testing the empirical adequacy of parafield theories

Parafermions theories of order 2: parastatistics applied to electrons

Experiments of the first type were performed by Deilamian et al. (1995). Using Drake's predictions for an anomalous helium atom's energy level shifts, they irradiated a beam of helium atoms by photons and focused on the anomalous spectral lines. No such lines were detected (See Figure 2.1).

The second kind of experiment, this time focusing on detecting forbidden transitions, was performed in 1990 by Ramberg and Snow. The idea was to focus on the x-rays that would be emitted if an electron was cascading down to a 1s state already doubly occupied, using Greenberg and Mohapatra's β's parametrization for anomalous states. The experiment, run at Fermilab during two months, consisted in detecting the eventual shift of x-rays when external electrons were supplied to a strip of copper–i.e., when electrical current was passing through the copper strip. It did not yield any positive result for the paronic program, but instead reduced the limit put by Greenberg and Mohapatra on "the probability that a new electron added to an antisymmetric collection of N electrons to form a mixed symmetry state rather than a totally antisymmetric state" (Ramberg and Snow 1990, 438) from $\leqslant 10^{9}$ to $\leqslant 10^{-26}$. This was not the end of the paronic program, still used in two ways: one is to further reduce the bound on the β parameter and thus strengthen the experimental justification of Pauli's principle. This is for instance done in the VIP_1 and VIP_2 experiments, currently underway in Europe at the LNGS (Shi et al., 2016). The other extension of the program consists in introducing infinite statistics that interpolates between para-Bose and para-Fermi statistics, using a q-mutator going from 1 to -1 that averages over Bose and Fermi statistics. Such a program, usually referred to as "quon-statistics", is still connected to the initial parastatistics program, inasmuch as the para-Fermion and para-Bose algebras are recovered for $q = -1$ and $q = 1$ respectively (Meljanac et al., 1996), but it cannot be made relativistic (Greenberg, 2000), (Greenberg and Mohapatra, 1989).

Instead of a confirmation of the para-Fermion and paronic program, experiments carried out to test possible violations of Pauli's principle after the 1980's turned into a confirmation of the PEP. By itself, it constitutes an interesting result, since no quantitative test of the principle could have been done without the rival parastatistics program. However, this interesting result should not make us forget that what it showed is the empirical inadequacy of the parastatistics program as an experimental program.

Parafermions of order 3: Parastatistics applied to Quarks

For our review of the further developments of the parastatistics program to be complete, a last attempt at formulating a proper rival to the colour model must be considered: the locally gauge-invariant formulation of parastatistics given first by Greenberg and Macrae (1983), then Govorkov (1982) and Govorkov (1991). Remember that it is sometimes suggested that the paraquark model was eventually abandoned because, although equivalent to the colour, it was sufficiently “obscure”, “unfamiliar” (Pickering and Cushing 1986, 218-220) and “disagreeable” (Hartle and Taylor 1969, 178) to discourage physicists to pursue it. Historically however, the interest in parastatistics only faded away after the SU(3) theory was gauged, while the paraquark model could not be. As stated in French (1995), the paraquark model was eventually dismissed precisely because it could not be made gauge-invariant:

> What is important from our point of view is that the colour model was able to be gauged whereas the parastatistics theory was not. This fact is acknowledged by Greenberg himself (...): ‘The SU(3) colour theory became more popular that the parastatistics version because (a) the former is more familiar and easy to use, and (b) up to now nobody has been able to gauge the parastatistics theory, while the gauging of the SU(3) colour gives quantum chromodynamics. Let me be explicit, the two theories are equivalent quantum mechanically, but they are apparently not equivalent from the standpoint of quantum field theory’(private correspondence). It is left open as to which factor carried more weight (1995, 103).

Why was the gauging of the SU(3) colour quark model such a decisive factor for the success of the coloured quark model? Gauging the SU(3) was crucial in unifying the colour quark model with the strong force. Like Greenberg’s proposal to consider quarks as parafermions of order 3, the model proposed by Han and Nambu (1965) consisted in adding an extra degree of freedom to quarks, to antisymmetrize their states. Unlike Greenberg’s model though, this extra degree of freedom was conceived as a *gauge* degree of freedom: their colour model was an ordinary field system with global symmetry. This toy model became Quantum Chromodynamics (henceforth QCD) only when the global gauge symmetry U(3) was replaced by a local SU(3) one: the colour now had a dynamical role to play since the local colour charge couples with the strong force. Such a coupling requires eight "colours", i.e., eight types of gluons[12], given that the number of generators of a SU(3) group is $N^2 - 1$. As noticed by Greenberg[13], Freund and Govorkov, only an SO(3) group can be gauged using Green's ansatz, and if SO(3) is understood as the basis of gauge colour, then only three gluons[14], or types of colors, would be allowed.[15] Furthermore, as demonstrated by Ohnuki and Kamefuchi (1982, chapter 11), the kinematical properties of paraquarks gauged that way cannot explain the mechanism of quark confinement. An attempt to circumvent the limitations of Green's parafield formulation by using a complex Clifford algebra instead can be found in Greenberg and Macrae (1983). It leads to interesting restrictions on the number of antiparticles physically possible, and thus to a matter-dominated universe due to this particle-antiparticle asymmetry in initial states.[16]

A summary of the parafield theories ménagerie

When the history of the parastatistics is reviewed in the literature, the conclusion drawn is very often that the parastatistics program was eventually given up because this program was equivalent to the coloured quark model, but way more disconcerting. The question of which parafield theories are accurately described in such a way is very often left vague or not addressed. As a way to fill this gap, I summarize in this section which parafield theories are possible, which parafield theories have found interesting applications, and what are the possibilities left for

[12]It seems at first that, given the three possible color charges, nine type of gluons should obtain by combination of them. However (and in rough terms), the linear combination of red/anti-red +blue/antiblue+green/anti-green would give a "white" gluon, i.e., a color singlet gluon not interacting with other gluons and therefore behaving like a free particle. Since such a gluon is not observed, one of the nine combinations must be discarded and only eight gluons accepted.

[13]See the added note in Freund (1976).

[14]The dimension and number of generators of an SO(n) group is $\frac{n(n-1)}{2}$.

[15]The colour-gauge field that couples with the Green index a of the quark fields must belong to the same family as the latter: this means that the gauge field must be a paraboson of order 3 whose internal degrees of freedom take on three possible values.

[16]For a detailed analysis of this asymetry, see Govorkov (1991).

developing interesting parastatistics program.

Roughly speaking, a parafield theory is a theory that generalizes the framework of canonical quantum field theory by extending commutation rules to *trilinear* commutation rules. Since the statistics that particles obey are defined through commutation rules, a parafield theory also corresponds to a generalization of statistics, that recovers Bose-Einstein and Fermi-Dirac statistics for the special case p=1. In order for these generalized commutation rules to be imported within Quantum Field Theory, one must find a way to represent the operators obeying these commutation rules, i.e. the parafield operators that will act on the vacuum state and create the units of charge that can at least naively be interpreted as particles. One way to do this, and the most used one historically, is the Green ansatz that gives a Fock-like representation of parafield operators. The Green ansatz, although *sufficient* for describing parastatistics of order p, is not *necessary*–when the limitations of Green paraquantization where demonstrated for gauging parafield theories, Greenberg and Macrae indeed suggested other possible choices of operators within the context of a Clifford algebra.

When developed by Green in 1953, the parastatistics program was simply a mathematical framework with no known application. The discovery of the Ω^- in 1964 offered to this program its first possible application: if quarks were not fermions, but para-fermions of order 3, then the statistics problem could be easily solved: quarks states could be symmetrized with respect to their visible degrees of freedom, but antisymmetrized with respect to an extra unobservable degree of freedom taking the form of a three-valued charge. Although this program was crucial in developing the idea of a colour degree of freedom, the impossibility of gauging a parafield theory in the context of Green's quantization was an insurmontable obstacle compared to the gauged colour quark model. After the SU(3) gauge symmetry model and strong interactions were unified, two reactions were possible with respect to the parastatistics program:

- One possible reaction was to focus only on the resolution of the statistics problem through a hidden degree of freedom, and hence on their equivalence. From this point of view, i.e., the possibility of symmetrizing baryon states, the two hypotheses, and hence the two potential theories that can be developed on their basis, are equivalent in their predictions: they predict the same spectrum for excited states of baryons, and can equally account for phenomena whose effects depends on the numbers of colors only–the neutral pion decay rate for instance, or the ratio of the cross-section of electron-positron annihilation to hadrons to the cross-section for annihilations to muons pair (see Greenberg 1993, 11-12). Notice however that the predictive equivalence holds only for a restricted domain of phenomena, and ignores part of the known phenomenology of quarks. The proof of equivalence given by Drühl et al. (1970) is formulated in this context, i.e., with a global gauge group and not a local one.

- Another possible reaction was to focus on their differences and consider the paraquark theory refuted, given the impossibility to gauge it except for the special case of SO(3). The basis of QCD, indeed, is the coupling of the extra degree of freedom to the local SU(3) color symmetry and its eight gluons, mediating the strong interactions like photons mediate the electromagnetic force. Freund (1976) for instance, seems to suggest such a reading:

> Unfortunately, not all eight components of the color current are among the Green components of the color of (the paraboson of order three) $j_\mu^{(+)}$ [...], so that no "clever" coupling of $j_\mu^{(+)}$ to gauge fields can lead to an SU(3)-color gauge theory. In view of the present confidence in color gauge theory this looks fatal to the paraquark model (1976, 2323).

Although "refuted" seems to be a strong word in this context, one has to remember that many phenomena that were decisive for the acceptance of the quark hypothesis and for an explication of their phenomenology (for instance, the reason why no free quark had been observed) depend on the dynamical role played by the colour degree of freedom. A model such as the global SU(3) symmetry initially suggested by Han and Nambu does not properly account for the quark phenomenology: as shown by Govorkov (1982), if

> SO(3) is the basis of the color symmetry, then the number of quarks species would be restricted to two. Such a theory would be hard to reconcile with the observation of six flavors of quarks and would impose severe restrictions on quarks/gluons interactions. Likewise, a SO(3) gauged-theory would admit as singlet states not only mesons (a pair quark/antiquark $q\bar{q}$) and baryons (qqq) but also diquarks qq , anomalous baryons $qq\bar{q}$ and quark-gluons qg.

A second possible application was found for the Green's paraquantization with Ignatiev and Kuzmin model. Previously inhibited by the superselection rules constraining the Hamiltonian of a system of N identical particles, possible models for observing violations of the PEP were finally developed in the form of hindered parafermions of order 2 and quons algebra. This program was both an amazing success, in the sense that it provided the first possible test of the PEP, but also a complete failure; for no gross violations of the PEP were observed and models with small violations were quickly shown to be inconsistent. This program still continues nowadays, not because a parafield theory of order 2 is seriously pursued as the empirically adequate or true theory, but because it provides the only model thanks to which high-precisions tests of the PEP can be run. In sum, paraparticle theories of order 2 were interesting and fruitful as a way to justify the PEP inasmuch as they are not equivalent to ordinary particles theories.

I pause here to insist on the fact that the Green's ansatz implies a very specific interpretation of parastatistics, that finds its origin in the initial context where parastatistics were developed. Parastatistics can be interpreted as describing cases of exact degeneracy with respect to a internal coordinate, where indiscernible particles obey parastatistics of order corresponding to the degeneracy number. This is somewhat the interpretation forced upon parastatistics by the use of Green's ansatz. Green's ansatz is a convenient way to give a Fock-like representation of parafield operators, since the parafield operators are represented in the form of the direct sum of operators of ordinary Fermi or Bose fields satisfying anomalous commutation rules, usually referred to as the "Green components". Given that indices of these components are in principle unobservable, it is easy to think about parafield operators as equivalent to ordinary

<table>
<tr><td rowspan="4">Parastatistics of finite order, with p an integer (discrete parastatistics).</td><td>Para-fermions of order 1.</td><td colspan="2">Coincides with ordinary Fermi statistics.</td></tr>
<tr><td>Para-fermions of order 2.</td><td colspan="2">Empirically ruled out for electrons (Deilamian, Gillapsy, Keller, 1995). Offered the basis for developing deformation statistics.</td></tr>
<tr><td rowspan="2">Para-fermions of order 3 and higher order.</td><td>Non-gauged parafield model.</td><td>Quasi-equivalent to an ordinary field theory equipped with a global gauge group.</td></tr>
<tr><td>Gauge-invariant parafield theories.</td><td>*With Green's ansatz: the theory is gauged only up to SO(3). Only three gluons and two species of quarks predicted.
*With Clifford algebra : the theory has an asymmetry between matter and antimatter different from the predictions of QCD.</td></tr>
<tr><td>Parastatistics of finite order, with 0<p<1 (deformation statistics).</td><td>Paronic theories.</td><td colspan="2">Empirically ruled out by the bounds put on β^2 by Ramberg and Snow (1990), VIP1 and VIP2.

Govorkov has shown that such small violations are not consistent, for they include states with negative norms.</td></tr>
<tr><td rowspan="2">Parastatistics of infinite order.</td><td>Quon theories.</td><td colspan="2">Non-local quantum theory.
Still explored as a possible source of small violations of the PEP in NRQM.</td></tr>
<tr><td>Infinite statistics superselection sectors in AQFT.</td><td colspan="2">No realization of infinite field operators known.</td></tr>
</table>

Figure 2.2: The Para-Fermi field Ménagerie

operators with a hidden additional degree of freedom. But there are several things that must not be forgotten. First, an interpretation of parastatistics as degenerate states is not necessary, and it was actually rejected by Greenberg and Messiah in their 1965 paper[17]. Second, even if this interpretation is adopted, such an equivalence is a restricted equivalence, as parafields impose very strong restrictions which are precisely what makes the parafield representation useful heuristically. As we have seen for instance, a gauge-invariant parafield theory only be gauged up to SO(3) using Green ansatz, as the state space thus obtained is not rich enough to model the eight components needed for the colour current, required by the eight-dimensional vector space of SU(3). The Green ansatz representation, that facilitates the importation of trilinear commutation rules into the usual framework of Quantum Field theory, restricts the original state space of Green fields to a Fock space, that contains less states than the former. The Fock representation is only one of infinitely many possible irreducible representations of the commutation rules, singled out notably by the choice of a vacuum state $|0\rangle$ such that this ket is annihilated by all the annihilation operators α_k. Therefore, the Fock-like representation of parafields contains only one state $\alpha^*(k)\,|0\rangle$, whereas the Green fields state space contains $p\,:\,\alpha^*_{1}(k)\,|0\rangle\,,...,\alpha^*_{p}(k)\,|0\rangle$–this is actually why Greenberg and Messiah rejected the degeneracy interpretation. Moreover, the exact degeneracy could be a first approximation of a broken symmetry–and indeed, Green-gauged parafield theories are incomplete, as SU(3) is in this context a broken symmetry. Finally, the Green paraquantization is sufficient to describe parastatistics of order p, but is not necessary; and other locally-gauged invariant parafield theories have been built on different paraquantization schemes.

2.2.4 To be equivalent or not to be

Given the above assessment of the parastatistics program, it seems surprising that Baker et al. were able, in 2015, to prove the theoretical equivalence of *every* parafield theory with an ordinary field one. If the real scope of the equivalence thesis is the non-gauged paraquark

[17] See footnote 7 of this paper and its discussion in Govorkov 1984, section 5.

model, the proof of equivalence would not achieve much, as this theory might be shown to be equivalent only to a toy model of ordinary fermions and bosons, that does not have the resources to account for the strong interaction in terms of the coupling of the colour charge and gluons. We do not have any reason to think that this theory is the most likely to be true, and thus to be realist about it. Its equivalence with a para-field theory does not, if so, create any difficulty for the realist–two distinct theories cannot be true together, but they can certainly both be false. Why then construct the rivalry between the two potential theories, not even fully developed, as a case of underdetermination, when the mature theories are not even empirically equivalent?

If the scope of the equivalence thesis is really as general as claimed, and if every interesting and fruitful aspects of the parafield program is captured by the proof of the equivalence, then Baker et al. would have shown that, given a particular interpretation of a parafield theory, theories that are demonstrably empirically inequivalent are yet theoretically equivalent. Escaping such an inconsistency would hint at a very impoverished definition of the theoretical content of a theory. And indeed, the challenge created by paraparticles is extremely interesting to gain insight into different degrees of equivalence between theories, insamuch as what made these programs worth pursuing was both their equivalence, as hypotheses providing an answer to the statistics probem and the basis for developing new theories, and their predictive inequivalence as providing the first experimental way of testing of the PEP. One can see already how difficult it is to offer a notion of theoretical equivalence that would solve a possible underdetermination problem for the parafermions of order 3 theories while at the same time preserving the fruitfulness of the parafermionic theory of order 2.

2.3 Putting theoretical equivalence to work

The physical equivalence[18] of ordinary quantum field theory and para-quantum field theory was accepted by a large part of the community by the end of the 1970's, despite no rigorous proof provided, based on their predictive equivalence for the baryon spectroscopy and quark properties that only depend on the number of colors. As early as 1961, Araki tried to show that the Green field algebra of a parafield theory could be re-parameterized such as to be made equivalent to an ordinary field algebra–the Green components of the parafield operators can be decomposed into ordinary bosons and fermions operators, but with anomalous commutation relations. Drühl et al. (1970) further extended this work, by showing that, given some global gauge group, both the algebra of observables and the superselection structure could be recovered. Doplicher and Roberts (1990) used their Reconstruction Theorem to show the equivalence of any local field theory with a *complete*[19] parafield theory on their algebra of observables, observables outside the local algebra and the superselection structure of the field system. More recently, Baker et al. have tried to extend this result to show the *theoretical* equivalence between complete parafield theories and ordinary field theories and exclude incomplete parafield theories as not physically admissible. As they put it,

> a proof of the full theoretical equivalence of paraparticle theories with certain theories of bosons and fermions would be the holy grail from the standpoint of the scientific realist. There would be no mystery as to why the latter appear adequate by themselves to describe nature (2014, 938).

In other words, they take it that the availability of both paraparticles and ordinary particles constitute a classic case of underdetermination, in that the theories are predictively equivalent but offer different ontologies, and as such, a threat for scientific realism. Thus, they offer what

[18]We leave the term equivalence unqualified on purpose here, since the equivalence claimed by Haag and Kastler mentioned below is physical equivalence. Given that part of our task is to situate this notion of equivalence in the hierarchy of possible equivalence relationships, we leave open what "physical" amounts to in this section.

[19]'Complete' means, roughly speaking, that the field system must include all the DHR states and only DHR states, i.e., not exclude any of the DHR states but not include any non-DHR states. In other words, complete theories must be local in sense defined by DHR states. I go back to this notion in more details later in this section.

they think is the "holy grail" for the scientific realist: a proof that these two theories are not only predictively equivalent, but also theoretically equivalent–that is to say, that they are notational variants of one and the same theory.

The reasoning underlying their proof is grounded in three steps, that can be formulated as follows:

- The technical notion of quasi-equivalence provides a sufficient condition for theoretical equivalence.

- The DR reconstruction theorem proves that complete (para)field systems are quasi-equivalent to ordinary field systems.

- Only complete field and parafield systems are physically admissible, for incomplete fields do not satisfy the Charge Recombination Principle.

- Conclusion: Therefore, any physically admissible parafield theory is quasi-equivalent to a theory with ordinary statistics.

In 3.1, I start by analyzing the notion of quasi-equivalence on which relies Baker et al.'s argument. 3.2 presents the Doplicher-Roberts theorem and Baker et al.'s conclusion.

2.3.1 Quasi-equivalence

First, let us consider what is involved in the notion of quasi-equivalence. As stressed by the authors, theoretical equivalence is usually taken by scientific realists to be a stronger relation than empirical equivalence, insofar as two theoretically equivalent theories not only make the same observable predictions (as empirically equivalent theories do), but also posit the same unobservable entities. Theoretical equivalence is what is required for the realist to escape the underdetermination argument: since a realist is entitled to the reality of both observables and unobservable entities postulated by the theory she is a realist about, two theories that posit equivalent observables but disagree on the unobservable reality they admit cannot have the same theoretical content.

Does quasi-equivalence meet this condition in the context of Algebraic Quantum Field Theory (hereafter AQFT) within which Baker et al. present their proof? The *motto* behind AQFT is that the whole physical content of the theory is encapsulated in the abstract C*-algebra of observables $\mathfrak{A}$, a collection of operators whose self-adjoint elements denote physical quantities. Two theories sharing their abstract algebra of observables are predictively equivalent, in the sense that the states they accept assign the same expectation values to corresponding observables. Theoretical equivalence, however, requires more than that, since the collection of observables refer only to measurable physical quantities. Baker et al. thus strengthen their notion of equivalence by including some observables outside the abstract algebra of observables: two theoretically equivalent theories must also agree, they say, on which states are physically possible and on the expectation values they assign to operators outside the algebra of observables, the so-called parochial observables.[20]

Since quasi-equivalence is defined as a *-isomorphism between the algebras containing the parochial observables–the Von Neumann algebra that corresponds to a concrete representation of a C*-algebra closed under the weakest topology of the space of bounded operators on a Hilbert space, or weak closure $\pi(\mathfrak{A})^{-}of(\mathfrak{A})$–that preserves the structure of the algebra of observables $\mathfrak{A}$, two quasi-equivalent theories will agree on the expectation values for all observables and all parochial observables. Hence, quasi-equivalence is taken to be a sufficient condition for the theoretical equivalence of field theories by the authors, inasmuch as it includes both the quantities considered physically relevant by the defenders of AQFT and the global "parochial" properties associated with the concrete representation of the canonical commutation relations.

Mathematically speaking, a concrete representation of the abstract algebra of observables

[20]It has been shown (see for instance 2011, 134) that some physically significant quantities are defined in the von Neumann algebra affiliated to a concrete representation ($\mathcal{H}$,π) of $\mathfrak{A}$ that have no correlate in $\mathfrak{A}$. These quantities are referred to as "parochial observables".

$\mathfrak{A}$ is a Hilbert space $\mathcal{H}$ associated with a *-homorphism π from $\mathfrak{A}$ into $B(\mathfrak{A})$, the collection of bounded operators for this Hilbert space. Quasi-equivalence can thus be defined as follows:

> Representations π and π' of $\mathfrak{A}$ are quasi-equivalent iff there is a *-isomorphism α from $(\pi\mathfrak{A})$ onto π' $(\mathfrak{A})$ and α $(\pi(\mathfrak{A})) = \pi'(A)$ for all A belonging to the algebra of observables $\mathfrak{A}$ (945).

2.3.2 The Doplicher-Roberts Reconstruction Theorem and the equivalence of field algebras

The Doplicher-Roberts Reconstruction theorem (henceforth DR theorem) is usually taken to prove that *complete* parafield theories are quasi-equivalent to complete ordinary field systems. I provide here a simplified version of the DR Reconstruction theorem based on Baker et al. (2014), Halvorson and Müger (2006) and Doplicher and Roberts (1990). Baker et al. take this theorem as a starting point, to which they add a physical principle ruling out *incomplete* parafield theories. With the addition of this principle, deemed the Charge Recombination Principle, they consider the DR theorem as becoming a proof of the quasi-equivalence of any physically admissible parafield theory.

The two basic notions of AQFT are a C*-algebra of observables, understood as a collection of operators denoting physical quantities, and a state of that algebra. States assign expectation values to corresponding quantities. Once the algebra of observables is defined, relativistic restrictions are added on those tools by defining a subalgebra $\mathfrak{A}(O)$ of $\mathfrak{A}$ for every region O of Minkowski space-time. The collection of local subalgebras $\mathfrak{A}(O)$ is called a net of observables. The main goal of the DR reconstruction theorem, and, more broadly speaking, of the Dophicher-Haag-Robert analysis of the superselection structure of the theory upon which the DR theorem depends, is to show that all the structure of Quantum Field Theory–field systems and gauge groups included–can be obtained from the mere algebra of observables, granted that this algebra is associated with a privileged representation. In other words, the goal is to show that the superselection sectors of the theory can be derived based on the axioms of AQFT and on the DHR analysis, instead of merely posited.

The DHR analysis starts by suggesting a criterion to determine which representations are physically admissible representations. The idea is to choose a net of observables $\mathfrak{A}$ endowed with a privileged vacuum state ω_0 and its representation $(\mathcal{H}_0, \pi_0)$. In order to satisfy the DHR selection criterion, any representation $(\mathcal{H}, \pi)$ of $\mathfrak{A}$ has to be unitarily equivalent[21] to π_0 and must have finite statistics. Roughly speaking, this means that the representations deemed as physical are those whose states -the DHR states- are "localized", i.e., differ from the privileged vacuum state only locally.[22]

Here begins the DR reconstruction theorem properly speaking. What this theorems shows is that, for any net of observables $(\mathfrak{A}, \omega_0)$, a unique complete[23] field system $(\mathcal{F}, \mathcal{H}, \pi, \mathcal{G})$–with $\mathcal{G}$ its gauge group–can be reconstructed that has normal commutation relations. In other words, for a given field algebra $\mathcal{F}_1$, it is always possible to extract a net of observables such that a field algebra $\mathcal{F}_2$ can be reconstructed that has normal commutations relation and is quasi-equivalent to the former. In technical terms, for two complete fields systems satisfying the DHR condition $(\mathcal{F}_1, \mathcal{H}_1, \pi_1, \mathcal{G}_1)$ and $(\mathcal{F}_2, \mathcal{H}_2, \pi_2, \mathcal{G}_2)$ for $(\mathfrak{A}, \omega_0)$, the representations $(\mathcal{H}_1, \pi_1)$ and $(\mathcal{H}_2, \pi_2)$ are quasi-equivalent:

- If a field system $(\mathcal{F}, \mathcal{H}, \pi, \mathcal{G})$ is complete, then the folium[24] of $(\mathcal{F}, \mathcal{H})$ is constituted of all and only DHR states.

- Since $(\mathcal{F}_1, \mathcal{H}_1, \pi_1, \mathcal{G}_1)$ and $(\mathcal{F}_2, \mathcal{H}_2, \pi_2, \mathcal{G}_2)$ are complete, the folium of $(\mathcal{H}_1, \pi_1)$ is equal to the folium of $(\mathcal{H}_2, \pi_2)$.

[21] Unitary equivalence requires a unitary map U:$\mathcal{H} \to \mathcal{H}'$ such that $U^{-1}\hat{O}_i'U = \hat{O}_i$ for all i. In other words, there is a one-to-one correspondence between the sets of observables associated with the two representations, and another one-to-one correspondence between the sets of state.

[22] Note that, at this stage, non-local field theories such as theory with quon statistiscs and other field theories with infinite statistics are already excluded.

[23] A field system is complete iff every DHR representation of $\mathfrak{A}$ is a subrepresentation of π and every subrepresentation of π is a DHR representation

[24] The folium of a state ω is the set of states that can be expressed as density matrices on the GNS representation associated with this state

- If the folium of $(\mathcal{H}_1, \pi_1)$ and the folium of $(\mathcal{H}_2, \pi_2)$ are equal, then the Von Neumann algebras affiliated with the DHR states are isomorphic, which is tantamount to saying that there is an *-isomorphism between the weak closure algebras of $\pi_1(\mathfrak{A})^-$ and $\pi_2(\mathfrak{A})^-$.

- $(\mathcal{H}_1, \pi_1)$ and $(\mathcal{H}_2, \pi_2)$ are quasi-equivalent.

Hence, Baker et al. consider that the DR theorem can be accepted as a proof of the equivalence thesis, with the restriction that only complete parafield theories are considered: any complete parafield theory satisfying the DHR selection criterion is quasi-equivalent to a complete normal field theory. Baker et al. aim to bring the final stone to this edifice by extending the proof to incomplete field systems. They do so by dismissing, as physically inadmissible, any incomplete (para)field theory, *via* what they call the Charge Recombination Principle. This principle states that we should always prefer a theory that does not violate Charge Recombination:

> given that Q is a physically possible value for the charge of a region, we should prefer (as better motivated or less ad hoc) theories according to which it is physically possible for Q to be the total charge of the whole universe (2014, 954).

This result allegedly proves that *any* physically admissible parafield theory is quasi-equivalent to any ordinary theory with an additional hidden charge degree of freedom with internal gauge symmetry.

Note that this principle rules out a gauge-invariant parafield theory using Green's ansatz, since it only recovers SO(3) as the basis of color symmetry and therefore constitute an *incomplete* parafield theory. SU(3) is a broken symmetry for a Green-gauged theory, which means that there are states in the DHR sector of charge Q that are admissible according to the DHR selection criterion but are not contained in the Hilbert space associated with this parafield theory.

It should also be noticed that the Charge Recombination Principle is not the only thing added to Doplicher and Roberts's proof by Baker et al. An important twist is also made by the move from physical equivalence, as understood by Doplicher, Haag, Roberts and other advocates of AQFT, to theoretical equivalence. In the next section I will analyze the consequences of such a twist in terms of what a realist interpretation of AQFT amounts to.

I contend that, in the version of AQFT they defend, and despite their efforts to extend the physical core of the theory to parochial observables, there is no difference between Baker et al.'s realism about parafield theories and an instrumentalist interpretation of them–no difference that makes a difference, mostly due to the fact that the operationalist motivations that led to AQFT to begin with shaped the structure of the theory in such a way that determining which notions of equivalence can play the role of empirical and theoretical equivalence respectively becomes extremely difficult.

2.4 The resources of AQFT

The idea underlying the use of the theoretical equivalence strategy is to escape the threat of underdetermination by 1) giving a criterion of physical significance that captures everything physically relevant in both theories under comparison, 2) assuming that a vocabulary exists where a proof that every element or aspect of Theory 1 that satisfies this criterion can be mapped into another element or aspect of Theory 2 in a way that preserves the meaning[25] and structure of 1 and 2, and 3) proving that such an isomorphism holds. Let us consider these respective assumptions in the context of AQFT and of the Doplicher-Roberts theorem in particular.

2.4.1 The motivations for AQFT

The core tenet of an algebraic approach to QFT is that all the physical content of the theory is contained in the abstract local observable algebra $\mathfrak{A}$, as opposed to the affiliated concrete representation in Hilbert space. Both in the early paper of 1964 and in the later book *Local*

[25] Sklar has convincingly shown that a formal commonality of form is not sufficient to ground theoretical equivalence, as the meaning of the terms mapped to each other is crucial to questions of equivalence. See the details of the argument in (Sklar 1982,93).

Quantum Physics, Haag and Kaastler emphasize two different motivations for justifying the algebraic *canon*[26]: on one hand, avoiding the problem created by unitary inequivalent representations. This explains the choice to locate the physical content of the theory in the *abstract* algebra of observables instead of locating it in the concrete C*-algebra of observables. On the other hand, the algebraic interpretation is supported by a strong operationalism[27], according to which the physically significant concepts of the theory are the ones analyzable in terms of "operations", i.e., in terms of possible experiments. Both motivations converge into a similar result: a thinner notion of physical content than other quantum field theories, and the embedding of the theory into an operationalist framework upon which it becomes difficult to impose a realist grid.

Segal (1947), who Haag cites as the pioneer of this algebraic approach, suggested that a focus on the abstract algebra of observables could help circumventing the problem arising with the breakdown of the Stone-von Neumann theorem in QFT. In Quantum Mechanics, this uniqueness theorem guarantees that, for any system with a finite number of degrees of freedom, all irreducible representations of the canonical commutation relations (henceforth CCRs) are unitarily equivalent. Schrödinger's and Heisenberg's representations constitute a famous case of competing theories that finally have been proved equivalent thanks to this theorem. But this uniqueness theorem fails in QFT, wherein systems have an infinite number of degrees of freedom. As a consequence, QFT features uncountably many unitary inequivalent representations of the CCRs. Why is this a potential problem for a consistent interpretation of QFT? Unitary equivalence insures that there is a bijective map between the sets of observables associated with the two representations, and another one between the sets of state, so that two unitary equivalent representations associate the same values with corresponding observables. For this reason, they are said to be physically equivalent: they are "simply and unalarmingly different ways of expressing the same (...) kinematics" (Ruetsche 2011, 14). Conversely, many take that

[26]Philosophers tend to insist on another motivation, that of putting quantum field theories on firmer mathematical grounds free of the inconsistencies forcing renormalization procedure upon QFT. See for instance the introduction of Halvorson and Müger (2006).

[27]For a more careful definition of "operationalism", see Bridgman (1927).

representations that are *not* unitarily equivalent are also *not* physically equivalent. Now if unitarily inequivalence implies physical inequivalence, as many authors claim it does, then the choice between two inequivalent representations is not a matter of convention anymore–since two representations which disagree cannot be correct at the same time, then it seems that a representation *must* be chosen over the others[28]. It is not clear though on which grounds–if any–such a choice could be made. Nevertheless, if the whole physical content of the theory is considered to be encapsulated in the abstract C*-algebra of observables, i.e., if nothing of physical importance is located in the CCRs representation, then the problem of unitarily inequivalent representations vanishes altogether. Since UIRs share their algebra of observables, or, in other words, share everything of genuine physical importance according to the AQFT *credo*, unitary inequivalence is not really a problem for proponents of AQFT. Unitary inequivalent representations have no bearing on the physical content of the theory:

> The relevant object is the abstract algebra and not the representation. The selection of a particular (faithful) representation is a matter of convenience without physical implications. It may provide a more or less handy analytical apparatus (Haag and Kastler 1964, 851-852).

> [T]he specification of a special representation is physically irrelevant, all the physical information being contained in the algebraic structure of the abstract algebra A alone...It [interpreting weak equivalence as physical equivalence] shows indeed that the physically relevant object is not a concrete realization of A but the algebra A itself, since any two different concrete realizations (i.e., faithful *-representations, or representations with zero kernel) will be physically equivalent (Kaastler 1964, 180-181).

Thus, one of the most important consequences of locating the physical content of the theory in the net of observables is the formulation of a Quantum Field Theory in a mathematically con-

[28] As mentioned in the introduction, representations disagree only to the extent that one considers absolutely precise states.

sistent framework where the problem of UIRs does not appear. Haag and Kaastler's dismissal of part of the structure of the theory as superfluous dissolves the problem altogether.

The limiting of the physical content of the theory to $\mathfrak{A}$ is also intimately related to what they consider to be the goal of a physical theory, namely, accounting for possible observations and for the outcomes of experiments. This goal is made very clear in Haag and Kastler (1964), where states and observables are defined only in terms of possible experiments:

> We are concerned with two categories of objects: "states" and "operations". The term "state" is used for a statistical ensemble of physical systems, the term "operations" for a physical apparatus which may act on the systems of an ensemble during a limited amount of time producing a transformation from an initial state to a final state. (...) We may say that we have a complete theory if we are able in principle to compute such probabilities for every state and every operation when the state and the operation are defined in terms of laboratory procedures (1964, 850).

As a result, the parts of the theory that do not play any direct or indirect role into computing probabilities, i.e., into generating empirical predictions, are also superfluous–a theory is 'complete' even when dispensing with these theoretical terms. AQFT is thus a theoretical framework where the problem arising with the UIRs is solved by limiting the resources of the theory and adopting a thinner notion of content than that traditionally adopted. Moreover, the theory is motivated by a strong operationalism that not only contribute to the adoption of thinner notion of content, but also taints the entire vocabulary of AQFT. As an example of this operationalist flavour that underlies AQFT, I focus in the next subsection on the concept of equivalence as understood by Haag and Kaastler and their successors, and on how equivalence can be interpreted in realist terms in such a framework.

2.4.2 Equivalence in AQFT

Haag and Kaastler adopt weak equivalence as the appropriate encoding of physical equivalence. Given their operationalism, no distinction is made between theoretical equivalence and empirical equivalence, for no element of the theory that is not accounted for in terms of measurements or results of measurements, i.e., in terms of its observational consequences, is considered physically relevant. Hence, *physical* equivalence is the only notion of equivalence that really matters to them.

Weak equivalence was first introduced in Fell's theorem and is intimately related to the finite accuracy of an experiment. Indeed, given any state associated with the representation ($\mathcal{H}$,π), there is always a state associated with ($\mathcal{H}'$,π'), such that ($\mathcal{H}'$,π') is unitarily inequivalent to ($\mathcal{H}$,π) and both states have the same expectation values, up to some degree of error $\epsilon > 0$. Since the finite accuracy of the experiments does not allow to differentiate between the expectation values corresponding to those representations, then ($\mathcal{H}'$,π') and ($\mathcal{H}$,π) should be considered as physically equivalent for Haag and Kaastler[29], given their explicit operationalism. As mentioned in section 2.3 however, Baker et al. have extended the notion of physical equivalence of Haag and Kaastler such as to include parochial observables, i.e., properties that can only be defined relative to a concrete representation. Whereas Haag and Kaastler consider that weak equivalence is sufficient for physical equivalence, Baker et al. (2014) require a stronger condition, that of quasi-equivalence. If weak equivalence can be interpreted as empirical equivalence, then the stronger degree of equivalence that quasi-equivalence constitutes may include enough content to be a possible candidate for theoretical equivalence. So, is an extension of Haag and Kasstler's notion of equivalence enough to qualify as theoretical equivalence?

Note first how tricky it is to answer this question and how ill-posed it appears in the context of AQFT. Theoretical equivalence collapses into empirical equivalence within AQFT, for the epistemological stance that shapes the theory denies the relevance of such a distinction. Can we

[29] For a similar analysis of Fell's result, see Wallace (2011), 54 and Lupher (2016), 5.

thus reintroduce it consistently within this framework, and, if so, what would be the appropriate encoding of theoretical equivalence? In other words, given the very thin notion of content to which Haag and Kaastler restrict themselves, can weak equivalence really be accepted as empirical equivalence and quasi-equivalence as theoretical equivalence?

Consider first the case of empirical equivalence. Lupher (2016) has already shown that 1) weak equivalence between concrete representations fails to capture cases of observational equivalence, and that 2) some representations that are obviously not physically equivalent yet qualify as weakly equivalent representations. On one hand, physically equivalent representations– in the sense suggested by Haag and Kaastler themselves– do not always meet the condition of weak equivalence. Consider for instance two KMS states whose temperatures differ, but by such a small amount that it cannot be distinguished by any experiment–note how similar this case is to the one underlying the adoption of weak equivalence based on Fell's theorem. The representations induced by these two states fail to meet the conditions of weak equivalence, despite their physical equivalence from an operationalist point of view. On the other hand, weak equivalence deems as physically equivalent theories that are obviously not so. Unitarily inequivalent representations such as those of a free and an interacting system in QFT meet the condition of weak equivalence, thereby qualifying as physically equivalent![30] My point here is not to rigorously demonstrate that weak equivalence is not sufficient for empirical equivalence, but to emphasize that weak equivalence seems too poor of a notion of physical content to satisfy even an operationalist–yet alone a realist.

As we have seen however, Baker et al. extend their notion of equivalence to include more physical quantities than weak equivalence gives access to–notably the parochial observables mentioned in section 2.3. This move seems at first glance well-justified: if quasi-equivalence is stronger than weak equivalence and includes those quantities that a Hilbert space conservative would consider as significant, then quasi-equivalence is a good candidate for theoretical equivalence. Put differently, if one includes in their notion of equivalence more quantities than

[30]Details of the argument can be found in Lupher (2016), sections 5-7.

required by empirical equivalence, then one can be confident that their concept of equivalence captures more than the predictions made by the theory–it captures part of its theoretical content as well.

Such a stance is however problematic, given the strong role they attribute to their equivalence proof, i.e., that of pairing parafield theories and theories with ordinary bosons and fermions as *one and the same theory*. As I mentioned above with the case of free and interacting QFTs, weak equivalence is already too weak to qualify for empirical equivalence, since it pairs as empirically equivalent representations that are obviously not, even from the most radical instrumentalist point of view. Therefore, demanding more than weak equivalence by no means guarantees that the strengthened relation of equivalence offers a solid basis for *theoretical* equivalence. More importantly, although content has been added to the theoretical content considered by requiring quasi-equivalence, it does not mean that the *relevant* content has been included. Indeed, quasi-equivalence still fails in delivering a degree of equivalence that would hold at the level of the field systems where parastatistics are defined. Yet, if a genuine case of underdetermination is at stake, field operators should be taken seriously[31] and included in the scope of equivalence, especially given that a notion of equivalence holding at this level is available in the context of the DR theorem.

The Doplicher-Roberts theorem states that, given a net of observables $(\mathfrak{A}, \omega_0)$, there exists a field system with gauge symmetry $(\mathcal{F}, \mathcal{H}, \pi, \mathcal{G})$ that is complete and has normal commutation relations, and that any complete and normal field system for $(\mathfrak{A}, \omega_0)$ will be equivalent to it up to DR-equivalence. DR-equivalence is a very strong condition, since it requires a unitary

[31]See section 2.5.2.for a detailed analysis of what should be the status of parafield operators for a case of underdetermination to arise.

operator W:$\mathcal{H}_\infty \to \mathcal{H}_\in$ such that:

$$
\begin{aligned}
&W\pi_1(A) = \pi_2(A)W, \forall A \in \mathfrak{A}, \\
&WU(G_1) = U(G_2)W, \\
&W\mathcal{F}_\infty(O) = \mathcal{F}_\in(O)W.
\end{aligned}
\tag{2.9}
$$

Note that the last two conditions require an isomorphism between the gauge groups and between the field algebras. This degree of DR-equivalence is how Halvorson and Müger (2006) define theoretical equivalence in their introduction to AQFT (see 11.3.1-11.3.2), as this condition not only requires an isomorphism between the net of observables such that the structure of these nets is preserved, but also requires unitary equivalence, i.e., a bijective mapping, between the field algebras $\mathcal{F}_1$ and $\mathcal{F}_2$. This equivalence condition is only met by theories with normal commutation relations. If $\mathcal{F}_\infty$ is a parafield algebra, then from it a net of observables can be extracted and a field algebra $\mathcal{F}_\in$ reconstructed that will have normal commutation relations. $\mathcal{F}_\in$ is then *quasi-equivalent* to $\mathcal{F}_\infty$, a condition that does not demand equivalence between the local field systems–and indeed, such a an equivalence would not hold. What is interesting here is that the initial aim of the Doplicher-Roberts theorem was to prove that nothing of physical importance that is not already included in the net of observables can be found in the fields and gauge groups, which therefore constitutes superfluous structure whose utility is at best heuristic. Given a net of observables for a privileged vacuum state, a field algebra can be uniquely reconstructed from the net up to DR-equivalence with any complete field system with normal commutation rules. In other words, the argument goes as follows: if the fields and gauge groups can be fully derived from the net of observables, then they are dispensable structure, inasmuch as they are already 'hidden' in the net of observables and one can be an eliminativist about them. Such a reasoning requires that the reconstructed field algebra is *unique*, i.e., that field systems and gauge groups reconstructed from the net via the DR theorem are equivalent in such a way that they can be paired as one and the same. Yet, when it comes to parafield theories, Baker et al. (2014) lower the standards of the DHR analysis and insist that quasi-equivalence is enough to pair parafield and ordinary field theories as one and the same. How could quasi-equivalence play this role when it ignores part of the theoretical content that obviously mattered for the proof of uniqueness of the field systems?

2.4.3 Equivalent...to what?

Consider now what the conjunction of the previous two subsections, i.e., the conjunction of the resources of AQFT and the consequence of the adoption of quasi-equivalence as a plausible candidate for theoretical equivalence entails for the scope of the equivalence proof. As emphasized by Halvorson and Müger (2006) and Wallace (2011), very often one motivates their choice of working within the context of AQFT by stressing the mathematical rigor of this framework. But this rigor comes at a cost, that of not being in a position to describe any quantum field theories that could exist in nature. Indeed, there is no known algebraic formulation of any interacting quantum field theories in four dimensions, although we do live in a world in four dimensions where no system is in complete isolation and thus free of interactions. But let us leave aside the oddity of formulating a proof of theoretical equivalence in this context for the sake of the argument. Knowing that the chosen context of the proof is AQFT and, within AQFT, the DHR analysis, what does the claim "any parafield theory is equivalent to an ordinary field system" really mean?

The parafield theories that have been historically developed are para-Fermi theories. Remember that our para-Fermi field ménagerie comprehended parastatistics of finite order, deformations statistics (the paronic program) and parastatistics of infinite order. Parastatistics of infinite order include the infinite statistics superselection sectors in AQFT, which do not have a known realization in terms of infinite field operators and are therefore not addressed. The quon theory constitutes a very interesting case, because it is the best theory for exploring small violations of PEP so far, but it fails to satisfy the Haag and Kastler axioms. Quon statistics satisfies the CPT theorem, do have a relativist kinematics for free fields, and obey locality principles such as the cluster decomposition principle. However, they violate the spacelike commutativity of observables required by Haag-Kaastler axioms[32], and are therefore left aside given the context of the proof. The paronic program is also not considered, for the DHR analysis explores para-bose and para-Fermi statistics only of positive integer order.

Only parastatistics of finite, integer order are thus left from the entire possibility space of parafield theories. This leaves us with: para-fermions theory of order 2, which provided the basis for the search of anomalous-to-anomalous transitions in Goldhaber and Deilhamian's experiments; para-fermions or order 3, including the non-gauged paraquark model, the gauge-invariant paraquark model based on Green's ansatz, the gauge-invariant paraquark model based on Clifford algebra, and non-gauged (and -gaugeable) para-Fermi theories of higher order. However, the proof of equivalence only bears on non-gauged parafield theories and excludes gauge-invariant paraquark models.

This already considerably restricted possibility space is then further narrowed down by the adding of two other criteria of physical significance: only theories that are local in a very specific sense, that of the DHR criterion, are addressed. According to the DHR analysis of AQFT, only those states whose charge is localized, i.e., which differ from the vacuum only within some finite region, are admissible states. This criterion excludes perfectly admissible theories with long-range forces such as electromagnetism. Eventually, Baker et al. (2014) top this list of criteria of physical admissibility with a new one, the Charge Recombination Principle, whose role is to exclude incomplete parafield theories.

Now, what about the other side of the debate, that of ordinary field theories? What kind of quantum field theories with bosonic and fermionic commutation rules fall within the scope of the proof? As we have seen already, there are no known algebraic formulations of interacting QFTs in four dimensions, and, as acknowledged by the authors themselves (2014, 962), it is not clear how their proof would extend to interacting systems. Among the free-field QFTs, QED is dismissed by the DHR criterion, as well as QCD which requires a local gauge group

[32]Quon theories suggest interesting connections between the way locality is implemented in QFT and the apparition of parastatistics. I will return to this point in Chapter 2.

and not a global one. Gauged-invariant models developed after the 1980's are also left aside So, what the equivalence really reads is the following: any non-interacting, local in Haag and Kaastler's sense and localized in the DHR criterion's sense, non-gauged parafield theory of finite and integer order that can be given an algebraic formulation is quasi-equivalent to an ordinary field theory with global gauge group that satisfy the same *desiderata*.

Two things should strike us at the end of this analysis. The problem of underdetermination allegedly arises because the world seems to accomodate both a description in terms of para-particles and a description with only bosons and fermions. In other words, both descriptions, although incompatible from a realist point of view given that they deliver two different stories about what the world we live in is made of, capture equally well what we know about the world. But do the theories that survive the criterion of physical significance upon which the proof of equivalence is based really present such an underdetermination problem? Given how restricted the resources of AQFT are, and the added requirements of the DHR analysis, can we construct in this context a rivalry between a parafield and an ordinary theory that could pretend at successfully describing the world? This question is all the more important that claiming that this equivalence solves the underdetermination problem means that nothing physically interesting or relevant has been left aside in the proof. Yet, we have seen in section 2.2 that para-Fermi theories of order 2 have been developed into an experimental program looking for anomalous-to-anomalous states transitions or accumulation in anomalous states–like an electron cascading down to an already occupied state. How come then, if this program is not empirically equivalent to an ordinary theory of fermions satisfying the PEP, that Baker et al. could show the *theoretical* equivalence of the two programs? In sum, the following two questions emerge from our analysis of the resources and the vocabulary of AQFT:

- What should be the status of the parafield operators that introduce parastatistics in AQFT for a case of underdetermination to arise? Is AQFT compatible with such a status, i.e., is AQFT a legitimate framework for constructing and addressing a threat to realism? This question can be rephrased in terms of what the scope of the equivalence should be:

among all the notions of equivalence in AQFT, which one is a reasonable notion of theoretical equivalence that can both play its role of pairing the rival theories as one and the same while being compatible with the idea that parastatistics create an underdetermination problem?

- Are the interesting features of the parafield program preserved in an algebraic formulation of a parafield theory? Or does the proof of equivalence ignore what made this program worth pursuing?

I tackle these two questions in the coming section.

2.5 Underdetermination undermined

In this section[33], I start by identifying the general conditions that must be satisfied for an underdetermination problem to be a threat to scientific realism, before defining more specifically what a realist take on paraparticles amounts to. More precisely, I explain what being a realist about bosons or fermions commit oneself to and extend this analysis to paraparticles, before showing that these commitments are negated by the equivalence strategy. I then proceed to analyzing whether AQFT can capture theories satisfying these commitments and suggest another way out of underdetermination in the case of paraparticles, *à la* Laudan and Leplin (1991).

2.5.1 Let's be real!

The common take on Van Fraassen's argument from underdetermination (1980) consider this problem as a a weapon against scientific realism.[34] This argument states that the existence of empirically equivalent rival theories equally supported by data suggest that we should not believe one of these theories to be true, but rather revise our ambitions and accept both of them

[33]The title of this section is a reference to that of chapter 8 of Psillos (2005a).
[34]See Van Dyck (2007) for another reading of Van Fraassen's argument.

as empirically adequate. It seems reasonable to require, for a case of underdetermination to constitute a genuine threat to scientific realism, that the following conditions are met:

1. The theories must be empirically equivalent theories, in that they make the same *actual* and *possible* predictions. Weak cases of underdetermination are no threat to realism, for a scientific realist can simply decide to suspend their judgment about which of the two is true until further evidence is collected.

2. The theories must be empirically successful: the rival candidates must compete for the title of true theory for a threat to arise. A realist has no difficulties to accept both theories as false.

3. The alleged rivals must disagree on the unobservable reality they posit, i.e., offer two incompatible pictures of what the world is like.

4. The unobservable reality upon which they disagree must have a theoretical role to play, i.e., constitute more than descriptive fluff or superfluous structure. In other words, if a realist accepts as (approximately) true a given theory, and some irrelevant fluff is added to this theory that makes no difference whatsoever either for its observational consequences or for the picture of the world it delivers, then the realist can maintain their belief in the truth of both theories without contradiction.

According to condition 4, paraparticles or parastatistics must have some role to play, i.e., not be mere superfluous structure with no explanatory power for a case of underdetermination to arise. So, what does it mean to be realist about bosons, fermions or parabosons and parafermions?

In non-relativistic Quantum Mechanics, this question is easy to answer: it means that the theoretical terms “bosons”, “fermions”, or “para-bosons and -fermions” refer to something in the world, whose behavior is successfully captured by the mathematical description the theory provides. Since particles are defined by the kind of statistics they obey, “being a boson” means “obeying bosonic statistics”, i.e., “being invariant under permutation”. Particles are bosons if the wave-function describing them is symmetric under permutation. Thus, being a realist about bosons commits oneself to believing that there is, in the mind-independent world described by the physical theory, a particle or structure that actually exhibits such a behavior.

The reasoning in non-algebraic QFT is similar. Since a "particle"[35] in QFT is nothing but the excitation of a quantum field, defined over spacetime, the statistics apply this time to creation and annihilation field operators and are determined by their commutation rules. Consider the action of a bosonic creation operator α^* acting on the vacuum state and generating a system with two momentum states i and j. The creation operator is *bosonic* if it does not matter in which order you are putting a particle in state i or j. Since bosons are symmetric under permutation of particles, creating one before the other or vice-versa must not have any observable consequences. In other words, the creation operators commute:

$$\left[\alpha_i^\dagger, \alpha_j^\dagger\right] = \alpha_i^\dagger \alpha_j^\dagger - \alpha_j^\dagger \alpha_i^\dagger = 0 \tag{2.10}$$

Similarly, a creation operator is fermionic because changing the order in which particles are created will only affect the sign of the state. The fermionic operators $c^\dagger$ anticommute, such that:

$$\left[c_i^\dagger, c_j^\dagger\right] = c_i^\dagger c_j^\dagger + c_j^\dagger c_i^\dagger = 0 \tag{2.11}$$

In both cases, bosons and fermions are defined through their behavior under permutation–permuting two bosons leaves all the observable values and the field operators values invariant; whereas permuting two fermions leaves the observable values invariant but change the value assigned to field operators. The difference between bosons and fermions is really expressed in the behavior of field operators under permutation.

Now, as we have seen in section 2.2, there is another difference between bosons and fermions: the former can be put up to any number into the same quantum state, while no

[35]We leave aside the discussion about whether QFT can be interpreted in terms of particles or not, since the argument stays the same whether we are talking about bosonic particles or bosonic fields. Likewise, the same argument applies whether we talk about some entities referred to as "particles" or to some structure instantiating bosonic or fermionic statistics.

two fermions can occupy the same quantum state. This difference is also expressed in the expectation values of creation operators for a given state. A bosonic creation field operator for a system of N particles will give:

$$\alpha_i^* |n_1 ... n_N\rangle = \sqrt{n_N + 1}\, |n_1 ... n_N + 1\rangle \,, \tag{2.12}$$

when a fermionic creation operator gives:

$$c_i^* |n_1 ... n_N\rangle = (-1)^{\Sigma_N} |n_1 ... n_N + 1 ...\rangle \,. \tag{2.13}$$

Again, the same state will assign different expectation values to bosonic and to fermionic operators (and, likewise, to parafield operators). Hence, it seems reasonable to think that being realist about paraparticles or parastatistics commits oneself to grant some theoretical significance to creation and annihilation field operators, even if they do not correspond to observables. Do the parafield theories under scrutiny in the Doplicher-Roberts theorem attribute some physical significance to these field operators?

2.5.2 The status of parafield operators in AQFT

According to the AQFT *credo*, all that and only that which belongs to the net of observables is physically relevant. Nothing of physical importance can be found in the concrete representation of the abstract C^*-algebra that was not already present there. Where does this leave us with respect to parastatistiscs? Are they physically significant, or, in other words, are they the kind of things a realist should think of as genuinely referring within this framework?

Recall that, in Algebraic Quantum Field Theory, one has to define subalgebras $\mathfrak{A}(O)$ of $\mathfrak{A}$ for every region O, the collection of which is called a net of observables. Then, in order to introduce parastatistical commutation relations within this framework, the algebra of observables $\mathfrak{A}$ is made a subalgebra of the field algebra $\mathcal{F}$, containing the unobservable field operators representing the parastatistical commutation relations along with the observables of $\mathfrak{A}$. The field algebra is then equipped with a gauge group $\mathcal{G}$, such that $\mathfrak{A}$ is the subalgebra invariant under $\mathcal{G}$. At first sight, the distinction between the observable algebra $\mathfrak{A}$ and the field algebra $\mathcal{F}$ seems to fully capture the traditional distinction between the observational terms of a theory and its theoretical terms–i,e., between the terms to which the instrumentalists restrain the content of the theory and the unobservable entities that the realist accept as real. Thus, a realist interpretation of a parafield theory should consider that not only the elements of $\mathfrak{A}$ are real, but also the unobservable fields–or at least those unobservable fields operators that introduce parastatistics into the theory. Parafield operators must be among those theoretical terms that make a difference for the theory in the case of an underdetermination problem, and, as such, included in its theoretical content. Yet, Baker et al. explicitly denies this attitude with respect to unobservable fields:

> What about operators in $\mathcal{F}$ that are not elements of $\mathfrak{A}$? For purposes of adjudicating this question, the term standardly used to refer to these quantities is unfortunate. "Unobservable fields" sound like exactly the sort of thing a scientific realist should be a realist about. But when their theoretical role is taken into account, it becomes far from clear that anyone should accept realism about operators living in $\mathcal{F}$ but not in $\mathfrak{A}$ (Baker et al., 943).

Their main motivation for denying the physical significance of unobservable fields living outside of $\mathfrak{A}$ relies on the fact that those unobservable fields are not gauge-invariant. If a transformation under the internal group of symmetries defined as a gauge group of $\mathcal{F}$ is comparable to a mere coordinate transformations and if unobservable fields are changed by these gauge transformations, then unobservable fields should not be considered as physically significant quantities. My point, here, is not to go against this argument. Rather, it is to show that such an argument does not allow any form of realism about paraparticles, and hence seems to undermine the idea that paraparticle theories create an underdetermination problem to begin with. If you deem only those quantities that are gauge-invariant as physically significant, and if in the framework you chose field operators describing parastatistics are not, then you are not a realist about paraparticles–you do not think that 'paraparticle' is a theoretical term that makes a difference within your theory and genuinely refers.

The proof of theoretical equivalence is thus obtained at a very high cost. First, it holds only because the domain considered is extremely restricted, actually too restricted to account for the phenomelogy of quarks as we know it nowadays. As a result, the theories under comparison in the proof looks like toy theories rather than fully developed theories brought to maturity about which a realist can be realist about. Finally, the dismissal of parts of the possibility space of parafield theories at each step of the proof leads to losing the capacity to model the interesting features of parafield programs that made them fruitful programs to develop. Is this cost worth it, when a safer and simpler strategy is available: to bet that their empirical equivalence will eventually break down?.

2.5.3 Escaping the underdetermination à la Laudan and Leplin

Be it from the point of view of the status of parafield operators or from the point of view of how the degree of equivalence that quasi-equivalence constitutes, parastatistics have never been included in the physically significant quantities or theoretically useful terms of the theory in the context of the proof of equivalence. The identical rivals strategy pairs parafield theories and theories with ordinary bosons and fermions as one and the same *inasmuch* as statistics are considered as descriptive fluff. Appealing to this strategy is thus tantamount to denying that the availability of a paraparticle theory creates a serious case of underdetermination. In other words, the theoretical equivalence strategy is successful to the extent that the ontological or physical significance of the terms upon which the disagreement bear is denied, and the case of underdetermination undermined.

In what remains, I want to insist on the fact that there are no good reasons for a realist to dismiss parastatistics as superfluous structure, and that doing so comes at a significant cost. More precisely, I want to insist on the fact that at least some of the parafield theories were developed into experimental programs and that this point is totally missed by the equivalence proof. Such a fact hints at how impoverished the notion of content used in this proof really is.

According to Magnus and Frost-Arnold (2010), two conditions can help in deciding whether a disagreement on the unobservable reality posited by rival theories is an ontological disagreement or a mere verbal difference. One is the future discriminability condition, according to which the identical rivals strategy is untenable " if future developments will allow for scientists to observationally distinguish between the rival theories" (2010, 5.1). The second is that of heuristic utility, which states that the identical rivals response is inappropriate "when the peculiar posits of a theory are heuristically useful and guide scientists in developing the theory" (ibid, 5.2). None of these conditions is satisfied in the case of paraparticle theories. Although at the time when the non-gauged paraquark theory and the colour quark model with a global gauge group were true rivals, i.e., competing for the title of the 'correct' theory, paraparticle theories had yet to be turned into an experimental program, the experiments of Goldhaber and Ramberg and Snow, followed by the discovery of the IK oscillator model in 1987 and its further use by Greenberg and Mohapatra (1987) changed the situation. Not only were experiments designed to observe anomalous-to-anomalous transitions within the theoretical framework of para-fermi theories of order 2, but these experiments–and their negative results–were instrumental in the eventual confirmation of Pauli's exclusion principle. The confirmation of Pauli's exclusion principle could not be accounted for if one were to claim that para-Fermi statistics do not make any physical difference compared to fermionic ones.

Likewise, after 1983, two ways of gauging a parafield theory were suggested, based respectively on the Green and Clifford algebras, that gave birth to gauge-invariant paraparticle theories not empirically equivalent to QCD. Remember that QCD was born from the unification of quarks and the strong force, and that such a unification required the coupling of the extra 'colour' degree of freedom with the gluons. Since the Green-based gauge invariant formulation of a parafield theory of order 3 has a SO(3) instead of a SU(3) symmetry, the theory predicts only three gluons, which means that if a full coupling is required to preserve the property of asymptotic freedom, then the numbers of species of quarks must be limited to two.[36] However, six flavors of quarks have been observed to this day, the last one in 1995.[37] The Clifford-based parafield theory does recover this full phenomenology, but violates the C-symmetry and predicts an imbalance between matter and antimatter. In sum, the paraquark toy model developed in the 1960's is no longer empirically adequate, for it only accounts for a restricted number of phenomena compared to the phenomenology of quarks presently known. The parafield theories that eventually superseded this initial program are demonstrably not empirically equivalent to their ordinary field rivals: para-fermionic theories of order 2 and the Green formulation of gauge-invariant parafield theories of order 3 are actually empirically inadequate.

Rather than asserting that parastatistics introduce superfluous structure in the theory as a way out of an underdetermination dilemma, a safer bet and more obvious strategy to escape the underdetermination is thus to deny the empirical adequacy of paraparticle theories. Not only we have good reasons to attribute physical significance to parastatistics and to reject their 'occamization', but the experimental triumph of one of the most important principles in Quantum Mechanics, the Exclusion Principle, would be a mystery if paraparticles made no observational difference compared to a theory of ordinary bosons and fermions. However, AQFT does not seem in a position to model these experimentally and empirically interesting features of para-field programs.

2.6 Concluding remarks

In the previous section, I have argued that the proof of equivalence provided by Baker et al. (2014) can be interpreted as a proof of theoretical equivalence only at the cost of dismissing parastatistics as superfluous structure, given that the unobservable field operators that introduce parastatistics within the theory are not considered physically significant quantities, and the equivalence deemed *theoretical* equivalence does not hold between the field systems. I have

[36] Govorkov (1982), 1131-1132.
[37] See Carithers and Grannis (1995).

also argued that the dismissal of parastatistics as superfluous is not justified, for parafield programs with interesting experimental consequences can be developed that cannot be accounted for by the impoverished notion of physical content upon which AQFT is based. Furthermore, it is not clear how to reconstruct appropriate notions of empirical and theoretical equivalence within a theory so obviously shaped by the operationalism of its founding fathers. As a result, it seems that one can only accept the proof of theoretical equivalence at the cost of denying the existence of a case of underdetermination and thereby the interesting features of the parafield theories.

The history of paraparticle theories is especially telling with respect to the cost of the theoretical equivalence strategy. As made obvious by the later developments of the paraparticle program, even a program that found its first application in the addition of an unobservable degree of freedom can eventually be developed into a theory no longer predictively equivalent to its initial rivals. In the first steps of their history, the paraquark model and the colour-gauge quark model, both based on the adding of an extra degree of freedom to quarks, seemed to constitute trivially interconvertible theories, and as such ideal candidates for the appeal to the identical rivals strategy. However, twenty years later, the subsequent developments of the program into gauge-invariant formulations of parafield theories and a 'paronic' theory challenged this status and backed up Laudan and Leplin's thesis that there could never be empirically equivalent theories, and that to this extent the identical rivals strategy is never appropriate.

But then, what can be the role of theoretical equivalence if not to solve cases of underdetermination? Is this concept a useless one? Before addressing this question, I want to pause and remind the reader of the context within which the proof of equivalence was reached. First, the proof has been developed within the theoretical framework of AQFT, which was originally motivated partially based on the need to dismiss unitary inequivalent representations as superfluous structure to avoid inconsistencies. AQFT was thus built on an very restricted notion of physical content, with a strong operationalist flavour. The Doplicher-Roberts theorem requires an even stronger stance on what counts as physically significant, that Baker et al. (2014) top with a third one, excluding incomplete field theories. So, one way to read their result is the following: the maximal degree of equivalence that may be established between a parafield theory and theory with normal commutation relations is that of quasi-equivalence, and such a degree of equivalence holds only for complete theories localized in a specific way, i.e., for very unrealistic assumptions given that we know already that long-range forces theories such as electromagnetism violate the DHR criterion. In other words, it provides the degree of equivalence that can be reached and under which conditions. Thus, the equivalence proof can be considered as a test-case that delivers insight into what needs to be better understood before the differences between the two theories can be fully elucidated. More precisely, it defines a program of research that sets the basis for improving our understanding of parastatistics, by inviting us to test which of these conditions can be stretched and which one would not resist any attempt to generalize the scope of the equivalence result:

- Complete vs. incomplete parafield theories: the Doplicher-Roberts theorem is partially based on Drühl et al. (1970)'s insight that incomplete theories are somehow pathological theories, in a sense that is not yet defined. This theorem therefore invites further research on finding a physical justification for why incomplete parafield theories should be excluded, a challenge taken by Baker et al. (2014).

- Short-range vs. long-range forces: the DHR analysis of the superselection structure only holds for short-range forces. Charges whose effects propagate arbitrarily far, such as charged states in electromagnetism, are excluded by the DHR criterion. Since we know already that this condition is unrealistic, but that two of the most spectacular results obtained within AQFT–a full analysis of the superselection structure, derived from the net of observables; and the equivalence thesis–only holds if this criterion is meant, the Doplicher-Roberts theorem invites researchers to generalize their result by testing how much the DHR criterion can be loosened before the proof breaks. Such a task has been undertaken in Buchholz and Fredenhagen (1982), where the DHR analysis was successfully extended to charges localized in a single space-like cone, and further developed in

Buchholz and Roberts (2014).

- Locality and parastatistics: the proof of equivalence and the examination of different parafield theories that do not fall within its scope reveals an interesting characteristic of parafield theories. Violation of statistics seem to be connected to violation of locality, but the nature of the locality principle that would be violated is not fully elucidated. Quon theories for instance are local in the cluster decomposition principle's sense, but not in the sense of spacelike commutativity of observables. Theories where the spin-statistics theorem is not stipulated but derived automatically exclude parastatistics. An interesting question to tackle in the future is thus to pin down in what sense exactly paraparticles are non-local.

I therefore suggest to regard theoretical equivalence as a tool for exploring the structure of competing theories, instead of as a tool to explore the structure of reality. The theoretical equivalence strategy can be redefined as the strategy consisting of first identifying the maximal degree of equivalence that can be established between rival theories, then determining under which conditions this maximal result holds, and finally assessing the extent to which these conditions can be stretched. In the case study I examined for instance, the maximal degree of equivalence that obtains is that of quasi-equivalence, under the conditions that one excludes incomplete theories, accepts the DHR locality criterion, thereby restraining the scope of the equivalence to short-range forces, and focuses on field theory with a global gauge group only. These conditions allow one to define where the structure of these theories needs to be clarified: can the claim of quasi-equivalence be generalized to long-range forces? Can we consider incomplete theories as pathological and if so, why? What kind of locality condition exactly is violated by paraparticles? In that sense, the concept of theoretical equivalence is better conceived as a regulative ideal–i.e., not as the assertion that there is a equivalence relation that can be found both each pair of rival theories such that it captures everything physically significant in rival theoretical structures and allow for a fair comparison, but as the principle governing the search for such an equivalence relation, with no guarantee that a mathematical mapping can be found that will actually be able to play this role. In other words, the role of the theoretical equivalence strategy should consist in determining the conditions under which a bijective mapping with maximal scope can be found, i.e., a one-to-one mapping between a maximal amount of physical quantities and relations between them than the rival theories admit. Inasmuch as the mapping that could capture everything considered as relevant by both parties is in practice extremely difficult to determine, I contend that the theoretical equivalence strategy should be conceived as a tool for clarifying the structure of rival theories, as a way to learn more about their respective structures, rather than as a claim that such a strategy can legitimately determine when competing theories are "one and the same" theory. By analyzing where exactly the equivalence breaks down, i.e., which loosening of the equivalence conditions is crucial in breaking the maximal degree of equivalence, one can hope to define where the interesting differences between the competitors lie, and which parts of the theory are really responsible for its observational consequences and their differences. Pinning down in what sense exactly parafield theories are non-local could be one of the insights provided by such a use of theoretical equivalence.

Chapter 3

Easier said than unsaid: Artifacts in Quantum Theories

3.1 Introduction

The role of theoretical virtues in privileging a theory over its empirically equivalent rivals has been a central topic in discussions of underdetermination.[1] The fact that two theories account equally well for empirical data, i.e., that no empirical evidence can be used to justify the choice of one over the other, does not entail that this choice is eventually a matter of subjective or pragmatic preferences that has nothing to do with what the world is really like — or so the realist defense of theoretical virtues goes. Precisely how a body of data relates to (or is entailed by) a set of axioms or principles might differ from a theory to its rivals. Such contrasts might provide non-empirical yet *epistemic* justification for why a given theory is more likely to be true than its empirically equivalent competitors. In particular, it has been argued that, *ceteris paribus*, the greater simplicity or the greater explanatory power of a theory constitutes indirect evidence that should play a role in theory appraisal.

The criterion of simplicity has been notoriously difficult both to use and to justify. Bunge

[1] See for instance McMullin (1987), Boyd (1973), Boyd (1980), Boyd (1984), Salmon (1970), Salmon (1985), Salmon (1990),Churchland (1985), Glymour (1980), Psillos (2000), Psillos (2005b).

(1961), for instance, has shown how ambiguous the concept of simplicity is–under one single concept are subsumed many kinds of simplicity that are not necessarily compatible with each other. A theory could be simpler in an economic sense, in that it entails the same body of evidence with a smaller number of axioms or principles required. But it could also be ontologically simpler, as postulating a more parsimonious ontology; or experimentally simpler, being easier to test than its rivals; or simpler in terms of its intelligibility. More problematically, considering simplicity as a factor of truth instead of a mere pragmatical virtue requires an extra assumption- that the world we are describing through scientific theories itself is simple. Yet, we have no reason to believe such a claim *a priori*-actually, the universe described at very small scale by quantum theories is anything but simple, and is even stranger at very large scale, if considering the geometry of spacetime of a universe dominated by a positive cosmological constant like ours. The role that explanatory power plays in confirming a theory, however, seems at first sight easier to justify for grounding the choice of a theory. Suppose that you have a body of evidence e_i(i=1,...,N) and two theories T and T'. Suppose now that the entire set e_i can be deduced from the set of axioms–and the subsequent laws derived from them–that T postulates, while in T' a subset (e_3, e_4) requires the addition of an extra-postulate. This extra assumption is introduced as an *ad hoc* patch, whose only purpose and justification are to recover e_3, e_4. Although T and T' both account for e_i[2], T seems to be in a stronger epistemic position than T': it gets for free what T' only obtains by hand. For such a strategy to be successful though, rival theories have to agree on what requires an explanation, and to some extent on what counts as an explanation.[3] Imagine a case where T' introduces more structure than T as a way to make T' more intelligible. Suppose now that the extra assumption needed to recover empirical adequacy is simply the acknowledgment of this extra structure and its local removal to get (e_3, e_4) back. Would we still have the same epistemic grounds for privileging T

[2]By "account", here, I mean that their scope is equivalent and that neither T nor T' is contradicted by any $e \in e_i$.

[3]Given that our focus is on explanatory power as a possible way for realism to escape the underdetermination threat, we will ignore non-realist accounts of explanation aiming to disconnect the concept of "explanation" from that of "truth" like that defended by Van Fraassen (1980).

over T' in that case? Would we still consider that (e_3, e_4) are *explained* in T, but not in T'?

In this paper, I evaluate the appeal to this strategy in order to privilege interpretations of Quantum Mechanics that pretend to derive the Symmetrization Postulate instead of merely postulating it. Recently, Brown et al. (1999), Bacciagaluppi (2003), Dürr et al. (2006), and Sebens (2016) have argued that any interpretation of Quantum Mechanics that can offer a no-go theorem against paraparticles possesses an explanatory advantage over other interpretations and should, as such, other things being equal, be favored over others. Given that a system of paraparticles is described by partially symmetric states, providing a no-go theorem for them amounts to *deriving* the Symmetrization Postulate, which only allows fully (anti)symmetric states for describing systems of identical particles.[4] De Broglie-Bohm mechanics provides such a no-go theorem, Brown et al. and Dürr et al.[5] argue, as the topological approach of the configuration space predicts the non-existence of paraparticles and is particularly natural in the Bohmian framework. The vast majority of physicists and philosophers, however, do not consider paraparticles as a problem — as something whose absence should be explained — and either dismiss them as mere surplus structure, a mathematical possibility with no counterpart in the physical world, or consider a paraparticle theory as theoretically equivalent to a theory with ordinary bosons and fermions. The explanatory advantage strategy thus seems to have little to no impact on physicists and philosophers, inasmuch as they consider the Symmetrization Postulate as simply removing superfluous structure from the formalism. The success of the explanatory strategy requires that the rival interpretations all treat paraparticles as not merely surplus structure.

In this paper, I evaluate the strength of the arguments on both sides of the debate. More

[4]As we will see below, the Symmetrization Postulate imposes a superselection rule on physically admissible states: states describing a system of identical particles must be fully (anti)symmetric, excluding partially (anti)symmetric states. Since the Indistinguishability Postulate already excludes non-symmetric states, I consider the question of justifying the Symmetrization Postulate as logically equivalent to ruling out paraparticles. Sebens does not agree with this reading of the Symmetrization Postulate, and considers that justifying "the symmetry dichotomy" amounts to explaining why identical particles behave differently than non-identical ones (Sebens 2016, 49).

[5]Bacciagaluppi does not make use of the topological approach and instead uses the extra structure added by Bohmian Mechanics to the standard axioms of Quantum Mechanics.

specifically, I identify what is required to explain the Symmetrization Postulate and show that the topological approach fails to meet these requirements. In parallel, I argue that the dismissal of paraparticles on the basis that they constitute surplus structure relies on a misapplication of this concept. Given the failure of these two strategies to solve the problem and to provide a compelling resolution of the debate, I suggest a shift in focus. I hypothesize that paraparticles are actually artifacts, and show how this hypothesis sheds light on how the distinct resources of standard Quantum Mechanics and Bohmian Mechanics affect the way they construct the explanatory target. I argue that one's epistemological stance on what constitutes a good scientific theory ultimately shapes the structure of the theory, in that it determines what counts as physically relevant structure and as superfluous structure. Furthermore, it determines what is a fact requiring explanation, the nature of the *explanandum* and the structural resources available within the theory for providing an *explanans*. This case study therefore illustrates why the appeal to the explanatory power of a given interpretation of Quantum Mechanics cannot be an effective strategy for privileging one interpretation over another; but also why such a strategy is very likely to fail, especially to convince the rival side. Given that what requires an explanation, what constitutes a relevant explanation and the resources available for providing one are determined within a given theory, a comparison of distinct theories in terms of their explanatory power is not neutral enough to be persuasive.

In Section 3.2, I detail the consequences of quantum particles' indiscernibility and how they have been addressed through the Indistinguishability and the Symmetrization Postulate. I then proceed to present in what sense the topological approach of the configuration space can *derive* the Symmetrization Postulate while in standard Quantum Mechanics, by contrast, this postulate does not receive any theoretical justification. In section 3.3, I criticize the explanatory dimension of the topological approach and the application of the concept of "surplus structure" to paraparticles: if surplus structure corresponds to different mathematical representations available to describe one and the same physical system, then scrutinizing the properties of paraparticles shows that parabosons (parafermions) cannot represent the same physical system as bosons (fermions). Finally, I defend the hypothesis that partially symmetric states — paraparticles — are merely artifacts of creating distinctions between states that the theory has no resources to handle, and re-define on this basis what an explanation of the symmetry requirement imposed in the Symmetrization Postulate should be, both in Bohmian Mechanics and in standard Quantum Mechanics.

3.2 Why does the Symmetrization Postulate need to be justified?

3.2.1 From identical particles to the Symmetrization Postulate

Imagine that two identical quantum particles, arbitrarily labelled 1 and 2, collide. After the collision, a particle is detected in the region A, whereas the other one is detected in B. The system can be described by the state $|\psi(r_A, r_B)\rangle = |\psi_1(r_A)\rangle\,|\psi_2(r_B)\rangle$. Now, since the particles are identical, the labels could be permuted and the system described by a different state:$|\psi(r_A, r_B)\rangle = |\psi_2(r_A)\rangle\,|\psi_1(r_B)\rangle$. The actual state of the system is unchanged by this permutation but there are at least two mathematical vectors that are not along the same ray and yet suitable for that state. This ambiguity already generates difficulties, since one needs to know the vector describing the final state of the system to calculate the probability of this result. These two states represent mutually exclusive results and yet there is no possible measurement that could help to distinguish one from the other. This already bad result gets even worse when considering which kets are suitable kets to represent the *initial* state of the system. Consider a system of two particles — again, say 1 and 2 — with spin $\frac{1}{2}$. To each particle is associated a state space, respectively $\mathcal{H}_1$ and $\mathcal{H}_2$, along with observables acting on this state space. If A_1 constitutes a complete set of commuting observables in $\mathcal{H}_1$ and the particles are identical, then there is an observable A_2 whose components also constitute a complete set of commuting observables in $\mathcal{H}_2$. The common eigenvectors $|1 : a_i, 2 : a_j\rangle$ of A_1 and A_2 span a common basis such that an observable A can be defined for the two-particles system. As in our previous example, there is no possible measurement of either A_1 or A_2, but only of A for each of the particles. Thus, the ket $|1 : a_i, 2 : a_j\rangle$ could as well be $|2 : a_i, 1 : a_j\rangle$. By the superposition principle, *any* normalized vector belonging to the two-dimensional subspace spanned by these two vectors could be used. That is to say, any state of the form $b\,|1 : a_i, 2 : a_j\rangle + c\,|2 : a_i, 1 : a_j\rangle$, where b and c are complex numbers, is a possible description of the state: there is an infinity of different descriptions available to represent the physical state. This problem is one of exchange degeneracy: the specification of the complete set of observables' eigenvalues does not uniquely determine one corresponding state.

One can alleviate this difficulty by rejecting some of the possible mathematical descriptions of the state as physically meaningless. Since particles are by definition indistinguishable, then a natural requirement seems to be that of permutation symmetry — if the particles have exactly the same properties, then it should not make any difference whether they are permuted or not. This requirement, usually referred to as the "Indistinguishability Postulate", is a restriction on the observables that qualify as physically admissible: the states representing systems of identical particles have to be permutation invariant, in the sense that any observable must commute with the permutation operator. Roughly speaking, this means that the eigenvalue assigned to an observable must be exactly the same whether it applies to the permuted or the non-permuted system.

Consider a system of three identical particles for instance. In that case, the permutation group contains one trivial and five non-trivial elements, that are represented by unitary operators acting on the Hilbert space $\mathcal{H}_n = \mathcal{H}_i \otimes \mathcal{H}_j \otimes \mathcal{H}_k$, where $\mathcal{H}_i$ is the one-particle Hilbert space for particle i, in the following way:

$$I = i \to i, j \to j, k \to k \Rightarrow \hat{I}\,|\psi_{ijk}\rangle = |\psi_{ijk}\rangle \tag{3.1}$$

$$P_{ij} = i \to j, j \to i, k \to k \Rightarrow \hat{P}_{ij}\,|\psi_{ijk}\rangle = |\psi_{jik}\rangle \tag{3.2}$$

$$P_{ik} = i \to k, j \to j, k \to i \Rightarrow \hat{P}_{ik}\,|\psi_{ijk}\rangle = |\psi_{kji}\rangle \tag{3.3}$$

$$P_{jk} = i \to i, j \to k, k \to j \Rightarrow \hat{P}_{jk}\,|\psi_{ijk}\rangle = |\psi_{ikj}\rangle \tag{3.4}$$

$$P_{ijk} = i \to j, j \to k, k \to i \Rightarrow \hat{P}_{ijk}\,|\psi_{ijk}\rangle = |\psi_{jki}\rangle \tag{3.5}$$

$$P_{kji} = i \to k, j \to i, k \to j \Rightarrow \hat{P}_{kji}\,|\psi_{ijk}\rangle = |\psi_{kij}\rangle \tag{3.6}$$

The Indistinguishability Postulate dismisses all observables that do not commute with the representation of the permutation group thus defined. The kets obtained by permutation span a six-dimensional state space with four subspaces:

- the symmetric one: $|ijk\rangle = (|ijk\rangle + |ikj\rangle + |jki\rangle + |jik\rangle + |kij\rangle + |kji\rangle)/\sqrt{6}$
- the anti-symmetric one: $|ijk\rangle = (|ijk\rangle - |ikj\rangle + |jki\rangle - |jik\rangle + |kij\rangle - |kji\rangle)/\sqrt{6}$
- a partially symmetric one, spanned by two mutually orthogonal vectors : $|\alpha\rangle_1 = 2\,|ijk\rangle + 2\,|jik\rangle - |ikj\rangle - |kji\rangle - |kij\rangle - |jki\rangle)$ and $|\alpha\rangle_2 = |kji\rangle + |kij\rangle - |ikj\rangle - |jki\rangle$
- another partially symmetric one, also spanned by mutually orthogonal vectors: $|\beta\rangle_1 = |kji\rangle - |kij\rangle - |ikj\rangle - |jki\rangle$ and $|\beta\rangle_2 = 2\,|ijk\rangle - 2\,|jik\rangle + |ikj\rangle + |kji\rangle - |kij\rangle - |jki\rangle)$.

Notice that the first two subspaces are one-dimensional, while the last two are two-dimensional. The Symmetrization Postulate states that only the first two are physically relevant: that only fully symmetric states (for identical particles of integer spin, or bosons) and antisymmetric states (for identical particles of half-integer spin, or fermions) are admissible, or, equivalently, that states should correspond to unique rays in a Hilbert space. As stressed by Messiah and Greenberg (1964), this extra selection rule does not strictly follow from the Indistinguishability Postulate. The condition imposed on the possible states is stronger than required by the mere indistinguishability of quantum particles, as symmetric and antisymmetric states do not exhaust the possibilities left open by the mere invariance under permutation.By definition, irreducible subspaces are associated with different types of permutation symmetries. Since irreducible subspaces of the Hilbert space correspond to irreducible representations of the permutation group, and different irreducible representations correspond to different particles, we can conclude that to these partially symmetric multi-dimensional subspaces should correspond a type of particles distinct from bosons or fermions — the so-called "paraparticles". In the case of parabosons of order p for instance, the state vector describing the system can be antisymmetric up to p paraparticles and symmetric for the remaining parabosons.

Although the Symmetrization Postulate agrees with empirical facts, as no paraparticles have ever been observed, it could be interpreted as a mere stipulation that paraparticles do not exist; as an *ad hoc* clause added to the theory in order to restrain possible states and to recover empirical adequacy. As a result, an interpretation of Quantum Mechanics able to derive the Symmetrization Postulate could be favored as possessing a greater explanatory power over its rival; one could then *explain* using its resources why only bosons and fermions are observed, whereas rivals could only take note that paraparticles are not observed in nature and fix their interpretation consequently. This is precisely the project defended by Brown et al. (1999), Bacciagaluppi (2003) and Dürr et al. (2006):

> Some implications of this topological approach to the treatment of identical particles within the framework of the de Broglie-Bohm 'pilot-wave' formulation of quantum theory have recently been studied. The purpose of the present paper is principally to stress one point not emphasized in [Sjöqvist and Carlsen, 1995], namely that the multiple connectedness of the reduced configuration space, which seems somewhat ad hoc in the standard formulation of the topological approach, receives a natural justification within de Broglie-Bohm theory (Brown et al. 1999, 230).

> It is the purpose of this paper to give a natural proof that parastatistics are excluded

> and a derivation of the three known symmetry types in the framework of two pilot-wave theories, namely de Broglie-Bohm theory and Nelsons stochastic mechanics (Bacciagaluppi 2003, 2).
>
> The topological factors we derive are equally relevant and applicable in orthodox quantum mechanics, or any other version of quantum mechanics. Bohmian mechanics, however, provides a sharp mathematical justification of the dynamics with these topological factors that is absent in the orthodox framework (Dürr et al. 2006, 791).

Note that this is also how authors criticizing this approach interpret their work:

> Even those who think that the absence of paraparticles does not pose a deep problem for standard QM generally agree that a version of quantum theory that successfully predicts the impossibility of paraparticles would possess an explanatory advantage over the basic theory. A number of interpretations, including Bohmian mechanics, stochastic mechanics, and modal interpretations, have offered no-go theorems for paraparticles (see Baciagaluppi ([2003]), Drr et al. ([2006]), Nelson ([1985]), and Kochen ([unpublished]) (Baker et al. 2014, 932).

3.2.2 Topological approach to configuration space

Let us now examine how this explanatory advantage would work. The topological approach has been developed notably by Leinaas and Myrheim (1977), in their influential paper *On the theory of identical particles*, by Girardeau (1965) and continued by Bourdeau and Sorkin (1992). The starting point of Leinaas and Myrrheim's reasoning is that, once the Symmetrization Postulate is added, the formalism of Quantum Mechanics presents a redundancy that could be avoided by adopting the correct approach of permutation invariance–namely, by applying permutation invariance to the configuration space instead of applying it to observables. Once the topology of a quantum system of identical particles is properly understood, the symmetry requirement emerges, without requiring anything more than the Indistinguishability Postulate:

> Since the indistinguishability of the particles is taken into account in the definition of the configuration space, no additional restriction, corresponding to the symmetrization postulate, is added on the state functions (1977, 3).

The topological strategy rests on three steps. First, it requires moving from a full configuration space to a "reduced space". Taking the classical limit of the indiscernibility of quantum particles as a model, the reduced configuration space proposed as the classical solution to Gibbs paradox is transferred to the quantum state space. The configuration space of N quantum particles is therefore not a Cartesian product of the single-particle spaces, but a multi-connected space. Imagine N identical particles moving in a X-coordinates space, knowing that to each particle corresponds a Hilbert space $\mathcal{H}_1$,$\mathcal{H}_2$,...,$\mathcal{H}_N$ and that the full configuration space is given by the tensor product of these single-particle state spaces: $\mathcal{H}_i = \mathcal{H}_1 \otimes \mathcal{H}_2 \otimes ... \otimes \mathcal{H}_N$. Since particles are identical, points in the full configuration space differ only by the ordering of their coordinates. Therefore, the appropriate configuration space should not be X_N, but $\frac{X_N}{S_N}$, obtained by the action of the permutation symmetry group S_N on the configuration space. The second step is then to remove from $\frac{X_N}{S_N}$ all singular points corresponding to several particles occupying the same spatial position at the same time[6]. The appropriate configuration space thus becomes a multi-connected set Q= $\frac{X_N}{S_N}\Delta$, with Δ the set of coincidence points, and naturally satisfies the following symmetry condition:

$$\psi(X_{P^{-1}1}, X_{P^{-1}2}, ..., X_{P^{-1}N}) = e^{i\gamma}\psi(X_1, X_2, ..., X_N) \tag{3.7}$$

where N is the number of particles, P any permutation on the set 1,2,...,N, and $e^{i\gamma}$ the phase factor. Since in three dimensions the phase factor is necessarily either +1 or -1, the permutation invariance applied to the configuration leaves only bosons and fermions as possible solutions.

[6]This step solves the difficulties associated with the definition of a Hamiltonian at singular points and with the violation of Fermi-Dirac statistics induced by the existence of such points.

The Symmetrization Postulate is therefore not an extra postulate anymore, but a way of formulating explicitly what was already implicitly present in the topology of a system of quantum identical particles.

This topological approach is not exclusively related to Bohmian Mechanics. In fact, it could be applied to any interpretation of Quantum Mechanics. However, the removal of the singular points Δ would be completely *ad hoc* in standard Quantum Mechanics, as it would be grounded in the assumption that indistinguishable particles are also impenetrable. This assumption, which might be verified for fermions, is yet non-justified. More importantly, it certainly does not hold for bosons, which can be accumulated in the same state. By contrast, this assumption is fully justified — or so the authors argue — in Bohmian Mechanics. In the Bohmian framework, an ensemble of N particles is described by a pilot-wave, its wavefunction, which is a complex-valued function defined on the configuration space q of the system: $\psi(q,t) \in \mathbb{C}, q = (q_1, ..., q_N) \in \mathbb{R}^{3N}$. The configuration space variable is given by $Q = (Q_1, ..., Q_N) \in \mathbb{R}^{3N}$, where $Q_k \in \mathbb{R}^3$ is the actual position of k_{th} particle. Two equations give then the evolution of $\psi(q,t)$ and of $Q(t)$:

- the Schrödinger equation for the former: $i\hbar\frac{\partial\psi}{\partial t} = H\psi$;
- the guidance equation for the latter: $\frac{dQ_k}{dt} = \frac{\hbar}{m_k} Im\frac{\psi^*\partial_k\psi}{\psi^*\psi}(Q_1, ...Q_N)$

with m_k the mass of the k_{th} particle, Im(z) the imaginary part b of a complex number $z = a + ib$, and ∂_k the gradient with respect to the generic coordinates $q_k = (x_k, y_k, z_k)$ of the k_{th} particle. The wave-function determines the instantaneous velocities of the corpuscles by inducing a velocity-vector field, whose integral curves are the trajectories of the particles. Given that the guidance equation is a first-order equation, the trajectories are known once the positions q_1,...,q_N of the particles are specified. It is then easy to explain the removal of the coincidence points: since the wave-function is a single-valued differentiable function of the position and since the past and future trajectory of a particle is determined entirely by the wave-function and the position of the particle at t, Bohmian trajectories cannot cross in configuration space.

Therefore, coincidence points are inaccessible from non-coincidence points. As a consequence, Brown et al. claim that:

> within the topological approach to identical particles the removal of the set Δ of coincidence points from the reduced configuration space $\mathbb{R}^{Nd}/S_N$ thus follows naturally from de Broglie-Bohm dynamics as it is defined in the full space $\mathbb{R}^{Nd}$ (1999, 7).

3.3 Explanatory power and surplus structure

There are two distinct ways to understand the claim made by the Bohmians. The claim may be a general claim, according to which Bohmian Mechanics should be preferred over standard Quantum Mechanics given that they can explain the dichotomy bosons/fermions — i.e., that only bosons and fermions are physically admissible particles. It could also be of a narrower scope, and simply asserts that the topological approach of the configuration space can be naturally implemented in Bohmian Mechanics, while it would be *ad hoc* in standard Quantum Mechanics. In that case, the claim could be the basis of theory choice only if the standard implementation of permutation invariance is shown to be inherently flawed and that the topological approach does not suffer from this flaw. Note that no conclusion could be drawn from the narrow claim without this supplementary addition, other than acknowledging that there are different ways of implementing the indiscernibility of quantum particles, and that permutation invariance applied to configuration space suits better the Bohmian approach than the standard one. In this section, we will thus focus on the general claim, hoping to gain some insights about the logic of the narrower one by doing so.

3.3.1 What counts as an explanation?

The first thing to elucidate is what exactly needs to be explained. The controversy about paraparticles has persisted for four decades and yet, the nature of the issue itself is still not clear:

some authors formulate the problem of paraparticles in terms of whether they exist or not, some other authors in terms of whether a justification of the Symmetrization Postulate is required, while some others focus on why paraparticles are not observed (Nelson et al. (2013), Nelson et al. (2016)). This confusion stems from the fact that nobody agrees on the status that should be granted to paraparticles. The aim of this section is to clear up what should be explained to begin with.

There is a range of questions that I take to be equivalent, given the introduction to paraparticles above:

- Why are fully (anti)symmetric states the only states admissible, when the Indistinguishable Postulate admits more?
- Why should the eigenvalues of permutation operators be only ± 1?

These three questions all assume that only bosons and fermions are possible, and that what needs to be explained is the reason why paraparticles are not physically admissible states. This, I argue, is what is subsumed under the broader question: "How do we justify the Symmetrization Postulate?" The question "Why do paraparticles not exist?" can be taken to be a close equivalent, but only granted that you adopt a realist attitude towards scientific theories. On the other hand, asking "Why are paraparticles not observed?" is a logically very different request: unlike the first four questions, it assumes that paraparticles are actually a physical possibility, presumably following the idea that to every irreducible representation of the permutation group corresponds a possible type of particles and that every possibility is realized. If one endorses the idea that what can exist *should* exist, i.e, that to every mathematical quantity is associated a physical entity[7], then paraparticles should indeed be observed in the absence of a law prohibiting their existence. So, what is asked here is: given that paraparticles should exist, what is preventing us from observing them?

[7]This "principle of plenitude" was defined as follows by Lovejoy in 1936: "[T]here is a fullness of realization of conceptual possibility in actuality" and can often be seen at play in arguments to the existence of the magnetic monopole, positron, tachyons or even the ether. See Kragh (1990), pp. 270-274.

To answer the latter question, what I will call the observation problem, we need to make explicit the conditions under which a paraparticle would be observed.[8] There is a well-known obstacle to observing paraparticles. Given the requirement imposed by the indiscernibility of particles on the Hamiltonian, transitions from normal states to anomalous states or from anomalous states to normal states are forbidden: if the Hamiltonian must be invariant under the permutation of two particles, then it cannot change the sign of the wavefunction of the system. Thus, an observation of paraparticles cannot be a transition from a bosonic (fermionic) state to a para-bosonic (-fermionic) one. Hence, if one wants to test Pauli's exclusion principle against paraparticle violation — since having several parafermions occupying the same state amounts to violating this principle, one would have to either come up with a model slightly modifying the original paraparticle theory to overcome this difficulty, or to design an experiment allowing for the observation of anomalous to anomalous transition. Furthermore, such a violation should have direct observational consequences, in particular in the chemical behavior of atoms with anomalous electronic configuration. Despite models built explicitly to overcome the above objection, no such violations have ever been observed.[9]

An answer to the first kind of worries may take two different forms. First, one can accept that these questions do require an explanation. Then, one would have to show that symmetric and anti-symmetric states are the only possible states and exhibit the physical law that paraparticles violate. The Bohmian topological approach is meant to be such an explanation: the conjunction of permutation invariance properly implemented — i.e., applied to the configuration space instead to the observables — and of the dynamical laws of Bohmian Mechanics forbid all non-symmetric and partially (anti)symmetric states and this conjunction, the Bohmian

[8]Another possible answer could be the following: paraparticles are a physical possibility from the point of view of the irreducible representation argument, but their existence would violate a physical law and is therefore prohibited. The very early literature on paraparticles indeed focused on whether they violate the locality requirement expressed by the cluster decomposition principle. I will not tackle this possible answer here, since whether paraparticles violate this principle is still an open and controversial question. See Steinmann (1966) and Vo-Dai (1972) for arguments to the conclusion that paraparticles violate the cluster property, and Hartle and Taylor (1969) and French (1987) for a criticism of this line of argument.

[9]See Chapter 2, section 2.2.2 for a detailed overview of attempts to violate Pauli's exclusion principle by either modifying the paraparticle theory or by designing experiment to observe transitions from anomalous to anomalous states.

defenders argue, constitutes an *explanation* properly speaking. In other words, the conclusion that permutation operators can only take ± 1 as values follows necessarily from this conjunction and the set of covering laws and definitions admitted by Bohmian Mechanics. Second, one could dismiss the relevance of these questions and argue that they are not the kind of questions that require an explanation: partially symmetric states are not the kind of possibility to which the principle of plenitude applies, but merely surplus structure that should be ignored and removed whenever necessary. In the remainder of section 3.3, I show that none of these strategies is an appropriate response to the explanation demand formulated in 3.1.

3.3.2 What the topological approach is hiding under the rug

It seems, at first sight, that the topological approach embedded within Bohmian Mechanics constitutes a proper deductive-nomological explanation of why the eigenvalues of permutation operators can only be ± 1, and thereby of the Symmetrization Postulate. The *explanandum*, i.e., that the phase factor can only be ± 1 in equation 2.2.1, is logically deduced from the *explanans*, the axioms of Quantum Mechanics retained in Bohmian Mechanics in conjunction with its dynamical laws, granted that the indiscernibility of quantum particles is taken into account by applying the permutation invariance requirement to the configuration space instead of the observables. The axioms and laws of Bohmian Mechanics qualify as general covering laws, and the permutation invariance requirement as an initial condition, inasmuch as it follows from the fact that quantum particles are indiscernible. By contrast, in standard Quantum Mechanics the explanation would fail, because the assertion made in the conclusion would contradict a consequence of the premises, since the set of coincidence points that appear in the multi-connected topological space violates Fermi-Dirac statistics.

This is not the end of the story, however. As our reader might recall, the statement that the eigenvalues of permutation operators should be ± 1 is usually enforced in standard Quantum Mechanics by imposing that unique rays in a Hilbert space are the only physically admissible states. The very problem of the Symmetrization Postulate, indeed, is that it rules out *by fiat* those states that correspond to generalized rays, i.e., to collection of vectors satisfying the Indistinguishability Postulate. If the reduced configuration space was by itself restricting the possible values of the phase factor to ± 1, then indeed one could consider the topological approach as a better way to understand permutation invariance, if not as an *explanation* of the symmetry requirements imposed on many-particle systems of identical particles. However, the reduced space does not give you that alone. One has to decide how to quantize the multiply-connected space obtained after the removal of singularities. As stressed by French and Krause (2006) already, this quantization, and not the reduced space approach, is doing all the work in deriving the appropriate phase factor for excluding paraparticles, since the quantization procedure actually imposes the same unique rays that we saw at play in standard quantum mechanics:

> First, we introduce for each point x in the configuration space a corresponding one-dimensional complex Hilbert space h_x.[...] And we assume the state of the system to be described by a continuum of vectors $\Psi(x) \in h_x$ [...]. That is, Ψ is assumed to be a single-valued function over the configuration space, whose function value $\Psi(x)$ at the point x is a vector in h_x (Leinaas and Myrheim 1977, 13).

What's doing all the work here in the derivation of bosons and fermions statistics is the same assumption that was deemed *ad hoc* in the standard approach. Leinaas and Myrheim themselves acknowledge this point when defining the permutation operator $\hat{P}_x$ in the reduced space: if the vector h_x is one-dimensional, $\hat{P}_x$ is just the phase factor $\hat{P}_x = e^{i\gamma}$, and γ will either be equal to 0 or to π. For a three-dimensional configuration space, one even needs an extra assumption, that of enforcing the hermiticity of permutation operators — since the configuration space is doubly connected for three dimensions, the one-dimensional quantization does not necessarily yield the desired eigenvalues for permutation operators. In order to do that, one needs to guarantee that γ is independent of the point x, such that $P_x = P'_x = P(x', x)P_xP(x', x)^{-1} = e^{i\gamma}$[10], and imposing the hermiticity of **P** gives you exactly this result. The hermiticity of permuta-

[10] $P(x', x)$ is the linear operator that transports parallely the vectors of the one-dimensional Hilbert space h_x into $H_{x'}$, along some curve joining x to x'.

tion operators in the topological approach guarantees their independence with respect to the particles' coordinates. Without these two extra-assumptions, not only parastatistics are back in the formalism, but even more exotic statistics make an apparition -ambistatistics and fractional ambistatistics (Imbo et al. 1990[11]).

If the Symmetrization Postulate could not be an explanation of the symmetry dichotomy, then it is hard to see how the topological approach, that relies on exactly the same assumptions, could be one. The topological approach may have its own merits as a way to implement the requirement of permutation invariance, but cannot be an explanation to the questions formulated above.

There is still an important lesson to be drawn from the topological approach. Leinaas and Myrheim explicitly state that they want to provide a theoretical justification for the Symmetrization Postulate (1977, 1). But they also conceive the topological approach as a conceptually simpler way of formulating the problem, that is very much needed in order to find a justification. Indeed, the authors insist that the "introduction of particles indices [...] brings elements of nonobservable character into the theory and tends, therefore to make the discussion more obscure"(1977, 2). According to them, the redundancy appearing in the formalism when one accepts states such as $|\psi(p(x_1, ..., x_N)|^2 = |\psi(x_1, ..., x_N)|^2$ is the root of the problem, as it creates a distinction between two states *while at the same time* asserting some kind of identity between them. In section 3.4, we will follow up on this idea that particle indices could be symptomatic of a deeper conceptual problem.

3.3.3 Can the surplus structure strategy work?

Paraparticles are often described in the literature as superfluous structure, in two different senses. In the physics literature, very often one sees the term "paraparticle" taken as just another way of saying "bosons (fermions) with an extra hidden degree of freedom". In other

[11] Imbo et al. distinguishes three inequivalent statistics for three-dimensional configuration spaces: Bose, Fermi, and 'half-Bose half-Fermi' statistics. The term 'ambistatistics' refers to the latter.

words, a paraparticle theory and an ordinary particle theory are considered as equivalent theories that can both adequately describe the furniture of the world. We have criticized this use of theoretical equivalence in chapter 1, since the notion of theoretical equivalence is an empty one when not embodied in a fully fleshed out epistemological package, bringing together "a theory of meaning, of evidence, of ontology, of truth, of explanation and of equivalence itself"[12] into a consistent whole. There is, however, another sense in which paraparticles could constitute superfluous structure that we examine below.

In fact, we can consider that labelling identical particles introduces a redundancy in the formalism, as Leinaas and Myrrheim emphasized, since it confers a physical meaning to particle permutation. However, if quantum particles are genuinely indiscernible, then there is no matter of fact whether two particles are permuted or not. Thus, the Symmetrization Postulate would simply be removing from the formalism the surplus structure created by these labels. In this case, paraparticles would be a mere mathematical possibility induced by an arbitrary labeling, and the Symmetrization Postulate would not require any theoretical justification. The concept of surplus structure that underlies this reasoning is drawn from Redheads conceptualization of surplus structure, that can be found in Redhead (1975). In this paper, Redhead analyzes the mathematical formalization of a physical theory as an operation of embedding a physical theory in a mathematical model: a theory T can be embedded in a mathematical structure M if and only if there exists an isomorphism — a one-to-one structure preserving correspondence — between T and a substructure M of M. Thus, when embedded, the physical theory inherits a surplus structure from the complement of M in M, which allows many-to-one relations between the elements of the model in M and the states described by T. This surplus structure is illustrated in Figure 1 below.

From the possibility of many-to-one relations between the mathematical elements and the physical ones has been derived the idea that surplus structure manifests itself by the emergence of multiple representations available for a unique physical state. Remember that the reason

[12]See Sklar (1982), p. 90.

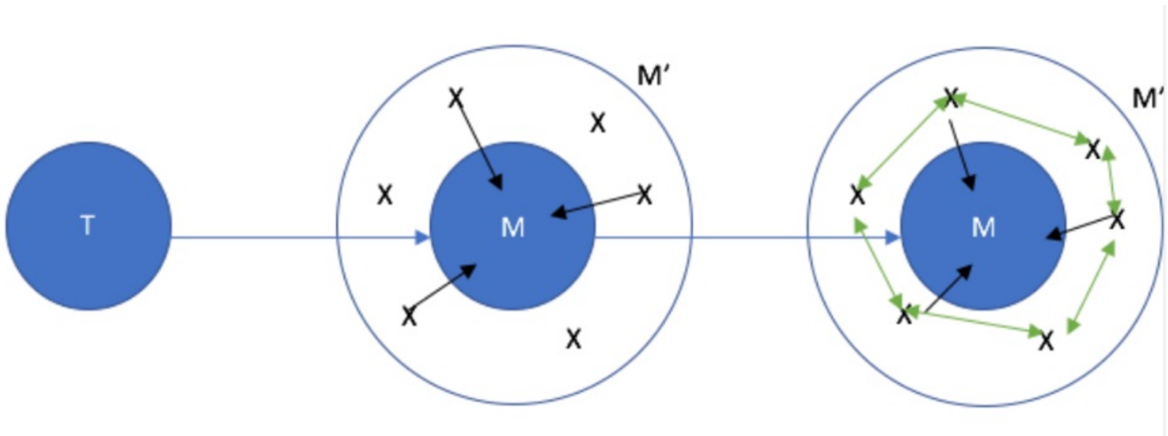

Figure 3.1: Redhead's representation of surplus structure. The theory T isomorphic to the substructure M is embedded in the bigger model M'. Both the surplus structure elements — black crosses — and the relations between these elements — green arrows — are inherited from the surplus structure in M'.

traditionally offered for considering paraparticles as surplus structure is that the arbitrary labelling of indiscernible particles introduces a redundancy in the formalism. In the example given in section 3.2 for instance, the mathematical state obtained by permuting particles differs from the former only in a difference of labels – the physical system itself is left unchanged. Thus, the state and the permuted state describe one and the same physical state, and differ only in the choice of labels imposed on the subsystems. Choosing between these two states is only a matter of convenience, and each of these states could be qualified as surplus structure. Such an account of paraparticles can be found in Massimi (2005):

> Thus, permutation invariance is itself playing a systematizing role in the quantum mechanics framework in the sense of making the already known quantum statistics (Fermi-Dirac and Bose-Einstein) follow from a more general group-theoretical prescription that also discloses new symmetry types. This is an example of surplus structure, to use Redheads terminology: a physical structure P (e.g. the quantum statistical behaviour of an ensemble of indistinguishable particles) is not represented by the mathematical structure M (e.g. Fermi-Dirac an Bose-Einstein statistics) with a one-one structure-preserving map between P and M. Rather, P is repre-

> sented by a larger mathematical structure M (e.g. permutation invariance), hence a surplus structure M-m (e.g. generalized rays) in the representation of P by means of M (2005, 178).[13]

Despite its popularity, there are several issues with this account of non-symmetric and partially symmetric states as surplus structure. In the definition of surplus structure seen above, the main criterion to diagnose whether mathematical representations constitute surplus or not is whether there are multiple ways to mathematically represent one and the same physical situation. However, there is no sense in which non-symmetric states and symmetric ones, or partially symmetric states and fully symmetric ones can be considered as referring to one and the same physical situation. As we saw in section 3.2.1, the Indistinguishability Postulate rules out observables that do not commute with permutation operators precisely because these observables would admit states that are non-symmetric under permutation. In other words, it would make a difference whether particle 1 is in state ϕ and particle 2 in state ψ or particle 1 in state ψ and particle 2 in state ϕ, thereby contradicting the very idea that these particles are qualitatively identical. Non-symmetric states are those which make an observable difference, and are therefore excluded as meaningless, or maybe even inconsistent, states. If particles are indeed indiscernible, then those states should not be admitted as physically relevant states, or then particles would not actually be indiscernible.

Now the problem becomes more subtle when it comes to partially-symmetric states, as these states do satisfy the Indistinguishability Postulate. As a consequence of the theoretical equivalence between paraparticles and bosons or fermions entertained in the Algebraic Quantum Field Theory literature, one can very often read the claim that a paraparticle state could be as well replaced by a bosonic or fermionic state with an extra hidden degree of freedom, a claim that keeps entertaining the idea that the mathematical representations for paraparticles and for ordinary states correspond to one and the same physical situation. This claim, however, should come as a shock when one remembers what paraparticle states allow, i.e., a violation of Pauli's

[13]One can find an equivalent account in French and Krause (2006) or French and Rickles (2003).

principle. Two parafermions can occupy the same state, and such a fact has immediate physical consequences. If two electrons behave as parafermions instead of fermions for instance, then the electronic configuration of the atoms to which they belong would differ dramatically, along with its chemical properties. How could we then consider them as describing one and the same physical state and the availability of two distinct mathematical representations as a mere embarrassment of riches?

There are, to my eyes, two possible reasons that underlie such a claim, neither of which is convincing. The first reason stems from the fact that proofs of equivalence were developed in the context of Algebraic Quantum Field Theory (hereafter AQFT), as a way to prove the equivalence of the colour quark model and the parastatistics model. Since the motto of AQFT[14] is that the physical content of the theory is located in the abstract algebra of observables, and that nothing of physical importance can be added by defining concrete operators on a Hilbert space, statistics of all kinds were already dismissed as not belonging to the physical content of the theory, but as some kind of redundancy merely making explicit what was already present in the algebra of observables. This line of thought was even more prevalent after the publication of the Doplicher-Roberts theorem, whose aim was to show that the field algebra where statistics are defined can be uniquely reconstructed from the algebra of observables. Thus, proofs of equivalence were already based on the fact that parastatistics, and hence paraparticles obeying them, had no physical significance, and from this initial statement, were aiming at deriving the conclusion that a bosonic/fermionic representation is merely a notational variant of a parabosonic/fermionic one. Not only is this statement made in a very specific context, that of Algebraic Quantum Field, whose tools are not easily transferable to any other models that cannot be treated within AQFT, but it was based on the implicit premise that statistics have no genuine physical significance and are superfluous once an algebra of observables is given. Appealing to proofs of theoretical equivalence to argue that paraparticles describe the same physical state as ordinary particles, and hence constitute surplus structure, is therefore begging

[14] At least, the motto of the founding fathers of AQFT, i.e., Haag and Kaastler.

the question.

Another reason that could explain why physicists are so eager to accept that paraparticles are surplus structure could be that violations of Pauli's principle such as the ones described above have never been observed, despite experiments run at very high precision. If the only way to observe a paraparticle is by observing a violation of Pauli's principle, and such a violation does not happen, then it is easy to see why dismissing paraparticles has having no physical significance would feel so natural. This conclusion yet would rely on a confusion between the observation problem and the Symmetrization Postulate problem. If one takes paraparticles seriously, i.e., as a physical possibility, and consequently attempts to observe them, the failure to observe the consequences derived from the paraparticle hypothesis does not constitute evidence that paraparticles constitue surplus, but that the paraparticles program is refuted. One cannot accept at the same time that paraparticles are surplus structure, *given that they have never been observed*, and that paraparticles describe exactly the same state as ordinary particles. It would actually worsen the 'ad hocness' of the Symmetrization Postulate: the main motivation for considering paraparticles as a physical possibility — and thus taking the observation problem seriously — is the argument that the conjunction of the formalism of Standard Quantum Theory, complemented with the Indistinguishability Postulate and the plenitude principle, *predict* the existence of paraparticles. If the existence of paraparticles is then refuted, and one had already committed themselves to these three premises, then there is something fundamentally wrong with the fact that partially symmetric states describing paraparticles appear so naturally in the formalism, something that cannot be solved by simply adding the Symmetrization Postulate and justifying its introduction by the removal of surplus structure. Hence, the theoretical equivalence developed in AQFT and the non-observation of paraparticles do not constitute compelling reasons to consider partially symmetric states as surplus structure, and the very definition of surplus structure furthermore excludes the possibility that non-symmetric states describe the same physical situation as fully or partially symmetric ones.

3.4 Explaining or Unsaying?

In section 3.3, I have detailed why I deny that the topological approach and the surplus structure strategy provide an explanation to the Symmetrization Postulate. In this section, I explore a third hypothesis: that paraparticles in Standard Quantum Mechanics are neither surplus structure in the technical sense nor a physical (im)possibility, but an artifact of creating distinctions between states that the theory has no resources to handle. In Quantum Mechanics, these artificial distinctions appear through the use of labels and their failure to uniquely fix a reference, but labels only contingently signal a deeper conceptual problem — that of implementing the indiscernibility of quantum particles through permutation invariance, i.e., that of requiring that these states are distinct enough that talking about permuting them make sense while at the same time asserting their qualitative identity. In section 4.1., I unpack the claim made in the Indistinguishability and the Symmetrization Postulates and evaluate how my hypothesis elucidates the apparent logical gap between them, before reviewing in section 4.2. two possibilities for what an appropriate answer to the questions created by the Symmetrization Postulate could be on this basis. In section 3.4.3, I test my hypothesis in the context of Quantum Field Theory, where no labels are used.

3.4.1 Paraparticles as artifacts

Imposing the Symmetrization Postulate, as we saw above, amounts to requiring that applying a permutation operator twice gives you back the state you started with. From a conceptual point of view yet, it comes as a surprise that such a requirement should be enforced: if one assumes as a starting point that quantum particles are indiscernible, how could exchanging any number of them possibly make a difference? A concrete example can help us elucidating this point.

Take three black marbles with exactly the same properties, on which you put a red, green and blue sticker respectively, and place them in a jar. Thanks to the stickers, you can unambiguously describe the state you have — for instance, the red marble is on the left side, the green one in the upper middle, the blue one on the right. You know, however, that these stickers should not make any physical difference when describing the properties of the marbles. Thus, after shaking your jar and shuffling the marbles, you make sure to take into account this rule when writing down the new state of your system: the marbles' stickers are not a relevant physical property, and any physical difference that seems to arise on the paper when you exchange two particles is ultimately a result of you having put these stickers. In other words, what your rule says is that, for all physical purposes, your marbles really are black. From a more formal point of view, this means that any observable whose expectation values differ depending on whether it is measured on the initial system or the shuffled one is not really a physical observable, but an artificial construction built up from the colours you added.

Your rule, however, still maintains that there is a meaningful distinction to be drawn between a state in which marble X is on the left and marble Y on the right or vice-versa. These states correspond to two distinct states in the possibility space that your configuration space represents, but permutation invariance asserts that they are somehow one and the same state. Because we can associate these marbles with a trajectory, and picture in our mind what a permutation of two black marbles would look like, we can qualify more accurately what this "somehow one and the same state" means in our example: these states are synchronically identical but diachronically distinguishable. If one has access to the history of the states, and follows the marbles' trajectories, one can maintain the claim that these two states correspond to two distinct possibilities despite their not making any measurable difference. If the initial and final states are the only information one has access to both in fact and in principle, then the distinction maintained in the permutation invariance requirement has no grounds anymore.

This is, I contend, the role of the Symmetrization Postulate: cancelling the distinction maintained in the permutation invariance requirement, based on the facts that when considering a system of identical particles, the theory has no resources to handle this distinction. First, they do not have a definite trajectory but randomly jump from a state to another. Second, particles are not localized in the same sense as marbles. They do not occupy a definite region of spacetime. Instead, the wave-function ψ_x assigns a probability $\int |\psi_x|^2 dx$ of finding the particle in a given interval between x and dx, and these wave-packets can overlap. In our example, one way to understand the Symmetrization Postulate is to assert that our three marbles are better conceived as three drops of black ink: the number of drops is known — maybe a graduated pipette was used to pour the drops in the jar, but once put in the jar, there is no point in considering the black ink it contains as a system with distinguishable parts that can be individually referred to. Within our theory, there is no individuating property or individuating causal relation that allows to unambiguously determine the referent of these labels in this context. As a consequence, labels should be momentarily neutralized.[15]

In sum, the need for two distinct postulates to implement the indiscernibility of quantum particles is a mere acknowledgement of the fact that labelling an object is really making two distinct assumptions:

- That of attaching a specific label to a particle. The Indistinguishability Postulate states that, whatever the labels imposed on particles are, they should not make any physical difference when systems of only identical particles are under study.

- That of asserting that particles are the kind of things that can be labelled. Note that this assumption is not necessarily a metaphysical one, as argued by Redhead and Teller. It is a claim about what your theory has the resources to handle or not. The Symmetrization Postulate is a statement that standard Quantum Mechanics does not have enough structure to give a meaning to the distinction maintained in the permutation invariance requirement, and that, as a consequence, from the point of view of its resources, two states that only differ by a permutation of their indices are one and the same possibility. Hence, the Symmetrization Postulate asserts that the indices i, j, k, that index the one-particle state spaces $\mathcal{H}_i$, $\mathcal{H}_j$, $\mathcal{H}_k$ from which the Hilbert space of the system as a whole

[15] I am not arguing here that something went wrong in the construction of the Hilbert space that could have been avoided. I am arguing, rather, that the size of the Hilbert space depends on an assumption that cannot be dispensed with given the resources of Quantum Mechanics. I will show, in section 3.4.3, that switching to a occupancy number representation faces exactly the same difficulty as long as the particle ontology is considered.

has been built refer to one and the same possibility space.

Two remarks should be made before concluding this subsection. First, asserting that $i = j = k$ is making a claim about the state space of your system, not about the particles themselves. The cardinality of the system remains unaffected by such a claim. Since the state space represents all possible states your system can be in, you are asserting that from the point of view of your theory, points in $\mathcal{H}_i$ that only differ from points in $\mathcal{H}_j$ by their indices constitutes one unique possibility. This is not tantamount to saying that your system is not a two-particles system.[16] Consider our previous example of the three marbles with coloured stickers. Imposing the Indistinguishability Postulate on your system guarantees that colour cannot be used as a physical observable, or be the basis of physical distinctions. However, you can still consider the three marbles as permutable entities, and list all the different states you can obtain by permuting or transposing the marbles. Two transpositions will always give you the state your started with back. But two permutations will not. If your permutation involves exchanging both the red and the blue, and the blue and the green marbles, applying the same permutation twice will not give you back your initial state. Starting with the initial state $(red_1, blue_2, green_3)$, and applying the permutation operator $\hat{P}_{312}$ twice will give you $(blue_1, green_2, red_3)$ instead. The Symmetrization Postulate does not state that there is only one particle in your system, but that there is only one vector in the Hilbert space $\mathcal{H}_N$ that can describe your state: the one that gives your initial state back if you apply your permutation operator twice.

Second, our hypothesis is that paraparticles are a mere artifact of creating distinctions when modelling one-particle states or many-particle states where particles are of different kinds in a way that cannot be accounted for anymore when considering many-particle systems where particles belong to the same family. In standard Quantum Mechanics, labels/indices are a

[16] Imagine a story, told by a narrator now dead, about two soldiers. We know that the story involves two soldiers, but they are only referred to using the pronoun 'he'. The story relates what happened to these two soldiers after their paths split, but does not give any information to differentiate who is the referential target of 'he' at any point. Soldier A could as well be the hero of one part of the story, and soldier B of the other, or the other way around. You can with no contradiction accept both the claim that there are indeed two soldiers and that, with no more information available, the story in which A is responsible for x and B for y is not a distinct story from the one where A is responsible for y and B for x.

symptom of these artificial distinctions, but are not necessary for our hypothesis to be true. Since this point is difficult to see in non-relativistic Quantum Mechanics, I will discuss in section 3.4.3 the consequences of our hypothesis in Quantum Field Theory, where labels are dispensed to begin with.

3.4.2 What does an explanation of the Symmetrization Postulate look like?

In this subsection, I examine what a justification of the Symmetrization Postulate could be, given our hypothesis. First, one could justify this postulate by adding extra structure to the theory, in order to provide grounds for distinguishing permuted states. This answer, as we will see, is not so much an explanation as a dissolution of the problem. Second, one can accept that the Symmetrization Postulate is a mere acknowledgment that what we have said when labelling one-particle state spaces must be unsaid in some specific contexts — in other words, that it expresses a connection among the structure of a physical theory, its resources, and how systems can be represented using these resources.

Let us consider first how supplementing the theory with some additional structure can adequately address the questions raised in 3.1. If my hypothesis is correct, then the Symmetrization Postulate should no longer be needed in the extended theory. Granted that its role is merely to deny that the distinction between two permuted states is a relevant one with respect to the resources of the theory, then the adding of extra structure, precisely meant to provide such resources, should make the Symmetrization Postulate dispensable, while at the same time not reintroducing paraparticles. Such a strategy is exemplified by Bacciagaluppi's derivation of the postulate. Roughly speaking, his derivation consists of extending the Indistinguishability Postulate such that Bohmian trajectories fall within its scope, and showing that the added structure of particles trajectories is enough to recover the symmetry requirements that the Symmetrization Postulate imposed by hand:

> Since pilot-wave theories include more structure than the standard formulation of quantum mechanics, namely particle trajectories, it is now possible to formulate stronger conditions of indistinguishability than merely requiring (2), and this will allow us to derive all the standard symmetry properties for wavefunction (Bacciagaluppi 2003, 5).

Start with a wave-function defined in terms of its amplitude R and its phase S such that $|\psi\rangle = Re^{iS/\hbar}$. Since we know already from the Indistinguishability Postulate that the Hamiltonian must be symmetric, the proof must show that S and R are symmetric at a given instant. If the permutation invariance requirement is extended to the velocities of particles, such that the velocity of particle 1 is equal to that of particle 2 when exchanged, then one obtains:

$$\nabla[S(\mathbf{x}, \mathbf{y}, t) - S(\mathbf{y}, \mathbf{x}, t)] = 0. \tag{3.8}$$

From 4.2.1 it follows that, except for the cases where S is undefined -i.e., when R=0-, if **x**=**y**, then

$$S(\mathbf{x}, \mathbf{y}, t) = S(\mathbf{y}, \mathbf{x}, t) + \gamma(mod 2\pi) \tag{3.9}$$

and γ=0 or π for all t. Bacciagaluppi then demonstrates (2003, 7) that from .9 the symmetry of R follows, such that:

$$R(\mathbf{x}, \mathbf{y}, t) = R(\mathbf{y}, \mathbf{x}, t), \tag{3.10}$$

and then generalizes his proof to spinors.

We find ourselves here in the same case as with the three black marbles. The Indistinguishability Postulate guarantees that the labels attached to particles are not interpreted in such a way that some properties could be built up from them and differentiate them. All physical observables, including the well-defined position and velocities of these particles in Bohmian mechanics, must commute with the permutation operator. But the Symmetrization Postulate is not needed, because Bohmian mechanics has the resources to account for the distinction between the permuted states: even though these two states are indistinguishable from a synchronic point of view, they are not from a diachronic one, and there is an individuating property that now uniquely fixes the reference of the labels. In a nutshell, the extension of the Indistinguishabilibility Postulate to velocities and positions guarantees that generalized rays would not satisfy this postulate anymore.

Although this strategy is a perfectly sound one, it should not be taken as an *explanation* of the fact that states representing identical particles must be fully (anti)symmetric under permutation. What this strategy does actually is to deny that particles are indiscernible. This helps us realize what the deeper conceptual problem is with the topological approach presented in section 3.2: it aims at solving a problem that Bohmian Mechanics does not have, with tools that are not theirs. Bohmian Mechanics does not have a problem to begin with in explaining why symmetric states are the only admissible, while the Indistinguishability postulate admits more — the Indistinguishability Postulate does not admit more, when adequately extended such as to include the extra structure postulated by Bohmian Mechanics. If Bacciagaluppi is right, then partially symmetric states do not even arise in the formalism, without any need to appeal to the topological approach. However, using the same tools as standard Quantum Mechanics, i.e., ruling out paraparticles through the standard quantization procedure, is *ad hoc* in the context of Bohmian Mechanics, for Bohmian Mechanics already has enough structure to dispense with such a requirement. Bohmian Mechanics simply cannot assert that the distinction made in the permutation invariance is groundless and needs to be neutralized.

As a result, defenders of Bohmian Mechanics cannot argue that their theory has a greater explanatory power than standard Quantum Mechanics based on their explanation of the Symmetrization Postulate only. At best, defenders of Bohmian Mechanics can claim that the Symmetrization Postulate is not necessary in this specific theoretical framework, and make explicit the relationship between the structure of the theory, the observables it admits, and the consequences of modifying the scope of the Indistinguishability Postulate. Furthermore, the addition of extra structure will not find any justification within physics, but either in a metaphysical claim about the individuality of quantum particles or about their point-like nature, or in an philosophical claim about what a physical theory should be like — for instance, that the theory should be complete in a sense satisfying the EPR argument.

Another way to address the problem of justifying the Symmetrization Postulate is to simply admit that, given the structure of standard Quantum Mechanics, consequences of labels must be locally removed when the theory does not provide any individuating condition uniquely fixing their reference. This denial can be understood in two ways: either as a metaphysical assumption that particles are not the kind of things that can bear a name, or — and more interestingly to my eyes — as a linguistic statement. In that case, the postulate is simply stating that we do not have any theory of reference that can allow the use of labels when studying systems of identical particles, for no individuating condition is available that could unambiguously fix the reference of these labels. Thus, the Symmetrization Postulate simply is a way to circumscribe the use of labels to situations where the differentiation operated by labels can be accounted for, and to deny their use when it becomes illegitimate. From that point of view, the Symmetrization Postulate is not *ad hoc*: it is a mere acknowledgement of the structure of standard Quantum Mechanics, of its resources, and of when some distinctions that have been introduced become meaningless. In other words, the Symmetrization Postulate is an acknowledgment of which systems can be represented using labels or indices and which cannot, based on the structure of the theory itself — the physical quantities it admits and the relations among them.

3.4.3 Proof-of-the-case: how paraparticles arise in Quantum Field Theory

In section 3.4, I have argued that paraparticles in standard Quantum Mechanics are artifacts, only appearing in the formalism because of the lack of resources to distinguish between permuted states within the theory. Since paraparticles arise not only in Quantum Mechanics, but also in Quantum Field Theory, the latter gives us a good opportunity to test our hypothesis. If paraparticles are indeed artifacts of how systems are modelled in quantum theories, we should be able to trace back the origin of paraparticles in Quantum Field Theory in a similar manner.

In Quantum Field Theory (hereafter QFT), statistics can be encoded in two ways: either by encoding them in creation and annihilation operators, or by encoding them in field operators.[17] These two ways of encoding statistics differ in what they consider to be the bearer of statistics. In the first case, clearly, particles do this job, as statistics are encoded in the operators that create and annihilate particle-states. In the second case, no reference to particles is made or needed: fields bear statistics and are either bosonic or fermionic, depending on whether they commute or anti-commute at space-like separation. I will argue that introducing statistics in Quantum Field Theory through creation and annihilation operators leads to exactly the same situation as in Quantum Mechanics: given that Quantum Field Theory has no resources to distinguish between particles-states created or annihilated by these operators, maintaining a distinction between them through permutation invariance generates artificial statistics in the form of trilinear commutation rules.

Let us consider first the role of creation and annihilation operators. At the energy scale described by Quantum Field Theory, the number of particles considered does not remain constant — particles can be created and annihilated. Thus, one has to define creation and annihilation operators $\hat{a}_k^\dagger$ and $\hat{a}_k$ that will act on the vacuum state $|0\rangle$ so as to create or annihilate particles with momentum k. Introducing operators in Quantum Field Theory amounts to providing a representation of observables that satisfy Heisenberg equations of motion

$$\delta_\mu \phi(x) = i[P_\mu, \phi(x)], \tag{3.11}$$

where P_μ is the total energy-momentum 4-vector for the field $\phi(x)$; and allows for the expansion of fields in terms of positive and negative frequencies components:

$$\phi(x) = \sum_k a(+k)\phi_+(x) + a^\dagger(-k)\phi_-(x) \tag{3.12}$$

[17]For a more detailed introduction to statistics in Quantum Field Theory, see the first chapter of Bain (2016) and Greenberg (1998), by which this section is inspired.

where $\phi \pm (x)$ represents a complete set of orthonormal functions with positive and negative-frequency components. The relativistic 4-momentum is expressed in terms of creation and annihilation operators in the following way:

$$P^{\mu} = \sum_{k} P_{k}^{\mu}[a^{\dagger}(k), a(k)].^{18} \tag{3.13}$$

The scheme of quantization, i.e., the commutation rules, guarantees that the proper relations hold between the terms $\phi(x)$, $a(p)$, $a^{\dagger}(p)$ and P_{μ}. Commutation rules must satisfy the equations of motion while at the same time making sure that creation and annihilation operators respect the symmetry requirement defined for bosons and fermions; i.e., that creation and annihilation operators that (anti)commute will create or annihilate many-particle states that are (anti)symmetric under the permutation of single-particle substates; and that the order in which particles are put in states (k) or (k') does not make any difference. This is what is achieved by the ordinary bilinear commutation rules imposed on creation and annihilation operators:

$$\begin{array}{ll} \{a(k), a(l)\} = 0 & [a(k), a(l)] = 0 \\ \{a(k)^{\dagger}, a(l)\} = \frac{1}{2}\delta_{kl} & \left[a(k)^{\dagger}, a(l)\right] = \frac{1}{2}\delta_{kl} \\ \text{for fermions,} & \text{for bosons.} \end{array} \tag{3.14}$$

Here is the problem however. As shown by Green (1953), these commutation rules can be generalized into trilinear commutation rules that also satisfy Heisenberg equations of motion:

$$\begin{array}{ll} [a(k), [a(l), a(m)]] = 0 & [a(k), [a(l), a(m)]] = 0 \\ \left[a(k), \left[a(l)^{\dagger}, a(m)\right]\right] = \frac{1}{2}p\delta_{kl}a(m) & \left[a(k), \left\{a(l)^{\dagger}, a(m)\right\}\right] = \frac{1}{2}p\delta_{kl}a(m). \end{array} \tag{3.15}$$

[18] This relation holds for bosons. Commutators must be replaced by anticommutators for fermions.

As was the case in non-relativistic Quantum Mechanics, these commutation rules correspond to a para-statistical behavior that is not observed in nature, but should be if one accepts the principle of plenitude. The imposition of bilinear commutation rules, therefore, seems as *ad hoc* as the Symmetrization Postulate appeared at first sight. Notice that again, distinctions are made that the theory has no resources to handle: the "particles" that are created and annihilated are quanta of excitation in fields, i.e., chunks of energy $\hbar\omega_k$ that are indistinguishable in the same way that quantum particles were indistinguishable. In other words, there is no predicate or quality that can account for the distinction between $a(k)$ and $a(l)$ when $k = l$, as is the case when considering identical particles. The distinction maintained between those two annihilation operators in the commutator that defines them does not rely on any physical grounds. Generalized vectors were obtained in standard Quantum Mechanics by combining vectors describing states that the theory has no resource to distinguish. Likewise, generalized commutation rules obtain from combining operators describing the creation or annihilation of particles that Quantum Field Theory has no resource to distinguish. Hence, the equations of motions could equally well be satisfied by fermionic creation and annihilation operators such as the following

$$\begin{aligned} \hat{a}_i^\dagger \hat{a}_j^\dagger + \hat{a}_j^\dagger \hat{a}_i^\dagger &= 0 \\ \hat{a}_i \hat{a}_j^\dagger + \hat{a}_j^\dagger \hat{a}_i &= \delta_{ij} \end{aligned} \tag{3.16}$$

or, as shown by Green (1953), by parafermionic operators as defined below:

$$\begin{aligned} \hat{a}_i^\dagger \hat{a}_j^\dagger \hat{a}_k^\dagger + \hat{a}_k^\dagger \hat{a}_j^\dagger \hat{a}_i^\dagger &= 0 \\ \hat{a}_i^\dagger \hat{a}_j \hat{a}_k + \hat{a}_k \hat{a}_j \hat{a}_i^\dagger &= \delta_{ij} a_k \\ \hat{a}_i \hat{a}_j^\dagger \hat{a}_k + \hat{a}_k \hat{a}_j^\dagger \hat{a}_i &= \delta_{ij} a_k + \delta_{kj} a_i. \end{aligned} \tag{3.17}$$

Now contrast this case with the second way of encoding statistics in field operators, that which can be seen at play in Wightman's axiomatic Quantum Field theory. Field operators $\phi^{\dagger}(x)$ and $\phi(x)$ are constructed from a linear combination of operators and act on the *real* space: they create and annihilate particles localized *at particular spatial locations*. In that case, quanta are not only defined by their momentum as they were above, but also by their coordinates. Thus, encoding statistics in field operators guarantees that particle-states created by field operators are always given a unique description in terms of their coordinates that distinguish them from other particle-states. It does so through a locality principle, according to which fields either commute or anti-commute, depending on whether they are bosonic or fermionic:

$$[\phi(x), \phi^{\dagger}(y)]_{\mp} = 0, \text{for spacelike (x-y).} \tag{3.18}$$

The commutativity of fields at spacelike separated points makes the theory relativistic, in that it implements the prohibition of superluminal signals. 3.18 is slightly stronger than mere local commutativity, in that it also acts as a definition: fields that commute are bosonic, and fields that anti-commute are fermionic–although nothing is said about a possible relation between spin and statistics. Now, the equation 3.14 that was excluding parastatistics by hand in the first approach *follows* from this statistics-locality assumption, once a Fock space is given, and paraparticles are naturally excluded.[19]

Does it follow from this that Wightman's approach has a greater explanatory power than, say, Weinberg's or the Lagrangian approach, as it excludes paraparticles? The reader will probably anticipate my answer here: the two approaches do not address the same problem. In the latter, equation 3.14 introduces bilinear commutation rules among creation and annihilation operators as a way to implement the permutation invariance of single-particle states within many-particles states. By contrast, Wightman's approach dispenses with any reference to particles and introduces statistics for fields operators, where statistics do not refer to the behavior of particles under permutation; but to the independence of fields that are spacelike separated. 3.18, therefore, is genuinely a locality principle, as it follows from the restrained

[19]See Bain (2016), chapter 1.

Lorentz invariance of time-ordered functions and of Wightman's functions. 3.14, by contrast, becomes a locality principle — usually referred to as a "causality" principle — only when bilinear observables are constructed from the creation and annihilation operators. As emphasized by Greenberg (1998), the spin-theorems that can be built from these assumptions are also distinct theorems and distinct ontological claims:

> I have two purposes in this note. The first is to make clear the difference between the spin-statistics theorem: *particles* that obey Bose statistics must have integer spin and *particles* that obey Fermi statistics must have odd half-integer spin, and what I suggest should be called the spin-locality theorem: *fields* that commute at spacelike separation must have integer spin and *fields* that anti-commute at spacelike separation must have odd half-integer spin (Greenberg 1998, 144).

3.5 Conclusion

In this paper, I have examined arguments to the effect that Bohmian mechanics should be privileged over its empirically equivalent rivals based on its ability to explain the Symmetrization Postulate. Neither this strategy nor its denial, based on the claim that paraparticles are surplus structure, were found to be satisfactory answers to the question of why states describing systems of identical particles should be fully symmetric or antisymmetric. As a result, I have suggested to change our perspective on the problem and to ask instead how can systems be represented given the resources offered by the structure of Quantum Mechanics. More specifically, I have argued that the Symmetrization Postulate should be understood as the statement that, when it comes to systems of identical particles, Quantum Mechanics lacks the resources needed to unambiguously determine the reference of a label, and that the distinction between two distinct states maintained in the very idea of permutation invariance must be erased. Similarly, in Quantum Field Theory, particle states are abstract entities that the theory has no resource to distinguish, therefore generating artificial relationships among particle creating operators than cannot be uniquely determined. From the point of view of the structure of the theory, we can now understand 1) why the explanatory strategy in this case is neither genuinely explanatory nor in a position to convince rival approaches, as committed to additional structure whose introduction can only be justified by metaphysical or philosophical assumptions; 2) how paraparticles artificially emerge from an implicit assumption made in the permutation invariance requirement, i.e., that a distinction can be maintained between two states only differing by a permutation of labels, 3) that the Symmetrization Postulate is a mere acknowledgement of this referential ambiguity: when it comes to systems of identical particles, the reference of labels is no longer uniquely defined and the consequences of these labels must therefore be neutralized.

Chapter 4

On Robustness in Cosmological Simulations

4.1 Introduction

The standard ΛCDM cosmological model describes a very nearly homogeneous early universe, where very small density inhomogeneities evolve with time through gravitational collapse to form the large-scale structures we now observe. An essential component of this cosmological model is a rather mysterious kind of matter called ‘dark matter’, that only interacts gravitationally. Whereas the collapsing of ordinary, baryonic matter is opposed by the outward radiation pressure of photons, dark matter is not opposed by such a force and starts collapsing in dark matter haloes earlier than ordinary matter, thus providing the scaffolding where stars can merge with gas and form galaxies, cluster of galaxies, and all the large-scale structures of the universe.

Properties of dark matter are mostly unknown, beyond the fact that it must be dissipationless[1] and that it cannot be *hot*. Hot dark matter is made of particles moving at relativistic speed, whose speed would permit their escaping from the small density inhomogeneities and to wash

[1]If dark matter were to convert its kinetic energy in photons radiation, then the same radiation pressure that prevents the collapse of baryonic matter would halt its collapsing and structure formation would happen way later than observed.

any small scales fluctuations out. In such a scenario, only the bigger lumps would survive and the large-scale structures would form earlier than galaxies, born from the fragmentation of the latter, in contradiction with our observations of the universe. Cold dark matter, on the other hand, is made of more massive particles, whose non-relativistic speed is compatible with the survival of lumps at small scales. Such a scenario, based on cold dark matter, predicts with great accuracy the statistics of hierarchical clustering: mass function and clustering of dark matter haloes, their evolution with redshift, the non-linear evolution of the dark matter power spectrum, correlation functions, or the topological properties of large-scale structures. However, while the Cold Dark Matter (hereafter CDM) model is well supported by evidence on large scales, it does not fare as well on small scales, where simulations fail to reproduce the observed abundance and demographics of dark matter haloes structure. Since different models of dark matter such as cold, warm, or self-interacting dark matter agree on large-scale, but differ precisely on their predictions about the structure of dark matter haloes, understanding how mass is distributed in the haloes is crucial for determining the nature of dark matter. At such a scale though, only numerical approaches to determining what a hypothesis implies for haloes' mass distribution are possible. Non-linear effects related to star formation and gas dynamics make it impossible to determine the halo mass distribution analytically. Hence, numerical simulations are needed to determine the mass distribution; and these simulations are a crucial part of evaluating the CDM model and various rival hypotheses.

Understanding in which case a simulation can succeed in (dis)confirming a model is, however, still a challenge in cosmology. As mentioned, dark matter models differ on their predictions with respect to the haloes' structure. On small scales, the CDM model seems to do worse than its rivals: simulations predict, for instance, much more substructure within dark matter haloes than is actually observed. Prior to 1998, however, the problem was the exact inverse; simulated dark matter haloes did not present enough substructure compared to observations. When a model has been shown to be so sensitive to modelling assumptions, what is the conclusion that should be drawn from a mismatch between simulation outcomes and observations?

How can we assess whether the missing satellite problem stems from numerical artifacts or constitutes a genuine failed prediction?

In biology and climate science, evaluation of when evidence confirms a model and in what sense this confirmation must be understood has been based on robustness analysis (Levins 1966; Wimsatt 1981/2012; Weisberg 2006 and Weisberg 2012). In cosmology, astrophysicists have been relying on a methodology similar to robustness analysis, in which results that resist a change of values of the numerical parameters or the use of different codes are considered trustworthy. In this paper, I will argue that robustness is not a sufficient criterion for determining when a prediction is reliable in N-body simulations, for simulations can be made to converge on mutually exclusive sets of results. Even more worrying, numerical artifacts can generate robustness, inasmuch as they can turn a specific result into an attractor solution, on which all simulations, regardless of coding choices, will unavoidably converge. As a result, I propose another methodology, that of crucial simulations, meant to put the numerical or physical origin of a prediction under a crucial test.

In 4.2, I introduce the reader to the fundamentals of N-body simulations and to the way robustness analysis is used to assess the physical significance of their outcomes. Section 4.3 offers two arguments against a first kind of robustness analysis that astrophysicists have dubbed 'convergence studies', which consists of looking for a region in the parameter space where outcomes of simulations are independent from the value assigned to numerical parameters. Section 4.4 extends this criticism to a possible other instance of robustness analysis, based on the search for robust predictions across different codes. Finally, in section 4.5, I present the methodology of crucial simulations and suggest to re-direct the use of code comparisons, in order to extract from them hypotheses to test in crucial simulations.

4.2 Robustness and Convergence Studies

In this section, I introduce the methodology used by astrophysicists for evaluating the trustworthiness of a simulation outcome and justify the subsumption of these so-called 'convergence studies' under the broader concept of robustness analysis. Based on the work of van den Bosch and Ogiya and Baushev et al., I highlight the limits of convergence and its failure to deliver what it is supposed to: a reliable indicator of when predictions can be trusted to be *physical* predictions, as opposed to *numerical* artifacts.

4.2.1 The methodology of N-body simulations in cosmology

N-body simulations have first been used in the 1970's in order to study whether gravity alone could be responsible for the formation of clusters of galaxies. The simulation of the gravitational collapse of a cloud of 300 particles detailed in Peebles 1970 was considered the first realistic simulation of cosmic structure formation. N-body simulations can be used to represent the temporal evolution of cosmic structure at large-scale–the so-called 'cosmological simulations'–[2] or the evolution of individual dark matter haloes–or 'zoom-in' simulations–, with initial conditions generated in accordance with the parameters defining the ΛCDM model. The distribution of particles depends on the problem to solve. If the simulation is a cosmological one, then the distribution of particles will be nearly homogeneous and particles will have the same mass. If the intent is to simulate a smaller region with higher resolution, then many small particles will be distributed in the region of interest, with a few large ones in the rest of the volume. The evolution of the distribution of particles is then treated using a conjunction of the collisionless Boltzmann equations for dark matter particles and the Poisson equation for the gravitational potential.

The way gravitational forces are calculated differ from one code to another. In Peebles 1970, these forces were calculated using direct summation, i.e., by summing up all contribu-

[2] See for instance the Millenium, the Bolshoi or Illustris simulations.

tions from all particles. These codes are sometimes called Particle-Particle codes. This technique is almost abandoned nowadays, given its computational cost–the number of operations needed to calculate the forces scales as N^2. One can, instead of summing the contributions of all particles, appeal to a Particle-Mesh code using a three-dimensional mesh covering the cubic domain of the simulation. The idea is simple: calculate the density field for every node of the mesh, using a technique called Cloud-in-Cell density assignment[3]; solve the Poisson equation for the gravitational potential; advance velocities, coordinates and time and repeat for every time step. This code not only discretizes the time but also the space by covering the domain of the simulation with a mesh. The advantage of Particle-Mesh methods is that the resolution can be increased wherever needed by adapting the mesh size and placing smaller cubic cells in the regions of interest. This technique is known as 'Adaptative Mesh Refinement'. Another popular way of calculating gravitational forces resides in tree-codes, that do not calculate the contribution of individual particles or the field density but instead group particles hierarchically and replace individual contributions with a single multipole force for the whole group. An oct tree algorithm is used to group particles, based on a given threshold per cell–e.g., if the threshold is one particle per cell, and the number of particles in a cell exceeds this number then the cell will split into smaller cubic cells, and so on until each cell contains only one particle. Once the tree is constructed and the information relative to mass distribution stored in each cell, forces are found by testing, for each cell of size l, whether the opening angle $\theta = l/d$ is greater than a specified threshold. In this case the force contributions from the cell are ignored and the cell 'opens', so that the opening angles of the cell's children are tested instead (see figure 1). The force contribution from the cell is accepted once the opening angle is smaller than the specified threshold. The idea is to treat individually only the nearest particles, while the distant ones are treated collectively, as a group, to diminish the overall computational cost.

For each of these codes, there are essentially four purely numerical parameters that need to

[3]The rough idea of the Cloud-in-Cell technique is to calculate the distance between the center of the mesh cells and the particle and to assign, based on this distance, a weighted fraction of the total particle mass to the nearest 2, 4 or 8 mesh cell centers.

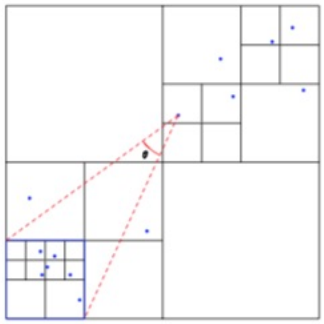

Figure 4.1: An example of particle grouping algorithms for TREE codes from Klypin 2017. The red dashed lines show the opening angle θ for a particle close to the centre and a given cell (the blue square).

be calibrated:

- The mass resolution N_p, which refers to the number of particles used in the simulation;

- The time-step Δt: the N-body problem in astrophysics consists of solving the Newtonian equations of motions and finding the velocities and coordinates of a number N of massive particles only interacting through Newtonian gravity, given their initial coordinates and velocities. If r_i and m_i are the coordinates and masses of the particles, then the equations of motion that must be integrated are:

$$\ddot{r}_i = -G \sum_{j=1, i \neq j}^{N} \frac{m_j (r_i - r_j)}{|r_i - r_j|^3} \tag{4.1}$$

  In the simplified case[4] where one would use Euler integration method to find the new coordinates r_i of the i^{th} particle $r_i(t + \Delta t) = r_i(t) + \Delta t v_i(t)$, then the time-step Δt is simply the time step between the initial time and the later time at which coordinates and velocities must be found.

- The force accuracy θ: the accuracy of the force computation has a distinct meaning

[4]In real simulations, Euler integrator is never used because of its low accuracy. Rather, a second-order integration scheme called 'Leapfrog' is used. See Klypin 2017, section 4 for a detailed discussion of both integrators and their merits.

depending on the codes. In a tree code, it corresponds to the opening angle θ above which force contributions are ignored. In a Particle-Mesh code, it corresponds to the size of the grid.

- The force softening ϵ: real dark matter particles are substituted by heavy particles, which means that gravitational forces can generate very large, unphysical accelerations when two particles get very close to each other. Force softening is used to smooth the gravitational potential and suppress these accelerations below a typical distance–the 'softening length'. Like force accuracy, force softening has a different meaning for different codes: mesh codes define softening based on the size of the cell elements, while tree codes use a method referred to as 'Plummer softening" and replace the distance $\Delta r_{ij}^2 = |r_i - r_j|$ in equation 2.1.1 with the expression $(\Delta r_{ij}^2 + \epsilon^2)^{1/2}$.

4.2.2 What is robustness analysis?

Ever since the 1970's, simulations based on the Cold Dark Matter model have been incredibly successful in reproducing the observed structure of the universe at large scale. In 1982 for instance, the remarkable match between the first extensive 3D galaxy survey and the outcomes of large-scale simulations was considered a huge success for the CDM model and largely contributed to abandoning its hot dark matter rival (Frenk and White 2012). Almost 50 years after this first attempt at a 'realistic' N-body simulation, simulations still constitute an indispensable tool in order to extract predictions from dark matter models about the distribution and properties of clusters of galaxies. With a close difference: simulations now benefit from faster processors, parallel-computing methods, increased computational power allowing to run simulations exceeding 10^6 particles and to model dark matter but also baryonic components such as gas, stars, supermassive black holes and their energetic feedback. As a result, it has become more and more difficult to track the causal contributions of distinct components of the simulations–e.g,the physical model implemented, the numerical parameters, the gravity solver, the integration algorithm–, and to determine what conclusion should be drawn from mismatches between observations and simulation outcomes.

As mentioned in the introduction however, the CDM model faces a number of problems at small scale. It predicts for instance way more substructure in a dark matter halo of the size of the Milky Way than is actually observed. Only 59 satellite galaxies[5] seem to orbit our Milky Way, whereas several thousands of them are predicted by the CDM model–hence the name of "missing satellite" problem. Likewise, the density profile drawn from this model by Navarro, Frenk and White predicts a steep, cuspy profile in the central region of dark matter haloes, with an infinite density at the center. Yet, observations favor a 'cored' profile, with a more shallow, flatter density profile as the radius tends toward zero. Observations for dwarf and low-surface-brightness galaxies are especially problematic, not only because the Navarro-Frenk-White (hereafter NFW) profile is supposed to be a universal density profile, but more importantly because these galaxies are mostly made of dark matter, which means that this discrepancy will not easily be washed away by adding more baryonic physics to the simulations. [6]

Given that the predictions made by the CDM model are not drawn from first principles, but from analytical fits to dark matter-only simulations, assessing the extent to which these two problems challenge this model is a very difficult task to undertake. How can we assess whether the discrepancy between the simulated systems and the observed ones stems from the physical model at play or from an erroneous code? How can we determine whether the simulated outcome is altered by numerical artifacts or constitutes a genuine failed prediction? Astrophysicists have been heavily relying on robustness analysis in order to decide when the outcome of a simulation is reliable. Robustness analysis has been famously suggested by Levins in 1966 as a way to assess the trustworthiness of models in population biology in the absence of a background theory providing analytically soluble equations, while appreciating the complexity of

[5] The discovery of the two dwarf galaxies Carina~II and ~III in January 2018 and of the low-surface-brightness galaxy Antlia 2 in November 2018 brings the current number of observed satellite galaxies to 59. See Torrealba et al. 2018a and Torrealba et al. 2018b.

[6] In this paper, I will focus only on these two problems, given that they constitute prime discriminators for understanding the nature of dark matter. More exhaustive reviews of all the controversies arising at small scale for the CDM model can be found in Weinberg et al. 2015 and Bullock and Boylan-Kolchin 2017.

the many parameters to take into account. Since models have to be simplified to get predictions to measure against nature, a method must be developed in order to evaluate the impact of these simplifications and to determine "whether a a result depends on the essentials of the model or on the details of the simplifying assumptions" (Levins 1966, 423). This is the role played by robustness analysis, which consists of addressing the same problem with a diversity of models relying on different simplifications, so as to test the agreement of these models on their predictions and to confirm the independence of the latter from the simplifications made:

> (...) [I]f these models, despite their different assumptions, lead to similar results we have what we can call a robust theorem which is relatively free of the details of the model. Hence our truth is the intersection of independent lies (1966, 423).

Levins's account seems to rely on a kind of eliminative reasoning, although not explicit, that exposes it to a famous criticism first formulated in Orzack and Sober 1993. Indeed, each of the three models considered by Levins to assess the reliability of the proposition "In an uncertain environment species will evolve broad niches and tend toward polymorphism" excludes a specific possibility: the first two make distinct modelling choices to represent the diversity of environments and its relationship to fitness, but ignore the impact of genetics that the third takes into account. According to Orzack and Sober however, the robustness reasoning will yield valid inferences if and only if an exhaustive set of all possible models is examined, and such an exhaustivity is excessively difficult to achieve.

In 1981, Wimsatt further strengthened the notion of robustness analysis by making explicit the procedure underlying this analysis. According to Wimsatt, the robustness of something–be it a property, an experimental result, or a prediction–is determined through the following four-steps procedure:

- To analyze a *variety* of *independent* derivation, identification, or measurement processes.
- To look for and analyze things which are *invariant* over or *identical* in the

conclusions or results of these processes.

- To determine the *scope* of the processes across which they are invariant and the *conditions* on which their invariance depends.

- To analyze and explain any relevant *failures of invariance*.

This definition includes the use of different assumptions to build models deriving the same result, but also "using different experimental procedures to verify the same empirical relationships or generate the same phenomenon", or "using different sensory modalities to detect the same property or entity". According to Wimsatt, robustness analysis is defined through the following three principles: first, it is a procedure aiming at distinguishing the "reliable from the unreliable", and second, it requires to show the invariance of that which reliability is scrutinized over different processes or models, in order to build confidence in their independence from these; and finally to determine the scope of this invariance. Eliminative reasoning seems absent from Wimsatt's account, inasmusch as the ensemble of models is not constructed in such a way that each of them excludes a possibility. The robustness of a property is justified through its overdetermination by independent models, but not by the exclusion of all possible sources of artifacts. Although this independence needs be qualified, such an account does not require to consider all possible models; but only that models are independent 'in an appropriate way'. In what follows, I will consider any procedure satisfying these Wimsattian features an instance of robustness analysis.[7]

[7]More recently, Weisberg (2012) has been building on Levins' account and suggested a two-step procedure for searching robust theorems. This two steps involve, first, examining a group of similar but distinct models, searching for a common result deemed the 'robust property'; then finding the core structure giving rise to the robust property. However, the lack of modularity exhibited by simulations in cosmology undermines any attempt to find the common structure responsible for the common property, and the subsequent formulations of a robust theorem. Such a search requires to develop a fully fleshed out methodology to gain some modularity back or to overcome the lack thereof. For this reason, I will leave aside Weisberg's work and focus on Wimsatt-based definitions of robustness.

4.2.3 Robustness analysis and Convergence Studies

Robustness analysis transferred into astrophysics takes the form of 'convergence studies'. Given that cosmological parameters such as the matter density parameter Ω_m, the dark energy density parameter Ω_λ, the Hubble constant H_0, the scalar spectral index n_s, and the amplitude of the power spectrum σ_8 are constrained by the observations from WMAP and the Planck collaboration (see Dunkley et al. 2009 and Planck et al. 2014), the idea behind convergence studies is to explore the role of the unconstrained four numerical parameters listed in 2.1. More precisely, it is to define the conditions under which the structure of a simulated halo does not depend on the value assigned to these purely numerical parameters and can thus be deemed 'appropriately resolved'.

One of the most influential convergence studies is that of Power et al. 2003, which contributed to set up the parametrization of N-body simulations for the last fifteen years. Their methodology can be summarized as follows: first, they derived scalings between numerical parameters from analytic estimates, in order to constrain the parameter space. An example of this scaling is the following. Artificial discreteness effects, due to the limited number N of simulated particles, requires the number of time-steps Δ_t to increase as the value of the force softening ϵ gets smaller. As a consequence, efforts to limit the computational power cost dictates the use of large softenings, in order to limit the number of time-steps needed. On the other hand, large softening jeopardizes the spatial resolution of the simulations. Thus, the relationship between N, Δ_t and ϵ is such that an optimal value of ϵ must be found to reduce the computational cost while limiting discreteness effects and preserving the spatial resolution. The following constraints on these parameters are drawn from this scaling analysis:

- The time-step must be shorter than the orbital timescale: $t_{circ}(r) \geq 15(N_{\Delta_t}^{-5/6} t_{circ}(r_{200})$, where Δ_t is the total number of timesteps, $t_{circ} = 2\pi r/V_c(r)$ the circular orbit timescale, and r_{200} the virial radius, the radius of a sphere of mean density contrast 200. This criterion applies under the condition that the force softening value guarantees that particle discreteness effects are negligible, i.e., that $\epsilon \geq 4r_{200}/\sqrt{N_{200}}$, where N_{200} is the number of particles contained within the virial radius.

- Enough particles must be enclosed so that the average two-body relaxation timescale within the region $t_{relax} = (r/V_c(r))N(r)/(8ln\Lambda_C)$ is comparable to the age of the universe t_0.

.

Then, they simulated a large cosmological volume with low resolution, tracking the structure growth seeded by Gaussian primordial density fluctuations up to redshift $z = 0$,before zooming-in on targeted haloes and re-simulating them at much higher resolution. Once they obtained a sample of haloes, several hundreds of simulations (typically with a resolution of 32^3 particles) were run, allowing to survey the reduced parameter space by systematically varying the numerical parameters, draw preliminary convergence results, further refine the convergence criteria and finally confirm these convergence with another series of simulation of higher mass resolution –a series of run with 64^3 particles, and a few (given how expensive they are) run with 128^3 and 256^3 particles. Granted that the convergence criteria guaranteeing that the mass profile of the halo is reliably resolved are satisfied, if in this region of the parameter space the predictions–in this case, the circular velocity as a function of the radius–remains the same despite the increased resolution, then one can be confident that they are independent of the values assigned to the numerical parameters and thus not affected by it. As robustness was a guarantee of the independence of the property or prediction from the specifics of the experimental techniques or models used, likewise, the convergence of the simulated profile is supposed to warrant its independence of the numerical parameters and thus confirm that "the mass profiles are unlikely to be affected by numerical artifacts" (Navarro 2003, 4).

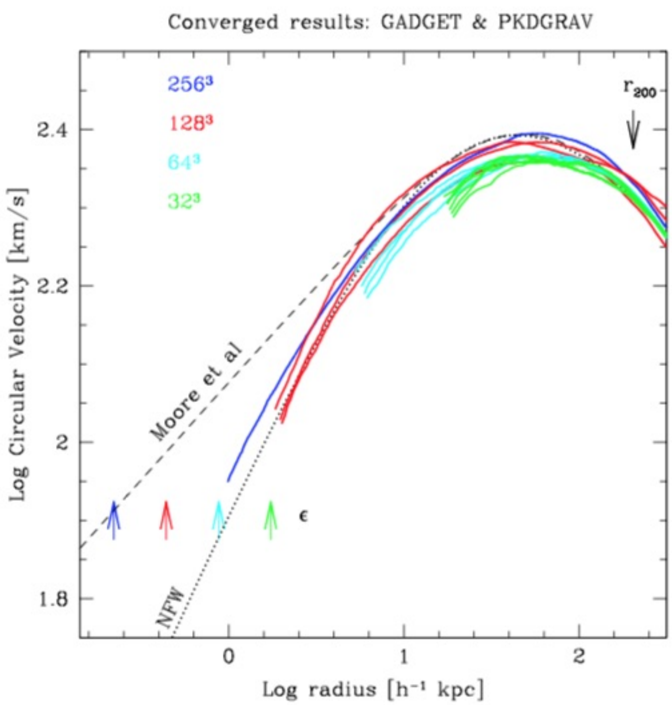

Figure 4.2: An example of converged circular velocity profiles for dark matter haloes from Navarro 2003. The profiles are plotted for a large number of converged runs with different resolutions and interpreted by the author as "roughly independent of the number of particles" (p. 9, figure 3) and thus confirming the reliability of the NFW profile.

4.3 Against Convergence

4.3.1 Convergence is necessary but not sufficient

Predictions extracted from the ΛCDM model *via* N-body simulations are taken to be reliable when it comes to the amount of substructure that should be observed in dark matter haloes. The main reason underlying this confidence is that, as one should expect given the above, these predictions resist convergence studies really well above a resolution of 50-100 particles per subhalo and seem not significantly affected by an increase on resolution above this limit. Springel et al. 2008[8] for instance, followed a methodology very similar to Power et al. 2003: starting with a simulation of halo formation with a very large periodic box, they targeted haloes for re-simulation at higher and higher resolution, in order to assess whether the substructure would be affected by a change of resolution. The negative answer, according to them, forces some really strong conclusions upon us:

> The results presented above demonstrate that we have created a remarkably accurate set of simulations, reaching very good convergence for the dark matter density profile and the substructure mass function over the maximum range that could be expected. Even the location, mass and internal structure of individual large dark matter subhaloes reproduce well between simulations of differing resolution, a level of convergence which exceeds anything previously reported in the literature (2008, 1709).

As briefly mentioned in the introduction however, the amount of substructure predicted in dark matter host haloes is extremely sensitive to modelling assumptions. Up to the close of the last century, simulations were suffering from an 'overmerging' problem, in that not enough substructure was predicted to match the observations. Several culprits had been proposed by then, with no consensus on the exact cause of the subhaloes disruption, but with an agreement on the *numerical* nature of the most likely responsible. Moore et al. 1995 and Klypin et al. 1999

[8] See also Onions et al. 2012, Knebe et al. 2013, or Griffen et al. 2016.

blamed inadequate force softening. Carlberg 1994 and van Kampen 1995, on the other hand, argued that a low mass resolution could cause two-body heating and artificially enhance matter disruption. Since this problem was eventually superseded by a 'missing satellite' problem as the resolution of simulations increased, no definitive conclusion was drawn about what exactly was causing the overmerging. Yet, as shown in van den Bosch et al. 2017, subhaloes disruption is still "extremely prevalent in modern simulations, with inferred fractional disruption rates (at z=0) of $\sim$ 1 percent per Gyr", which means that "$\sim$ 65 percent of all subhaloes accreted around z=1 are disrupted by z=0" (2017, 2). One question that arises is thus the following: are N-body simulations still suffering from overmerging, or can they really be considered reliable, based on convergence studies? In other words, is the subhaloes disruption due to a physical mechanism or the result of numerical artifacts?

This question is precisely that addressed by van den Bosch and Ogiya 2018. Their goal in this paper is both to investigate whether N-body simulations still suffer from an overmerging problem and to gain a better understanding of the non-linear effects of tidal stripping of dark matter subhaloes. 'Tidal stripping' refers to the escape of matter due to the tidal forces exerted by the external host halo on the subhalo. Beyond the limit known as the Roche limit, the tidal forces exerted by the host halo overcome the gravitational force bounding the subhalo system together, resulting in the system's dislocation. Such tidal processes are extremely difficult to describe, as the stripping of matter causes the subhalo remnant to fall out of virial equilibrium[9], and then to re-virialize by expanding, thereby provoking more stripping of matter, and to fall out of equilibrium once again (2018, 4). No analytical theory is available to describe such complicated processes of de- and re-virialization. Understanding such a prevalent mechanism for matter disruption, therefore, requires to invent new ways of investigating its consequences on the stripped subhalo's density distribution. Given that, in the absence of a background

[9]The virial theorem applied to celestial bodies states that the kinetic energy of a system is equal to half its potential energy–here, its gravitational potential energy–, or, in other words, that its gravitational potential energy is equal to twice its kinetic energy. If this equality does not hold, the system is either collapsing –the gravitational potential energy exceeds the kinetic energy– or expanding. A system for which this equality applies is considered in *virial equilibrium*.

theory providing some guidance about this mechanism, only simulations can provide a window into how the density distribution of the subhalo is affected by tidal stripping, it is of the utmost importance to disentangle what really pertains to this stripping mechanism and what artificially results from an inadequate parametrization. In order to do so, van den Bosch and Ogiya 2018 came up with an idealized numerical scenario where the *physical* hypothesis of tidal stripping could be tested again the *numerical* hypothesis that matter disruption is mostly caused by inadequate force softening. The idea is the following: consider the tidal evolution of an isolated subhalo on a circular orbit. Its isolation permits to avoid having to consider 'galaxy harassment', i.e., high-speed encounters with other subhaloes causing more matter to disrupt. Likewise, considering a circular orbit excludes the possibility of other physical mechanisms enhancing disruption, such as tidal heating due to the fast pericentric passage on an eccentric orbit.[10] In such a scenario, only the host halo's tidal field and the numerical parameters can have an impact on the disruption of subhaloes. The hope, based on this methodology, is thus to determine whether the disruption is caused by the former or by the latter, by studying how the bound mass of subhaloes evolve when varying systematically the strength of the tidal field, the mass resolution, the force softening, the force accuracy and the time resolution.

The figure below shows the bound fraction of mass as a function of time and suggests that decreasing the orbital radius, and thereby increasing the strength of the tidal field, does not lead to the disruption of subhaloes, except when the orbital radius is chosen so that $r_{orb}/r_{vir,h} = 0.1$, i.e., for the smallest value of the orbital radius.[11] Even after 60 Gyrs, i.e, well beyond the age of the universe, a significant bound remnant survives the tidal stripping.[12] However, at $r_{orb}/r_{vir,h} = 0.1$, the subhalo totally disrupts after $\sim$ 13 Gyrs. This results concords with the results of the Millenium and the Boshoi simulations according to which $r_{orb}/r_{vir,h} = 0.1$ is the

[10]A fast pericentric passage implies a rapid change in the tidal field that results in the orbiting body's deformation, and thus to frictions and internal heating, which eventually will lead to more loss of mass.

[11]$r_{orb}/r_{vir,h} = 0.1$ is the orbital radius of the subhalo expressed in units of the host halo's virial radius, with r_{orb} the distance between the centres-of-mass of the host and subhalo and $r_{vir,h}$ the virial radius, defined as the radius inside of which the average density is Δ_{vir}=97 times the critical density for closure.

[12]All these fiducial simulations are run with the following parameters: initial mass of subhalo $m_{s,0} = 1$, mass of host halo $M_h = 1000$, concentration of subhalo $c_s = 10$, concentration of host halo $c_h = 5$, mass resolution $N_p = 10^5$, time-step $\Delta t = 0.02$, force softening $\epsilon = 0.05$, tree opening angle $\theta = 0.7$.

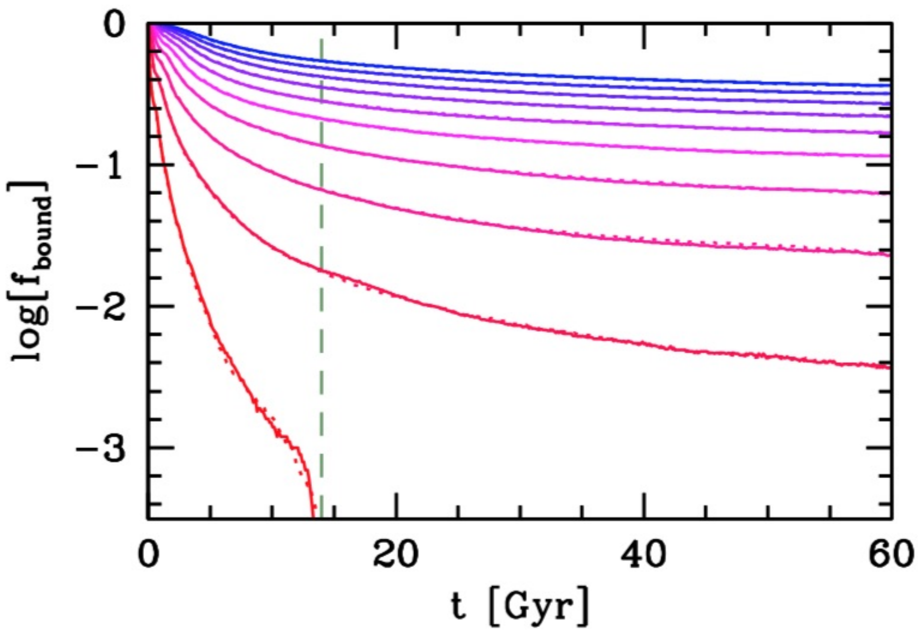

Figure 4.3: This figure shows the bound fraction of the subhaloes' mass as a function of time. Colours from blue to red correspond to values of the orbital radius ranging from 1.0, 0.9, ..., to 0.1, and the solid and dotted lines correspond to runs with different codes. See van den Bosch and Ogiya 2018, figure 4, 4071.

typical radius at which subhaloes undergo disruption. Now, if this prediction is a physical prediction, as it is taken to be by most astrophysicists, it should not be significantly affected by a variation in numerical parameters.

Now, look at what happens in van den Bosch and Ogiya 2018 when examining the impact of individually varying each numerical parameter. Runs of simulations varying the time-step Δt and the force accuracy θ, with other parameters kept at their fiducial values, show that the amount of mass loss is not significantly increased for any value of $\Delta t \leq 0.08$ and of $\theta \leq 1$. As a consequence, the values assigned to Δt and θ that will be kept fixed for exploring the effects of the two others numerical parameters are chosen such as to be as conservative as possible, without thereby increasing the computational cost unnecessarily–$\Delta t = 0.02$ and $\theta = 0.7$. According to Power et al. 2003, the optimal value for force softening is that for which the maximum stochastic acceleration caused by closed encounters with individual particles is smaller than the minimum mean-field acceleration in the halo. This criteria, along with other criteria that can be found in the literature[13], suggests an optimal softening ranging between 0.02 and 0.06. Since a force softening too large usually results in a too small density distribution at small radii and less dense systems are more exposed to tidal stripping, it seems natural to predict that a force softening larger than the optimal value would enhance stripping and, subsequently, increase the amount of matter disruption. Likewise, as we have seen earlier, a softening too small is more exposed to two-body relaxation effects, which contribute to flatten the central density profile and thus to enhance disruption. Thus, based on this naive analysis, force softening should have no other consequences on the amount of matter observed than increasing the mass loss. Surprisingly however, the simulations show the opposite: for $\epsilon < \epsilon_{opt}$, the bound remnants are larger and survive longer. Thus, one can already suspect that physical disruption is very sensitive to the value assigned to ϵ. Finally, one must assess the consequences of increasing or decreasing the mass resolution. As mentioned earlier, simulations are necessarily run with a limited number of particles, which unavoidably creates artificial discreteness noise that translates into highly divergent behavior in the bound fraction of mass and artificially triggers subhaloes disruption. The smaller the mass resolution, and the smaller the orbital radius, the higher the divergence is: the standard deviation in $f_{bound(t)}$ after 60 years from a simulation to the exact same simulation can reach 0.4dex *even for a very high mass resolution* of $N_p = 300000$ (2018, 4075).

Consider now what happens to simulations run with the fiducial parameters at the orbital radius $r_{orb}/r_{vir,h} = 0.1$ at which subhaloes are supposed to undergo disruption, when the mass resolution and the force softening are increased together. If you look for a region in the parameter space where the discreteness-driven instability is kept under control [14] and a 'converged'[15] fraction of bound mass $f_{bound}(t)$ remaining after a given time can be found, the upper-left corner of the figure below will give you the closest you can hope for to such an ideal. In this upper-left

[13] See for instance Van Kampen, 2000.

[14] The sensitivity to discreteness noise is characterized by the authors using the variance σ_{logf} in $log(f_{bound})$.

[15] 'Converged' here means that "(i) no significant changes occur when N_p is increased further, and (ii) the standard deviation in f_{bound} after one Hubble time is sufficiently small (i.e., $\sigma_{logf} \leq 0.05$)" (van den Bosch and Ogiya 2018, p. 4076.

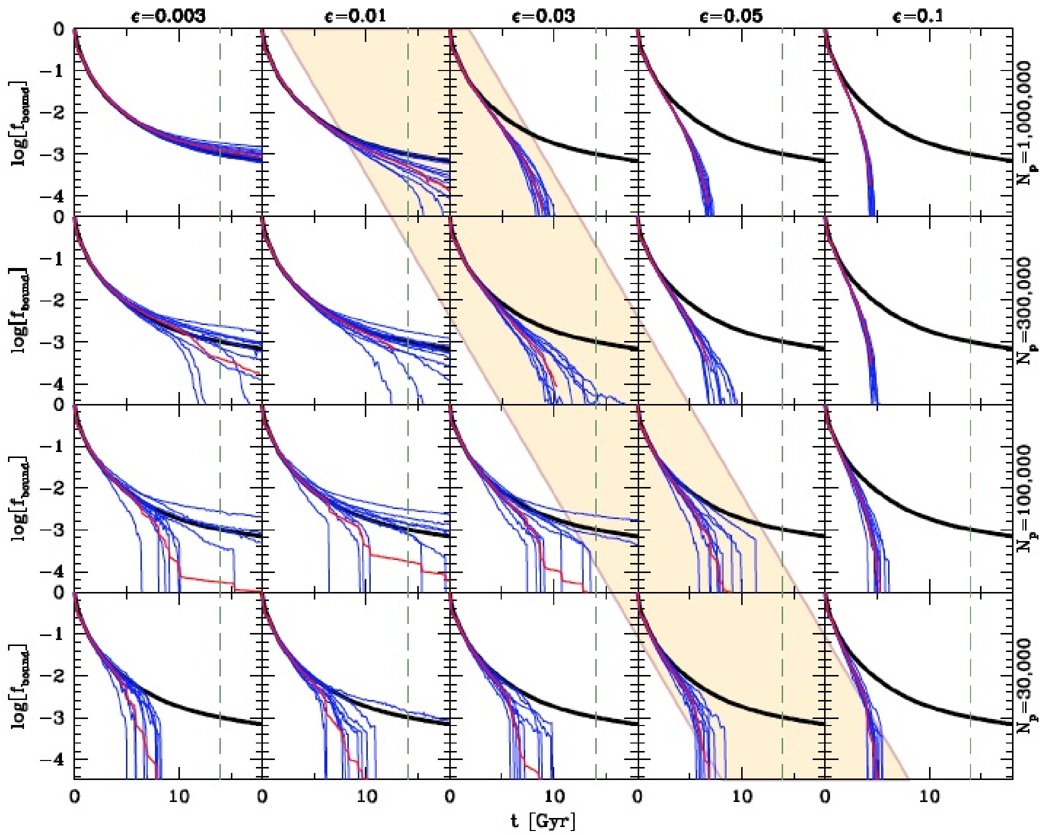

Figure 4.4: This figure shows the bound fraction of mass as function of time for simulations with different mass resolution N_p and different force softening ϵ with physical parameters, time step and force accuracy kept at their fiducial values. The black line shows the 'converged' results of a simulation with $N_p = 10^7$ and $\epsilon = 0.003$; the blue line the results from 10 simulations; the red line their average. See van den Bosch and Ogiya (2018), figure 10, 4077.

corner however, subhaloes do not disrupt after 13 Gyrs. On the contrary: a large fraction of bound mass survives, which seems to indicate that resolving the dynamics at $r_{orb}/r_{vir,h} = 0.1$ requires very high mass resolution and even stronger force resolution.

Note especially the contrast between this result at large N_p and small ϵ and the ones observed along the yellow-shaded band, which corresponds to the scaling between mass resolution and force softening defined by Power et al. 2003. This scaling is obeyed by most of the state-of-the-art simulations since the publication of this convergence study. If you focus on the red lines, i.e., on the averaged results of simulations, the bound fraction of mass appears converged: the red line prediction stays more or less the same despite increasing the mass and the force resolution, which is precisely why it is taken to be a robust prediction by Power et al.'s standards. Yet, this red line indicates a full disruption of the subhalo between 5 and 8 Gyrs. Thus, simulations converge on predicting that subhaloes fully disrupt after 8 Gyrs for a given range of values but also on predicting that they survive after 13 Gyrs for a different range.

What should alert the philosopher here is not so much that only one of these contrary results can be accurate, or that the results displayed in the upper-left corner might more likely be exact than the outcomes of simulations corresponding to Power et al.'s scaling. What must be emphasized, rather, is that *convergence alone will not tell you which one of these results is the correct one, or whether one of them is*. Given the range of the optimal softening defined by Power et al. 2003, i.e., $0.02 < \epsilon < 0.06$, increasing the mass resolution and the force softening will give you good agreement among simulations and confidence in your prediction that subhaloes at $r_{orb}/r_{vir,h} = 0.1$ will not survive after one Hubble time. But if you do not follow this scaling and keep increasing the resolution beyond the range defined by the convergence studies, you will find convergence on another prediction, that contradicts the former. As summarized by the authors, convergence is "not a sufficient condition to guarantee that the results are reliable" (van den Bosch and Ogiya 2018, 4067).

4.3.2 Pseudo-convergence or convergence?

In the previous section I have argued based on the fascinating work done by van den Bosch and Ogiya that robustness analysis in the form of convergence studies is not enough to exclude numerical artifacts and that robust predictions cannot be considered genuine physical predictions on such grounds. In this section, I push this thought further and argue that convergence can actually result from artifacts in some cases. Instead of the missing satellite controversy, I will focus in this subsection on the cusp-core problem, i.e., the fact that the central density profile apparently predicted by the CDM model in dark matter only simulations does not match galaxy observations, especially observations made for the galaxies mostly made of dark matter–low-surface brightness and dwarf galaxies.

In N-body simulations, real dark matter is substituted by a limited number of heavy test bodies, so that the computational task is made tractable while preserving the averaged density of the system. However, whereas dark matter is thought to be collisionless, heavy test bodies can undergo collisional relaxation, which is a decisive factor in determining the density profile of dark matter haloes. Baushev 2014, for instance, has shown that a moderate energy relaxation inevitably forms a core in the center of the halo, while a very intensive energy relaxation would contribute to a cuspier profile. Collisional effects of test bodies can be characterized by the relaxation time $\tau_r = \frac{N(r)}{8 ln\Lambda . \tau_d}$,[16] with N(r) the number of test bodies inside a sphere of radius r, $ln\Lambda$ the Coulomb logarithm and τ_d the characteristic dynamical time of the system at radius r. As a consequence, Power et al. 2003 recommended, as one of their main convergence criteria, that convergence would be reached in a time $t \leq 1.7\tau_r$, with τ_r the relaxation time, since the first sign of the influence of collisions seems to be the apparition of the core profile after $1.7\tau_r$. The stability of the density profile up to $1.7\tau_r$ was actually taken to be the proof of its physical significance.

Baushev's claim that violent relaxation generates cuspy profiles is very intriguing and raises important questions. Indeed, dark matter haloes undergo violent relaxation when they collapse, as the density inhomogeneities appearing lead to small scale gravitational fields that mediate the exchange of energy among dark matter particles (Baushev 2015, 48). This mechanism, however, is only efficient at the moment of the collapse. The halo, once formed, has a stationary gravitational field. How come then that N-body simulations based on the cold dark matter model predicts cuspy density profiles for formed haloes? If the cuspy profiles stems from collisionality, where does this phenomena come from? It is artificial or physical?

This question was addressed by Baushev et al. in a beautiful paper in 2017, with a methodology very similar to that at play in van den Bosch and Ogiya (2018). First, they proposed an idealized, unrealistic scenario where all sources of collisionality could be kept under control except the one under scrutiny–here, they simulated an isolated halo, to avoid the tidal influence

[16] See Binney and Tremaine 2011, eqn 1.32.

of nearby haloes affecting the energy exchange between particles and the gravitational capture of more mass from the surrounding that could lead to a secondary violent relaxation. Then, they identified a prediction resulting from artificial relaxation. They simulated a halo with a Hernquist density profile, i.e., behaving exactly like the NFW profile in the central region, but describing a stationary, fully stable halo. Given that such a halo has a constant gravitational potential field $\phi(r)$, the density and velocity profiles should remain the same and all (the implicit functions of) the integrals of motion such as the specific energy $\omega = \phi(r) + \frac{v^2}{2}$, the specific angular momentum $\vec{K}$, and the apocenter distance r_0[17] should be conserved. Thus, any variation of them should be attributed to numerical effects. Then, they run these simulations and determined whether the prediction extracted from the artificial hypothesis was verified. Figure 5 (2017, 6) tracks the variations of the integrals of motion as a function of radius over 200 snapshots and clearly shows that the integrals of motion vary significantly in one single timestep.[18] Thus, the convergence criterion advised by Power et al. 2003 by no means guarantees that all of sources of numerical artifacts have been excluded.

This preliminary conclusion becomes even more interesting when one starts, with the authors, to wonder why the density profile remains stable–which, as our reader might recall, is precisely the convergence criterion emphasized in the literature–if the integrals of motion do vary. In other words, one might wonder whether the fact that the cuspy profile $\rho \propto r^{-1}$ is very stable is a mere coincidence, or a direct result of the numerical effects observed in figure 5. If energy and angular momentum exchange occur between particles, then the system is not

[17] r_0 is the maximum distance on which the particle can move off the center and depends only on the integrals of motion: $\omega = \phi(r_0) + K^2/2r_0$.

[18] Here is the more detailed methodology of Baushev et al. 2017, 5:

- Start by ordering the 10^6 particles according to their r_0 in the initial snapshot,
- Divide them into 200 groups of 5000 particles with same r_0 and characterize these groups by the averaged initial r_0 of their members
- Calculate the deviations $\Delta r_0/r_0 = (r_0(i+1) - r_0(i)/r_0)$ and $\Delta K/K_{circ} = (K(i+1) - K(i)/K_{circ})$ for each particle on each timestep
- Find the root-mean-squares of $\Delta r_0/r_0$ and $\Delta K/K_{circ}$ averaged over each group and for each snapshot and finally average them over all the timesteps. These final averaged values are denoted by $\langle\hat{\Delta} r_0/r_0\rangle$ and $\langle\hat{\Delta} K/K_{circ}\rangle$. i is the number of the snapshot and K_{circ} the angular momentum for circular orbit r_0.

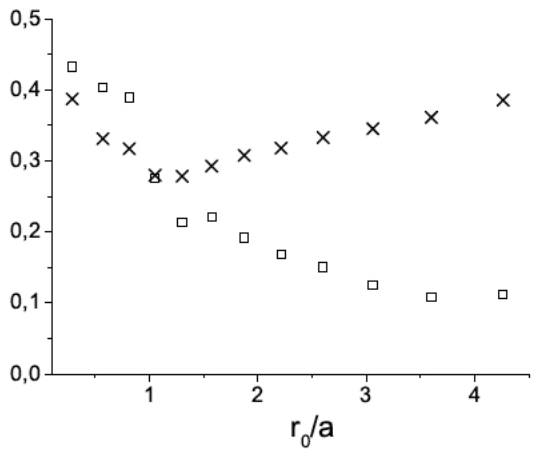

Figure 4.5: Averaged variations $\langle\hat{\Delta}r_0/r_0\rangle$ (crosses) and $\langle\hat{\Delta}K/K_{circ}\rangle$ (squares) of the integrals of motion. See Baushev et al. (2017), 6.

modelled by the collisionless Boltzmann equation $df/dt = 0$, but can be by an 'essentially collisional' (Baushev et al. 2017, 7) equation, i.e., that of Fokker-Planck. The Fokker-Planck equation is used in astrophysics in order to model the evolution of stellar systems due to weak binary encounters. Roughly speaking, this equation describes the energy changes as the cumulative effect of such encounters. Given that dynamical friction and diffusion constitute the two main processes at play in the dynamics of collision, and since the Fokker-Planck takes into account both processes, it is considered a good approximation of the exact collisionless Boltzmann equation, which includes strong encounters and is thus harder to handle. Although the Fokker-Planck equation is not exact, it offers good insights about the full collisional Boltzmann equation, as some of its stationary solutions hold for this equation too.[19]

If the system is modelled using this collisional equation, then the Fokker-Planck diffusion streams created by the particle interactions could compensate each other and contribute to form a stable density profile. And, as it turns out, the Fokker-Planck equation does have a stationary solution close to the NFW one. If the cusp is a product of this Fokker-Planck diffusion, then the

[19]The Fokker-Planck equation is derived from the exact collisional Boltzman equation by expanding the collision integral in a Taylor series and truncating the series after the first two terms given that only these terms matter for small velocity changes. See Binney and Tremaine 2011, section 7.4. for the derivation of this equation from the collisional Boltzmann equation and the simplifications needed for such a derivation to hold, and Evans and Collett (1997) for a first application of the Fokker-Planck equation to the cusp-core problem.

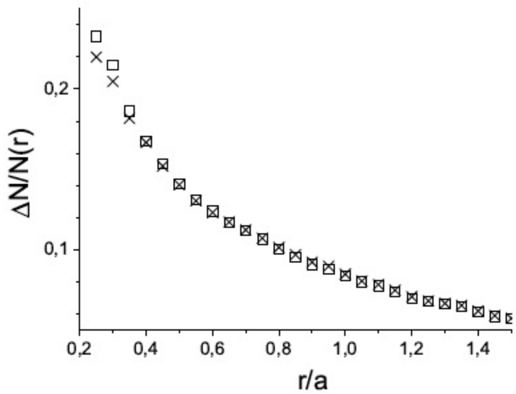

Figure 4.6: The upward $\Delta N_{+}(r)/\Delta t$ (squares) and downward $\Delta N_{-}(r)/\Delta t$ (crosses) Fokker-Planck streams of particles divided by the total number of the particles $N(r)$ inside r.

artificial collisions must form a downward and an upward stream of particles, with increasing and decreasing r_0 respectively, that compensate each other and thereby explain the stability of the profile. Note, again, that this stability is taken to be the proof of the physical significance of the density profile since at least 2003. This prediction can be tested by taking two adjacent snapshots, preferably at the beginning of the simulation to avoid the core formation effects, and calculating, for an array of radii r the number of particles $\Delta N_{+}(r)$ of particles which had $r_0 < r$ at the first snapshot and $r_0 > r$ at the second one and the number of particles $\Delta N_{-}(r)$ of particles which had $r_0 > r$ at the first snapshot and $r_0 < r$ at the second one.

The results, presented in figure 6 (2017, 9), demonstrate not only that the Fokker-Planck streams exist, but that they compensate each other very well outside of $r = 0.4a$.[20] Moreover, these streams are important, important enough for being responsible for the shape of the density profile, as they represent more than 2% of the total mass of the halo at each time step.

Two conclusions force themselves upon us at this stage:

- First, the variations of the integrals of motion unambiguously demonstrate that N-body simulations are still suffering from artificial collisional effects, due to the limited number of particles simulated, even though the convergence criterion usually prescribed is

[20] a is the scale radius, i.e., one of the two parameters that define the NFW profile along with the characteristic contrast density.

supposed to exclude them.

- The stability of the density profile cannot be considered as a convergence criterion warranting the reliability of the density profile, as it likely results from numerical artifacts.

4.3.3 Discussion

Although convergence failed, in the two cases discussed above, to deliver a reliable methodology for excluding artifacts, one still has two options to rescue robustness analysis. One way to do so is to argue that van den Bosch and Ogiya and Baushev et al.'s studies actually constitute instances of robustness analysis, thereby showing its efficiency.[21] Remember that we defined robustness analysis, along with Wimsatt, as an inference to the reliability of a phenomenon from its invariance over multiple models or processes. We argued that convergence could be subsumed under this concept, given that convergence studies aim at determining the conditions under which a prediction is reliable, based on its invariance over multiple numerical parametrization. One could therefore consider that what is actually shown by van den Bosch and Ogiya and Baushev et al. is the unreliability of the predictions examined, and hence their lack of robustness, based on their unsteady behavior over different parametrizations.

The methodology at play in these two papers cannot, however, be considered a case of robustness analysis, for at least two reasons. Let us consider first what is under investigation in the paper by van den Bosch and Ogiya. What the authors scrutinized here is not the reliability of the simulations outcomes, but the convergence criterion itself. This point is made clear when discussing the unrealistic setting of the simulations. The authors insist that the goal of the numerical experiments is not realism or the search of robust properties, but to understand whether "the dominant cause of (..) [the] prevalent disruption of subhaloes in numerical simulations (...) is artificial (numerical) or real (physical)"(2018, 4067):

> The experiments conducted here correspond to idealized set-ups that one will never

[21] Thank you to M. Weisberg for pointing this to our attention.

> encounter in nature. In reality, subhaloes will already have been affected by the tidal field of the host halo well before it reaches the starting point of our simulation. If realism is the goal of the simulation, one has little choice but to simulate the system in its proper cosmological setting (i.e., run a cosmological simulation). The goal of the idealized experiments described here, though, is to gain a physical understanding of the complicated, non-linear and numerical processes associated with the tidal stripping of dark matter subhaloes (2018, 4068).

The main conclusion of this paper is that “most, if not all, disruption of substructure in N-body simulations is numerical in origin”, a conclusion that “questions whether the fact that subhalo mass functions appear to be converged down to 50-100 particles per subhalo implies that results are reliable” (2018, 4084). In other words, the authors showed that, even though convergence was reached, numerical artifacts had not been excluded; and that the results backed up through convergence were still not reliable. What is at stake here is not whether the amount of substructure predicted through N-body simulations is a reliable prediction, but whether the reason it is taken to be a reliable prediction is a sound one.

One could insist that robustness analysis has a positive and negative use: it serves both to show that predictions are reliable when they are robust, and that they are not when the robustness analysis breaks down. Indeed, one could assert that van den Bosch and Ogiya (2018) showed that predictions made about the number of satellite galaxies that should be observed in a dark matter halo are not reliable, because their alleged robustness breaks down when the resolution is increased a little further. However, this reconstruction of their argument is misleading: what we see in their paper is not that the appearance of convergence of the results based on Power et al. breaks down, while ‘true’ convergence is reached for higher resolution. What we see instead is a first instance of converged results for a region of the parameter space and a second one for another region. Asserting that the robustness of the first *breaks down* when increasing the resolution is assuming that we have more reasons to trust the second set of results than the former. On which grounds could such a claim be made, if robustness is the only criterion on the basis of which reliability is assessed? The authors actually carefully avoided to make any claim about which ones among those two should be taken as the correct prediction, if any, for there would be no way to justify such a choice based on robustness only. If robustness is the basis on which reliability is assessed, then we have equally good reasons to accept one or the other converged results.

Even more undoubtedly, robustness analysis has no role to play in showing the failure of convergence in Baushev et al.'s paper. The aim of the paper, like van den Bosch and Ogiya's, is not to test the reliability of the NFW density profile *per se*, but to test whether its stability can really constitute a convergence criterion. Furthermore, the demonstration that convergence does not guarantee reliability does not involve any attempt to make the robustness of this property break down. The point of this paper is not that the alleged convergence of the simulations disappear when the resolution is further increased. On the contrary, it shows that convergence does not and will not break down, precisely because it results from numerical artifacts. Simulations converge on the NFW profile because the artificial diffusion streams generated by numerical collision effects correspond to a stationary solution to the Fokker-Planck equation that is very close to the NFW profile. In other words, the NFW profile is a robust property, and will remain so as long as these artificial collisional effects are not suppressed. The lesson to be drawn from this paper is that robustness by no means says anything about the *physical* nature of the prediction made, and even less about its reliability.

There is, however, another way to rescue robustness analysis. One could deny that convergence is robustness, based on the facts that different numerical parametrizations do not constitute an appropriately diverse set of simulations to compare. One could argue, say, that different values assigned to numerical parameters–be it an increase in resolution–are not *independent* in the relevant sense, and that invariance should therefore not be searched for over changes in these values. Orzack and Sober refer to the search of robust predictions within a single model as 'internal robustness', a kind of robustness that the authors deem 'useful' but no indication of truth, given that robust predictions can be found in incorrect models (1993, 540). Thus, convergence studies could be interpreted as an internal form of robustness that does not fully capture the strength of the concept of robustness, which necessitates to examine a diversity of independent models. This is, to my eyes, a more convincing attempt to preserve robustness analysis[22], especially given the lack of consensus in the literature on defining what constitutes an appropriately diverse set of models to compare. I devote the following section to assess whether comparisons across different codes could be considered instances of robustness analysis, and, if so, whether they do any better than convergence studies.

4.4 Robustness and Code Comparisons

In this section, I present two examples of code comparisons recently done on hydrodynamical simulations–i.e., on simulations that include not only dark matter, but also baryonic physics. I chose to focus on these two specific code comparison projects for two reasons: first, they constitute two of the most recent ones, and of the most important ones in terms of the number of different teams involved. Second, they are based on sharply different methodologies, both of which can teach us a lot about robustness analysis. I start with the most recent and still ongoing project AGORA and deny that this methodological enterprise for code comparison can really constitute an instance of robustness analysis, based on the problem of sufficient diversity mentioned in section 4.2. I then proceed to analyze the methodology of another project, named AQUILA, and the lessons that can be drawn from this project despite its failure to find any convergence across codes.

4.4.1 Code Comparison: The AGORA Project

The Assembling Galaxies Of Resolved Anatomy (hereafter AGORA) project was launched in July, 2012, in an impressive attempt to insure that comparisons across codes are actually pos-

[22] Thank you to E. Winsberg for pointing this argument to my attention.

sible, i.e., that apples are compared to apples.[23] This project currently gathers fourteen teams across the world, with more than 150 participants and 60 institutions involved. [24] The project bears not on large cosmological volumes simulations or on dark matter haloes "zoom-in" simulations, but on galaxies formation, which requires to model baryonic physics–dissipation, heating and cooling of gas, the formation of stars and supermassive black holes, magnetic fields, and so on. These simulations thus necessitate to take into account not only collisionless dark matter, but also astrophysical phenomena that requires fluid motion–'hydrodynamics'–calculations. Different codes tend to prioritize different energy feedback and to implement them through different subgrid physics, making them difficult to compare. This is the reason why the underlying aim of AGORA is to make sure that different codes actually target identical astrophysical systems. In other words, in order for a comparison to be meaningful, and to provide a relevant basis for robustness analysis, one must determine whether distinct codes with distinct subgrid physics are simulations of one and the same system. Indeed, whereas dark matter-only simulations have only numerical parameters left unconstrained, hydrodynamical simulations have also unconstrained degrees of freedom in implementing the physics of the cooling, the shocks, or even more significantly the interstellar medium (ISM). Not only different codes have different preferences for deciding on the values of these parameters, but parameters may sometimes not have the same significance across different codes. However, if one cannot assess whether the galaxies simulated are actually intended to be the same, then nothing can be learned from a disagreement or an agreement among distinct codes. As a result, the methodological core of the AGORA project is to develop a framework guaranteeing that codes share their initial conditions and astrophysical packages and can be read using common analysis toolkit.

More precisely, the project as described in the Flagship paper consists of comparing 5 codes–two Smoothed-Particle-Hydrodynamics (SPH) codes GADGET and GASOLINE, and

[23] Private correspondence with Ji-Hoon Kim, first author of the two AGORA publications and project coordinator.

[24] Information retrieved from the website of the project https://sites.google.com/site/santacruzcomparisonproject/outline on March, 4th 2019.

three adaptative mesh refinements (AMR) codes ART, ENZO and RAMSES. Two distinct sets of initial conditions were generated using the platform MUSIC (Hahn and Abel 2013) for haloes with $z = 0$ ranging from 10^{10} to 10^{13} solar masses,i.e., from dwarf galaxies as well as of galaxy groups. The first set corresponds to galaxies forming with a quiescent merger history, while the other describes a violent one, with many mergers between $z = 2$ and 0. A low-resolution large volume is simulated with cosmological parameters chosen in accordance with the ΛCDM cosmology and the WMAP results, while the astrophysical package for gas-cooling, UV background, the stellar initial mass function and mass loss, star formation and supernovae energy feedback is implemented through GRACKLE, which provides a standardized primordial chemistry for H and He as well as a cooling library. Other subgrid physics parameters, especially those regulating stellar feedback processes–star formation density threshold, star formation efficiency, initial mass of star particles, stochasticity of star formation–, are calibrated on an isolated disk galaxy scenario, by varying the feedback parameters and the mass and spatial resolutions until succeeding in simulating a realistic disk galaxy. Eventually, all simulations outcomes are analyzed with the common analysis platform *yt*.[25]

A test of the AGORA set-up is then performed to ensure that "1) each participating code can read the common "zoom-in" initial conditions generated by the MUSIC code, 2) that each code can perform a high-resolution cosmological simulation within a reasonable amount of computing time, and 3) that the simulation output be analyzed and visualized in a systematic way using the common analysis *yt* platform" (Kim et al. 2013, 11). The test consists of simulating a dark-matter only cosmological simulation of a galactic halo of intermediate size at $z = 0$. This 'proof-of-the concept' test shows great agreement overall, especially on the mass distribution around the central halo, the target halo mass, and the density profiles[26] and the Flagship paper thus concluded on a rather optimistic note about the possibility of comparing

[25] See Turk et al. 2010 for a detailed introduction to *yt*.

[26] The agreement does not hold for the substructure mass distribution. Possible culprits identified by the authors are a) a small deviation in density distribution evolving into a significant difference, b) a timing mismatch in the numerical integration of the equations of motion, c) an intrinsic difference in solving Poisson equation–i.e., in the gravity solvers.

different codes:

> We have found that the dark matter density profiles as well as the general distributions of matter exhibit good agreement across codes, providing a solid foundation for future hydrodynamic simulations. Throughout the test we have demonstrated the practical advantage of our common initial conditions and analysis pipeline by showing that each code can read the identical 'zoom-in' MUSIC initial conditions and that each simulation output can be analyzed with a single *yt* script independent of the output format. By doing so, we have produced evidence that the cumbersome barriers in comparing galaxy simulations can be, and are, removed.

The second paper, published in 2016, extended this methodology to nine codes total, whose convergence was tested on an isolated Milky Way-size disk galaxy and its properties such as gas/stellar disk morphology and kinematics, the thermal structure of the ISM, or the star formation relation. The conclusion of this paper was even more optimistic than that of AGORA-I:

> Our experiment reveals the remarkable level of agreement between different model simulation tools despite their codebases having evolved largely independently for many years. It is also reassuring that our computational tools are more sensitive to input physics than to intrinsic differences in numerical schemes, and that predictions made by the participating numerical codes are reproducible and likely reliable. If adequately designed in accordance with our proposed common parameters (e.g, cooling, metagalactic UV background, stellar physics, resolution (...)), results of a modern high-resolution galaxy formation simulation are likely robust (Kim et al. 2016, 26).

4.4.2 Robustness Analysis and the Problem of Sufficiently Diverse Models

The AGORA project developed an impressive and very much needed apparatus to ensure that codes could actually be compared, i.e., to ensure that code comparisons are 'apple-to-apple comparisons'. This attempt to develop common platforms, including common initial conditions (ICs), common astrophysical packages and common analysis tools, was indeed required to make sure that outcomes of simulations were representing the same system. Furthermore, a lot was learned about the codes themselves through this enterprise and will still certainly be in the future. For instance, the AGORA teams decided to take what they called a '0 Myr snapshot' when testing the effectiveness of common platforms, i.e., a snapshot immediately after the ICs were read, in order to determine whether different codes were reading the ICs consistently. This step not only allowed to correct all the possible ways in which the ICs could be misread by the codes–wrong units, wrong definition, wrong convention–[27], but also permitted to gain some insights about intrinsic differences between mesh-based codes like ART I and II, ENZO, RAMSES, GIZMO and particle-based ones like GADGET, GEAR, CHANGA or GASOLINE. An example of this is illustrated by figure 1 of Kim et al. 2016, that shows the differences between these hydro-solvers for the surface density of disk galaxies: particle-based codes seem "to smooth out the strong density contrast in the ICs at the edge of the initial gas disk" (2016, 10), due to the way the density is reconstructed from the positions of particles in these codes.

This effort to make comparison possible is nevertheless in tension with the search of robust properties. In this section, I will argue that this effort to make comparisons possible is not compatible with achieving the relevant diversity needed for robustness analysis, and more generally speaking that the methodology at play here is not sufficient to exclude artifacts.

The first question that arises about this methodology is whether it can succeed in excluding

[27]The nature of these corrections is not detailed in the paper itself, but was in private correspondence with Ji-Hoon Kim.

the artifacts diagnosed in van den Bosch and Ogiya 2018 and Baushev et al. 2017. The proof-of the-concept simulations described in Agora-I make me doubt that it can be the case. Indeed, in this zoom-in, dark matter-only simulation of a halo of virial mass $1.7 \times 10^{11} M_{\odot}$ at $z = 0$, the particle resolution ($\sim 3.38 \times 10^5 M_{\odot}$) and the force softening (322 comoving pc from $z = 100$ to $z = 9$, 322 proper pc after for particle-based codes; 326 comoving pc for the others[28]) follow the recommendations made by Power et al. 2003 and fall along the yellow-shaded band highlighted in fig.3 by Van den Bosch et al. Given the extremely high resolution prescribed by the latter to appropriately resolve a dark matter halo, one could suspect that a project like AGORA would not have been able to detect any artifact generated by an inadequate force softening, despite the convergence reached across codes. It is all the more true when it comes to the halo density profile: in that case, the convergence could as well be the symptom of the artificial collisionality still present in the way dark matter is modeled. In his Baushev et al. 2017, Baushev proceeded to a code comparison–although of much smaller extent–between the direct summation code Ph4 and the tree code Gadget-2. The idea behind this code comparison was to determine whether the artificial Fokker-Planck streams likely stabilizing the density profile have their origin in the potential calculation algorithm. Numerical effects potentially affecting the density profiles include discretization effects, the algorithm of particle trajectory evaluation, inadequate softening and the potential calculation algorithm. Since Ph4 and GADGET-2 differ only on the latter, the impact of the potential calculation algorithm–had it been the responsible for the pseudo-stability–should have appeared through the absence of Fokker-Planck streams in the direct summation code. This, however, was not the case[29]; which means not only that the artifact could not be excluded through this code comparison, but that AGORA could not exclude it either if it stems from the first two possible culprits listed. Discretization, for instance, is a necessary idealization made for any simulation. As such, its effects, as the effects of any of the necessary idealizations made, could not be diagnosed through a code comparison, even one with the breadth and infrastructure of AGORA.

[28]See section 5.1 of Kim et al. 2013 for the dark matter-only simulation set up.
[29]See figure 7 and its discussion in Baushev et al. 2017, section IV.

Our second worry with the AGORA project bears on whether this enterprise preserves the kind of diversity needed for robustness analysis. The effort undertaken by the AGORA teams to make a comparison across codes possible relies on the use of common platforms to generate ICs, common toolkit to implement part of the astrophysics, and common analysis tools. While I agree that an apple-to-apple comparison requires the development of such a common infrastructure, this infrastructure comes with its own problems too. How can one then guarantee that these common tools are not introducing new sources of numerical artifacts? Consider the example of MUSIC (MUlti-Scale Initial Conditions), for instance, which is conceived as a way to generate common cosmological initial conditions for zoom-in simulations readable by all the codes involved in the comparison. Remember that for zoom-in simulations, one must first carry out a a low-resolution simulation of a large volume, in order to have an idea of the large-scale cosmic environment where haloes structure will develop, before zooming-in on a particular object at higher resolution. The challenge is thus to generate initial conditions for both scales: since the density perturbations in the cold dark matter are responsible for structure formation, and these perturbations extend from solar system size to giga parsec scales, a reliable simulation of an individual halo in its cosmic environment must be able to track both smaller scale perturbations directly impacting the halo structure and the large-scale perturbations. The aim of MUSIC is to provide an algorithm generating multi-scale initial conditions, i.e., generating "Gaussian random fields that follow a prescribed power spectrum and act as source terms for density and velocity perturbations in Lagrangian perturbation theory" (Hahn and Abel 2013, 2).[30] MUSIC itself is a code, based on a number of assumptions, idealizations and numerical parameters, including force softening, that expose it to numerical artifacts. While the comparison across codes made both in Hahn and Abel 2013 and in the AGORA project is reassuring about the fact that codes do read initial conditions in a similar way[31], this fact alone does not

[30]More precisely, MUSIC uses Fast-Fourier Transformations convolutions to obtain the density field from a hierarchical white noise field, and an adaptative multigrid Poisson solver for displacement and velocity fields. See Hahn and Abel 2013, sections 2 and 3 for more details.

[31]As shown in figure 1 of Kim et al. 2016 however, there are intrinsic differences between particle-based codes and mesh-based codes when it comes to properties such as surface density.

mean that these initial conditions are free of errors and immune to numerical artifacts that would similarly impact all codes. The same reasoning can be applied to GRACKLE for the astrophysical sub-packages it implements, and for the analysis tool *yt*. These common platforms serve their role, inasmuch as they do allow for a comparison on similar grounds for all codes, but they also hinder the search for the diversity needed to proceed to a sound robustness analysis, genuinely in a position to assess whether predictions made by simulations are *physical* predictions.

What about the astrophysical physics that is not handled through common platforms, like the star formation and stellar feedback parameters? Can the AGORA strategy provide some grounds for thinking that the agreement of different codes is enough for trusting this part of the physics? Before addressing this question, we need to pause and look at an older code comparison undertaken in 2012. This project, called 'AQUILA', greatly contributed to motivate the methodology of AGORA, for its disappointing results[32] were thought to entirely stem from the failure of the project to offer comparable targets. Hence, the AGORA project focused on building a common infrastructure to allow for a genuine comparison of similar targets. However, AQUILA, with its own methodology, was able to deliver very interesting insights that must not be forgotten when looking at AGORA's results.

AQUILA, indeed, was based on a comparison among different codes but also different versions of the same code, i.e., of one code with different implementation of the subgrid physics. AQUILA is a code comparison based on a zoomed-in simulation of one of the haloes of the Aquarius project named 'Aq-c'.[33]. Thirteen different hydrodynamical simulations

[32]The AQUILA paper ends with these words: "Although numerical convergence is not particularly good for any of the codes, reasonably good convergence is found for the properties of the stellar component, such as total mass and median age. Less well converged are the internal properties of the galaxy, such as the half-mass radius, or the fraction of stars in a rotationally supported disc" (Scannapieco et al. 2012, 1742). What follows is even worse: "Aside from these considerations, perhaps the main result of the AQUILA project is that, despite the large spread in properties spanned by by the simulated galaxies, none of them has properties fully consistent with theoretical expectations or observational constraints in terms of mass, size, gas content and morphology" (ibid, 1742).

[33]Aquarius is a collaborative project similar in scope and scale to the Millenium simulation, which provides ultra-high resolution simulations of 6 Milky-way size individual dark matter haloes, named Aq-A, B, C D, E and F. See table 1 in the flagship paper of the project for more details about the haloes' properties (Springel et al. (2008)).

based on nine different codes were run at two different resolutions: seven Smooth-Particle-Hydrodynamics codes including six versions of the codes GADGET3 and GASOLINE, one adaptative mesh-refinement (RAMSES) and a moving mesh (AREPO), each of which has its own preferred treatment of radiative cooling, star formation and its own numerical treatment of feedback–injecting the feedback energy in the interstellar medium as thermal energy, kinetic energy, or temporarily decoupling the gas from the interstellar medium. GADGET3 and RAMSES were also run three times with different subgrid physics modules: GADGET3 only included supernovae feedback whereas G3-BH also considered the energy feedback of supermassive black holes and G3-CR that of the energy deposition of cosmic rays. Likewise, RAMSES was run three times, one with longer star formation time-scale as compared to the fiducial run of RAMSES (RAMSES-LSFE)—, and one adding the feedback energy of an active galactic nuclei associated with a supermassive black hole. Looking at this diversity in the physics implemented itself, the deceptive results of AQUILA seem no longer as surprising as they appear at first glance, and not even as worrying as presented by the authors of the project. The divergence of the results observed by different codes could very well be explained by the fact that these codes do not consider the same physics to begin with. As a result, no rushed conclusion should be made about the unreliability of N-body simulations from AQUILA's divergent results–the results could be different merely because they compare different things.

However, AQUILA does deliver a lot of knowledge with respect to the impact of different astrophysical packages that should inform our interpretation of AGORA's more optimistic assessment. In particular, the results of AQUILA about galaxy morphology highlight the importance of stellar feedback and star formation for the morphology of galaxies. The delayed star formation of the RAMSES-LSFE version of the code, with delayed star formation, shows that the later the gas turns into stars, the more prominent the disc will be. Delaying star formation gives time for the gas to accrete into a centrifugally supported structure and thus promotes the apparition of a thin disk. On the other hand, the earlier stars form, the more galaxies will tend to be spheroidal.[34] These conclusions are supported by the fact that G3 and AREPO, which share their subgrid physics but differ on their hydro solvers, both lack discs in the simulated halo, making it more likely to find the culprit in the astrophysics than in the numerical scheme; and that codes with more efficient feedback, preventing the stars to form too early, do exhibit a disc, although less prominent that the one displayed by RAMSES-LSFE. However, star formations parameters and feedback modules[35] in the AGORA project are individually tuned so as to produce a realistic disk, thus erasing the differences met in the AQUILA project:

> It is of primary importance to understand how each individual code needs to be calibrated to reproduce various observational constraints. In a comparison like the AGORA project, it is even more important to cross-calibrate stellar feedback processes of the various codes using an idealized set-up such as an isolated disk. This is precisely the goal of this second type of initial conditions: we would like to model a realistic galactic disk using our various codes and their feedback parameters and the mass and spatial resolutions. By doing so, subgrid star formation and feedback prescriptions in various code platforms will be tuned to provide a realistic interstellar and circumgalactic medium (Kim et al. 2013, 6).

How then can any conclusion be made about the final agreement of these codes on galaxy morphology? How can one determine whether this agreement stems from the reliability of the predictions made or from this forced initial agreement obtained by individually tuning the codes? This is all the more worrying that the tuning is made to produce a realistic disk galaxies, when in the AQUILA project, none of the codes–including GADGET and RAMSES, which are involved in both code comparisons– were able to produce realistic results, compatible with theoretical expectations and observations. One might be concerned, in this case, by the extent to which codes have been tuned to agree on a realistic disk.

In sum, I doubt that code comparisons can provide solid grounds for robustness analysis,

[34]See figure 4 and its interpretation, Scannapieco et al. 2012, 1731-1732.

[35]More specifically, the relevant tuned parameters includes: star formation density threshold, star formation efficiency, initial mass of star particles, stochasticity of star formation. See section 3.2. of Kim et al. 2013.

because of the incompatibility that a project like AGORA exemplifies between 1) making sure that the comparison bears on comparable simulation outcomes, and 2) providing a diverse set of codes to compare in order to eliminate all possible sources of numerical parameters. Codes already share some necessary assumptions or idealizations whose impact cannot be evaluated, like that of the discretization of dark matter. But the effort to make sure that codes target similar systems multiply these shared and unexamined sources of artifacts, first through the development of a common infrastructure to generate initial conditions, implement the subgrid physics and analyse the snapshots, and second *via* the forced agreement of different codes on a given scenario through their thorough tuning. The independence of the set of codes under comparison is already a difficult requirement to satisfy, given the limits on computational power and the subsequent idealizations that must be made by all codes. But this requirement becomes even more difficult to achieve when one wants to ensure that these codes simulate one and the same system, given that this effort demands that the codes under scrutiny all share some common assumptions, i.e., in contradiction with the independence relied upon by robustness analysis. As of today, it is difficult to conceive a code comparison that would not necessarily fall prey of this trap, and thus would constitute a proper instance of robustness analysis.

4.5 Alternatives to Robustness

In sections 4.3 and 4.4, I have shown the pitfalls of robustness analysis when applied to N-body simulations and argued that robustness analysis cannot be a reliable method for determining whether the outcome of a simulation is fully explained by the physics that it implements or is affected by numerical artifacts. Section 4.5 is devoted to build upon the positive insights met in both sections and to offer, based on them, a new methodology that could play the role that robustness analysis fails to satisfy. In this section, I detail the two facets of this methodology, which I referred to as the method of Crucial Numerical Experiments or Crucial Simulations, after Bacon's famous *experimentum crucis*.

4.5.1 Reviving Crucial Experiments

One thing that may have struck our reader already is the similarities exhibited by van den Bosch and Ogiya and Baushev et al.'s reasoning to evaluate the effectiveness of convergence studies. In both cases, the aim of their study was to address a specific hypothesis opposing a *numerical* against a *physical* explanation to a given property considered as predicted by the ΛCDM model. In the former case, the author asked whether the matter disruption that is prevalent in N-body simulations is caused by tidal stripping or by inadequate force softening. In the latter case, the author investigated whether the cuspy density profile was a consequence of a proper modelling of collisionless dark matter, or the result or artificial collisionality. Likewise, both started by listing all the possible physical *explanans*, and constructed a idealized scenario where all these possible physical culprits would be eliminated but one, thus offering an exclusive alternative between a numerical and a physical mechanism. van den Bosch and Ogiya simulated an isolated halo on a circular orbit to eliminate other possible physical mechanisms like tidal heating or galaxy harassment. Baushev focused on the central density profile of a stationary Hernquist halo to rule out other sources of collisionality such as a violent secondary relaxation through late mass accretion, tidal influences of nearby structure, or substructures within the haloes. Finally, both studies decided between the two hypotheses by extracting from one of them a prediction that could be verified in the given idealized scenario suggested: in the overmerging problem, Van den Bosch hypothesized that the amount of substructure disrupted would not be affected by a joint increase in mass resolution and force softening if caused by tidal stripping.[36] Baushev conjectured that, if the density profile was affected by artificial col-

[36]Of course, one could argue that this joint increase of resolution is an instance of convergence studies, properly conceived, and does not as such constitute an objection against this methodology. Such an argument, however, would ignore that this specific way of testing the numerical hypothesis is due to the nature of the possible culprit, i.e., an inadequate force softening. Such an hypothesis, by definition, necessitates to test other values of force softening, and thus to increase the mass resolution given the scaling between these two parameters. For any other numerical culprits, the methodology would be different: the integration scheme, for instance, can be tested *via* a comparison between an Eulerian- and a leapfrog-based method, other things being equal; two-body artificial effects can be tested through mass segregation effects and so on. The increased resolution is specific to the nature of the hypothesis tested, and does not constitute by itself a methodology. See section 4.3.3. for a more detailed discussion of why this increase of resolution does not constitute an instance of robustness analysis.

lisionality, this exchange of energy and angular momentum should be turned into variations in the constants of motion, and proceeded to track their changes over successive snapshots.

We can abstract away from their reasoning the following steps to construct the methodology of what we will hereafter call crucial simulations:

- Step 1: Select a property, seemingly predicted by simulations, whose physical significance is at doubt.
- Step 2: List all the possible physical mechanisms $P_i, P_j, P_{.}.., P_n$ and numerical artifacts $N_i, N_j, N_{.}.., N_n$ that could generate the scrutinized property.
- Step 3: Select among all numerical artifacts the most likely culprit N_i and fix the values of other parameters such that this value is as conservative as possible without unnecessarily increasing the computational cost.
- Step 4: Design a simplified scenario where all these possible culprits can be ignored but one on each side, so as to construct a solid explanatory alternative $P_i \veebar N_i$ between a physical or a numerical origin for the property under discussion.
- Step 5: Extract from one the two hypotheses a prediction E that is not satisfied by the rival hypothesis.
- Step 6: Carry out the simplified simulation and verify whether E is observed or not.

This methodology revives the idea of crucial experiment forged by Bacon and convincingly argued against by Duhem. Recall that a crucial experiment, or so the story goes, opposes two competing theories, say T_1 and T_2. T_1 entails the prediction that E, whereas T_2 entails that not-E. A crucial experiment is an experiment favouring either E or not E such that the (non)observation of P will disprove (T_1) T_2, and indirectly confirm (T_2) T_1 through the elimination of its rival. In a similar manner, I call a 'crucial simulation' that simulation which proposes an idealized, simplified scenario where a physical hypothesis can be tested against a numerical one, by allowing the observation of a prediction P drawn from one of the hypotheses and absent from its rivals. The observation of the phenomena P in the outcome of the simulation then disproves one of the alternatives, thereby confirming the other; i.e., it disproves or confirms the numerical or physical nature of a given property of a simulation outcome.

The methodology of crucial experiment was heavily criticized by Duhem in his 1991, and rightly so to my eyes. Duhem's attack goes against the two branches of the crucial experiment methodology: crucial experiments cannot be used to confirm a theory based on the elimination of its rivals, for physical theories are not contradictory, but contrary hypotheses; meaning that they could very well be false together. As a result, the elimination of T_2 is not in itself sufficient for confirming T_1. Can crucial experiments still, however, be enough to definitely rule out an hypothesis, and thus at least be used for refuting theories? Not so, according to Duhem. Part of Duhem's famous holism[37] aims at showing that this account of testing is an illusion, since no prediction can be derived from an hypothesis taken in isolation. As Duhem himself puts it, "the prediction of the phenomenon whose nonproduction will cut off the debate does not derive from the disputed proposition taken in isolation but from the disputed proposition joined to this whole group of theories" (Duhem 1894, 82). In other words, if the phenomenon predicted is not produced, then the entire theoretical scaffolding involved in the prediction is "shown to be wanting", which also means that any part of this scaffolding can be amended such as to recover the phenomena. Hence, a crucial experiment is never sufficient to refute a given hypothesis, given that the blame could be put on any other part of the theory involved in the prediction, and that these parts can always be amended such as to recover empirical adequacy. These criticisms were justifiably considered as devastating for the methodology of crucial experiments. So, how could such a methodology be of any help when it comes to assessing the reliability of simulations?

Let us first consider the confirmation branch of the dilemma, i.e., the idea that a physical theory cannot be confirmed by the exclusion of its rival, for the two theories can both be false. This criticism loses its relevance when applied to simulations, for the alternative at stake does

[37]For a detailed account of Duhem's holism, see Gueguen and Psillos 2017, 62-65.

indeed exhaust all possibilities: the property whose reliability is scrutinized is either caused by a physical or a numerical mechanism, but cannot be caused by anything else. These two possibilities are contradictory, which entails that the refutation of one amounts to the confirmation of the other. Note, however, that this argument only works if one can be sure that they came up with a legitimate alternative, i.e., that the list of physical culprits was exhaustive, and the other possible candidates successfully ruled out in the simplified scenario where the alternative is tested. We will come back to this point in 4.5.2.

The criticism bearing on the disproving power of crucial experiments is trickier to handle, for Duhem's point with the holist threat seems particularly relevant when considering the lack of modularity of N-body simulations. One of the main challenges in assessing the reliability of simulations is indeed, as we mentioned earlier, their lack of modularity. The difficulty to assess the soundness of simulations is precisely grounded in the fact that the praise or blame that results from comparisons with observations cannot be distributed to individual components rather than to the simulation as a whole, given our lack of understanding of their causal contribution to the overall outcome of the simulations. Consider for instance the case of force softening that we have discussed extensively. N-Body simulations approximate the density of real stellar systems by simulating fewer, but more massive, particles. Force softening is then needed to prevent divergences in the gravitational force when massive bodies get close to each other. As shown by van den Bosch and Ogiya however, smoothing the gravitational potential to avoid these numerical errors tends to artificially enhance matter disruption, and so to reduce the amount of substructure predicted in dark matter haloes. As this example illustrates, it is difficult to determine precisely how a particular feature of a simulation depends upon the basic physics, and to separate genuine results from numerical artifacts. How can we hope then to escape Duhem's objection?

I need to emphasize that crucial simulations are meant to be cases where the contrast between competing hypotheses can be made particularly sharp, given their focus on idealized situations where the holistic challenges associated with modularity can be minimized. The role of crucial simulations is not to confirm or disconfirm a model, but a very specific component of this model.The simplified, idealized scenarii suggested in step 3 above serve precisely to isolate and clarify the impact of very specific aspects of the simulation, by making sure that one or several physical factors are minimized or fully turned off. The conception of such a simplified scenario is an essential part of the methodology and is precisely meant to counter worries associated with holism. This step would be problematic if the goal of the methodology was to test simulations against observations, since such a goal would require simulations to be as realistic as possible. Crucial simulations, on the contrary, aim first and foremost at gaining a better understanding of what originates from the numerical scheme and what is caused by the physical model. Therefore, a simplified scenario does not jeopardize the assessment of the simulation's reliability, but rather serves its purpose to better isolate the role of different modules, and thereby to detect artificial consequences of the numerical scheme. to recover in this context.

For these reasons, I think that crucial simulations do not fall prey of Duhem's criticisms and constitute a viable, if not advantageous, alternative to robustness analysis. I do acknowledge nevertheless that the merits of crucial simulations heavily depend upon how well constructed steps 2 and 3 of the methodology are, i.e., on whether the lists of possible culprits examined is exhaustive and whether the scenario on which the crucial simulation is based succeeds in properly isolating the two components under test. Section 4.5.2. is therefore arguing in favor or re-directing the use of code comparisons such as to supplement and strengthen these two steps of the methodology.

4.5.2 What do we learn from code comparison: on supplementing crucial simulations

As touched upon in the previous subsection, one challenge for the methodology of crucial simulations is to provide a solid alternative to explore, and that providing such an alternative requires to have already a sense of which components of the simulations have what effects. But how could we have such an understanding of the causal impact of different components, when the ‘fuzziness’[38] of their roles is exactly what creates the challenge for assessing the trustworthiness of simulations? This apparent circularity is what motivates the need to supplement this crucial simulation method for ruling out artifacts. In this subsection, I go back to the methodological contrast between the AQUILA and the AGORA project and the kind of knowledge that can be built from the strategies underlying these two code comparisons.

I emphasized earlier that the AGORA project was born from the frustration generated by the methodology of AQUILA, as many astrophysicists felt that the pessimistic appraisal of state-of-the-art simulations of the latter only stemmed from an attempt to compare non-comparable things. Hence, from the beginning, AGORA focused on developing an infrastructure that would allow for a relevant comparison. I have argued that, while this goal might indeed have been reached, it was at the detriment of others goals, such as testing the robustness of predictions. Here, I want to argue that a similar argument can be made for AQUILA: while it failed at providing the basis for the apple-to-apple comparison they were aiming at, it might still have reached one of its original targets, that of getting a better understanding of “what determines the morphology of a galaxy, what the main feedback mechanisms are and what role they play on different mass scales and at different times”, and of “whether the difficulties in reproducing realistic discs are predominantly a consequence of insufficient numerical resolution, inappropriate modelling of the relevant physics, or a failure of the cosmological model” (Scannapieco et al. 2012, 1728). And the responsible for this success, I contend, is precisely the specific methodological choices made by the AQUILA group.

Remember that AQUILA is a comparison among different codes but also different versions of similar codes, i.e., of one and the same code with different implementation of the subgrid physics. In particular, GADGET3 and RAMSES were run three times each with different subgrid pysics modules. Although this methodology does not promote the diversity that robustness analysis has to look for, it offers some very insightful overlapping between codes

[38] This term refers to the “fuzzy modularity” discussed in Lenhard and Winsberg 2010.

Table 1. Summary of code characteristics and subgrid physics.

Code	Reference	Type	UV background (z_{UV})	(spectrum)	Cooling	Feedback
G3 (GADGET3)	[1]	SPH	6	[10]	Primordial [13]	SN (thermal)
G3-BH	[1]	SPH	6	[10]	Primordial [13]	SN (thermal), BH
G3-CR	[1]	SPH	6	[10]	Primordial [13]	SN (thermal), BH, CR
G3-CS	[2]	SPH	6	[10]	Metal dependent [14]	SN (thermal)
G3-TO	[3]	SPH	9	[11]	Element-by-element [15]	SN (thermal+kinetic)
G3-GIMIC	[4]	SPH	9	[11]	Element-by-element [15]	SN (kinetic)
G3-MM	[5]	SPH	6	[10]	Primordial [13]	SN (thermal)
G3-CK	[6]	SPH	6	[10]	Metal dependent [14]	SN (thermal)
GAS (GASOLINE)	[7]	SPH	10	[12]	Metal dependent [16]	SN (thermal)
R (RAMSES)	[8]	AMR	12	[10]	Metal dependent [14]	SN (thermal)
R-LSFE	[8]	AMR	12	[10]	Metal dependent [14]	SN (thermal)
R-AGN	[8]	AMR	12	[10]	Metal dependent [14]	SN (thermal), BH
AREPO	[9]	Moving mesh	6	[10]	Primordial [13]	SN (thermal)

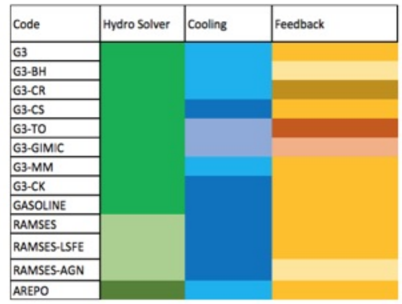

Figure 4.7: On the left, the table taken from (Scannapieco et al. 2012, 1729) summarizes the different types of hydro solvers, their preferred way of calculating the radiative cooling, the kind of feedback considered (Supernovae, Black Holes, Cosmic Rays) and the way it was implemented (through the injection of thermal or kinetic energy into the ISM or into the gas itself). On the right, I reproduced and colored this table to show the overlapping between codes like G3 and AREPO, or different versions of RAMSES.

and their modules, as one can see on the figure below: Notice, for instance, that GADGET3 and AREPO have exactly the same subgrid physics, but differ on the type of hydrodynamical solvers they rely on. This overlapping allows for very interesting insights in the results of AQUILA. The codes disagree on properties of the simulated halo such as the stellar mass of galaxies. AREPO predicts twice as many stars forming as G3, despite the fact that they share their subgrid physics. Such a disagreement thus indicates that the numerical scheme adopted play a role in the star formation efficiency that impacts the outcomes of the simulation. Another interesting characteristic of these two codes is that they both fail to produce a galaxy with a prominent thin disk. This agreement of both codes, despite the different numerical methods they adopt, is a good indication that the morphology of a galaxy is not affected by the type of hydrodynamical solver used, but that the prevalent mechanism in determining its morphology is to be found in the feedback efficiency.[39]

These two conclusions provides hypotheses to be tested in the context of crucial simulations, and support that these hypotheses are well-grounded. Considered separately, the code comparison and the crucial simulation are not sufficient to definitively conclude on whether

[39]We emphasize these two cases of agreement and disagreement as they are the ones that the AQUILA group insists on. The same reasoning, however, applies to other codes with relevant overlapping between their modules.

the numerical scheme or the physical model should be held responsible for generating the halo property under discussion. Together however, the code comparison offers hypotheses whose relevance is supported by the adequately constructed comparison between codes with overlapping modules that the crucial simulation is then in charge of confirming or disproving. The conjunction of these two methods can give us confidence that the results of crucial simulations can be trusted.

Based on these insights delivered by the AQUILA project, I suggest to re-direct the use of code comparisons such as to become a new source of hypotheses to test in crucial simulations. A code comparison is a very expensive enterprise to undertake, be it in terms of logistics, of time or in terms of computational power. Constructing them in such a way guarantees that code comparisons are optimized and knowledge gained about the different roles of the simulation components even in the case where the codes widely diverge on the properties they predict. These re-directed code comparisons should not be substituted to an effort to develop semi-analytical models offering some guidance for what the properties of a realistic halo should look like, but constitute a resource to supplement them whose power has yet to be exploited.

4.6 Conclusion

In this paper, I have argued that robustness analysis, in the form of convergence studies, is certainly not sufficient to exclude the presence of numerical artifacts in N-body simulations; and thus to warrant the conclusion that the simulation outcome is reliable. Even worse, the convergence used as a criterion for the robustness of a prediction is sometimes the direct product of numerical artifacts, as it appears to be the case for the stability and universality of the density profile predicted by simulations.

Since one could object that convergence studies do not exhibit the kind of diversity needed for a relevant robustness analysis, and thus deny that convergence studies constitutes proper instances of it, I have examined whether the search for robust predictions across code comparisons would better capture the notion of robustness. However, in order to fill that role, code comparisons would have to achieve two tasks that seems extremely difficult, if not impossible, to reconcile: developing a common infrastructure that guarantees that the comparison bears on similar systems, while at the same time preserving the diversity needed for the code comparison to constitute a genuine instance of robustness analysis. Since the common infrastructure itself is a possible source of artifacts, and that this infrastructure is not itself scrutinized; and given that not all parameters can be implemented through these common platforms but rather need to be individually tuned to agree on a given scenario, the code comparison thus constructed is not in a position to exclude all possible sources of numerical interference.

As a result, I suggested another methodology to fill the task initially assigned to robustness analysis, that of crucial simulations. The gist of crucial simulations is to provide a simplified scenario where the numerical or the physical origin of a prediction can be tested. Crucial simulations, more generally speaking, are useful as a way to overcome the holist threat–or 'fuzzy modularity'–that makes the assessment of the reliability of N-body simulations such a challenge to begin with. Used more specifically, i.e., as a confirmation method confirming or disproving the physical or numerical nature of a given phenomena, crucial simulations need to be done in conjunction with code comparisons with an adequate methodology. The confirmation or falsification power of crucial simulations depends upon the exhaustivity of the list of culprits considered and on the scenario subsequently investigated in the crucial simulation–that is, on whether it successfully isolates two and only two candidates. Code comparisons based on adequate overlapping of codes modules offer a better understanding of the–sometimes far detached–consequences of numerical artifacts, thereby providing confidence in the fact that all possible mechanisms, physical or numerical, have been reviewed and that the alternative tested is a relevant one.

Chapter 5

Concluding Remarks

I would like to close this dissertation with some concluding remarks, emphasizing three of the main themes that emerge from this work.

5.1 Theoretical Internalism

In chapters 2 and 3, I have insisted on the relation between the aim of a theory, its structure, and the way distinct theoretical structures might differ in how they construct their explanatory targets. In chapter 2, exploring these relations led me to conclude that the theoretical equivalence strategy, as a realist weapon against cases of underdetermination, presupposes that the structure of rival theories can be analyzed in terms of what is physically relevant and what is superfluous in a common and neutral vocabulary. In practice however, such a vocabulary is not available. Thus, the comparison is established on the basis of one of the rival theories, although their vocabulary is embedded in an epistemological stance on what constitutes a good theory that favours the content of this theory, while dismissing significant parts of the other. The algebraic formulation of Quantum Field Theory, for instance, is embedded in a strong operationalism, and has been developed based on a very thin concept of physical content. The proof of equivalence formulated in this context clearly missed fruitful features of the parafield program. Notably, it missed what made it possible to develop parafield theories into an experimental program to test Pauli's exclusion principle, as it did not seem to have the resources to take these features into account. In sum, I do not think that the structure of a theory can be interpreted, especially when this interpretation amounts to separating the relevant from the superfluous, independently of the epistemic considerations that motivated its construction. And given that formal approaches presuppose that one can simply read off from the theory its relevant content, I do not consider that formal approaches to equivalence can really do justice to the interesting features of the disadvantaged theory–by which I mean that pairing these theories as one and the same will most probably amount to loosing the characteristics of the theory that could be heuristically fruitful and should be preserved for this reason.

In chapter 3, I criticized the appeal to theoretical virtues such as explanatory power in order to discriminate between empirically equivalent theories. I argued that one's stance on what a good theory–or, in that case, a good interpretation of Quantum Mechanics–should be like not only shapes what counts as physically relevant and as superfluous structure; but that it also determines what constitutes a fact requiring an explanation, the nature of the *explanandum* and the resources available within the theory for providing an *explanans*. Focusing on comparing interpretations of Quantum Mechanics in terms of their theoretical virtues hides the fact that "justifying the Symmetrization Postulate" does not define a similar explanatory program in both interpretations given their different resources. In Bohmian Mechanics, what needs to be explained is why the Symmetrization Postulate is not needed at all, once the scope of the Indistinguishability Postulate is extended so as to apply to the particles' velocities. Likewise, in standard Quantum Mechanics, the Symmetrization Postulate does not require an explanation, but should be understood as the mere acknowledgement of the referential ambiguity created by the use of labels. In this context, this postulate is nothing but the statement that the assumptions underlying the use of labels need to be neutralized for systems of identical particles, as the referential ambiguity they create, combined with the linear nature of the laws that quantum systems obey, generates artifacts.

I call 'theoretical internalism' the position according to which what constitutes relevant and superfluous structure, and what constitutes an *explanandum* are defined within a given theory, and thus do not allow for a neutral comparison across theories. This does not mean that criteria of physical significance, or explanatory targets, never overlap in different theoretical contexts. But the burden should be on showing the existence of such an overlapping, before proceeding to such a comparison. The use of theoretical equivalence or theoretical virtues to either pair theories as one and the same or privilege one over the others should be based on a demonstration that such a comparison is possible, meaningful, and is fair to both theories. I hope to have shown in chapters 2 and 3 that such a basis is not always present, and that comparing theories in that sense can be damaging, misleading or masking the features worthy of interest of the theories under comparison.

Does it mean that we should never try to compare rival theories? That theories are incommensurable? This is by no means what I mean by 'theoretical internalism'. First, the kind of comparisons that I deem unfruitful and misleading is a very specific kind, those comparisons made across alleged empirically equivalent theories in the hope of escaping cases of underdetermination. Comparisons like these are made to deny that there are really two theories, or to favour one over the others. Such a goal necessitates 1)to establish that the entire content considered relevant by both sides is included in the notion of theoretical equivalence at play, and 2) that the explanatory demand upon which the claim of greater explanatory power depends is fair to both parties. Second, as I mentioned above, I am not excluding that sometimes such a basis can be found and used as grounding an underdetermination escaping strategy. Nevertheless, this common basis cannot be merely assumed, it must be demonstrated. Finally, my suggestion to use theoretical equivalence to clarify the structure of rival theories instead of merging them as one and the same shows that comparisons are not only possible, but fruitful for clarifying the structure of rival theories and pinning down where exactly the interesting differences between them lie.

5.2 Eliminating superfluous structure is counterproductive

The theoretical equivalence strategy and the appeal to theoretical virtues are supposed to be answers to an underdetermination problem, i.e., they are supposed to provide a way for the scientific realist to escape this threat by either denying that the theories are genuine rivals, or by offering a solid epistemological reason to choose one over the other. None of these strategies, however, seem to have ever been in a position to convince the other side. Yet, in many cases, had they been listened to, they would have prevented the exploration of fruitful features of these theories. Let us take a look at theoretical equivalence first. Greenberg (Greenberg and Mohapatra 1989, Greenberg 2000) has eventually abandoned his non-relativistic quantum mechanics of parastatistics because the violations of Pauli's principle it implies would be gross and already observed; his locally gauged parafield model because of the difficulties to formulate a local field theory that would be consistent with small violations of the PEP–i.e., not already empirically ruled out, while Govorkov continued to develop possible parafield theories ((Govorkov 1991), and physicists such as Nelson (Nelson et al.) kept working on defining conditions in which an pair of paraparticles could be observed.

In parallel, Bohmiam Mechanics and standard Quantum Mechanics are sometimes presented as cases of notational versions of one and the same theory. Yet, Bohmian Mechanics clearly accepts more structure than the latter, and gives a role to this structure: to recover classical physical concepts, or provide a complete theory in the Einstein-Podolsky-Rosen sense for instance. No counterpart to this extra structure can be found in the standard formulation. A proof of theoretical equivalence, if reached, would then necessarily rests on the dismissal of this additional structure, and thus fail to convince the Bohmian side. More generally, as pointed out by Coffey (2014), there is no agreement on an unambiguous criterion for theory equivalence, and more broadly on which theories to pair as theoretically equivalent. Coffey explains this lack of consensus by the focus on formal relations between theoretical structures, which according to him fail to take into account the differences in individual intuitions with respect to theoretical equivalence (2014, 834):[1]

> But logico-structural relationships between formulations hold independently of individual intuitions regarding which formulation pairs are theoretically equivalent. (Whatever the formal relations between Newtonian gravitation theory and Cartan theory, for example, those relationships are fixed, regardless of intuitions about their theoretical equivalence.) So if agents disagree about whether two formulations are intuitively theoretically equivalent, an appeal to an analysis based on the logico-structural relations holding between those formulations is unlikely to account for the diverging intuitions.

However, Coffey does not think that this failure undermines the theoretical equivalence strategy in general, but only the formal approach to theoretical equivalence. He suggests as an alternative the project of 'interpretive equivalence', which he defines as follows:

> Two theoretical formulations are theoretically equivalent exactly if they say the same thing about what the physical world is like, where that content goes well beyond their observable or empirical claims. Theoretical equivalence is a function of interpretation. Its a relation between completely interpreted formulations.

I share, to some extent, Coffey's anti-formalism, although I do not agree with his motivations for rejecting formal approaches. I do not think that our intuitions with respect to a notion as subtle and complex as theoretical equivalence should have any weight in the discussion. In many ways, our physical theories are not intuitive at all, be it at very small or at very large scale. As a result, I do not consider that intuitions are the relevant place to look at to inform our judgments about theoretical equivalence. But I tend to support his idea that formal approaches are never enough, in that they do not consider all the relevant aspects that should be considered

[1]The second reason he offers to explain this failure is the fact the theoretical equivalence judgments are identity judgments, which should be symmetric judgments, but that many cases of formal approaches to theoretical equivalence ends up with asymmetrical judgments. Many consider–although again, there is no consensus on this– for instance that the Newtonian and the Lagrangian approaches to dynamics, are theoretically equivalent, but that the other has advantages that the first does not have. (Coffey, 2014, 830-831).

to deem two theories equivalent. Nevertheless, while I agree with the fact that, in order to play the role it is supposed to play, our concept of theoretical equivalence must include more than the empirically confirmable or disconfirmable, and necessitates a common interpretive project, I am not at all optimistic that many cases of rival theories accept a common interpretation. Moreover, I do not think that those rare cases for which a common interpretation is possible would exhibit interesting cases of rivalry. The algebraic quantum field theory of ordinary particles and the algebraic parafield theory have a common interpretive project. But they do not present a genuine case of underdetermination, as neither of these theories are empirically adequate and take seriously 'paraparticle' as a theoretical term in a realist sense.

The same inconclusive results seem to be reached in the case of theoretical virtues. It does not seem that any defender of the standard Quantum Mechanics has been convinced by the Bohmian argument to the point of switching sides. Those who are convinced that paraparticles are surplus structure will continue to think so. Likewise, they will certainly not be convinced of the greater explanatory power of Bohmian Mechanics given that this explanatory power explains away something that does not require an explanation, and rests on hidden variables that they consider superfluous as well.

One way to formulate the general worry I have with eliminating structure deemed superfluous is the following: either this strategy will remain inconclusive, and a considerable amount of time and energy will be devoted to suggest arguments that can only convince people already on your side. Or these arguments will have an impact, and most probably lead to drop parts of the theories that could have been useful in eventually breaking the empirical equivalence of the rivals, as was the case for the parastatistics program, ignored by most physicists after the 1970's. According to Laudan and Leplin (1991), most of if not all the examples of underdetermination provided are cases of *transient* underdetermination, whose equivalence will ultimately break down when sufficient progress in technology will have been done so as to revise the auxiliary hypothesis thanks to which predictions get extracted from scientific theories. If they are right to think so, we should make sure to preserve these features that differ from a theory to another.

Betting on the transient character of the underdetermination seems to be a much safer bet than forcing a choice upon us or dissolving the problem: the scientific realist remains unthreatened, for she can simply suspend her judgment with respect to which theory is the correct theory, and meanwhile work on clarifying the structure of the alleged rivals, from the point of view from which they have been developed, to find potential sources of experimentally exploitable disagreements, or to get a clearer understanding of the theory itself.

Consider the following example. As we have seen, the Indistinguishability Postulate states that only those observables that are invariant under permutation are physically relevant to describe a system of indiscernible particles. Dirac, when introducing what is now referred to as the Indistinguishability Postulate, justified it based on observationalist principles. According to him, the postulate was nothing more than an expression of the observational indistinguishability of a quantum state describing indistinguishable particles and another state where these particles would be permuted. But then, if the PI stems from crude observationalist standards, how come that we need to add another stipulation such as the Symmetrization Postulate to recover the observed facts? One could argue that the Indistinguishability Postulate is justified in a different way, for instance experimentally or logically. However, a postulate that states that, for measurements made on indiscernible particles, it should not make any observable difference whether the measurement is made on the initial system or the permuted one is obviously not testable experimentally. And, as we have already seen, the indistinguishability postulate as we know it, i.e., as applying to observables, is not logically required by the axioms of Quantum Mechanics, for other ways to understand permutation invariance are available.

Another option would be to embed metaphysical considerations into the formulation of the IP such as the individuality or non-individuality of quantum particles. Weyl, for instance, was taking the IP as an expression of the metaphysical nature of quantum objects. Redhead and Teller, on the other hand, were arguing that assuming that quantum particles are individuals introduces surplus structure in the theory. Putting aside any consideration of what should be the role of metaphysics when it comes to decide on physical principles, metaphysics does not commit us either to one or the other understanding of the permutation group.

It seems then that if a justification has to be found, it is only in the action of the permutation group itself and in what it implies for the structure of the theory. The question of whether paraparticles are superfluous, of whether the Symmetrization Postulate needs to be explained and the Indistinguishability Postulate justified have to be put into brackets until one really understands what is meant by permutation invariance and how it relates to the structure of the theory. If this understanding does not rely on any experimental, metaphysical, observational or logical reasons, then one might hope that it would possibly stem from the structure of the theory itself. By switching the focus from attempts to eliminate superfluous structure or warrant the explanatory power of one of the rivals to clarifying the meaning of permutation invariance in different theoretical structures, based on different assumptions, I contend that one has better chances to clarify how these three problems need to be differently addressed in an interpretation of Quantum Mechanics, which does not have the resources to distinguish between permuted states describing indiscernible particles, and another which can distinguish between them at least diachronically.

5.3 Crucial simulations

In chapter 4, I have criticized robustness analysis as an effective method to identify reliable predictions in cosmological simulations. I offered an alternative to robustness analysis with the methodology of crucial simulations. In a crucial simulation, a very simplified scenario is provided that makes it possible to rule out all the possible numerical and physical responsible for a given prediction but one of each, considered the most likely culprits. Then, the idealized simulation is run to verify or infirm the prediction extracted from the numerical or physical hypothesis and absent from the other, so as to confirm the artificial or physical nature of the mechanism at play.

In the future, I would like to extend this work to other scientific areas heavily based on simulations, like the study of climate models. Similarly to cosmological simulations, interactions between distinct climate processes are so complex that only simulations can track them. Thus, one would like to know when a prediction made through simulations is likely to be true, and when it should be discarded. Wendy Parker, in her 2011, has shown that robustness analysis fails to deliver a reliable methodology for sorting out trustworthy from non-trustable predictions: when distinct climate models agree on a hypothesis, it cannot be inferred that this hypothesis is likely to be true or that our confidence in it should increase. The premise on which the robustness argument rests, i.e., the assertion that "it is likely that at least one simulation in this collection is indicating correctly regarding hypothesis H" (2011) is not warranted, first because a comparison across an ensemble of models will not sample enough of the possibility space that such a claim can be justified, but will only span that uncertainty range corresponding to the models actually used in such a comparison. Second, one cannot trust the performance of models which successfully predicted past climate, for this success may be artificially inflated and the models tuned to reproduce the known data. Crucial simulations could thus be a good candidate to replace robustness analysis in evaluating climate models too, but only under the indispensable condition that simulations can be simplified in a way similar to the cosmological ones. In other words, one has to assess whether an alternative of mutually exclusive hypotheses can be tested in this context, and this requires to have enough control on the simulations to proceed to such a simplification. Given the complexity and the diversity of the phenomena intertwined in formulating a prediction about future climate, the extent to which crucial simulations can be successfully applied to climate models deserves careful scrutiny, and as such goes beyond the scope of this dissertation.

Remember, however, that our primary concern was to find a methodology to exclude artifacts from scientific theories, and that we were hoping to find in the study of simulations methods that could be applied to clarifying the structure of theories. The answer to whether crucial tests can fill such a task is not a definite and unequivocal one. Like the extension of crucial simulations to other scientific areas, it requires an assessment of whether one is in a position to provide a relevant alternative to test, and whether there is a context/scenario in which the discriminating prediction extracted from the artificial or the physical hypothesis can be verified. The difficulty with the first condition comes from the fact that artifacts might remain hidden because they come from unavoidable, or close to unavoidable, idealizations. Finding a scenario to test such an alternative might thus be really tricky. The difficulty with the second condition is similar to the one described for climate change: listing all possible simulation components responsible for a given property or prediction, and constructing a scenario where possible culprits can be 'turned off' and the remaining one tested is a very complicated task. This does not mean that it is an impossible one. My analysis of Bacciagaluppi's derivation and of Quantum Field theories in section 4 constitute attempts to construct such crucial tests. But the impossibility to list all possible assumptions that could introduce paraparticles as artifacts, decide on the most relevant alternative, and to 'turn off' the discarded assumptions accordingly clearly lessen the confirmatory power of such a crucial test. In sum, crucial tests might be to some extent used to analyze the structure of scientific theories, but will never meet the ideal conditions that one can find with cosmological simulations. Their effectiveness may thereby be lessened.

Note that the proof-of-the-case of section 4 is designed as a crucial test and not as an instance of robustness analysis, even though different models are compared. The relevant set of models, theories or interpretations to compare cannot be determined without already having formulated an hypothesis about the source of the artifacts. One does not know where to look at unless one already have in mind a possible culprit. Furthermore, as I mentioned already, robustness analysis would probably conclude that paraparticles are a robust property, for they seem to appear in all interpretations of Quantum Mechanics and even in QFT, whether or not labels are used– Bacciagaluppi's derivation put aside. If so, then robustness analysis and crucial tests reach different conclusions when it comes to paraparticles.

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