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„A structural realist account of structure in physics theories on the basis of symmetries of solution spaces"

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CONTENTS

Abstract

Structural realism is a widely discussed form of scientific realism, sometimes dubbed "the most defensible form of scientific realism" (Ladyman, 2016). As a consequence several arguments against it have been raised, perhaps chief among them the contention that the notion of 'structure' that it is reliant on is ill-defined or too vague to be able to be able to even properly discuss its claims. In this thesis we present a defense of structural realism on this ground by providing a more graspable and concrete candidate for the meaning of 'structure' in the theories of physics in particular. Together with previously established arguments relying on notions of structure, we make a case for structural realism based on the concept of dynamical symmetries of solution spaces to fundamental equations in physics.

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“The rule in the history of physics seems to be that, whenever a theory replaces a predecessor, which has however itself enjoyed genuine predictive success, the 'correspondence principle' applies. This requires the mathematical equations of the old theory to re-emerge as limiting cases of the mathematical equations of the new.”

(Worrall, 1989, p.160)

1 INTRODUCTORY REMARKS

Philosophy and science have an interwoven history and in a relevant sense science can be seen as the brainchild of philosophers with a particular naturalistic, empirical yet still first principle derived application of reason to the observed natural world. Gradually, philosophy and the natural sciences drifted apart as academic disciplines to become what we think of them today (a sketch of this development of the scientific method out of particular philosophical schools can be found in Andersen and Hepburn, 2016). Nevertheless, philosophers have never stopped asking questions about the natural world and in many instances have also engaged in meta conversations about science and philosophy themselves. What can be said to be known in the first place (Steup, 2018), how does science achieve all of the things it does (Andersen and Hepburn, 2016; Bogen, 2017; Niiniluoto, 2015), why is the world describable by mathematics and physical laws (Horsten, 2018; Mancosu, 2018; Russ, 2011; Suppes, 2011), how much stock should we give scientific knowledge over other potential sources of knowledge and what differentiates and demarcates science from pseudo-science or non-science in the first place (Hansson, 2017; Pennock, 2011; Pigliucci and Boudry, 2013)? These are merely a select few of the questions that have been asked in some form or another in philosophy from the beginning of the first philosophical musings and writings all the way to modern academia.

The questions of how science can achieve what it does and just in what way we should think about the content of scientific theories have become increasingly relevant and pressing in the past decades and centuries, as a modern and science- and technology-empowered world has emerged from our initially rather modestly successful attempts at describing and understanding the world. Philosophers and scientists alike debate these topics in the philosophical subdiscipline known as philosophy of science. In particular, the question of whether science tells us anything "true" about the world or how we are to think of scientific knowledge and the intuitively very 'exotic' objects science speaks about (e.g. electrons, DNA, energy) has been receiving much attention in the context of discussions around so-called scientific realism and anti-realism. These two not entirely mutually exclusive viewpoints hold either that in some non-trivial sense and domain of study science *does* tell us something true about the external world (realism) or that it does not (anti-realism). It is important to note, however, that neither of these views is monolithic and static. Indeed, various accounts of what scientific realism means or should mean have emerged and the same is true of anti-realism (see for example Chakravartty, 2017 or Chalmers, 1999 for an overview). Some philosophers are anti-realists about specific aspects of abstract science and not about a particular other class of theories which they consider distinguished somehow, for example by being observable without the use of "theory-laden instruments" (Bogen, 2017; Kuhn, 1962). Some realists think it is the actual objects of science that we should believe to exist whereas other realists think we should bestow our confidence on the structures contained *within* scientific theories. The latter view, dubbed structural realism, promises to be 'the best of both worlds' (Worrall, 1989) and combine under its banner the strongest arguments for both sides of this debate in a consistent manner. Many however have criticized that it is only apparently capable of doing this by being unclear about the intended meaning of 'structure' (e.g. Arenhart and Bueno, 2015).

This debate is vast and has been going on explicitly for over a century and implicitly for far longer than that. Understanding some of the strongest arguments for each side of this debate will be crucial to the purpose of this paper, which is to give an account of what "structure" could mean within structural realism as it applies to physics. To see why a concrete and tangible concept of 'structure' is crucial to this longstanding philosophical debate, we will begin in section 2 by giving a sketched historical account of the development of the realism and anti-realism debate leading up eventually to Worrall's famous short paper coining the notion of structural realism. After giving an introduction to structural realism we will see that it is anything but clear what "structure" might be intended to mean within structural realism and in what sense we are supposed to believe in the existence of structures of scientific theories – are we to believe in those structures because they are the only things we can know about (epistemic structural realism) or are we to believe in those structures because they actually are things that have the existence property instead of particular objects (ontic structural realism). Once the issue of the meaning of structure is made clear, section 3 will offer a candidate for the characterization of structure within the theories of physics which satisfy the requirements of structural realism.

The paper will thus argue in favor of a kind of structural realism or at least structuralism (the notion that a coherent sense of structure exists within scientific theories) towards certain aspects of theories of physics. We conclude by addressing potential criticisms of this characterization of structure in section 5, including a brief discussion of potential further places to take this argument.

2 THE REALISM / ANTI-REALISM DEBATE

Some very useful and in-depth compendia and monographies on scientific realism from both a systematic and historical viewpoints are (Chalmers, 1999), (Chakravartty, 2017) and (Dicken, 2016).

2.1 INTRODUCING THE DEBATE

The philosophical positions known collectively as scientific realism all share a central premise regarding the nature of scientific research: At least *some* aspects of at least *some* scientific theories tell us something about the actual nature of the world, at least in some approximate sense. The many qualifications included in this general premise hint at the large variety of positions that have been defended under the label of scientific realism – in general it is thus better to think of scientific realism as a *category* of philosophical worldviews rather than a worldview in itself.

The most easily described but also the most naïve sense of scientific realism would simply hold that science describes reality, the external world, and that the objects scientific theories speak of such as DNA or electrons actually exist and that scientific theories make true statements about them. There are, however, immediate problems with such an unsophisticated form of realism, such as the evident fact that the ontology of scientific theories changes or at least intuitively appears to change as new evidence from experiments and observation are obtained and as mathematical models are improved. We no longer believe in the existence of the luminiferous ether, the hypothetical medium in which light waves were thought to propagate (Michelson, 1881; Worrall, 1994). We also no longer believe in phlogiston, a hypothetical substance contained in flammable materials that was used to explain why some things are flammable and others are not (Weisberg et al., 2016), nor do we believe that diseases are caused by miasma or "bad air" (Karamanou et al., 2012). Our best scientific models once pictured the long extinct non-avian dinosaurs as sluggish cold-blooded, scaly animals while modern science describes them as very likely warm-blooded, highly active, specialized and often feathered (Allmon, 2006; Bakker, 1986). Hence it appears we cannot consistently believe in an external world independent of our own thoughts and beliefs and yet still believe that scientific theories describe truth in this very raw sense without running into countless contradictions.

To account for these issues philosophers have come up with several weaker and far more defensible forms of scientific realism. The first and most natural adjustment to realism is to slightly weaken its claim about truth into a claim about *approximate* truth. Scientific theories, one could hold, while not being *strictly* true, nevertheless make statements about the world that are true in some sufficiently approximate sense. Importantly, in order to be a consistent notion of realism this view would also have to hold that scientific theories improve over time, i.e. that successor theories are closer to the truth in some appropriate sense than or at least as close to it as its predecessors. Intuitively, this view would say that at first we only knew that the extinct non-avian dinosaurs existed in *some* sense, then we learned that they were reptilian, laid eggs, were related to birds and gradually as evidence and knowledge accumulated we improved our understanding of them to the point we are at now. Crucially, the point we are at now would be considered more true or more accurate and the possibility is granted (even demanded) that our understanding of dinosaurs may continue to improve in the future. This is the rather powerful but still modest form of realism that is implied by our use of natural language in most societies, variations on this sort of realism are sometimes labeled “entity realism”, since they hold that the entities described by science exist in some approximate sense.

We speak of people in ancient times *believing* X was true while we now *know* X is false. We speak of scientific *discoveries*: Newton *discovered* the law of gravitation, he did not *invent* it. Of course, natural language merely reflects a broad sense of collective human intuition and should not be used as an argument in itself unless one is also willing to concede that the sun literally rises and sets – a sentiment which given a realist interpretation of scientific theories we “now know is wrong”. As Worrall puts it:

> “What is the status of the genuinely theoretical, observation-transcendent content of our presently accepted theories? Most of us unreflectingly take it that the statements in this observation-transcendent part of the theory are attempted descriptions of a reality lying “behind” the observable phenomena: that those theories really do straightforwardly assert that spacetime is curved in the presence of matter, that electrons, neutrinos and the rest exist and do various funny things.” (Worrall, 1989)

Anti-realism conversely rejects the above views, again *in some sense*. The qualifier is needed once again because anti-realism is not monolithic either and in fact involves several nuanced positions about what should and should not be believed about scientific theories – it is again more of a category of philosophical worldviews than a worldview in itself.

Notably, many anti-realists are what one might want to call *selective* anti-realists: They are not anti-realists about *every* empirical claim of science but merely of those about a particular subset of things. The most popular variety of selective anti-realism is the notion that observability without tool use is the condition that should be adopted. In other words, this view would hold that we should take concepts like electrons and DNA as mere model entities and that we cannot or should not speak about them as if they *actually* existed, while something like a table or a knife can be perceived with human senses directly and thus are acceptable to include in one's ontology. Tool use to observe and test model entities such as for example microscopes and telescopes are thought of as *theory-laden*, meaning that the very reliance on them presupposes that we to some degree understand how they work. This, such anti-realists would argue, means that we cannot use tools to argue for things because the tools themselves rely on our theories of optics, electronics and mechanics and so on our scientific theories being accurate or true in the first place, causing a sort of circular argument.

The selective flavor of anti-realism described above has never really fully resolved the problem of drawing the line between the directly observable and unobservable in a satisfying and non-arbitrary sense. Not only can the human senses "directly" be fooled in many ways, many people also enhance their natural senses with things like corrective glasses which should count as tool use just as much as the use of telescopes since their use is equally theory-laden. It may seem at first glance as if this form of selective anti-realism avoids the vagueness of the modest realism's "approximate truth" described above but this is an illusion: What does it mean to be directly observable anyway? A table and chair might seem like an obvious example but what about the surface of Mars, what about a Mars rover we sent there? If a human were there it certainly would be directly observable but as it stands we can only use tools to glance at it. Are we to be anti-realists about the surface of Mars until an actual human lands on it? Are we to stop being realists about the Mars rover once it passes from the grasp of any living human's direct observation? And who has to make the observation, any human at all or all humans for themselves? Even if we were to introduce the also incredibly vague concept of "a suitably positioned observer could directly perceive it", any object or event in the past for example is not directly observable in this sense either and so unless we wish to never make any claims about ontologies in the past we would be required to come up with something like "a suitably positioned observer who can time travel" and the problem of specification would continue through other problems and continuously become more convoluted and unintuitive. The issue of coming up with an accurate characterization of "direct observability" that is self-consistent and tenable has proved to be of comparable difficulty to the problem of defining "approximate truth" in realism.

Some other anti-realists share the belief that the line drawn between the observable and unobservable is ultimately arbitrary. They thus either reject the claim about the existence of an external world as a whole or at least hold that what we can know about the external world is extremely limited and that most of our activities should more accurately be considered an active construction of models of perceived reality rather than a passive and descriptive investigation. This second form of anti-realism is known as constructivism and it holds that our best scientific theories are merely constructed models which do their job of "predicting and being useful" successfully and we thus keep them around. As a consequence they would hold that the concepts of truth and falsehood or existence and non-existence do not apply to the electron, the feathered non-avian dinosaurs, phlogiston and the luminiferous ether. Instead, many constructivists will simply judge models based on their usefulness and often adapt a Darwinian view of scientific theories in order to explain why they appear to be improving: We simply keep those that are more useful around and so inherently theories will tend to become more useful. No truth value ascription, not even any sense of approximate truth is needed from their view. The more radical anti-realist position which rejects the claim about the existence of an external world outright is a rabbit hole of radical philosophical skepticism which we will not further discuss in this paper – for a discussion and history of this idea and its critics see for example Machuca, 2018 or Zieminska, 2017.

These accounts of realism and anti-realism are by no means exhaustive. For a full understanding of these philosophical viewpoints and arguments, a study of the history of the debate as well as the several flavors of realism, anti-realism and constructivism are worthwhile. For our purposes, however, this brief introduction to the debate will suffice to motivate the idea of structural realism in section 2.4, the philosophical problems of which are the subject of this paper.

2.2 THE NO MIRACLES ARGUMENT

A fundamental problem of all of the anti-realist views according to the scientific realists, aside from the specific problems of particular versions of anti-realist views sketched above, is that they do not manage to account for why scientific theories seem to work. If electrons are not real in *some sense*, how come we can control them to such a precision based on our models that we can bundle them into lasers that could kill people or use them to image crystal clear details via electron microscopes. As Putnam put succinctly:

> “The positive argument for realism is that it is the only philosophy that doesn't make the success of science a miracle.” (Putnam, 1979, p.73)

What is actually going on according to the anti-realist and how do they explain the success of science if they deny the things science speaks of? This line of thinking is called the no miracles argument or sometimes just the miracles argument in reference to Putnam’s choice of language in the above quote and is often considered to be the basis of the most powerful arguments in favor of some kind of scientific realism (Chakravartty, 2017; Worrall, 1989). As a result, it is also one of the most hotly debated and most contested arguments surrounding the realism debate.

At the core of the argument lies the implicitly assumed acceptance of the immense success of scientific theories to accurately predict events. To provide an example which is not particularly standard in philosophy, consider that only recently in the year 2014 a probe by the European Space agency autonomously landed on a comet after a 10 year journey through space having been originally launched from Earth in 2004, its paths and intercept times with the comet having been predicted, calculated and planned years before the event actually occurred (Biele et al., 2015; Schulz et al., 2009; Simonin et al., 2012). The a priori difficulty of accomplishing such a task is immense and can barely be overstated. The success of the underlying physics principles without which this amazing success story could not have occurred requires *some* philosophical explanation. How is it possible that nature’s behavior is captured so accurately in the equations of physics? Realism provides a clear-cut answer: Scientific theories work because they are approximately true, i.e. they accurately capture parts of the external world’s actually existing laws and thus knowing these laws to a decent enough approximation allows scientists to predict all kinds of things and perform feats which would otherwise seem like magic.

Anti-realism is not defenseless before the no miracles argument. Most forms of anti-realism indeed acknowledge and respect the contributions of science towards technology and applications and only specifically reject the notion that science speaks about the ontology of the natural world. There is no grand-unified response that anti-realism gives to the no miracles argument. Instead, each of the many variants of anti-realism typically has its own set of critiques of realist viewpoints and arguments.

Some anti-realists might posit that the usefulness of scientific theories is in fact the result of a Darwinian selection rather than a process towards approximate truth (compare for example Kantorovich, 1996; Magnus and Callender, 2004; Musgrave, 1982). This argument shares a common idea with the gradual evolution of lifeforms to suit their given environmental pressures, in the case of scientific theories the pressure would be akin to “Does this theory work in practice?” or “Is this theory useful in practice?”. Anti-realists pursuing this counterargument see the gradual improvement of theories in the description and prediction of the world as the more or less simple result of selecting among candidate successor theories those which are more accurate and thus useful in their predictions. Such a methodology, they might argue, will lead to what we observe without any reference to ontology or approximate truth being necessary and thus the whole premise of the no miracles argument, that the vast success of scientific theories is in need of philosophical explanation, is contested.

Other anti-realists might find the no miracles argument to not be a convincing argument in the first place, pointing at all of the failed ontological interpretations of the past, such as the above-mentioned luminiferous ether or phlogiston. They may further advance that if we have currently no reason to believe those things exist, then who is to say that in a century we will not look back on the theoretical entities of current scientific theories in the same way? This line of argument is known as the pessimistic meta induction, one of the most widespread and strongest arguments in favor of scientific anti-realism.

2.3 THE PESSIMISTIC META INDUCTION

The name of the “pessimistic meta induction” derives from the fact that it concludes inductively from the failure of many (or perhaps even most or all depending on the philosopher making the argument) previous scientific theories to accurately capture the ontology of the natural world that the current best scientific theories will eventually suffer a comparable fate, hence the label "pessimistic". It is a *meta* induction in the sense that it reasons inductively about science, which perhaps might be seen as slightly ironic given that science itself is often viewed as an enterprise largely based on inductive reasoning.

The strength of this argument lies in the inevitability of its premises: Most realists will readily admit that there is no phlogiston and that the luminiferous ether eventually failed as a hypothesis and is thus no longer a part of the ontology of modern scientific theories. The crutch of "approximate" truth rather than actual truth can be invoked to weaken this argument slightly in some specific applications but unfortunately in general this fails as well.

One might for example consider the case of feathered theropod dinosaurs, specifically the well documented case of the development of our understanding of Megalosaurus, one of the earliest fossils discovered and actually named as "dinosaurs" in the 20th century. Looking at the progression of our understanding of Megalosaurus physiology and how it changed over the last century into modern times it is rather easy to at least intuitively picture this as a beginning with a very rough approximation – a scaly, vaguely reptilian, quadrupedal, carnivorous creature of enormous size – to a slightly more true approximation – a scaly, significantly more bird-like reptile, bipedal, carnivorous creature of enormous size – to our modern presumably even more approximately true understanding of Megalosaurus as a species belonging to the clade of *feathered*, carnivorous, bipedal theropod dinosaurs (Allmon, 2006; Bakker, 1986; Walters, 2012). At least in a qualitative sense, it is easy for a realist to point at a closer and closer approximation to truth in this series and thus argue that while our previous interpretations of Megalosaurus *were* inaccurate they were not *completely* wrong but in some sense approximately true.

Others of the above-mentioned examples, however, cannot be accounted for in this fashion: The luminiferous ether, anti-realists might argue, isn't retained at all and was simply found to not exist and was thus removed from physics. Likewise, it could be argued that phlogiston was simply removed from chemistry and that there is no sensible progression from phlogiston to our modern understanding of combustion that can reasonably be framed as a gradual trend towards better and better approximated truth – though attempts have been made within structural realist frameworks (Ladyman, 2011). It would at least initially appear that the ontological interpretations of some scientific theories or hypotheses of the past simply turned out to be wrong, providing fuel for the pessimistic meta induction.

Reframing the history of scientific theories with regards to an approximate truth being conserved across theories is one of the major attempts at disputing the pessimistic meta induction. The above example of Megalosaurus and phlogiston or the luminiferous ether provide examples where a very naïve approach to this line of argument appears to work for one case and perhaps appears to fail or at least be more difficult in other cases. In order to still salvage realism from the pessimistic meta induction, many suggestions have been made to account for what sense of approximate truth we should be looking for: Perhaps there is a sense of approximate truth in the successful scientific theories after all, even those which at first glance might appear to have ontologically and experimentally failed such as the luminiferous ether or phlogiston. The issue then becomes to actually *give* such a characterization of approximate truth instead of relying on intuitions. This is one of the major challenges of scientific realism.

The arguments around scientific realism and anti-realism are sometimes parodied as being an endless back and forth between the no miracles argument and the pessimistic meta induction. In fact, however, many philosophical arguments of the past decades have attempted to unify these two arguments into a compromise position between strong scientific realism and strong scientific anti-realism or constructivism. One of these attempts at resolving the conflict between the pessimistic meta induction and the no miracles argument and in fact of leveraging the argumentative weight of both of them is Worrall's structural realism (Worrall, 1989) and the many forms of structural realism based on it that have been developed since Worrall's initial proposal. Worrall describes the situation in the philosophical discussion around realism in the abstract of his 1989 paper:

> “The main argument for scientific realism is that our present theories in science are so successful empirically that they can't have got that way by chance - instead they must somehow have latched onto the blueprint of the universe. The main argument against scientific realism is that there have been enormously successful theories which were once accepted but are now regarded as false. The central question […] is whether there is some reasonable way to have the best of both worlds: to give the argument from scientific revolutions its full weight and yet still adopt some sort of realist attitude towards presently accepted theories in physics and elsewhere.” (Worrall, 1989)

As a consequence, Worrall’s proposed “structural realism” tries to translate the concept of approximate truth discussed very qualitatively above into structures – the claim becomes that what is preserved across theory changes is *structure* and it is precisely this structure which supposedly justifies a slightly modified realist position.

2.4 STRUCTURAL REALISM

Structural realism is the variant of scientific realism that is often considered "the most defensible form of scientific realism" (Ladyman, 2016). In this section we will sketch structural realism as it was introduced originally in Worrall's paper on the subject matter (Worrall, 1989). We will then describe some of the developments in structural realism that have been made since then in response to criticisms that have been raised against it by more traditional scientific realists as well as anti-realists.

As seen in the above quote in section 2.3, Worrall identifies the no miracles argument and the pessimistic meta induction as the two dominant arguments in the debate and desires to find a position favored by both of them:

> “The main interest in the problem of scientific realism lies, I think, in the fact that these two persuasive arguments appear to pull in opposite directions: one seems to speak for realism and the other against it: yet a really satisfactory position would need to have both arguments on its side.” (Worrall, 1989, p.140)

Worrall sketches a comparable path through the scientific realism debate as described in the introduction of this thesis, highlighting the aspects in which anti-realism leads to the odd lack of explanation for the immense success of science and the aspects of realism which result in too strong statements about ontology. He deems the latter incompatible with the historical ontology change in physics, the two most prominent examples cited in his work being optics and the development of electromagnetic wave formalism for light under Maxwell and the much debated transition from Newtonian mechanics to special and general relativity. To Worrall, the traditional accounts of realism and anti-realism fail in these examples but they fail in a way that may teach us something about properties of scientific theory change in physics. He explains this realization with regards to the example of the luminiferous ether:

> “Fresnel entirely misidentified the nature of light, his theory accurately described not just light’s observable effects but its structure. There is no elastic solid ether. There is, however, from the later point of view, a (disembodied) electromagnetic field. The field in no clear sense approximates the ether, but disturbances in it do obey formally similar laws to those obeyed by elastic disturbances in a mechanical medium. Although Fresnel was quite wrong about what oscillates, he was, from this later point of view, right, not just about the optical phenomena, but right also that these phenomena depend on the oscillations of something or other at right angles to the light. Thus if we restrict ourselves to the level of mathematical equations - not notice the phenomenal level - there is in fact complete continuity between Fresnel’s and Maxwell’s theories.” (Worrall, 1989)

It is some sort of not further specified and defined mathematical structure that Worrall notes is conserved across theory change. The example of the luminiferous ether is a particularly striking case but similar observations can be made about all sorts of theories in physics including the transitions from Newtonian mechanics to relativity, quantum mechanics and quantum field theories:

> “Fresnel’s equations are taken over completely in tact into the superseding theory - reappearing there newly interpreted but, as mathematical equations, entirely unchanged. The much more common pattern is that the old equations reappear as limiting cases of the new - that is, the old and new equations are strictly inconsistent, but the new tend to the old as some quantity tends to some limit.” (Worrall, 1989)

Worrall himself concedes towards the end of his paper that the concept of structure he is using is very rudimentary and anything but formally well defined. His paper is thus intended as more of a motivation of a research project rather than the final word on the matter.

2.5 CONNECTIONS TO THE PROBLEM OF DEMARCATION

Philosophy of Science as an academic field has occasionally been criticized by prominent scientists as being "as important to science as ornithology is to birds" (compare Murcho, 2006; Pernu, 2008), often attributing the origin of this phrase to the influential particle physicist Richard Feynman despite there not being any record of him having said this. An unspoken and quite problematic premise in that remark is that ornithology would not be immensely useful to birds if they could understand it in the same way that the study of humans and human biology has proven exceedingly useful to humans. Beyond this quip however, many scientists are at least cursorily familiar with core philosophy of science concepts such as Popper's falsificationism and will often even use such arguments and viewpoints in describing the purpose of science or the scientific method, so evidently philosophy of science has had at least some impacts on at least the perceptions of some scientists about themselves, if maybe not their active work processes.

There is an important point to take away from this notion, even if it is expressed in an unnecessarily mocking manner: The results of philosophical enquiry should probably not affect the *practices* of scientists to any significant degree. That is to say for the particular case of the realism / anti-realism debate that no matter the "result" of this discussion, it should not affect how scientists work and how they pursue novel research topics. This is contrast perhaps to philosophy of politics, law or morality where one might hope that findings and discussions within these fields find applications in the non-academic world as well but limiting the freedom of development of science in such a way would be doing a disservice to it. This in spite of, for example, the philosopher of mind Daniel C. Dennett quite correctly asserting that:

> "There is no such thing as philosophy-free science, just science that has been conducted without any consideration of its underlying philosophical assumptions." (Dennett, 2013)

The task of philosophy of science should, however, primarily be to understand how science can accomplish what it does and not to tell it what to do – the task is not philosophy-free science but science free of the particularities of realism and anti-realism. In other words, in this paper we will be seeking a *descriptive* account of the structure preservation in physics, not a normative one imposed on physics from the outside. This is not merely because such a philosophy might be seen to be in opposition to science as mentioned above but even just due to the requirements of intellectual honesty within structural realism itself we cannot pursue the path of trying to tell science what sort of thing needs to stay preserved across theory changes: Imposing a normative structural preservation from the outside is in fact the direct way to the intellectual death of structural realism, since it would equate to *demanding* that scientific theory change follow a particular sort of structural preservation and thus, if scientists happened to believe philosophers about this, structural realism would have made one of its strongest arguments – the argument from structural preservation across theory change – a forced and artificial component of the a priori solidly defined scientific method rather than a fascinating emergent property of science in need of explanation.

An enforced structural preservation across theory change would not be impressive nor in need of explanation – what *is* in need of explanation and what provides the basis for structural realism is that science, sufficiently free of the values and notions of realism and anti-realism, would in principle be open to follow evidence and follow new theories wherever they lead and yet *still* and in fact *in spite* of this we find strong preservation of structure in physics theories across decades and centuries.

Worrall addresses this point in his original paper on structural realism for the case of empirical adequacy and the no miracles argument – the no miracles argument in itself only works as a powerful argument if the predictions and success of science were not *built into* the theories to begin with but instead grew organically, not ad-hoc, and produced and predicted new phenomena independently:

> "Similarly the fact that creationist biology can be made empirically adequate with respect to, say, the fossil record clearly founds no argument for the likely truth of the Genesis account of creation. Such empirical adequacy can of course easily be achieved - for example by simply making Gosse's assumption that God created the rocks with the "fossils" there already, just as they are found to be. (Perhaps God's purpose in doing this was to test our faith). But the fact that this elaborated version of creationism is then bound to imply the empirical details of the fossil record is, of course, neither a miracle nor an indication that the theory "is on the right track". The explanation for this predictive "success" is, of course, just that it is often easy to

> incorporate already known results ad hoc into a given framework. Nor is the success of a theory in predicting particular events of an already known kind enough on its own to sustain a 'no miracles' argument in favour of a theory." (Worrall, 1989)

For comparable reasons, accounts of structural preservation across physics theory change can never be prescriptive and must always *be descriptive and explanatory* in nature.

So far all of the reasons presented in this paper for studying the question of scientific realism or anti-realism were academic in nature. These reasons related to what we can know and were justified by the common drive in all of philosophy to find the right questions to ask and attempt to give ways of thinking about such difficult problems (compare Dennett, 2013, section II-1). We have also discussed above that the scientific workflow itself should at least for the most part remain unaffected by discussions about realism. There are, however, some potential reasons outside of philosophy and in fact outside of academia as a whole to consider the question of realism and anti-realism to be of importance. Dennett speaks about philosophy as in some sense being blessed by its low impact on the rest of the world:

> "It is fortunate for us that philosophy is largely ignored by the rest of society, since otherwise we would have to conduct our business much more cautiously, guarding against overstatement and the sort of grand claims that get scientists in trouble with their peers when their advertisements for their hypotheses get distorted and further magnified by the press." (Dennett, 2017)

Unfortunately, however, this simplified view of the impact of philosophical arguments is no longer accurate in the age of the internet, if it even ever was at all. To give the relevant example for this paper, in a lot of anti-science and pseudo-science propaganda in all forms of online media one finds arguments like the pessimistic meta-induction and other arguments of anti-realism and constructivism, even if they are often simplified and even oversimplified versions. Many camps of enablers of pseudo-science have to a large degree used the arguments of anti-realists and constructivists to attempt to undermine the validity and importance of scientific research, leading not merely to a worse understanding of the world which might be jokingly said to be the worst consequence of a purely academic philosophical mistake, but to the direct loss of life due to the ignoring of scientific medical advice and the use of dangerous and unstudied “alternative” pseudo-medicine. This is but one form of pseudo-science that attempts to coopt philosophical arguments to undermine people’s perception of science.

The above Worrall quote on some forms of creationism also provides a *second* connection to pseudo-science and the problem of demarcation in that a second distinct type of pseudo-scientific endeavors espouse not to undermine scientific validity with arguments such as unpolished forms of the pessimistic meta induction but instead to ad-hoc make their own ideas empirically adequate and pretend to be in tune with scientific methodology. This kind of pseudo-science corresponds loosely to the argumentative weight of the no miracles argument, the most well-known example of this type being the modern form of creationism prevalent in the United States (see Nieminen et al., 2015; Rivers Singleton Jr., 1987 and Taylor and Ferrari, 2010 for philosophical discourse on this topic), while the type mentioned in the previous paragraph corresponds loosely to the pessimistic meta induction side of the philosophical discussion. Most of the modern esoteric forms of pseudo-science fall into the latter category.

The topic of pseudo-science is vast and the demarcation of pseudo-science from actual science is another widely discussed and extremely important issue of modern as well as historical philosophy of science (compare Pigliucci and Boudry, 2013), so an in-depth discussion of this issue is certainly beyond the scope of this paper. The important key point here is that philosophical academic discourse does not exist in an isolated bubble from the rest of humanity and that while philosophy should never self-censor truth out of fear of its consequences, philosophers need to be aware of the potential impacts and misunderstandings that can be caused in the public eye’s interpretations of their work. They should work diligently to make the implications as well as the non-implications which at first glance might *seem* to follow as clear as possible. Just as accurate science communication is increasingly gaining in importance as the complexity of scientific theories and their impacts on our lives increase, so does philosophy communication have to improve in many ways. The debate around scientific realism and anti-realism has found a false echo in the public sphere in the form of science versus anti-science and many forms of pseudo-science attempt to coopt philosophical arguments for their own gains at the expense of everyone.

Realism versus anti-realism in an academic philosophical setting is generally *not* science versus anti-science and neither the no miracles argument nor the pessimistic meta induction are intended to be entangled with the problem of demarcation to the degree that they are in the public sphere. The pessimistic meta induction in particular does *not* relate to the usefulness and importance of science but to what we can know about science and how to properly think about existence of model entities within it. Correspondingly, the no miracles argument does *not* lend credibility or scientific status to anything with ad hoc and post hoc artificially induced empirical adequacy.

2.6 EPISTEMIC VERSUS ONTIC STRUCTURAL REALISM

Very broadly, two kinds of structural realism can be distinguished: structural realism as an epistemological position and structural realism as a position about ontology. The distinction is however far less clear and also far less relevant to the present goal of this thesis than might initially appear.

Epistemic structural realism essentially holds that the reason we should be realists about structure rather than particular entities is that our access to the objects is in some sense inferior to our access to the structure – it does not claim that there are no objects but merely that the structures of scientific theories are more reliable than our object-based interpretations of them. Ontic structural realism on the other hand claims that the structures themselves are what we should be realist about in some non-trivial and not merely epistemic sense. In some sense then, ontic structural realism appears to be a stronger statement about structures of reality and scientific theories than epistemic structural realism but again it needs to be noted that the commitment to include structures themselves in one's ontology does not necessarily exclude objects or entities from also being part of one's ontology.

Ladyman lists four different broad categories of ontic structural realism based on the particular stance taken with regards to the connection between potential entities and relations as well as the existence of entities:

> “1) Eliminativism: there are no individuals (but there is relational structure) […]
>
> 2) There are relations (or relational facts) that do not supervene on the intrinsic and spatio-temporal properties of their relata. […]
>
> 3) Individual objects have no intrinsic natures. […]
>
> 4) There are individual entities but they don't have any irreducible intrinsic properties.” (Ladyman, 2016)

Unfortunately the significant differences of these variants of ontic structural realism lead to very specific counterarguments against each of them, leading to occasional confusion about which particular claims are being espoused. Nevertheless, since all of these positions take an ontological approach to structures within a structural realist setting the term “ontic structural realism” is applied to all of them.

2.7 Criticism of Structural Realism

In this section we will list some of the more common direct criticisms of structural realism and attempt to sketch answers that have been given to these criticisms in the philosophical literature. The final issue of structural realism we will encounter in this section is the question about the concrete meaning of structure within structural realism. An attempted answer to that particular pervasive problem of structural realist accounts is the primary topic of this thesis and will be covered in section 3.

2.7.1 Decay to entity realism

Claims have regularly been made that structural realism, if it were consistently pursued, would decay to a more naïve scientific realism or at least a form of entity realism. Chakravartty, for example, proposes a sort of “semi-realism” and holds that structural realism and entity based realism are equivalent in the sense that they each imply the truth of the other (Chakravartty, 2013, 2008, 1998). Psillos famously argues that Worrall’s account is either essentially just regular scientific realism again or not even coherent to begin with (Psillos, 2001, 1999, 1995). Ladyman proposes a strong eliminative ontic structural realism as a way out of such problems (Ladyman and Ross, 2007) and in a countermove Arenhart and Bueno recently argued that even the strong ontic forms of structural realism imply an ontology which includes entities as well (Arenhart and Bueno, 2015). In further contrast to Ladyman's approach, Esfeld and Lam have repeatedly defended and argued in favor of a moderate ontic structural realism that explicitly has no interest in denying the possible existence of entities, perhaps even entities with weak forms of intrinsic properties (Esfeld and Lam, 2011, 2006).

The contention that structural realism is not nearly as different as Worrall intended is thus definitely not a unique one and deserves to be given at least some attention in light of the large role arguments of this type play in the discussions surrounding this view. In this section we will thus sketch the arguments that some of the above mentioned philosophers as well as others have put forth to argue (either intended as positive argument or as a criticism) for the lack of the clear argumentative distinction between some forms of structural realism and entity realism that Worrall had clearly intended in his seminal paper. We will then defend the stance that structural realism indeed *does* imply a form of entity realism but also offer reasons as to why this is a feature of structural realism rather than a bug. The argumentative strength of structural realism is to be identified in its ontological and epistemological *focus* rather than its ontological *scope*.

Arenhart and Bueno, restricting their argument specifically to a structural realism which aims to eliminate objects or derives objects from more fundamental structures, contend that even such a view necessarily includes objects or entities and is thus contradictory and untenable (Arenhart and Bueno, 2015). While some other forms of structural realism in their characterization may still place entities as primary objects but prefer to speak of structures for epistemic reasons, eliminative ontic structural realism (as opposed to the moderate ontic structural realism of Esfeld and Lam) must give up the idea of "hidden entities" but as they see it fundamentally fails to do so. The core of Arenhart and Bueno's argument which we intend to address here is contained in the following section of their paper:

> "It is well known what set-theoretic structures are and how they are constructed: they can be characterized as ordered pairs […] consisting of a domain of objects and a family of relations among those objects, all of which are found in the set-theoretic hierarchy […]. Relations are then defined in terms of the objects that belong to the domain, and not the other way around.

> Given a structure, the existence of relations, as particular sets, depends on the existence of the elements of the domain: without the objects in D there would be no relations, and, hence, no structure in the set-theoretic sense. […] Thus, objects are basic in set theory […] However, […] objects are not allowed as primary entities in ontic structural realism. So, if the structural realist's characterization of structures is implemented in terms of set theory, some maneuver needs to be adopted to defuse the resulting commitment to objects." (Arenhart and Bueno, 2015, p.114)

To generate an intuition about the problem with this line of reasoning we may imagine, at least temporarily, that mathematical platonism is true, i.e. that there truly exists such a thing as a space of mathematics in which the topics discussed by mathematicians are timelessly present. Let us further get rid of the "physical world" insofar as this would be possible, consider the mathematical platonist's world to be the only thing worth the term "existence" for the purposes of this thought experiment. What are we to make of such a world in terms of the fundamentals being objects or structures? Evidently we would at least to some degree see sets, functions and numbers in the platonistic world so we might be inclined to state that in such a universe objects would exist in a truly fundamental way which cannot be reduced to structure. On the other hand, we may oppositely take note that the only way the objects can coherently be said to gain identity is through their relations to other objects in the platonist's universe. The number "2" gains its properties through its relationship to all the rest of the mathematical universe, it is fundamentally incoherent to imagine a platonist's universe in which only the number "2" solemnly exists without any other object or relation being present, for otherwise the number "2" would not have its properties and would hence not be able to be considered "2" instead of being considered any other object or number. In the language of Esfeld and Lam, the number "2" lacks intrinsic identifying properties that it has on its own which are not obtained through its relations with the other numbers and the structures of relations it is embedded in. Since it lacks such intrinsic identifying characteristics the only way identity can be conceived of in this thought experiment is to have relations and entities be co-dependent – they are not separable ontologically without reducing them to utter trivialities.

As Arenhart and Bueno argue, the notion of structure becomes difficult if not impossible to comprehend without some fundamental objects that share relations with each other – *relations without relata* edges very closely to a statement of absurdity. So even in this thought experiment of realism about mathematics, where we imagine mathematical platonism to not only be true but to be the only truth, we cannot reasonably try to get rid of objects nor get rid of structures without eliminating the entire universe or reducing it to a triviality. Objects *require* structures and relations to gain identity and structures and relations *require* objects and relata or else they are empty at best. The problem with arguing that objects are not allowed to occur within structural realism, and this also applies to many forms of ontic structural realism specifically, is that they inherently *have* to occur in it. In other words, this is not a bug, it is a feature without which the truth of structural realism would lead to an empty world which we evidently do not reside in. *Eliminative* ontic structural realism has to provide answers to these difficult questions but they cause no problems for ontic structural realism along the lines of Esfeld and Lam's moderate structural realism.

We can give another more visual intuition for the relationship between entities and relationships in such a moderate ontic structural realism: Objects and structures are analogous to the *apparent* two sides of a Möbius strip, one may be able to hold such a Möbius strip in hand and pinch it and thus touch "both sides" at once and have a sense of separation between them but upon further inspection these "sides" turn out to be inseparable and just one side after all. Being a realist about only one side of a Möbius strip immediately makes one a realist about the "other side" since there is not really an "other side", it is merely an illusion that one can exist without the other.

The continued philosophical focus on this distinction between ontic and epistemic structural realism partially obfuscates the actual point of why structural realism about science focuses on the structures instead of the objects. Whether as a matter of happenstance or a fundamental reason, structures are significantly more stable under alterations to the objects than objects are under alteration of the structure. This means that scientific theories will arrive at the appropriate structure sooner than they arrive at the appropriate object-based interpretation (if that is even something that is achievable). We can see this clearly within modern as well as historical scientific theories. While the objects may change as theories are improved and revised, the symmetries remain at the very least as limiting cases and approximations, for no theory will have as much predictive success as science does without hitting on *something* (i.e. the no miracles argument as discussed above). That particular something turns out to be the symmetries found in the theory which are conserved (at least in limiting cases) whereas objects are generally not conserved across theory change. The details of this claim are left for discussion and explanation in section 3.

Above we stated that this might be pure happenstance but there are reasons to think this is a fundamental property that could not be otherwise (at least stochastically). If science were to get the objects completely right, then by the nature of what it takes to know and characterize an object and to define the identity of an object, it would require knowing its relational properties to the objects it stands in interactions with. However, it is absolutely possible to get the structure and symmetries of certain interactions approximately right while being wrong about the objects involved. It does not require detailed knowledge of electrons to observe that the electromagnetic force has some semblance of a spatial rotational symmetry that has to remain present at least as a limiting case no matter how much our understanding of the constituents of carrying charge or even the concept of charge itself change. Electrons can be particles, waves, both, neither or some quantum object that human brains fundamentally cannot comprehend intuitively but it would make no difference to the fact that the discovery of rotational symmetry in space with regards to the electromagnetic force is a property of the universe that remains persistent no matter which interpretation of a particular charge carrier is the correct one. What we know from empirical science is that at least one element of the class of rotationally symmetric structures exists in the universe (among many other known properties). Structure can be preserved more easily upon object changes than objects upon structure changes. It is important to stress again that this does not mean structures do not require objects.

As mentioned above, the version of ontic structural realism that is being defended in this thesis thus bears a striking resemblance to what Esfeld and Lam call "moderate structural realism" (Esfeld and Lam, 2011, 2006), which ranks relations and objects on similar scales and does not ontologically prioritize one over the other unlike the eliminative ontic structural realism that Arenhart and Bueno are correctly criticizing in the above cited passages. Under this view objects lack intrinsic identifying properties (but might still have intrinsic non-identifying properties) and are given such properties and identities precisely by their relations. Relations and objects are thus inseparable under this view but for the above-mentioned reasons it is reasonable to expect scientific theories to converge much faster to structurally accurate properties than object based ones.

2.7.2 Incommensurability between theories

One might be inclined to argue with Thomas Kuhn or Paul Feyerabend that any comparison of non-equivalent theories is doomed to fail before it is even attempted, since successive theories are fundamentally based on such different premises and results that even immediate successor theories become incommensurable. In this section we will briefly sketch why one might think this is the case and how structural realism and even realism in general can defuse this criticism. We will argue that this point of view in fact only holds any weight if one already starts from an anti-realist viewpoint and hence this Kuhn-inspired contention does not affect the realism and anti-realism debate.

Kuhn’s characterization of scientific revolutions in terms of so-called “paradigm shifts” (Kuhn, 1962) has had widely influential impacts on the perception of science among philosophers and scientists alike. He contended that as big scientific revolutions occur, such as the move from Newtonian mechanics to relativity or quantum mechanics, the language of the old theory and the new theory, while they may appear superficially similar, are in fact incommensurable in the sense that they refer to completely different concepts, value completely different things as good science and consider very different approaches to particular problems. It is often said that a “world-change” occurs within such a Kuhnian framework, meaning that for example the world as seen by the Newtonian physicist is a wholly different one compared to that of the Einsteinian leading to insurmountable communication problems:

> “One of the most controversial claims to emerge from Kuhn's assertions about the incommensurability of scientific theories is that the proponents of different paradigms work in different worlds. Drawing on experiments in the psychology of perception, Kuhn argued that the rigorous training required for admittance to a paradigm conditions scientists’ reactions, expectations and beliefs, so that learning how to apply the concepts of a theory to solve exemplary

> problems determines scientists' experiences. So for example, where a proponent of the Newtonian theory sees a pendulum, an Aristotelian saw constrained free fall; where Priestley saw dephlogisticated air, Lavoisier saw oxygen; where Berthollet saw a compound that could vary in proportion, Proust saw only a physical mixture." (Oberheim and Hoyningen-Huene, 2016)

This may be raised as a criticism of structural realism or realism in general since one could argue that not only is there no progression towards approximate truth in science but that this statement is in fact neither true nor false but instead completely meaningless as we cannot even compare the physics post-Einstein with the physics pre-Einstein in a meaningful way – the definition of science would have changed during that revolution and thus the theories are fundamentally incommensurable.

This kind of characterization could only appear sensible from the perspective of non-scientists. In practice, however, physicists not only *can* discuss concepts across theory borders but in fact regularly do so. Newtonian mechanics as a previously very mature field of physics never "went away" and is still being used for various purposes by physicists (and engineers), who also study and understand relativistic physics and quantum mechanics. To deny this is to deny the reality of scientific day-to-day practice.

While the properties of many systems may change from the view of different physical theories, they are absolutely comparable and in fact often *are* compared. Beyond that, the notion that there is no continuity between the physics theories of the past is precisely what many anti-realist views *contend* and thus cannot be the starting point for an argument against realism. The incommensurability of theories is a claim rather than an argument and it is a claim that fails to explain how it is possible for physicists to be experts in both relativity and quantum mechanics in present day which are incompatible theories precisely because we *can* compare them, understand similarities and differences and see where the issues arise when attempting unification.

Lastly, it should be noted that the claim of world-change is in many ways analogous to the strong versions of the Sapir–Whorf hypothesis of linguistic relativity, which holds that the perception of the world is altered to such a strong degree by the language used that it is not possible to distinguish what is being said about something from how it is being said, i.e. the language framework being used is said to determine thoughts and perceptions rather than the other way around (Scholz et al., 2016). Under a language change (read: theory change) then, the strong Sapir–Whorf hypothesis would hold that the world according to scientists would shift dramatically and indeed since there is no shared language the theories would be incommensurable in this sense. This strong variation of the Sapir–Whorf hypothesis has however been rejected empirically more than once (Berlin and Kay, 1999; Rosch, 1973) and does not hold up to skeptical scrutiny.

Humans can communicate with little effort across such language barriers just as scientists can compare and contrast theories and even talk about similarities and the particular ways in which they can be thought to be analogous. Physics theories are not a priori incommensurable and in fact are evidently a posteriori comparable as demonstrated by the reality of scientists readily doing so and having done so both during and after all “scientific revolutions”.

2.7.3 The arbitrary nature of embeddings

Much work has been done in the philosophical literature to understand and define under which circumstances one is able to speak of one theory being *embedded* into another. The discussions on this subjects are often, but not always, given within the framework of model theory which is not the chosen framework for the present thesis. Aspects of model theory are nevertheless useful as they provide one example of a possible formalized framework. Elements of model theoretic language will be used in this section to give an adequate characterization of some of the problems with the concept of theory embedding and how they may or may not affect structuralism and structural realism. In this section we discuss the criticism of theory embeddings beginning with van Fraassen’s influential characterization in “The Scientific Image” (van Fraassen, 1980) and sketch its echo in modern day discussions on structuralism in science. We will return to the questions raised here in section 5, once the main proposition of this thesis for a characterization of structure in physics has been given in section 3.

The reason to think about the formal requirements of theory embeddings in the context of structural realism is that it requires there to be a certain sense in which the mathematical structure of one physics theory can be retained across theory change at least in some proper limiting case. One way to attempt to characterize such a limiting procedure is to think about embedding one theory into another but there are some immediate problems from the point of view of mathematics as well as physics which should be addressed: If embeddings are taken in the mathematical sense (i.e. theories are translated into the language of model theory), then isomorphisms or homomorphisms are certainly the wrong way to go about theory embeddings. This is evident since there is no coherent sense in which Newtonian mechanics can be considered a mathematical subset of quantum mechanics and neither is Newtonian mechanics a subset of special relativity and in fact it is not even isomorphic to a subset of these theories. When making the move from quantum mechanics or special relativity back to Newtonian mechanics we introduce slight, very slight errors that are negligible in the particular chosen limit (often not even within the realm of measurable deviations). Nevertheless, the fact that such errors exist in the theory, however minimal or unmeasurable they may be, means that they are not in the trivial sense subsets of another without first applying a certain kind of "dropping of negligible higher order terms" which typically is done in the form of a mathematical limiting procedure. Even more striking, perhaps, is the existence of incompatible statements outside of the domains of validity of a particular physics theory, such as the possibility of above light speed velocities in Newtonian mechanics while this is fundamentally not possible in special relativity. Since successor theories and predecessor theories are thus clearly not trivial subsets of another (nor are isomorphic to such a subset) the notion of a mathematical embedding for the purposes of structural realism may seem doomed from the start.

But perhaps there exists a way to embed a theory into another without having to rigidly retain all of its exact truths and thus neither the tiny error discrepancies within the domains of validity nor the very absolute and large discrepancies outside of these domains of the theories would cause the embedding to fail. This is essentially the task which Bas van Fraassen sets out to address in "The Scientific Image" (van Fraassen, 1980), although he does not do this for the sake of structural realism or any realism but for structuralism. Since structural realism is predicated on structuralism being true, i.e. there actually *being* a sensible structure in scientific theories in the first place, the arguments here still carry weight for the structural realism debate.

Van Fraassen defines the so-called 7-point-geometry with the intention of using its relationship to the commonly known Euclidean geometry as an analogy for structural embeddings of scientific theories under a semantic rather than syntactic view of the nature of scientific theories. He defines a particular geometrical structure using the following set of axioms:

> A_0: There is at least one line.
>
> A_1: For any two lines, there is at most one point that lies on both.
>
> A_2: For any two points, there is exactly one line that lies on both.
>
> A_3: On every line, there lie at least two points.
>
> A_4: There are only finitely many points. (van Fraassen, 1980, p.41)

Using these axioms, van Fraassen draws a diagram of what one example of an instantiation of this axiom system might look like, this is his 7-point-geometry. We reproduce a comparable image in Figure 1.

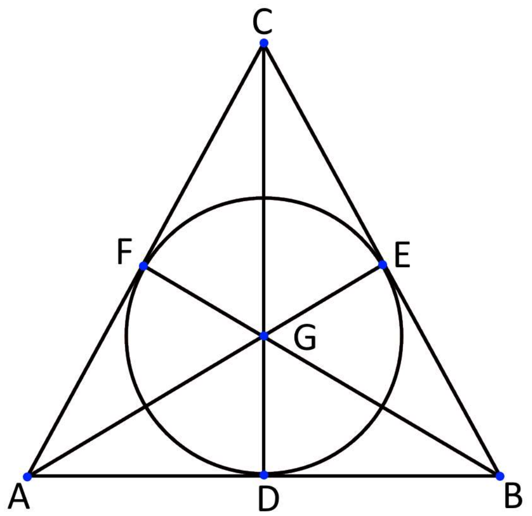

Figure 1: Visual representation of van Fraassen's 7-point-geometry embedded, as he says, in Euclidean geometry. (van Fraassen, 1980, p.42)

With regards to this figure he notes:

> "[…] [Y]ou will have noticed that I drew a Euclidean triangle to convey what the Seven Point Geometry looks like. For that seven-point structure can be embedded in a Euclidean structure. We say that one structure can be embedded in another, if the first is isomorphic to a part

> (substructure) of the second. Isomorphism is of course total identity of structure and is a limiting case of embeddability: if two structures are isomorphic then each can be embedded in the other. The seven-point geometry is isomorphic to a certain Euclidean plane figure, or in other words, it can be embedded in the Euclidean plane." (van Fraassen, 1980, p.43)

Van Fraassen may be considered guilty of omitting relevant details and limitations in his characterization of this embedding of the 7-point-geometry into Euclidean geometry as part of this mathematical analogy. For one, it is not at all clear how he selects the isomorphism in question. Halvorson elucidates this problem well:

> "What does it mean to say that the seven-point model is embeddable in the Euclidean model? What is the definition of "embedding" that is being used? Obviously, an embedding cannot be just any function; for example, the function that maps everything to a single point is not an embedding. Similarly, an embedding cannot simply be a one-to-one map because such maps could also mess up geometrical relations." (Halvorson, 2012, p.199)

The particular embedding is thus wholly non-unique and no sensible way to select one embedding as opposed to others exists within this purely formal framework with no empirical input. The concept of embeddability is more clear since it merely requires the existence of an embedding but such a concept of embeddability is insufficient for the purposes of a structural realist account of scientific theory change and we will thus not pursue mere embeddability further – we are looking for a concrete embedding and a more concrete formulation of structure after all.

Beyond the problem of selecting which isomorphism to choose, it is not even clear how one should think about this embedding: The axioms of the 7-point-geometry are not true in the Euclidean plane and there is in fact no subset of the Euclidean plane in which the axioms can be made true unless one is fluid with how one uses the term "line" and "point" (something that van Fraassen is explicitly willing to do). Furthermore, and this is the crucial point, if this map however it may be chosen does not actually reflect properties of the Euclidean plane itself but merely serves as a visual representation of one structure within the confines of another, then the process that is being describes is that of *representation* rather than that of embedding. We do not wish to merely represent Newtonian theory within relativity, however, we wish to demonstrate that Newtonian theory arises as a special case of relativity under a particular limiting procedure. This purely mathematical or model theoretic concept of embedding does not seem suited for this purpose.

Additionally, there is not even an a priori reason to believe that the way in which structure is retained across theory change is always the same and so the search for a consistent and future proof formalization of such a limiting process (or embedding process) in the language of model theory is quite possibly based on an empty hope in the first place. The particular way in which quantum mechanics retains Newtonian structure in a limiting case and the way in which special relativity retains Newtonian structure in a limiting case does not at first glance appear to be the same *type* of retainment or embedding. Being faced with the lack of uniqueness of such embeddings we are thus looking for some non-trivial and while perhaps not fully unique still *sufficiently* distinguished ways to demonstrate that one theory and another share mathematical structure in the context of physics. It is the contention of this thesis that the philosophical literature, predominantly in the language of model theory, has focused too much on the mathematical aspects of physics theories and not enough on the *specific* formalisms used in theoretical physics which contain a significant amount of empirical input and motivation.

By stripping the empirical content from scientific theories and thinking about mathematical "theories" as a good enough analogy, such as van Fraassen did in the above sketched 7-point-geometry example, model theoretic discussion easily misses the clearly empirically distinguished embeddings and makes them appear arbitrary when they are in fact distinctly implied by the formalisms used in physics such as the Lagrangian or Hamiltonian formalisms. Once the characterization of physics structure has been given in section 3, we will briefly return to the topic of the existence of distinguished embeddings in section 5 and argue how from the point of view of physics this problem does not arise.

2.7.4 What constitutes the relevant parts of theories?

Worrall himself touches on an issue on the intersection between the arbitrariness of embeddings and the incommensurability between theories when discussing the views of realists. To Worrall, it is apparent that the strong statements of some realists about ontological succession in scientific theory change cannot be considered true because in a strictly logical sense, if Newtonian mechanics were true then relativity would be wrong. While he acknowledges and in fact stresses that the mathematical equations of special and general relativity have mathematical limiting formalisms that yield Newton's equations, he challenges the notion of one theory being the extension of the other:

> "Professor Agazzi […] took the view that Newtonian physics remains true of objects in its intended domain and that quantum and relativistic physics are true of objects in quite different domains. But this position is surely untenable. Newton's theory was not about (its 'intended referent' was not) macroscopic objects moving with velocities small compared with that of light. It was about all material objects moving with any velocity you like. And that theory is wrong (or so we now think), […]. Moreover, it isn't even strictly speaking, right about certain bodies and certain motions and 'only' wrong when we are dealing with microscopic objects or bodies moving at very high velocities. If relativity and quantum theory are correct then Newton's theory's predictions about the motion of any body, even the most macroscopic and slowest moving, are strictly false. It's just that their falsity lies well within experimental error." (Worrall, 1989)

In Worrall's paper this particular paragraph is a mere footnote but it serves as a very important schematic for a potential criticism of the argument put forth in this thesis. A typical oversight of structuralism and structural realism is precisely the requirement of a domain of validity which represents the empirical content of the scientific theory. What we precisely mean when we speak of domains of validity will be discussed in detail in section 3.4. For now it suffices to think of the domain of validity of a particular theory simply as the subset of nature on which said theory has been predictively successful.

A scientific theory with an empty domain of validity is of course not a scientific theory at all but Newtonian mechanics' domain of validity is far from empty. Removing this empirical component of structural realism offers a potent counterargument to its opponents by making the notion of structural inheritance borderline impossible to define, since evidently Newtonian mechanics and relativity or quantum mechanics are only in particular limiting cases structurally similar or identical to the previous theories. The claim that *some* limit from one theory to another exists alone is not an impressive feat, since through all manners of correspondence and not otherwise justified limiting procedures one could obtain similar looking equations in degenerate scenarios – the reason *why* semi-classical limits are relevant and an important argument also in the philosophical sense is that they reproduce the older theories on the whole domain on which they have been proven to be empirically robust. Removing the argument of the domain of validity thus makes structural realism significantly weaker and gives it a feeling of artificiality since it would in this sense allow very absurd limiting behavior that would not respect the original theories experimental successes. Without empirical premises to provide boundary conditions on what should be considered as a proper embedding and what may "squeeze" theories too much to still be considered a sensible embedding structural realism becomes meaningless since all mathematical objects can in some sense be mapped to subsets of each other in some arbitrary way. We will introduce this concept of domains of validity properly in section 3.4.

For now we simply note that Worrall sees no conflict between his stance about domains of validity not being valid concepts of succession and his structural realism despite this giving odd loopholes to incredibly artificial concepts of structural inheritance. We return to this issue in section 5.

2.7.5 Ramsey sentences and Newman's objection

As previously stated, the uncritical application of highly formalized systems without appropriate mechanisms to account for the weight of empirical adequacy and novel predictive power within scientific theories is highly problematic but widespread. Nevertheless, there are of course things to gain and learn from a proper application of formal systems. In this section we will introduce one of the many formal approaches available to structural realists, Ramsey sentences and Ramseyfication, and then go on to describe an influential counterargument to structural realism that is based precisely on this formalism known as Newman's objection (see for example Ainsworth, 2009; Ladyman, 2016, section 3.2). We will also present a response to Newman's objection based on the argument put forth by Smithson (Smithson, 2017), revealing it as a mere artifact of the formalization scheme stripping away everything but the barebones logical structure with no account being given to what science is, does and can do. By only focusing on the formal structure, sight is lost on the powerful empirical nature of scientific theories and one is easily led to falsely believe that structural realist claims decay to simple or even trivial claims of cardinality about mathematical structure.

In order to understand Newman's objection we need to first introduce Ramsey sentences. Ramsey sentences are abstract second order logic sentences that can be generated from abstracted versions of scientific theories, the particular formalism often being attributed to Frank Ramsey (see Ramsey, 1990).

The concept of Ramsey sentences begins with a language of second order logic containing logical connectives (e.g. $\wedge$,$\vee$), quantifiers ($\forall, \exists$) as well as individual and predicate variables (e.g. x_1, x_2 and X_1, X_2) (compare Ainsworth, 2009). Additionally we require individual objects and particular relations or properties which we respectively label a_1, a_2 and A_1, A_2 and so on. These individual objects and relations are to be interpreted as the particulars of a given theory. For example, we can conceive of a particular property $A_1(a_1)$ which denotes that a_1 has property A_1, e.g. being charged or another property taking two inputs $A_2(a_1, a_2)$ denoting for example that a_1 and a_2 are atoms which attract each other. Under the presupposition of such a second order logic, a given theory is first expressed as a set of sentences (i.e. formulated with a syntactic view of scientific theories in mind) where observable and measurable relations and objects are labeled with the non-logical individual relation and object identifiers respectively and unobservable relations and unobservable objects of the theory are labeled with the corresponding logical variables. The Ramsey sentence is then obtained by taking all of the unobservable objects and relations and replacing them with a new logical variable and an existence quantifier over said new variable in front of the statement (if the a given object or relation occurs multiple times in a given sentence then they are of course replaced with the same new variable and the scope of the operators spans the entire sentence and thus all of the new variable's occurrences).

“Ramseyfication” is the term generally used to describe the process of taking a scientific theory and transforming it into its Ramsey sentence. It is necessary to say “transform” here instead of “translate”, because the word “translate” might suggest that this mapping is invertible in some sense when it is distinctly not – incompatible and different scientific theories can in principle have the same Ramsey sentence. Ramsey sentences are thus a logically much weaker form of the original scientific theory and it has been argued before that Ramsey sentences are what structural realists are or should actually be realists about since Ramsey sentences remove all of the unobservable entities and structures from the claims of the scientific theory and instead only speak of the existence of particular structures.

As an illustration of the Ramseyfication process, we give an example also discussed by Maxwell and later picked up again by Ainsworth and Smithson (Ainsworth, 2009; Maxwell, 1971; Smithson, 2017): Consider the logical sentence

$$\forall x\big[\big(A(x) \wedge D(x)\big) \Rightarrow \exists y C(y)\big],$$

which is taken to express a “toy theory” of sorts (an actual theory under a syntactic view of scientific theories would in fact be a set of such sentences). A concrete toy theory example for this sentence is that $A(x)$ and $D(x)$ refer to “x is a radium atom” and “x radioactively decays” respectively. $C(x)$ is the observational statement “x is a click in a measurement device setup properly to measure radioactive decay”, e.g. a click in a Geiger counter. Following the Ramseyfication guidelines above, the corresponding Ramsey sentence of this toy theory is

$$\exists X \exists Y \forall x\big[\big(X(x) \wedge Y(x)\big) \Rightarrow \exists y C(y)\big].$$

Before even looking at Newman’s objection an immediate objection to Ramseyfication as a tool for structural realism may come to mind: The fact that even the Ramsey sentence still clearly seems to think of object-structure relations as entity based, with relations acting on distinct objects rather than relations being part of what even defines the objects’ identities in the first place. Since we do not endorse an eliminative ontic structural realism that rejects entities altogether in this paper, this is however not an issue in urgent need of being addressed. It should nevertheless be noted and has been noted as a potentially problematic property of this formalization scheme (compare e.g. Smithson, 2017, p.995).

A further immediate disagreement might stem from the objection that scientific theories are not in fact sets of sentences at all (what is dubbed the “syntactic” view of scientific theories) but are rather contained in the semantics of the theoretical sentences. We have already seen aspects of this syntactic versus semantic view of theories in section 2.7.3 when discussing theory embeddings. Since most structural realists seem to favor a semantic interpretation of scientific theories, it might be seen as an odd starting point of any counterargument to structural realism to attack the process of Ramseyfication which distinctly demands a syntactically interpreted scientific theory (compare Smithson, 2017, section 2.2). Again, however, this will not keep us from still discussing Newman’s objection in part because in light of recent discussions around the syntactic and semantic view of theories and whether they are even as distinct as it is sometimes claimed we do not wish to make an exclusive choice among these two options.

Newman’s objection was originally raised against a version of realism that Russell endorsed (Newman, 1928) and was later rediscovered and applied to modern structural realism by Demopoulos and Friedman (Demopoulos and Friedman, 1985). It holds that Ramsey sentences are trivially true of any set with sufficient cardinality, meaning that if we are to reduce scientific theories to their Ramsey sentences before believing in their truth all that science would be doing is at best tell us about the cardinality of the universe. To see how Newman’s objection tries to make its case we look at a simple example due to Smithson (Smithson, 2017, p.996): Consider the following to be the Ramsey sentence of a given theory:

$$\exists X_1 \exists X_2 \exists x \exists y \exists z \exists t [X_1(x,z) \wedge X_1(x,y) \wedge X_2(z,x) \wedge (x \neq y) \wedge (y \neq z) \wedge (x \neq z) \wedge I_1(t)].$$

As we can see from the logical structure of this Ramsey sentence, the theory from which it would be derived includes one observable entity t with observable property I_1 as well as three distinct unobservable entities x, y, z and two unobservable relations between two entities X_1 and X_2. More precisely, the unobservable relation X_1 applies to the set of tuples $(x,z),(x,y)$ and the unobservable relation X_1 applies to the set of tuples (z, x). This is where Newman's objection becomes tangible: The existence of such relationships X_1 and X_2 is trivial in the sense that *any* set of objects with sufficient cardinality, i.e. in this case any set with at least three objects in it, will automatically satisfy this statement about the existence of such a relation by simply "putting the unobservable entities into ordered tuples in the appropriate way" (Smithson, 2017, p.997). Thus, if a structural realist were to claim that the Ramsey sentences of a theory include all of the actual content of a theory that we are supposed to take a realist attitude towards then that kind of structural realism would be empty of any meaningful statements about the universe except perhaps about the minimal cardinality of the objects in it. As noted by Smithson, however, this is not quite right: Since the Ramsey sentence still includes all of the observable sentences within it, it still technically contains more than the minimal cardinality of the universe but may still be accused of adding only trivialities to the observables:

> "For example, [the above example] says that some entity *t* instantiates the observable property I_1. It may sound strange to label the Ramsey sentence ''trivial'' given that it captures all of this empirical content, but one should remember that even a scientific anti-realist (such as a constructive empiricist) will agree that the observable consequences of the original theory are true. What separates the structural realist from the anti-realist is the further claim that there are certain extensionally-characterized relations that are instantiated in the unobservable world. Newman's charge is that this further claim is trivial […]" (Smithson, 2017, p.997)

In fact Ketland formally shows that "the truth of a Ramsey sentence is equivalent to a sort of combination of empirical adequacy and a Newman-esque cardinality constraint" (Ketland, 2004). He concludes that:

> Indeed, the 'structural content' of a theory […], at least if it is identified with what [its Ramsey sentence] 'adds' to the claim that [the theory] is empirically adequate, is just this Newman-esque cardinality constraint. (Ketland, 2004)

How can a structural realist respond to this criticism, since obviously structural realism was not intended to look at the unobservable aspects of physics as a trivial assessment about the number of things in the universe? Above we have already outlined a few issues one might have with the claim that Ramsey sentences are all that useful or important to structural realism in the first place but as stated we will not further pursue those arguments, instead favoring a more universal approach outlined by (Smithson,

2017) which leans on the no miracles argument. Since the no miracles argument is more or less a universal aspect of most forms of realism as well as structural realism, its use for the purpose of the defense of the consistency of structural realism requires no additional argumentative baggage which might come along with other counterarguments.

Smithson's response built on the no miracles argument has two components: First he argues that the origin of structural realism in the no miracles argument inherently means it is not a trivial position about observables and the cardinality of the universe, meaning that if Ramsey sentences suffer from this problem then that is a problem of Ramsey sentences and not structural realism. Secondly, Smithson offers a way to amend Ramsey sentences so that the resulting sentence properly specifies the epistemic commitments of the structural realist "so as to reflect her full epistemic commitments" (Smithson, 2017, p.1013).

With regards to the first part of his argument, Smithson notes that:

> "[…] if a theory was merely identifying structure involving relations of the type we already knew to be automatically instantiated anyway (so long as the world has a certain cardinality), how could its identifying that structure ever explain the theory's novel predictive success?" (Smithson, 2017, p.1004)

In other words, Smithson argues that trivial relations cannot satisfy what the no miracles argument is arguing for, so acceptance of the no miracle argument (as is the rule for structural realists) and adoption of a structural realism based on it is inherently non-trivial and not merely a statement about cardinality. If a given formalization yields trivial relations it is a problem of the particular formalization and not of the grounds on which structural realism is motivated and built. This notion is in fact also hinted at in the quote of Ketland above, namely that the Newman cardinality constraint is all the structure of a theory is "at least if [its structure is] identified with what [its Ramsey sentence adds]". This is a big if and since the no miracles argument can be used to sensibly argue against the conclusion of a valid formal logical argument then the premises of the formal argument are what must be discarded. In this case the premise that leads to the conclusion of triviality is that the Ramsey sentence entirely and accurately characterizes a theory's structure.

As for the second part of his argument, Smithson's proposal for an amended Ramsey sentence goes as follows:

> "[…] I will introduce a new one-place predicate N (''the [no miracles argument] predicate'') to both the first-order and second-order languages. The predicate is interpreted as follows: "Nx: x is such that the [no miracles argument] provides (direct) evidence for x." The rules for amending the Ramsey sentence with the N −predicate are very simple. Let $\forall \mathrm{x} \ldots \exists \mathrm{y} \ldots (\mathrm{P})$ be a sentence in prenex normal form from the original theory, where P is an expression involving Ramsified predicates. The first step is to amend each sentence of this form to $\forall \mathrm{x} \ldots \exists \mathrm{y} \ldots (\mathrm{P} \land N[P])$. The second step is to Ramsify the theory as normal, leaving the N −predicate interpreted." (Smithson, 2017, p.1005)

Smithson then goes on to argue that this solves the triviality of Ramsey sentences since it accounts both for the no miracles argument and the existence of unobservable relations. We will not delve too deeply into Smithson's proposed amendment of Ramsey sentences, for a complete exposition and defense of his account and whether or not his modified sentences adequately capture structural realism we refer to Smithson's original paper, as discussing this in detail would go beyond the scope of this thesis. Suffice it to say, however, that regardless of whether the amending of Ramsey sentences with the no miracles argument works and is fruitful or not, Newman's objection is still not a killing blow to structural realism since the issue itself arises because of the adoption of an inadequate formalized system as Smithson's no miracles based argument shows.

2.7.6 What even *is* structure?

Many of the above criticisms and also many of those that were left out for the sake of brevity share a common denominator: They all express confusion or criticism about the nature of *structure* within structural realism. Is structure intended to be a mathematical thing or perhaps a model theoretic one? What is the sense in which whatever concept of structure we are talking about is retained across theory change and what non-trivial set of shared premises exists to somewhat objectively determine whether such structure preservation has occurred?

Within the fairly mathematical science of physics at least, we contend that a sensible account of structure can be given for which structure preservation is evident from the semi-formal, semi-empirical framework of physics. The claim is that the focus on the semi-formal aspect of physics has distanced philosophical discourse too much from the actual practice of physicists to see the relevance of the semi-empirical aspects which allow for the selection of particular embeddings without the need for special pleading.

The section which follows contains the main argument of this thesis and will focus on introducing the relevant concepts one after another before bringing them together for the purposes of structural realism. We return to many of the above mentioned criticisms of structural realism to provide more specific answers to them in section 5.

3 CHARACTERIZING THE STRUCTURE OF PHYSICS THEORIES

3.1 THE RELEVANT CONCEPT OF SYMMETRY IN PHYSICS

Symmetries and specifically symmetry groups play a fundamental role in various fields of modern theoretical physics. Through the famous Noether theorem which states that all symmetries of physics have a corresponding conserved physical quantity one finds an inherent connection between statements about symmetries and statements about conservation properties such as the conservation of energy and time translation symmetry or the conservation of momentum and spatial translation symmetry. The Noether theorem thus marks the rigorous mathematical basis for a physicist's interest in symmetries. This is especially important in the field of particle physics, where one often lacks a complete mathematical theory of the system one wants to study but through the clever utilization of symmetries is still able to perform computations and simulations with significant predictive power. The concept of symmetries in the context of physics has also received some attention within philosophy of science and philosophy of physics, for example in Baker, 2010; Barrett, 2017 and Caulton, 2015.

Symmetries have played a major role in mathematics and by extension physics from the very beginning of the fields but were not thought of as a critical component of direct research within physics until the discoveries of "uncomfortable" violations of previously believed to be unconditional symmetries in particle physics experiments of the 20^{th} (in the language of the later sections of this paper, this caused a decrease in the size of the domain of validity of classical mechanics).

The present section's goal is to introduce the proper concept of symmetry that will regularly be referred to in the argument for structural realism. Many different and in fact occasionally mutually exclusive concepts of symmetry exist in physics and mathematics, so making sure that one agrees on which concept one is talking about is crucial. For example, in the physicist's language the revelation that the universe is not invariant under parity transformations (meaning that the universe does not have mirror symmetry) was shocking but in the conventional understanding of symmetry, it is a triviality that the universe is not mirror symmetric since after all, not even humans or the writing written on this very paper are invariant under mirroring in the sense that for example a perfect sphere would be. The reason physicists were shocked, whereas non-physicists may shrug about the lack of mirror symmetry of the universe is that they are not using the word symmetry in the same way. In order to understand what kind of symmetry defines the structure preserved in structural realism with regards to physics, we need to understand the physicist's conception of symmetry.

We will need clear terminology to distinguish these concepts of symmetry. We will call the concept of the non-physicist described above manifest symmetry. *Manifest symmetry* is the kind of symmetry where a given thing is transformed in some way but maps to itself under the transformation and leaves all of itself invariant. Examples of such manifest symmetries are rotating a circle by any angle desired or rotating a square by 90° or 180° and so on.

Naturally physicists also know of manifest symmetry and they even often exploit it when solving various problems such as engineering problems with cylindrical tubes or the motion of billiard balls to make the computations significantly easier. However, there is another fundamentally important concept of symmetry in physics, which regards the solution space of a theory (this term will receive a proper characterization in the coming sections) and is *not* a manifest symmetry. We will call this concept *dynamical symmetry* and give an example in which it occurs before characterizing it more abstractly once we have a better grasp on solution spaces.

Consider as the initial intuition producing example one of the simplest problems in Newtonian mechanics which is regularly posed to physics undergraduate students during their introductory classical mechanics classes: the dynamics of a ball throw. Given a particular set of initial conditions, such as angle with respect to the ground, knowledge of the strength of the gravitational field, knowledge of the mass of the ball and the force of the initial throw we can deterministically calculate the classical trajectory of the ball and make all kinds of very accurate predictions about its motion. The particular mathematical solution to this problem is not what we want to focus on, instead we wish to think about potential symmetries that such a solution includes.

Physicists will readily say that Newtonian mechanics has all kinds of symmetries, such as rotational symmetry, mirror symmetry and translation symmetry but intuitively at least in the manifest symmetry sense, such features are not present in the solution of a ball throw. There is a more fundamental concept of symmetry property in this process, however, namely that of *dynamical symmetry* of the solution space. We will focus on the example of mirror symmetry here, since it provides the most clear example of what is meant.

Evidently the universe (even before we knew about classical mechanics not being entirely accurate) is not mirror symmetric in the manifest sense – not even the writing on this sheet of paper or computer screen is invariant when mirrored in the way that a perfect circle is. Classical physics does *not* produce a manifest mirror symmetric universe but it *does* produce a *dynamical* mirror symmetric universe. What this means is that every process which is allowed by the physics equations governing the process in classical mechanics maps to another allowed process if we apply a mirror transformation (or "parity" transformation) to it. We may thus have also called this conception of symmetry *structural* symmetry or perhaps *legal* symmetry in reference to the fact that it is not being manifested in physical spacetime but rather a symmetry of the physical laws themselves. The colloquial phrase that "the laws of physics are the same everywhere in the universe" is nothing but a qualitative way of saying that the laws of physics as we know them have dynamical symmetry with regards to spatial translations and the colloquial phrase that "the laws of physics are the same now, in the past and in the future" is nothing but a qualitative way of saying that the laws of physics as we know them have dynamical symmetry with regards to time translations. The laws of physics as we know them do indeed have these particular spatial and time translation properties but it is a mistake of human intuition to think that they a priori *must* satisfy them – some such very intuitive dynamical symmetries have been shown to not hold without restrictions. Unfortunately, both of these concepts of symmetries are simply referred to as "symmetry" within physics itself and physicists are capable of distinguishing between the types of symmetry simply based on the particular context in which the word is used.

This extremely important difference between what we called manifest and dynamical symmetries in physics has been noted in the philosophical literature before as well, such as recently by David Baker who uses the same natural name to describe it:

> "The first step will be to make clear what symmetry signifies in physics. The word is used in a few different ways, but I'll be concerned here with *dynamical symmetries*. This concept admits an intuitive as well as a formal definition; the intuitive definition is: symmetries of a theory are transformations that preserve its laws." (Baker, 2010)

For the example of the ball throw above, consider the case where we throw a ball in a particular direction at a particular angle and have the experiment set up in a way that there is a large and clear planar mirror in which we can observe the mirrored process as in Figure 2.

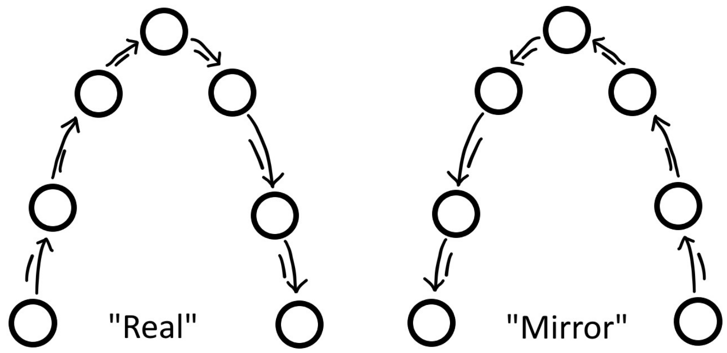

Figure 2: Example schematic of Newtonian ball throw in real world on the left and in mirror world on the right. The mirrored ball throw, while being a different particular solution to different initial conditions, would be equally allowed by the same laws of physics that describe the real ball throw. The classical universe and the mirror image of the classical universe follow the same physical laws.

The question now becomes if the mirror image, tentatively referred to as the "ball in the mirror world", follows classical mechanics as well or if some strange effects occur that mean that the mirror physics is fundamentally different from the real world. It turns out that (in classical mechanics but not in modern quantum field theory!) no such strange effect occurs: Every classical ball throw trajectory that we look at in the mirror can be described using exactly the same set of physical laws and it will look exactly like a "legal" ball throw would look in our own universe.

Perhaps this may appear as a triviality to some readers because the implicit assumption in their mind is that the universe for sure must be invariant under such mirror transformations, translations and rotations but it turns out that quantum field theory experiments show very readily that the universe breaks mirror or "parity" symmetry all the time, i.e. there is extremely strong empirical evidence that the "universe in the mirror" does not follow the same physical laws as our own. The important intuition to take away from this is that physics does not merely answer how a particular process will play out but that there are general solutions to how a *kind* of problem plays out (and appropriate mathematical formalisms to describe them). Using various kinds of transformations on these physical laws we can check if the resulting transformed physical system would still follow the same original laws of physics, even if the process of course will in general not be the *same* (i.e. there will not be *manifest* symmetry).

Getting slightly ahead of ourselves, to try and test if the physicist's concept of symmetry can be of use to the argument for or against structural realism one might ask "Are dynamical symmetries retained across theory changes in physics?". The answer to this question, at least in this naïve and direct sense, is *no*. Dynamical symmetries are *not* retained across theory change and the easiest way to see this is to work through a historical example where this was the case: the above-mentioned parity or mirror symmetry of the universe (in the dynamical symmetry sense, not the manifest one) provides one such adequate example.

Intuitively and without much of the mathematical baggage, parity symmetry as introduced above simply means that when one looked at the universe in a mirror, there would be no law of physics that would be violated in said mirror image world – there would be no physical law or physical property that would allow even the most knowledgeable and skilled of scientists to determine whether she is located within the mirror world or in the 'real' world - that is, if parity was an actual symmetry of nature. The belief that this was the case was practically undoubted, founded largely on the classical Newtonian intuition that had been so successful previously. The notion of a physical process that is allowed but whose mirror image is not allowed may have even seemed abhorrent to many. Nevertheless, as is the nature of science's continuous progress and self-testing, in the 20th century experiments were run on certain particle systems which would demonstrate that the laws of physics break parity symmetry, or in other words: Surprisingly, there *does* seem to be a way for a scientist to distinguish the mirror world from the real world through particular physical processes and this property of breaking parity would even become a defining feature of an entire mode of physical interaction – the so-called weak nuclear force.

The first thoughts that the natural world does not respect parity symmetry and thus is not ambidextrous came in 1956. Lee and Yang realized that there had so far not been experimental verification of parity symmetry in the case of the weak interaction, one of the four fundamental interactions of nature, though evidence for the parity of the strong and electromagnetic interaction were ubiquitous (Lee and Yang, 1956). The thus inspired quite sensitive and difficult to set-up experiment conducted by Wu et al.

would shake the particle physics community and cause significant refocus of both theoretical and experimental research (Wu et al., 1957).

The weak interaction is what causes the so-called beta decay, the process where a neutron becomes a proton (or a proton becomes a neutron) under emission of a beta particle, which is essentially an electron (or a positron in the other case), and an antineutrino of corresponding type. Yang, Lee and Wu set out to test the parity symmetry of the weak interaction by checking the results of such beta decays.

The chemical element Cobalt 60 undergoes such a beta decay, namely:

$$^{60}\mathrm{Co} \rightarrow {}^{60}\mathrm{Ni} + \mathrm{e}^{-} + \bar{\nu}_{\mathrm{e}^{-}}.$$

After aligning the spins of Cobalt 60 atoms to point in a particular reference direction, Wu observed in which direction the electrons from beta decay were emitted. To everyone's surprise the result of the experiments was that predominantly the electrons were emitted in the opposite direction of the direction in which the nuclear spin had been aligned. This demonstrated for the first time that the weak interaction broke parity, because in "the mirror world" the pseudo-vector of the nuclear spin would flip its direction since the rotation would now occur in the opposite direction but the emission of the electrons would still occur in the same direction, meaning that in the mirror world the electrons would predominantly be emitted *in* the direction of nuclear spin as opposed to in the opposite direction as in the experiment. Figure 3 shows a sketch illustrating how this experiment demonstrates broken parity by emitting an electron from the designated 'north pole' in the mirror world as opposed to the actually observed emission from the 'south pole' in the real world. This should be compared to the previously discussed example of Newtonian physics in Figure 3.

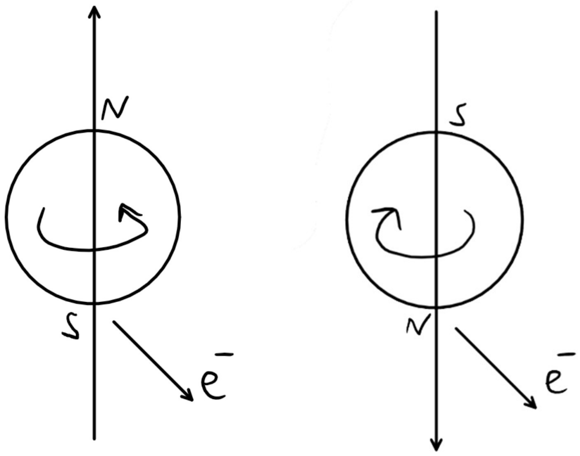

Figure 3: Sketch of how experiments showed parity violation by emitting an electron from the designated 'north pole' of a particle in the mirror world (on right) as opposed to the actually observed emission from the 'south pole' in the real world (on left). This means the mirror and real world follow somewhat different laws. Mirror symmetry is thus not a dynamical symmetry of the physical world.

This well documented historical example clearly and undoubtedly shows that dynamical symmetry is *not* preserved across scientific theory change in physics, at least not in this direct and naïve sense. Parity symmetry is a part of Newtonian physics but cannot be a symmetry of quantum field theory. This example is taken from (Griffiths, 2011) and we refer the relevant sections in that book for further details.

Nevertheless, using an appropriate concept of domains of validity as well as mathematical limiting procedures, we will show that in a still very important sense dynamical symmetries *are* in fact retained across theory changes on specific domains. This will form the groundwork for the symmetry based argument for structural realism.

3.2 THE LAGRANGIAN FORMALISM AND SOLUTION SPACES

Before moving on to discussing domains of validity and presenting the symmetry based argument for structural realism, we will discuss the Lagrangian formalism of physics as a concrete mathematical background on which we can do formal structural analysis regarding the above introduced concept of dynamical symmetry. The reason this is the chosen framework is that the Lagrangian formalism, while still being first principle based, requires empirical input in order to make any meaningful statements and is the foundation of most of modern physics. We have no reason to step too deeply into meta-theory frameworks like model theory when the actual theories themselves already allow structural analyses and comparisons within a unified framework such as the Lagrangian formalism without requiring the doubtful matching of our abstract model theory system to the way real scientific theories are formulated. When asking physicists what they think the correct formal framework for the analysis of structures and symmetries in physics is, their answer will be the Lagrangian formalism and not model theory. This paper among other things is an attempt to take this answer seriously.

The non-physicist, non-mathematician reader may choose to only skim the mathematical bits of this section. The Lagrangian formalism is just one very convenient and very useful framework for the discussions of structure in physical theories since all known physical theories, even those devised *prior* to the invention of Lagrangian formalism can be translated into it and analyzed from this vantage point.

As said, the Lagrangian formalism provides a framework from which the fundamental equations of a physical system can be derived. To understand it, we need three parts: The Lagrangian function itself, the concept of action and Hamilton's variational principle which allows one to move from the Lagrangian function and the action to the so-called Euler-Lagrange equations whose solution space are all physical processes which would be allowed in a world described by the chosen Lagrangian function. The goal of physics (or at least most of theoretical physics) thus reformulated turns into the search for the correct Lagrangian function, whose Euler-Lagrange equations are not only empirically adequate but manage to predict new phenomena. Often one also does the reverse and knows of certain phenomena from experiments or other observations and tries to find a Lagrangian function which contains said phenomena in its equations.

The Lagrangian function $\mathcal{L}(x, \varphi(x), \partial\varphi(x), \dots)$ in a certain sense that we will not need to delve into describes the energy and allowed interactions and couplings of a physical system. Depending entirely on the physical theory in question, the $\varphi, \partial\varphi$ and so on can have completely different interpretations ranging from classical particle coordinates or quantum particle fields to magnetic vector potentials in the case of electromagnetism – the universal power of this approach to describe physical systems is why it provides a suitable framework to discuss structures of physics theories.

The choice of $\mathcal{L}(x, \varphi(x), \partial\varphi(x), \dots)$ is where the empirical content of physical theories comes into play as it is a priori a completely free choice. The task as said is to find a Lagrangian function $\mathcal{L}$ which accurately captures the behavior of the natural world. Let's assume we have chosen such a Lagrangian function $\mathcal{L}(x, \varphi(x), \partial\varphi(x), \dots)$. We then define the action S of the physical system as the integral

$$S[\varphi] := \int_{\mathrm{M}} \mathcal{L}(x, \varphi(x), \partial\varphi(x), \dots) \mathrm{d}^{\mathrm{n}}x,$$

where the integral is over the spacetime manifold M. In the case of spacetime itself being part of the variation like in general relativity a further factor $\sqrt{-g}$ is part of this integration but this factor is simply 1 in all physical theories where spacetime is not dynamic so this does not affect the generality of the procedure. Note how the relevant variational variables or fields are written in the brackets after the action function *S*, indicating that those are the variables with regards to which we need to apply Hamilton's principle. Hamilton's principle is that the physics equations for a given Lagrangian function are obtained by minimizing the action, i.e. by demanding that

$$\frac{\delta S}{\delta \varphi} = 0.$$

The operator $\frac{\delta}{\delta\varphi}$ is a variational derivative - we will not delve into the specific mathematical meaning of how this acts on the action *S* because we would lose ourselves in calculus and variational calculus before getting anything done with regards to structural realism. For our purposes it suffices to note that it can be mathematically proven and beautifully derived, that given a Lagrangian function $\mathcal{L}(x, \varphi(x), \partial\varphi(x), \dots)$ and having constructed the action *S* as above, then using Hamilton's principle on said action one can derive the following system of equations (one for each generalized field φ) called the Euler-Lagrange equations:

$$\frac{\partial\mathcal{L}}{\partial\varphi} - \partial_\mu\left(\frac{\partial\mathcal{L}}{\partial(\partial_\mu\varphi)}\right) = 0,$$

where $\frac{\partial}{\partial\varphi}$ is the partial derivative with respect to the particular variational field and ∂_μ is derivative with respect to the μ-th spacetime coordinate.

This is a sketch of the Lagrangian formalism, not even attempting to contain proofs or in-depth mathematical descriptions. The aspects that matter here are how universal this formalism is and that for all of our established physics theories we have such a Lagrangian and thus a framework to compare these theories' structures. The elegance and power of this formalism cannot be overstated: The Euler-Lagrange equations above can with adequate choice of the Lagrangian function yield Newtonian mechanics, special or general relativity, non-relativistic quantum mechanics and even quantum field or string theory and more exotic non-commutative geometries if need be (Fliessbach, 2012; Ishibashi et al., 1997; Zwiebach, 2004). We can even describe entirely fictional toy model physics with this formalism and explore what a world based on such physics would look like in computer simulations.

The take-away here is that the physics of a system described by a particular Lagrangian function is contained in the Euler-Lagrange equations of the system. It is crucial to understand that the Euler-Lagrange equations are *not solutions of the physical system themselves* but that instead the solutions *of* the Euler-Lagrange equations define all of the physical processes which are allowed by a world governed by the given Lagrangian. We can now give a characterization of what we mean by solution spaces:

Definition (Solution spaces): Given a physical theory T in the language of the Lagrangian formalism, the solution space of T is the set of all solutions to its Euler-Lagrange equations. The solution spaces have a natural interpretation of all the physically allowed processes within the framework of theory T.

To summarize: The physics of a system described by a Lagrangian $\mathcal{L}$ is the solution space of the corresponding Euler-Lagrange equations. The Euler-Lagrange equations define what is physically possible and what is not - they contain in their solution space all of the physically possible processes (given that $\mathcal{L}$ is the appropriate Lagrangian).

Given this knowledge we can now return to the topic of structural realism and use this sketch of the Lagrangian formalism as part of an argument. The important realization here is that if the solution space of the Euler-Lagrange equations defines what is physically possible under a given theory then we can check, compare and try to define and understand the relationships between the solution space of one theory and the solution space of a successor theory. Even more importantly for our purposes, we can also check the solution spaces for dynamical symmetries in the sense of section 3.1. In fact, dynamical symmetries can now finally be given a proper abstract characterization:

Definition (Dynamical symmetry): We say that a mapping F defined on the solution space A of a given theory T is a dynamical symmetry of the theory T if for all solutions $\mathrm{d} \in \mathrm{A}$, we have that $F(d)$ is also a solution, i.e. $\forall d \in A: F(d) \in \mathrm{A}$.

The natural language interpretation of this definition is that a dynamical symmetry of a theory is an operation which when applied to a physically allowed process yields another physically allowed process. Transformations which take a given solution out of the solution space are not symmetries of the theory. We can also say:

Corollary: The dynamical symmetries of a given physical theory T are exactly the manifest symmetries of the theory's solution space.

We can now use this to better characterize the earlier examples of parity or mirror symmetry in classical mechanics and particle physics. The parity transformation, which takes a solution from solution space and produces its mirror image process by flipping the signs of the coordinates *does* yield another solution when applied to the solution space of classical mechanics but if it were applied to *some* elements of the solution space of particle physics (namely all of the solutions involving the weak interaction), we would obtain a physical process which is *not* part of this solution space. The universal quantifier is important here, since many solutions would indeed map to other solutions under parity but not all of them.

There are some further complications which can arise from this concept of dynamical symmetries. For example, we know of charge-conjugation-parity transformations on particular solutions of modern particle physics which would indeed yield another legal process but the two processes are not physically equal in that one occurs far more often than the other. We will not delve into such details and keep the definition of dynamical symmetry as above, leaving open the possibility of refining the term further to take such prevalence properties into account.

Discussions of this nature have partially appeared before in the philosophical literature. David Baker characterizes the concept of a "state space" as the set of all states of the universe allowed by the physical Lagrangian or Hamiltonian formalisms with a dynamical time evolution defined on it which makes it equivalent to the presently defined solution spaces:

> "Complete physical theories like Newtonian mechanics, relativity, and quantum mechanics can be formulated in a mathematical arena called a state space. We use that name because every element in state space stands for a physically possible (instantaneous) state of the world according to our theory. The experimental information we get from a theory comes in the form of predictions about how states will change over time. We call this account of temporal change a theory's dynamics; mathematically, the dynamics is sometimes represented by time-indexed transformations U(t') on state space that takes a state at time t = 0 to the state it will change into at time t'. So a theory's dynamics is a mapping from states to states. Transformations like rotations are also given by mappings T from states to states. Symmetries are then given by transformations that leave the dynamics […] unchanged. Mathematically, this means they must commute with the dynamics, so that U(t')T=TU(t') for every symmetry transformation T and every time t'." (Baker, 2010)

An important question to ask at this point is how we are to discuss the meaning of symmetries and approximating limits in physical theories without restricting science to a particular path of research (i.e. in this case that of the Lagrangian formalism)? If one were to ask a physicist how to accurately capture the symmetries and limit cases contained in physical theories in an abstract formal manner, they would likely also refer to the Hamiltonian or Lagrangian formalisms which are used in modern physics to deal with everything from classical mechanics to quantum field theory. Characterize symmetries as the symmetries of the solution space to the equations of motion for any given system originating from its Hamiltonian or Lagrangian formalism set of equations and you will likely find agreement with this procedure from particle physicists as well as engineers working with classical mechanics and even cosmologists and string theorists. However, while such an approach is possible (and this is what is presented here), we need to keep in mind that if as philosophers we start characterizing physical theories in this manner we run the risk of trying to shackle physics to these formalisms. The same physicists who would approvingly nod to philosophers using symmetries of the solution space to the Hamiltonian or Lagrangian formalism set of equations to characterize symmetries and structures in physics theories would disagree vehemently with the notion that any new physics would strictly have to follow and fit into this formalism.

While it *is* true that most future physics very likely *will* follow this formalism given its power and immense universality, restricting the options for future physics research in this manner would not be tenable nor acceptable from a philosophy of science framework. The question of scientific realism and anti-realism is ultimately a philosophical one, not a scientific one, and thus as argued above the answer to this question should only affect our philosophical approaches and perceptions and not those of science. Nevertheless, one needs a certain starting point to engage in a formal discussion of structure and since the Lagrangian mathematical formalism in particular is the universal language in which new physical theories are currently framed and all older physical theories can be translated into, we shall use this formalism as the starting point for the sake of keeping the formal framework of the discussion close to the actual science. However, any future physics will at the very least be based on *some* mathematical formalism which if it wants to help describe the universe will need to result in a solution space of physically allowed processes. This means that adapting the notions discussed in this paper to said hypothetical future framework should in principle be possible. To account for this we could alter the definition of solution spaces:

Definition (Solution spaces): Given a physical theory T in the language of the Lagrangian formalism (or a different mathematical formalism capable of all the things the Lagrangian formalism is presently used for), the solution space of the theory is the set of all solutions to the Euler-Lagrange equations (or equivalent fundamental equations derived from a potential future formalism). The solution spaces have a natural interpretation of all the physically allowed processes within the framework of theory T.

In his discussion on the different classes of symmetries of physical theories, Caulton (Caulton, 2015) also speaks of state spaces and dynamical symmetries in a similar fashion. First, he distinguishes between analytic and synthetic symmetries:

> To sum up, a theory's symmetries may be categorized as analytic and synthetic: the analytic symmetries do not generate a physical difference, while the synthetic symmetries do. This difference is of the utmost importance, since we glean information from it about which elements of the theory's formalism are physically representative represent, and which are not. Therefore, identifying a theory's analytic symmetries is bound up with identifying its physical content. (Caulton, 2015)

In addition, he discusses the distinction between what he calls variational symmetries and dynamical symmetries:

> The first type of symmetries are the variational symmetries, the group of transformations which preserve the Lagrangian function L. These will comprise both analytic symmetries and synthetic symmetries of the first kind. They cannot include synthetic symmetries of the second kind, since they are required to hold for all mathematical states, and thus all kinematically possible worlds. Whether they will comprise all of the analytic symmetries will depend on the details of the theory's interpretation, since L may or may not count as a physical quantity. (Caulton, 2015)

This concept might at first glance appear similar to the concept of dynamical symmetries introduced above but as Caulton himself notes, it is different in a subtle but important way. Let us first also note how Caulton's concept of dynamical symmetries matches the one we used in this paper:

> The second type of symmetries are the dynamical symmetries, which preserve the solutions of the dynamical equations. […] In systems subject to a variational treatment, this translates into preserving the fact that the Euler-Lagrange [equations hold]. [...] Clearly, therefore, the dynamical symmetries comprise all three kinds of symmetry. They include synthetic symmetries of the first and second kind, since their holding is restricted in terms of both quantities and states. The restriction of models to the dynamically possible worlds is surely one of the most salient for synthetic symmetries of the second kind. (Caulton, 2015)

The distinction between analytic and synthetic symmetries, while it can be made and has valid philosophical and physical uses does not matter to the concept of dynamical symmetries directly with the notable exception that dynamical symmetries happen to include both of these types. The distinction between variational and dynamical symmetries however is troublesome. Firstly, any transformation that leaves the Lagrangian of a given physical system invariant (and is thus what Caulton would call a variational symmetry) will inherently also have a directly corresponding transformation that leaves the Euler-Lagrange equations invariant if one defines a new transformation applying said variational symmetry to each occurrence of the Lagrangian in the Euler-Lagrange equations. This means that in fact variational symmetries can be distinguished *among* dynamical symmetries but not *from* them, as any variational symmetry is also a dynamical symmetry. The same does not necessarily hold in the opposite direction. As Caulton notes, dynamical symmetries also include the analytic symmetries that he speaks of since dynamical symmetries include any symmetry that remains within the solution or state space disregarding whether or not it creates an interpretational "physical" change in the world. In more than a technical sense even the trivial symmetry of applying an identity transformation to everything is a dynamical symmetry. So while these classes of symmetries Caulton discusses are intimately related to what is being discussed here, some of his distinctions come dangerously close to making distinctions for the sake of distinctions rather than their direct relevance for physics. In any case, these distinctions are subtleties which have little consequences for the argument presented here, so we will continue to refer to dynamical symmetries only since they play a crucial role in physics and are the broadest concept to which this argument can be applied.

3.3 THE CLASS OF THEORIES WITH NOVEL PREDICTIVE POWER

The idea of distinguishing theories based on their predictive power of *novel* results as opposed to their mere empirical adequacy with regards to already known results is not novel in itself and variations on such ideas have been discussed widely in the literature around scientific demarcation (e.g. Lakatos, 1970; Musgrave and Pigden, 2016, section 2.2; Popper, 1959; Thornton, 2017, section 7) as well as scientific realism (e.g. Alai, 2014; Barnes, 2008; Leconte, 2017; Leplin, 1997; Psillos, 1999, chapter 5; Segall, 2008). Leplin even bases his entire case for scientific realism on such a classification of theories with novel predictive power (Leplin, 1997). This is hardly surprising, since the no miracles argument is at its strongest when scientific theories made to explain a particular known but not understood phenomenon end up predicting an entirely unrelated and later verified one. This is also where Darwinian attempts to explain theory progress cannot fully account for why a theory developed under the selective pressure of particular experimental data should then also predict entirely unrelated and unknown phenomena unless *some* relevant elements of the structure of nature were adequately captured.

Solution spaces of physical theories and the concept of dynamical symmetries on solution spaces are two ingredients required for the intended argument for structural realism. While to some degree the Lagrangian formalism does include the empirical nature of science, it does so without a priori directly discriminating between useful theories and gibberish theories that describe nothing – it is scientists who have to do that given empirical information about the natural world through observation and experiments. The third ingredient is thus the concept of what we will call robust theories, which we will first define and then discuss.

Definition (Robust Theories): Given a scientific theory attempting to describe the physically allowed processes via its solution space, we will call it a *robust* scientific theory if it is empirically adequate *and* has been shown to be capable of significant predictions of previously unknown phenomena.

The very strong condition of empirical predictive power not merely of known phenomena (what we call empirical adequacy) but of previously unknown phenomena in the if condition of the above definition is not trivial, it is in fact essential in order for the claims of structural realism, whether epistemic or ontic, to not lend itself to a naïve reductio ad absurdum via all manners of historical scientific hypotheses that have eventually failed.

This is not truly a new addition as it has been present in the subtext of structural realism since its coinage by Worrall. In order for structural realism to defuse the pessimistic meta-induction and also incorporate the no miracles argument, it needs to refer to proper theory change or theory advancement and not merely to testing the waters via hypotheses by scientists. The no miracles argument speaks of the power of scientifically robust theories, not of the ideas and rough hypotheses that are still in the works and have not properly entered the playing field yet or even worse have already been discarded as dysfunctional.

Some philosophers may favor the label "mature" theories which is sometimes found in the literature but since the concept of "maturity" comes not only with linguistic connotations of age rather than power but is often also used to try and distinguish entire fields of science from each other (such as distinguishing the natural sciences from supposedly less mature sciences such as psychology), the label "robust" theories is preferred here.

Many things qualify for the above definition of robust theories such as most of the typical pivot points of philosophical discussion: classical Newtonian mechanics, Maxwell's electromagnetism, Einstein's special and general relativity and quantum mechanics as well as quantum field theory. Notably, currently in the works hypotheses such as string theory or super symmetry, which depending on what version one chooses will almost certainly be empirically adequate with regards to all or at least most present observations, do not make it to the status of a robust theory since neither has so far been able to produce observed novel phenomena with empirical backing.

It needs to be stressed that this definition is not intended as a tool for scientific demarcation – hypotheses and hypothesis building still have an immensely important role to play in scientific methodology and progress but barring empirical support we should probably abstain from even a preliminary judgement of ontology until they are either more fleshed out or discarded due to some empirical or theoretical failing.

3.4 DOMAINS OF VALIDITY

The last piece of the argument for structural realism based on symmetries of solution spaces relates to the domains of validity of particular theories. At a fundamental level, scientific theories stemming from the broad field of physics will include a *domain of* *validity*, i.e. the domain on which physicists believe the theory can yield accurate predictions and empirical adequacy. This domain of validity is not static throughout a theory's lifetime and may over time increase or more commonly decrease in size as our understanding and experimental knowledge increases. This last point is crucial for understanding why Worrall's argument discussed in section 2.7.4 does not work – it fails to appreciate that while most theories begin (and probably have to being) with a claim of a universal domain of validity, over the lifetime of a theory our understanding of its particular oddities improves and we begin to see areas where there is a discrepancy between the theoretical predictions of the theory and the empirical world. Thus, the domain of validity over time shrinks to only those areas in which predictions of the theory are highly accurate. This is an empirical process and at times it can be messy as science often is, hence it fundamentally cannot be seen if one only looks at scientific theories as fully abstract entities within a fully formalized framework unless perhaps if extreme care is taken to build this property into the formalization to begin with. Worrall realizes that certain not perfectly formalized aspects exist in scientific theories but in keeping those elements temporally rigid, he fails to account for the way physicists engage with their theories.

Newtonian physics, once thought to have the entire physical universe as its domain of validity, later turned out to be empirically adequate and strong in predictions only when dealing with the world at a macroscopic scale and sufficiently slow speeds as to be able to approximate the so-called speed of light as being infinitely large. Quantum mechanics is often seen as a theory which succeeded and replaced classical mechanics but classical mechanics is in fact, in a limiting case, obtainable from quantum mechanics. This means that the domain of validity of classical mechanics is contained in the domain of validity of quantum mechanics. Special relativity expanded the domain of validity in a different direction, namely in the direction where the speed of light is accurately understood to be finite. As Einstein introduced it, special relativity was incompatible with quantum mechanics. It took the later developments in quantum field theory (QFT) in order to obtain a theory that included all three of these: classical mechanics , quantum mechanics and special relativity as particular limiting cases of itself.

Like the previously discussed concepts, the idea of domains of validity which need to be established and can change over time is not unique to our argument here and has been discussed before in both the philosophical and the physics literature, sometimes even specifically under that name (e.g. in Gouesbet, 2014 and Holland, 1996). As Jennings puts it:

> "Although models cannot be proven correct, or strictly falsified, they can be judged and compared. Older theories are falsified, or to be more precise, they are shown to have a limited range of validity usually by a series of observations. The Michelson-Morley experiment on the speed of light in moving frames and experiments on atomic structure were inconsistent with the predictions of classical mechanics. This led respectively to special relativity and quantum mechanics. The models that replaced classical mechanics had greater predictive power, keeping the success of the previous models while also describing the new observations. Special Relativity has more predictive power than classical mechanics describing both slowly and quickly moving objects. Quantum mechanics replaced Newton's Laws because of its greater predictive power — it described microscopic as well as macroscopic systems. Following the Correspondence Principle as stated by Niels Bohr, quantum mechanics must reduce to and indeed does reduce to classical mechanics in those instances when classical mechanics provides a good description of the observations." (Jennings, 2006)

3.5 THE APPROXIMATE CONSERVATION OF SYMMETRIES AND STRUCTURE

Having introduced the four primary pieces of the symmetry argument for structural realism – (1) the concept of solution spaces of physical theories, (2) the concept of dynamical symmetries *on* such solution spaces, (3) the concept of robust scientific theories and (4) the applicability of different theories being restricted on particular domains of validity – we can present the claim of the structural realism pursued in this paper in a well-defined way:

Given a robust physical theory T_1, possessing empirical adequacy and predictive power on a domain of validity D_1, and one of its robust successor theories T_2, possessing empirical adequacy and predictive power on a domain of validity D_2 with $\mathrm{D}_1 \subseteq \mathrm{D}_2$, the correct way of thinking about the preservation of structure from T_1 to T_2 is to look at the symmetries of the solution space for the respective fundamental equations as well as the relationships between the solution spaces themselves. On the domain of validity $D_1 \subseteq D_2$, the theory T_2 will produce results predominantly compatible with

T_1 and in fact there will be a mathematical limiting procedure to move from D_2 to D_1 in which the fundamental equations from T_1 are reobtained.

This is the sense in which given a hypothetically perfect theory of everything at the end of such a potentially infinite theory chain T_n, we can still speak of $T_i, i = 1, \dots n - 1$ as approximately true in so far that they will limiting cases contained within T_n. By mathematical induction this means that the fundamental structures and symmetries observed even in the solution space of the initial first robust theory T_1 will be obtainable from T_n through successive limiting procedures when restricting the theory to a particular domain of applicability. This characterization of structure preservation, together with the no miracles argument and Worrall's initial argument for structural realism forms the argument for why it is reasonable to be a realist towards the structures and symmetries of physical theories, because they can be properly characterized and are in fact retained in limiting cases across theory advancement.

We need to stress here that theory progression does not need to be and in fact in reality is not linear and this is *not* what is being implied in the above argument. For example, classical mechanics branched out into special relativity and quantum mechanics in the way described above but for a while special relativity and quantum mechanics were valid on different domains. Theories can branch out in this way but the above will still hold either way. Physicists eventually managed to rejoin special relativity and quantum mechanics in quantum field theory but even in modern times, general relativity and quantum field theory span two different domains of validity while both still contain special relativity as one of their limiting cases. So the progression of theory should more accurately be thought of as a branching path that occasionally in moments of so-called theory unification manages to converge again:

$$T_1 \rightarrow \begin{array}{c} T_{2a} \rightarrow T_{2a'} \\ T_{2b} \end{array} \rightarrow T_3 \rightarrow T_4 \rightarrow \cdots \rightarrow T_n \ ,$$

rather than a completely linear view. Nevertheless, for each linear subbranch, which will show such linear behavior by construction, the above relationships will still hold.

4 EXAMPLES FROM PHYSICS

In this section we will perform a few relevant analyses of historical theory change to see if structure can be said to be retained across physics theories in the sense described above. The examples were chosen for their importance in both the scientific community and because these theory changes are typical points of contention among philosophers of science. In principle any theory change from one robust theory to another can be discussed in this manner. In all cases the analysis can be done very in depth in a mathematical sense and many additional physical parameters could be discussed. We will restrict ourselves to the mathematically simpler cases of the respective theories for illustrative purposes and give citations for those who wish to delve deeper into this well-developed subject matter.

4.1 THE CLASSICAL LIMIT OF SPECIAL RELATIVITY

We begin our brief look at the history of physics with the move from Newtonian or classical mechanics to Einstein's special relativity. General relativity is mathematically significantly more complicated and thus will not be discussed in detail, though the same kinds of arguments work for it as well with the exception that no unification exists for it and quantum field theory yet.

To start with the analysis we need to look at all of the four pieces discussed in section 3, beginning with the mathematical formalism which as stated we pick to be the Lagrangian formalism for the sake of analysis. The Lagrangian function for the simplest cases of Newtonian mechanics is

$$\mathcal{L}\big(t, x(t), \dot{x}(t)\big) = \mathrm{T} - \mathrm{V},$$

where T describes the system's kinetic energy and V describes the system's potential energy. That means in particular that for a completely free moving classical particle with mass m, with no potential being present $\mathrm{V} = 0$, the Lagrangian is just the kinetic energy of the free particle $\mathcal{L} = \mathrm{T} = \frac{m\dot{x}^2}{2}$. For n free particles and no potential being present $\mathrm{V} = 0$ the Lagrangian would be the kinetic energy of the system which is the sum over all the kinetic energies of the classical particles $\mathcal{L} = \mathrm{T} = \sum_n \frac{m_i \dot{x}^2}{2}$. These special case examples are given to illustrate the working procedure with the Lagrangian formalism, in principle this can be done for the general Newtonian mechanics as well.

The Euler-Lagrange equations in abstract form for this Lagrangian function turn into:

$$\frac{\partial \mathcal{L}}{\partial x} - \partial_t \left(\frac{\partial \mathcal{L}}{\partial \dot{x}}\right) = 0,$$

where of course we would obtain one such equation for each of the three spatial coordinates but we wish to keep things simple here so we will write only this one equation. Computing the derivatives of the Lagrangian function in the Euler-Lagrange equation explicitly turns the Euler-Lagrange equations into the fundamental equations of Newtonian mechanics. Let us do this for the case of the free particle $\mathcal{L} = \mathrm{T} = \frac{m\dot{x}^2}{2}$ for simplicity. Since

$$\frac{\partial \mathcal{L}}{\partial x} = 0\,,$$

and

$$\partial_t \left(\frac{\partial \mathcal{L}}{\partial \dot{x}}\right) = \partial_t(\mathrm{m}\dot{x}) = m\ddot{\mathrm{x}}\,,$$

we obtain for the Euler-Lagrange equations:

$$-m\ddot{\mathrm{x}} = 0.$$

We know from basic physics knowledge that the Newtonian fundamental equation for the movement of a particle is simply $\mathrm{F} = \mathrm{ma}$, where $\mathrm{a} = \ddot{\mathrm{x}}$ is the acceleration or change of velocity by time. In other words, the fundamental equation for a free Newtonian particle obtained by the Lagrange formalism is precisely that no force is acting on it and thus all of the allowed free particle movements must satisfy $\mathrm{F} = \mathrm{m}\ddot{\mathrm{x}} = 0$. This is exactly the result of the Euler-Lagrange formalism. The solution space of $m\ddot{\mathrm{x}} = 0$ fully describes all of the legal movements of a free particle in Newtonian mechanics and given a set of empirical initial conditions for a classical particle the physically allowed processes are fully determined. This can be done for the general case rather than merely the free particle for Newtonian mechanics as well but we will refrain from delving too deep into the mathematics and physics of these formalisms. The point here is that the Lagrangian formalism given the appropriate Lagrangian function $\mathcal{L}$ will yield exactly the fundamental equations of motion for the Newtonian system and that the solution space of the Euler-Lagrange equation describes all the allowed behavior of the system. From the symmetries of the mathematically resulting solution space (of the general case but it can also be seen for this special case of the free particle) we could now rather easily see that there are many dynamical symmetries in Newtonian mechanics, such as rotation symmetry, translation symmetry and the above mentioned parity or mirror symmetry. They are all manifest symmetries of the solution space to the above Euler-Lagrange equations.

The Lagrangian functions will naturally become increasingly complicated as we move through the history of physics and even now we have already made significant simplifications to Newtonian mechanics in order to illustrate the Lagrangian formalism. For example, we have so far omitted the potential which can be used to add electromagnetism and other forces to the physics of systems. The point here is to describe the procedure and how one can verify that structure is retained across robust theory change. Extending this to the general case is not difficult but a complete and in-depth mathematical analysis would likely be far longer and require a background in mathematics and physics to fully understand.

Moving on to special relativity, the shape of the Lagrangian function remains very similar but very important alterations happen to the form of the kinetic energy term. The 1 dimensional free (potential term $\mathrm{V}=0$) Lagrangian function for special relativity is:

$$\mathcal{L}\big(t, x(t), \dot{x}(t)\big) = -\mathrm{mc}^2\sqrt{1-\frac{\dot{x}^2(\mathrm{t})}{\mathrm{c}^2}},$$

which is just the kinetic energy term in special relativity and c is the constant speed of light. We wish to compare the symmetries and structures of the solution spaces of special relativity on its subdomain of validity defined by the domain of validity of classical mechanics so we need to compute the Euler-Lagrange equations, which in abstract form in this case are once again:

$$\frac{\partial\mathcal{L}}{\partial x} - \partial_t\left(\frac{\partial\mathcal{L}}{\partial\dot{x}}\right) = 0,$$

where of course now those derivatives will take different values since the Lagrangian function has changed. Performing this calculation for the relativistic free particle yields the vacuum relativistic version of Newton's second law

$$\partial_t\left(m\dot{x}\sqrt{1-\frac{\dot{x}^2(\mathrm{t})}{\mathrm{c}^2}}\right)=0.$$

Note how this differs from the Newtonian version we saw above which was

$$\mathrm{m}\ddot{\mathrm{x}}=\partial_t(m\dot{x})=0$$

only by the so-called gamma factor $\sqrt{1-\frac{\dot{x}^2(\mathrm{t})}{\mathrm{c}^2}}$. This term is always present in special relativity, regardless of the velocity of the free particle but intuitively one can already see how when the velocity $\dot{x}$ of the free particle becomes negligible compared to the speed of light, then $\frac{\dot{x}^2}{\mathrm{c}^2}$ will approximately vanish and the square root will simply be 1. This is a semi-formal way to see the connection from a free particle in special relativity to one in Newtonian physics. We can still formalize this a great deal more and even give precise error bars for the Newtonian classical limit and of course this could also be done for the general theories including potentials if desired.

The structural realist part of this is that the domain on which Newtonian physics was showing increasing signs of lack of predictive power and thus robustness was precisely that of high energy, high velocity objects. In the domains in which Newtonian physics remains valid and widely used to this very day, the solution space of $\partial_t(m\dot{x})=0$ and the solution space of $\partial_t\left(m\dot{x}\sqrt{1-\frac{\dot{x}^2(\mathrm{t})}{\mathrm{c}^2}}\right)=0$, i.e. their respective free particle Euler-Lagrange equations, agree with regards to their dynamical symmetries and which physical processes are allowed and in this case even more than just that, the relativistic solution space fully goes over into the Newtonian one as $\frac{\dot{x}^2}{\mathrm{c}^2}$ tends to zero.

Procedures like this can be done for the entire theory as opposed to these special cases rather easily and can be done for all of the important theories such as quantum mechanics, relativity and quantum field theory as well, see for example any of the following books covering this canonical material: Doughty, 1990; Lancaster, 2015;

Mandl and Shaw, 2010, section 2; Schwartz, 2013 and the fantastic textbook on symmetries in physics by Schwichtenberg, 2017.

4.2 THE CASE OF ELECTROMAGNETISM

Electromagnetism is perhaps one of the more striking examples for the presently proposed framework since the similar mathematical structure of it in all major physics theories (Newtonian dynamics, relativity, quantum mechanics and quantum field theory) is immediately obvious. The impressive retention of the symmetries and structure of electromagnetism from classical to relativistic and lastly to quantum field theory could perhaps even be used as an argument for this proposal in itself. In this section we will sketch the Lagrangian formalism and dynamical symmetries of electromagnetism across theory changes. Unfortunately electromagnetism is far more involved than a mere free particle, especially when touching on quantum field theory as well, so this section is only intended as a qualitative motivation following the language of physics. For more detailed information on these things, we refer to up to date quantum electrodynamics and classical electromagnetism textbooks such as Fließbach, 2012 and Zeidler, 2009.

Classical electromagnetism is typically characterized by Maxwell's equations and the equation for the Lorentz force on a moving charged particle. A canonical course on classical electromagnetism might thus begin by simply stating the Maxwell equations but for the present purpose a Lagrangian formalization of the theory is required. The Lagrangian function for classical electromagnetism including a charged relativistic particle of mass m and charge q can be readily found to be (up to arbitrary scaling with constants):

$$\mathcal{L}\big(\mathrm{A}(x), \partial A, t, x(t), \dot{x}(t)\big) = -\frac{1}{4}F^{\alpha\beta}F_{\alpha\beta} - \mathrm{mc}^2\sqrt{1-\frac{\dot{x}^2(\mathrm{t})}{\mathrm{c}^2}} - \frac{q}{c}A^{\alpha}\left(x^{\beta}(t)\right)\dot{x}_{\alpha}(\mathrm{t}),$$

where $F^{\alpha\beta} = \partial^{\alpha}A^{\beta} - \partial^{\beta}A^{\alpha}$ and the vector A^{α} is the electromagnetic 4-potential which characterizes both the electric and the magnetic fields. The first term in this Lagrangian characterizes the electromagnetic fields themselves and we can clearly recognize the second term in this Lagrangian as the relativistic free particle Lagrangian from before. The third term determines how the charged free particle *couples* to the electromagnetic field. In all of this, Einstein summation convention is used meaning that two occurrences of the same indices, one up and one down, imply that there is an implicit sum over that index.

On an interesting side note that is rarely discussed but still important for the internal consistency of the proposed conception of limiting procedures, we can see clearly that this theory of electromagnetism reduces to the simple free relativistic particle in the case that no electromagnetic fields are present, i.e. if $A^{\alpha} = 0$. Thus, if one considers "electromagnetism + relativity" as its own physical theory (and there really is no reason why one could not do this) then just like for special relativity into Newtonian physics discussed above there is a clear natural limiting procedure implied by the formalism itself to move to the domain of validity of the "predecessor" theory (which in this thought experiment is non-electromagnetic relativity). Typically "electromagnetism + X" is historically simply considered part of theory X but strictly speaking "electromagnetism + X" is a different and improved theory with a significantly larger domain of validity.

Solving for the Euler-Lagrange equations of this Lagrangian function one finds that it exactly reproduces Maxwell's equations and the Lorentz force per construction (a mathematically simple task which we will nevertheless skip here, see for example Fließbach, 2012, section 19 for the derivations). One can thus confirm that this is the correct translation of classical electromagnetism into the Lagrangian formalism. If we wanted to, we could couple the electromagnetic parts to the Newtonian particle instead of the relativistic particle, showing once again how the Euler-Lagrange solution spaces of special relativity show the same structure as Newtonian physics as we move to the Newtonian domain of validity.

Let us now have a look at the form of electromagnetism in a quantum field theory setting. "Electromagnetism + X" where X is quantum field theory has the charming name of "quantum electrodynamics" or QED for short. The Lagrangian for QED of an electron in particular has the following form (it becomes more complex when looking at general particles including bosons and fermions so we restrict to the relatively simple case of the electron):

$$\mathcal{L}(\mathrm{A}, \partial\mathrm{A}, \psi, \partial\psi) = -\frac{1}{4}F^{\alpha\beta}F_{\alpha\beta} + \overline{\Psi}\big(\mathrm{i}\gamma^{\mu}\mathrm{D}_{\mu} - \mathrm{m}\big)\psi,$$

where the right-hand term is responsible for producing the free Dirac equation as well as containing the coupling term for an electron in the covariant derivative D_{μ} and the left-hand term is evidently of the same form as that of classical electromagnetism discussed above. While this may appear like a trivial gluing together of different theories, the details of the internal theoretical differences here are subtle, non-trivial and physically important. From the point of view of Newtonian physics and special relativity the electromagnetic fields appear mathematically as classical fields which affect the particles via the Lorentz force coupling term while in the formalism of quantum electrodynamics the electromagnetic field just like the quantum fields ψ in the above Lagrangian become quantum objects themselves and behave just like “particles” themselves. In fact electromagnetism in the QED framework is conceived of as being mediated by an exchange of virtual photons. Still, these differences between the frameworks of the theories do not alter the fact that the mathematical structure of the electromagnetic aspect of the universe is clearly largely retained from classical to QED electromagnetism and one can even derive corresponding QED-variations of Maxwell’s equations from this Lagrangian.

Speaking more formally, both classical electromagnetism as well as QED treat electromagnetism as the result of an abelian gauge theory with symmetry group U(1). This and other symmetries are characteristic of electromagnetism across all of the different physical theories and are in fact one of many dynamical symmetries of the above electromagnetic Lagrangians.

What remains to be shown here of course is that not only the electromagnetic part of this reduces back to Newtonian or relativistic physics but that also the Dirac equation part reduces back to more classical non-QFT physics as well. To see that however, one would have to look at bosons and fermions and discuss all manner of interesting but involved physics of decoherence and semi-classical limits which would by far go beyond the scope of this thesis. Suffice it to say, however, that for the domains in which Newtonian physics or non-quantum relativity are wildly successful this can done but is quite mathematically involved and requires a deep understanding of classical physics, quantum mechanics and quantum field theory.

5 RESPONSE TO POTENTIAL CRITICISM

The core ideas intended to have been conveyed so far in this thesis are as follows:

1) Lagrangians and Euler-Lagrange equations and their solution spaces form a natural semi-formalized framework in which to discuss physics theories and in particular where and how structures may be found to be naturally embeddable into each other on particular domains of validity.
2) The present reality and the history of robust physics theories support the views put forth in this thesis and dynamical symmetries of solution spaces are retained on the domains of validity of predecessor theories (and indeed oftentimes the *entire* solution space is retained and not merely the dynamical symmetries, which is a far stronger statement than required for the above arguments).
3) There is no need for a further formalization step away from the language of physics itself as such a further step as argued above leads to the loss of sight on the empirical and evolving domains of validity which need to be considered when investigating structural inheritance and the retention of dynamical symmetries (or more) of the solution spaces and thus of the laws of physics restricted to said domains.

The present section intends to pre-emptively defend these points from some potential criticism that may be raised against these points based on arguments that have been made before in the philosophical literature.

5.1 EXISTENCE OF DISTINGUISHED EMBEDDINGS (AND THE TORTOISE)

Returning to the question of the uniqueness or even just existence of sensible embeddings from section 2.7.3, one might try to apply such thoughts to the proposed characterization of structure and ask if and in what way it makes the “appropriate” way to embed one theory within a successor theory sufficiently clear.

There is no a priori reason why such a strong condition as uniqueness should be required of structural realism, however, as all that should be required for a sensible concept of structural inheritance is the existence of a class of natural and distinguished embeddings among the infinitely many other ways to map from one theory to another which agree modulo some irrelevant parameters. This is precisely what the above characterization does, building on physicist's actual way of engaging with their theoretic frameworks.

Given two robust theories, one the predecessor and one the successor, their domains of validity will overlap. The claim is that on those domains of validity the solution space of both theories has the same symmetries and thus in an important sense retains the same structure and can even be brought into equivalent mathematical form barring at most some additional terms of the successor theory which vanish or are negligible on the specified domain of validity. The question of uniqueness here does not even arise since no claim about uniqueness is being made. It is a mistake, however, to believe that lack of uniqueness means that the position being advocated is trivial or meaningless since there still is a clearly indicated class of distinguished "embeddings" given directly by the physical Lagrangian (or Hamiltonian) formalism.

It should also be noted that the claim often found in the philosophical literature that there is no such distinguished class of ways to move from predecessor theories to successor theories on the mathematical and thus structural level should be seen as incredibly suspect to begin with given that physicists and mathematicians in fact do exactly this all the time and have even used such methods to learn new things about both the predecessor and successor theories. Thus, even if the characterization advocated in this thesis should prove entirely inadequate the fact still remains that non-trivial and distinguished mathematical limiting procedures exist and are directly indicated by the formalisms of our physical theories including their empirical domains of validity. It should be no surprise that abstracting all of the empirical content as well as the formalisms of physics away from the theory and placing them into the vacuum of a formal language ill-suited to deal with an empirical and scientific rather than a purely linguistic or mathematical theory results in many steps taken in physics appearing completely arbitrary or incomprehensible. In fact all of physics and science starts looking more and more impossible the deeper one delves into such fully abstracted logical systems of language analysis.

Likewise, when abstracting natural languages down into fully abstracted logical systems analogous to those applied to science one will start to perhaps see communication between different agents as fundamentally impossible as well since there is no clear way that the intended spoken sentence can ever be inherently maintained through the interpretation of the receiver, at least not unless such things are taken as empirical realities and placed into the system as premises.

Perhaps even more poignantly this is precisely the kind of mistake present in Zeno's infamous paradox of Achilles and the tortoise. Huggett tells a modern variant of it:

> "Imagine Achilles chasing a tortoise, and suppose that Achilles is running at 1 *m/s*, that the tortoise is crawling at 0.1 *m/s* and that the tortoise starts out 0.9m ahead of Achilles. On the face of it Achilles should catch the tortoise after 1s, at a distance of 1m from where he starts (and so 0.1m from where the Tortoise starts). […] [B]efore Achilles can catch the tortoise he must reach the point where the tortoise started. But in the time he takes to do this the tortoise crawls a little further forward. So next Achilles must reach this new point. But in the time it takes Achilles to achieve this the tortoise crawls forward a tiny bit further. And so on to infinity: every time that Achilles reaches the place where the tortoise was, the tortoise has had enough time to get a little bit further, and so Achilles has another run to make, and so Achilles has an infinite number of finite catch-ups to do before he can catch the tortoise, and so, Zeno concludes, he never catches the tortoise." (Huggett, 2018)

In said paradox not only does the empirically intuitive and evident victory of Achilles against the much slower tortoise become muddled down in language but *any physical movement at all* ends up sounding fundamentally impossible. This is evidently an artifact of the framing device (i.e. the formalization of the situation in a particular language) rather than a property of the actual situation (were such a race to occur). As Max Black put it:

> "It would be a waste of time to prove, by independent argument, that Achilles will pass the tortoise. Everybody knows this already, and the puzzle arises because the conclusion of Zeno's argument is known to be absurd. We must try to find out, if we can, exactly what mistake is committed in this argument." (Black, 1950)

Mathematical “solutions” of Zeno’s paradox have been abundantly discussed in the philosophical literature (e.g. in Black, 1950; Huggett, 2018; Wisdom, 1952) – in fact Zeno’s paradox being an artifact of the particular language does not arise in proper physical and mathematical analysis in the first place so it is not technically correct to speak of “mathematical solutions”. Whatever the case may be however, it should be self-evident to anyone that movement is indeed possible and Achilles would in fact win such a race. The discussion of Zeno’s paradox can thus only be a (admittedly very interesting and beautiful) language and formalization exercise.

In all of this, it is all too easy to lose sight of the original question that warrants inspection: *how* is it done and *why* can it be done. The question of *if* it can be done not only cannot be answered by purely logical systems that abstract out all of the important empirical aspects of observable reality, it is a rather blatant case of taking the formalized abstracted setting to be more powerful than the real world itself. I argue that the often posed question of existence of distinguished ways to move from predecessor to successor theories is also of this nature. The characterization of structure above is thus intended to explain *how* physicists are able to make use of limiting case retainment of older theories in newer ones given that this practice is ubiquitous and is known to work well. It is not an argument *that* they can do it, as the fact that they can and do is evident in scientific practice of the past centuries.

5.2 SHOULD REALISM BE SO SELECTIVE?

As mentioned in section 0, the idea of introducing stricter qualifications for the maturity of scientific theories based on their predictive power, specifically their predictive power for novel phenomena is not new to the philosophical literature. Novel predictions have received sufficient amounts of attention from philosophers of science in the realism / anti-realism debate that arguments against such positions have already been formulated and likewise scrutinized – one of the most recent collections of such criticisms of novel predictive power as a selective criterion for realism is given by Dana Tulodziecki in her 2017 paper “Against Selective Realism” (Tulodziecki, 2017).

Unfortunately essentially all of the literature specifically against such a “selective” sense of realism, including Tulodziecki’s paper, typically strays very far away from physics and go into biological, medical and occasionally chemical sciences for counter-examples. As this thesis exclusively concerns itself with the theories of physics (as they inherently already come with an empirico-mathematical form that is easy to work with) and thus also only advocates structural realism with regards to robust *physics* theories, the details of Tulodziecki's argument based on the nineteenth-century zymotic theory of disease which would eventually be succeeded by germ theory will not be relevant. We delay comments about the non-physics other natural sciences to section 5.5. Thus, instead of following Tulodziecki's zymotic theory based criticism, we will attempt to abstract the core of the structure of her arguments and see if similar concerns might be raised against physics.

Tulodziecki's core argument is sufficiently representative of other papers on the subject matter and applied to the present case and abstracted away from the concrete example she gives from the biological and medical sciences her argument could be summarized as standing on the following two pillars:

1) There is an example of a scientific theory A_1 which was successful in
   a) explaining observed phenomena
      and
   b) predicting novel phenomena.
2) For said robust theory A_1, we can clearly see that no relevant aspects or structure was passed on to successor theory A_2.

In Tulodziecki's paper the example theory A_1 she posits has these properties is the zymotic theory of disease and the successor theory A_2 lacking the relevant aspects is the germ theory of disease but as mentioned above we will not engage in a discussion on whether or not these examples actually satisfy what Tulodziecki claims about them since we are concerned with structural realism about physics.

Instead, we note that it is crucial that all of the above points (1a, 1b and 2) must simultaneously be true of a set of example theories in order for an argument against "selective structural realism" to have merit, at least if one attempts to argue along such lines. The characterization of structure in physics given above certainly makes no claim about there never being a physics theory change in history where no relevant aspects were retained. Evidently such changes have occurred especially in the early semi-scientific times of physics when most theories should rather be referred to as hypotheses and this observation is precisely why the concept of robust or mature theories is introduced in the first place.

The more noteworthy claim then is that a theory change occurred from a robust theory to another robust theory with no relevant aspects being transferred, not forgetting that in the case of the characterization given above this would have to be specified to no relevant aspects being transferred on the predecessor theory's domain of validity. Without attempting to give an analysis of *all* scientific theory change in physics as this would break the scope even of large monographs, it can at least be stated that the existence of such a theory change is highly doubtful just based on how physics works as opposed to other natural sciences.

Since on the domain of validity of the old theory the new theory inherently needs to produce the same predictions up to at most negligible terms (otherwise the new theory would fail to even be as good as the old one on that domain and would not be adopted as a successor in the first place) it is difficult to see how a mathematical framework could produce exactly the same phenomenology without producing identical or equivalent mathematical formulas to describe it. On the major theory changes that can readily be investigated and have historical significance such a theory change from robust predictive theories to another without significant structural inheritance has never occurred. Kepler's laws of planetary motion are present as limiting cases in Newtonian mechanics, Newtonian mechanics is present as limiting cases in special relativity, general relativity and quantum physics and quantum field theory. Quantum mechanics and special relativity are both limiting cases of quantum field theory as well.

Unless a successful criticism of this sort is raised specifically against physics theories, and to the best knowledge of the author there has not been a notable one, the multitude of criticisms of "selective" realism which may or may not succeed with regards to other natural sciences fail to pose a problem to the argument presented in this thesis.

5.3 THE RELEVANT PARTS OF THEORIES REVISITED

While there have not been many relevant arguments raised against selective realism in physics along the lines of Tulodziecki's argument, we have already encountered something with a vague resemblance to such an argument in section 2.7.4 where Worrall himself argued in his seminal paper in which he more or less coined modern structural realism that the step from Newtonian physics to its successors should not be thought of in terms of such domains of validity. We reproduce the relevant footnote in his paper which we already cited above once again:

> "Professor Agazzi […] took the view that Newtonian physics remains true of objects in its intended domain and that quantum and relativistic physics are true of objects in quite different domains. But this position is surely untenable. Newton's theory was not about (its 'intended referent' was not) macroscopic objects moving with velocities small compared with that of light. It was about all material objects moving with any velocity you like. And that theory is wrong (or so we now think), […]. Moreover, it isn't even strictly speaking, right about certain bodies and certain motions and 'only' wrong when we are dealing with microscopic objects or bodies moving at very high velocities. If relativity and quantum theory are correct then Newton's theory's predictions about the motion of any body, even the most macroscopic and slowest moving, are strictly false. It's just that their falsity lies well within experimental error." (Worrall, 1989)

The disagreement with Worrall resulting from our characterization of structure may turn out to be predominantly a semantic one but it merits discussion nonetheless. Worrall is entirely correct that what he calls the "intended domain" of Newton's theory was indeed universal and not merely intended to apply to "macroscopic objects moving with velocities small compared with that of light". To posit the latter would indeed be untenable given that it would require Newton and his contemporaries to have precognition of future failings as well as future successors of his theory.

Worrall's claim that Newtonian physics "was about all material objects moving with any velocity you like" (Worrall, 1989) is correct if one looks at the initial stages of Newtonian theory but as scientists noticed empirical issues of the theory, the domain of validity of Newtonian theory shrank, sometimes gradually, sometimes a lot at once to its current size. The successor theory is only claimed to be equivalent to Newtonian theory on the domain of validity which remains and on said domain, the smallness of velocities is inherent with the domain even if it is not inherent in the original Newtonian theory. Thus, the possibility for a “raw and original” Newtonian theory to accelerate objects beyond the speed of light is not a problem.

This is why in the definition of domains of validity caution was taken to account for the fact that physicists over time change the domain of validity of their theories, shrinking it when sufficient evidence accumulates in a specific region. The time-dependent nature of these domains is crucial to progress in physics, as shrinking domains of validity often indicate where in the theory the problematic assumptions lie. This is why many physicists often say that the discovery that something satisfies the predictions of present theories, while interesting and reassuring for the theory, is not as powerful a drive for improvement as finding out a result *not* predicted or explained by the theory.

Only when our accounts of the domains of validity of our scientific theories allow for the dynamic adjustment of theories to available empirical evidence does Worrall’s counterargument to this disappear.

Lastly, Worrall’s claim that

> “[…] if relativity and quantum theory are correct then Newton’s theory’s predictions about the motion of any body, even the most macroscopic and slowest moving, are strictly false. It’s just that their falsity lies well within experimental error.” (Worrall, 1989)

is true as well but trivially so since any position that would hold the opposite would have to hold that physics is finalized once a successful theory has once been established. The claims of a structural realism need not *nearly* be this strong. Since structural realism of the sort indicated in this thesis merely posits that a relevant sense of structure of the real world is struck by our most successful physics theories we need not try to defend the much stronger claim that *all* of a theory is retained one to one. Since Worrall goes on to defend a structural realism himself (it is the paper where he coins and defends it for the first time after all) he evidently realizes that this is only a problem if we were to try to be a naïve form of entity realist about common ontological language surrounding Newtonian theory and doesn’t work as a counterargument against a structure based view of Newtonian mechanics.

5.4 HIDDEN ENTITIES REVISITED

Previously, in section 2.7.1, we argued that the existence of objects within structural realist positions is not an issue and in fact even ontic structural realists can remain unaffected by claims of hidden entities within their view, since entities and relations are inseparable with neither having an ontological precedence over the other.

As would be expected of a structural realist position, no mention was ever made of objects when defining the relevant concept of structure in section 3. This should not be taken as an indication that entities are necessarily absent but only that their existence is not particularly relevant for the position.

Objects are merely in the background. We know *something* exists in nature which exhibits the structure and properties assigned to electrons but whether it actually is proper electrons or perhaps an excited vibrating string as string theory would suggest or something else entirely is irrelevant to the structural realism advocated here and we can remain entirely agnostic about the nature of individual objects as long as we do not assign to them fundamental, intrinsic identifying properties (compare Esfeld and Lam, 2011, 2006).

5.5 THE OTHER NATURAL SCIENCES

There are some natural limitations of the argument presented in section 3, which one might either consider features or bugs depending on one's background assumptions. For one, it works as a characterization of structure and argument for structural realism only on the basis of physics and leaves out other natural sciences for which forms of structural realism have also been proposed in the past such as biology and chemistry (see e.g. Hettema, 2017, p.238; French, 2017, p.324 or Sterpetti, 2016). However, with regards to the structures of biological theories such as the theory of evolution (or rather theories of evolution, as the theory of evolution has gone through major improvements over the decades since its first proposal), ontic structural realism would be a dubious position proposing as existing both the structures and symmetries of nature as well as somehow separately the existence of macroscopic structures which should probably be considered emergent phenomena rather than structures in themselves.

This is not a reduction of biology to physics, since there are significant differences in both experimental and theoretical methodology with regards to these sciences which are left fully intact. This is, however, a reduction of the ontology of biology to the ontology of physics – that is to say, the world according to biology does not contain anything that would not also be in the solution space of a sufficiently advanced physics, more or less by definition of how these different projects are set up. This is not an uncontroversial stance but also not a particularly uncommon one (compare for example notions in Boyd et al., 1991, section 3 and Melnyk, 2007). What this means about the ontology of *current* biological theories would need to be worked out separately.

The situation for chemistry is similar, except that at its most fundamental chemistry has a clear mathematical intersection with physics where the situation becomes more clear. However, since skepticism about the ontology of scientific theories are typically brought up regarding objects such as photons, atoms, electrons or quarks rather than humans or tigers (recall the concept of observable and unobservable entities above) and the task was to give a meaning to structure and symmetries in physics in particular, there is no reason to consider these limitations an acute problem of the characterization. The details of whether and how structural realism might be applied in a more direct way to the other natural sciences goes beyond the scope of this thesis but presents an interesting and worthwhile philosophical question in itself.

6 DISCUSSION

In this thesis we presented an argument for structural realism, both epistemic and ontic, on the basis of symmetry preservation in the solution spaces of predecessor and successor theories on the subsection of their domains of validity. Some of the major theory changes in the history of physics were cited as illustrative examples of this concept and can be readily found to satisfy the conditions of the argument and notably do not show any sign of theory incommensurability within the language of physics itself but mutual relationships via mathematical limiting processes. In this light the no miracles argument and Worrall's original argument for the adoption of structural realism are supported through a characterization of structure as dynamical symmetries of solution spaces which are very closely tied to actual physics research.

All throughout this thesis, a common theme has been the criticism of meta-formalizations of science and physics in particular. This may give the impression of a rejection of formalizations and idealizations in the first place but that is not the case. After all, physics itself is a case of formalization and idealization itself and would be impossible without it. The rigor and power of mathematical and logical languages are among humanity's greatest tools for progress. Instead, the intention of these arguments was that the commonly used formalizations in the philosophy of science suffer from a great deal of disconnection from the thing they are supposed to model and thus miss critical things about the phenomena. Results derived from such lacking formalizations which fail to reproduce what we see in the real world can only tell us something about the formalizations themselves and not about what is being modelled. Just like in physics, where not every mathematical model of a physical process is relevant but only those which accurately capture observed phenomena, a lot of care needs to be taken in philosophy to check if a particular formal system actually matches the empirical reality of the things it is meant to describe. Physics does not behave like many model theorists say it should and this is a problem of their models, not of physics. Establishing a formal system and then automatically taking the problems of the formal systems to be problems of the process it models is not unlike the mistake a physicist would commit if they tried to model the luminiferous ether, failed and then said that therefore light cannot actually propagate in the universe. It is not that which is *modeled* which is incomplete, it is that which is supposed to *do the modeling* which is wrong or incomplete.

The possibility of improving strictly logical systems to account for all of the empirical complexities such as time dependent domains of validity and dynamical symmetries of solution spaces is left open, of course, since there is no reason to think that such a program would a priori fail although the usefulness of such a new or adapted formalized language could be doubted on the grounds that meta-formalizing an already heavily formalized system such as physics is unlikely to produce any further insight.

The question of the standing of the other natural sciences such as biology and chemistry had to remain largely open and undiscussed due to the scope of the thesis but provides interesting grounds for philosophical investigation in its own right. In the brief section in which we discussed the other natural sciences, we argued that ultimately this might be addressed successfully with a physicalist approach but these views are controversial and thinking about alternative approaches to realism with regards to biology in particular is certainly warranted.

An obvious path to further strengthen the views presented in this thesis would be a more thorough and in-depth analysis of theory change and the particular Lagrangian formalisms of different theories. This is in principle possible and has been done within physics itself scattered throughout the textbooks and scientific literature but it would certainly be worthwhile to produce an up-to-date compendium of such limiting processes and on which domains they empirically should hold and do hold. The scope and mathematical depth required of such a program to be worthwhile is more suited for that of a monograph than a thesis, however.

7 DEUTSCHE ZUSAMMENFASSUNG (GERMAN ABSTRACT)

Unter den Begriff des wissenschaftlichen Realismus fallen alle jene philosophischen Meinungen und Auffassungen, welche in den Theorien der Wissenschaften (insbesondere der Naturwissenschaften) in einem nicht-trivialen Sinne Aussagen über die reale, extern existierende Welt sehen. Der strukturelle Realismus, welcher die über Theorienwechsel hinweg erhaltene Struktur wissenschaftlicher Theorien als Grundstein für eine Art von Realismus sieht, ist eine der am weitesten verbreiteten und populärsten Formen des wissenschaftlichen Realismus. Trotz oder eher sogar auf Grund dieses Status gibt es zahlreiche Kritiken an dieser Position, allen voran der Vorwurf keine genauen bzw. konkreten Aussagen bezüglich der Bedeutung des Begriffs der „Struktur“ anzubieten und daher leere Aussagen zu tätigen.

In dieser Arbeit wird ein Versuch der Charakterisierung der Struktur innerhalb der Physik in einem für den strukturellen Realismus relevanten Sinne formuliert, primär aufbauend auf dem Begriff dynamischer Symmetrien in physikalischen Lösungs- bzw. Zustandsräumen. Gemeinsam mit den typischen Argumenten für den strukturellen Realismus bildet dies eine Verteidigung des strukturellen Realismus vor anti-realistischen Argumenten einerseits und andererseits vor dem Vorwurf keine konkreten Aussagen über die Bedeutung des Begriffs der Struktur zu machen. Den Abschluss der Arbeit bildet eine Diskussion verschiedener möglicher Kritikpunkte an diesem Ansatz basierend auf relevanter philosophischer Literatur.

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