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REALITY IN PERSPECTIVES

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de Vrije Universiteit Amsterdam,
op gezag van de rector magnificus
prof.dr. J.J.G. Geurts,
in het openbaar te verdedigen
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door

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geboren te Hamedan, Iran

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Contents

Preface

Whether we can know reality is a long-standing philosophical question, about which people have been thinking since antiquity. The scientific realism debate concerns a relatively new version of that question: does scientific knowledge describe reality? From the time I first learned classical mechanics in high school, or even from earlier times in my childhood when I was looking up to the sky and thinking about the creation of stars, I have always had a desire to reflect on human knowledge of (natural) reality. Working on this dissertation allowed me to fulfill that deep-rooted philosophical desire.

Several contingent issues have influenced the chosen viewpoints with which I begin. For instance, Hans Radder has been my primary supervisor, so apart from his constructive comments and criticisms of my writings, his books and papers (especially, 2012[1984/1988] and 1996; and partly 2006) have been stimulating throughout my work on the dissertation. For another example, six years ago, when I just began my PhD studies, I wrote a course paper to argue that the constructive aspects of scientific theorizing are compatible with realism. Later in 2019, I studied Ronald Giere's books (1988; 1999; 2006a) and found his views more advanced than my earlier thoughts; then, I decided to critically review and develop Giere's accounts. Another contingent issue concerns my master's thesis, defended in January 2015, which was on the phenomenological-hermeneutical approaches to science and tool uses. From then on, I have followed the relevant literature, especially the studies regarding scientific realism and perception, which motivated me to write chapter 5. Some similar contingent issues may have influenced my choices of case studies as well. My interest in astronomy in the medieval Islamic world familiarized me with the Ptolemaic system, and then I started to think that Kepler's model may be able to restate the Ptolemaic successful explanations (see section 6.7). Furthermore, I was studying and discussing issues on the philosophy of physics with my physicist friends during a study group. As a result, I added the case of the Higgs boson to chapter 2 and composed chapter 7 on the theory of special relativity, as a case for perspectivism in terms of reference frames. Finally, perspectivism, entity realism, and structural realism are among current trends in the philosophy of science, so it is natural that I have paid attention to them.

Be that as it may, all these “contingent issues” are nothing but stories about my choices of the relevant literature. If I were in different conditions, my starting points would have been different. This unavoidable fact is unimportant, however. What really matters are whether I am successful in providing a coherent thesis and to what extent my arguments and cases are compelling and novel.

I would like to thank Hans Radder and Lieven Decock for their dedicated supervision of this project. Hans has studied several drafts of the dissertation patiently, and offered various comments on my arguments and writing style. He generously taught me how to develop a philosophical idea, how to compose a monograph on a coherent thesis, and how to write precisely, concisely, and clearly. I wish him a long and healthy lifetime. Lieven has also given me constructive feedback on different versions of the dissertation. In particular, his remarks taught me to consider the logical implications of my arguments carefully and to formulate them accurately. Furthermore, I am indebted to Javad Akbari Takhtameshlou for his detailed and helpful observations on chapters 5 and 6, to Javad Ebadi for insightful discussions about the cases of the Higgs boson and special relativity theory, and to Amir Sadat Mousavi for providing the details of the Ptolemaic system case-study. In the development of the ideas of the dissertation, I have also profited from the comments of many scholars, including but not limited to Mario Alai, Ebrahim Azadegan, Philipp Berghofer, Anjan Chakravartty, Hasok Chang, Matthias Egg, Roman Frigg, Don Howard, Don

Ihde, Alireza Kazemi, Ave Mets, David Papineau, James Read, Henk de Regt, Darrell Rowbottom, Juha Saatsi, Peter Vickers, Harald Wiltsche, and the anonymous referees of the journals to which I submitted earlier versions of the chapters of the dissertation. And warm thanks to Ehsan Mirzaee, whose companionship has been entertaining and soothing during my arduous work on the dissertation. Last but of course not least, I should like to thank, and dedicate this work to, my closest family members: my caring wife, my darling daughter, my always supportive mother, and my encouraging father. I am more than lucky and happy to have these lovely beings in my life. Although the pandemic, sanctions, wars, political and financial disasters, and other evils have already surrounded me and many others on earth, I feel our planet is still hospitable to those who care about others and about whom others care.

Chapter 1 Introduction: Science and Reality

1.1 The Realism Debate

This dissertation addresses the issue of realism, usually framed as the debate on the philosophical interpretation of human knowledge of reality. In particular, the dissertation concerns the scientific realism debate, about which several thinkers have thus far developed a variety of insightful views: perspectival realism, experimental or entity realism, referential realism, structural realism, transcendental realism, practical realism, instrumental realism, semirealism, constructive empiricism, social constructivism, (cognitive) instrumentalism, and so forth. It seems not interesting anymore to add another brand *besides* these viewpoints, but it is still fascinating to take advantage of them to develop a more comprehensive view. To do so, one should stand *upon* the shoulders of giants, so to speak, to better understand both science and reality.

In carrying out this project, my starting points are Hans Radder's and Ronald Giere's views, which are inclusive enough to take account of the different dimensions of science. Radder's view about science includes these dimensions: experimental action and production, conceptual-theoretical work, and formal-mathematical activity. Giere also speaks of experimental, instrumental, and theoretical dimensions of science. In addition to Radder's "referential realism" and Giere's different versions of realism (that is, "entity realism", "constructive realism", and "perspectival realism"), I discuss the (recent) views of entity/experimental realism as well as the phenomenological-hermeneutical approaches to the scientific realism debate. Also, a number of other (anti)realist viewpoints are addressed. Thus, relativism, constructivism, pragmatic realism, constructive empiricism, and instrumental realism are critically examined. I will also argue that structural realism is (partly) in line with the view I develop on intertheoretical continuity.

A feature of the realist views I discuss and endorse in the dissertation is that they are relatively modest accounts of realism, both ontologically and epistemologically. In general, recent realisms are much more modest than many of their predecessors, because they want both to preserve realist insights and to appreciate justified antirealist concerns. The arguments of (social) constructivists and of non-realist philosophers, such as Bas van Fraassen, Larry Laudan, and more recently Kyle Stanford, Brad Wray and Darrell Rowbottom, have made it difficult, if not impossible, for realists to make unqualified claims. For the same reason, while this dissertation often criticizes antirealism, the result is in several cases sympathetic to their ideas.

1.2 A Realist Perspectivism

The discussion of various other positions enables me to develop a view in which two (kinds of) concepts are of central importance. 1- Perspectivity: scientific knowledge is *perspectival*, inasmuch as it is conditional on instruments, theories/models, or reference frames. I will defend the view that non-perspectival knowledge of reality, or a view from "nowhere", is unattainable. 2- The concepts of persistence (and resistance), robustness, overlapping perspectives, replication, and explorability play a more or less similar role on the *realist* side of my thesis. These concepts are related, yet also distinct. Independent potentialities of reality are "persistent" (and "resistant"). Things, including ordinary objects and scientific entities or properties, are "robust" (if they are detectable/measurable in a variety of independent ways). The empirical evidence of a real entity is "explorable". Experiments and observations whose results can be obtained by means of several experimental processes are "replicable".[1] And perspectives (including instruments, models/theories, and reference frames) may be "overlapping". Thus, if I were to choose a name for my view, I could use the expressions persistent perspectivism, robust perspectivism, overlapping perspectivism, replicable perspectivism, explorable perspectivism. Or, in sum: realist perspectivism. Chapters 2 to 7 are designed to develop different features of this realist perspectivism, which I distinguish from other perspectival accounts in that the concept of "overlapping perspectives" is central to it.

Synchronic and diachronic versions of perspectivism have been introduced in the literature (see Massimi 2018b). The former emphasize that instruments and models (in the same historical period) represent reality from different perspectives. The latter regard successive theories or models in the history of science. Cases of synchronic perspectives have already been studied in detail (by Teller 2001, Rueger 2005, Giere 2006a, Plutynski 2020, among others). On the other hand, diachronic perspectives have not yet been investigated in sufficient detail; but at the same time, there are many discussions on theory change and the so-called pessimistic induction that relate to the issue of diachronic perspectivism. I bridge the gap between diachronic perspectivism and the latter discussions (in chapter 6), suggesting that the successes of a past theory may be restated from the

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[1] Over the past decade there have been a large number of discussions on the "replication crisis", the methodological problem that many scientific studies, in particular in medicine and psychology, are not replicable. These discussions acknowledge that replicability is central to scientific knowledge, and thus its lack causes a *crisis*. The importance of replication is underscored in this dissertation.

perspective of its currently acceptable successor, which entails that the no-miracle argument is supportable. Moreover, the synchronic/diachronic distinction is incomplete. It does not take into account the role of reference frames. Different reference frames in the theory of relativity are neither synchronous nor diachronous in the mentioned senses. Still, they are bona fide examples of perspectives, since according to the (special) theory of relativity several of the measured properties of objects are conditional on the choice of (inertial) reference frames. In this respect, chapter 7 addresses the role of (inertial) reference frames in special relativity theory. It also discusses the constancy of the speed of light, which is a "robust" property of light according to the special theory of relativity.

1.3 Approach and Overview of the Dissertation

The different chapters of the dissertation are designed to provide a unique and coherent thesis, which will be understood fully only when all are studied. Nonetheless, each chapter is written in a way to be intelligible independently. For this reason, some themes will be repeated. For instance, Giere's views are reiterated for different purposes: section 2.5 discusses his position to argue that my ontological view is compatible with perspectivism at the perceptual (and epistemological) level(s) of discussion; chapter 3 elaborates on his views to argue that entity realism and perspectivism are compatible and complementary; chapter 5 addresses his accounts to support the claim that there are strong affinities between perspectivism and the phenomenological-hermeneutical approaches to the philosophy of science; and chapter 7 critically examines his views on "partiality" and "conditionality" to argue that the latter concept is more suitable, especially for interpreting special relativity theory. Likewise, I reiterate Radder's views, entity realism and robustness in different sections for different purposes.

Furthermore, I employ different concepts in different contexts. For instance, in discussing, respectively, reality, experiments, perspectives, things, and empirical evidence (or its phenomenological equivalence: the perceptual horizon), I employ the notions of, respectively, "persistence", "replication", "overlapping perspectives", "robustness", and "explorability".[2] These different concepts denote different aspects of one comprehensive theme, namely realism (or more precisely, they denote the realist dimension of my realist perspectivism). Moreover, I use the ontological terms "entity", "object", and "thing" more or less interchangeably. Nevertheless, often entities are taken to be scientific entities (such as ions, black holes, viruses, the quantum state of a system), objects are ordinary objects (such as tables, trees), and things include both entities and objects. Another point is that, as it will be argued, scientific entities manifest their *properties* through empirical evidence obtained by utilizing technological instruments. Accordingly, when I discuss knowledge of scientific "entities", I primarily mean knowledge of the properties of those entities. As a result, the recent views of "entity" realism are actually kinds of "property" realism (see chapter 4). Finally, the fine-grained nuances between the metaphysical notions of "potentiality", "disposition", "power", "capacity", and so forth, are inconsequential for my purpose (in chapter 2 and other chapters).

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[2] William Whewell (1794–1866) is perhaps the founding father of the family of these realist notions. His "consilience of inductions" implies a theoretical unification which happens when a theory accounts for the empirical evidence gathered from different sources or when previously irrelevant empirical domains are united by the theory. He argues that "consilience" provides a criterion for reality (see Snyder 2005).

Several disciplines participate in the scientific realism debate. The ones employed in this dissertation include general philosophy of science, history of science, physics, metaphysics (of science), philosophy of language, philosophy of perception, history of philosophy (of science), science and technology studies, and philosophy of technology. In addition, my work is not limited to the analytic tradition. It is also inspired by phenomenological-hermeneutical ideas. In general, my preferred approach to philosophy is that different philosophical methods (in a broad sense including techniques and styles of thinking), such as conceptual analysis, formal logic, case studies, hermeneutical methods, and phenomenology, should collaborate in addressing specific philosophical problems. The employment of different methods to support one philosophical claim is an example of "replication" in philosophy.[3] Overall, the nature of the problem of scientific realism and my approach to philosophy have resulted in a highly interdisciplinary dissertation, whose writing required "persistence" and the overcoming of "resistance". Reading it may also need great patience and attention to detail.

Six chapters constitute the main body of the dissertation. Chapter 2 clarifies the notion of the real on the basis of two concepts: persistence and resistance. These concepts enable me to explain my ontological belief in the real potentialities of human-independent things and the implications of this view for the perceptual and epistemological levels of discussion. On the basis of the concept of "overlapping perspectives", chapter 3 argues that entity realism and perspectivism are complementary. That is, an entity that manifests itself through several experimental/observational methods is something real, but our knowledge of its nature is perspectival. Critically studying the

[3] On the possibility and desirability of replication in the humanities, see Peels (2019).

recent views of entity realism, chapter 4 extends the discussion of entity realism and provides a criterion for the reality of property tokens. Chapter 5, in contrast, develops the perspectival aspects of my view on the basis of the phenomenological-hermeneutical approaches to the philosophy of science. This chapter also elaborates my view of empirical evidence, as briefly expressed in sections 2.5 and 4.5. Chapter 6 concerns diachronic theoretical perspectives. It first explains my view of progress, according to which current perspectives are broader than past ones. Second, it argues that the successful explanations and predictions of abandoned theories can be accounted for from our currently acceptable perspectives. The case study of Ptolemaic astronomy supports the argument of this chapter. Chapter 7 serves as the conclusion of the dissertation by applying the central themes of the previous chapters to the case study of special relativity theory. I interpret frame-dependent properties, such as length and time duration, and the constancy of the speed of light according to realist perspectivism.

Chapter 2 Reality as Persistence and Resistance

2.1 Introduction

The scientific realism debate concerns the question of whether or not scientific knowledge describes reality. However, (anti)realists usually begin the discussion without clarifying the answer to this central question: what is taken as the meaning of the notion of the "real"? This leads to a general problem: reality is discussed, but its meaning is not clarified. Realists are faced with the issue more seriously since they advocate a realist interpretation of unobservables, the definition of whose reality is unclear. This question cannot be adequately answered merely by providing a "criterion" for reality. Also, using "truth" does not help realists in determining the meaning of reality because of the existence of non-realist theories of truth. This chapter aims to illuminate the notion of the real. I conceptually analyze the meaning of the real at three levels: ontological, perceptual, and epistemological. My approach in the chapter is mainly conceptual, except for section 2.7, which introduces a "normative" criterion.[1]

I start with Hans Radder's account saying that reality consists of human-independent, persistent potentialities or powers. I then explore the constructivist account of "resistance", which is a negative view about reality but which can be suitably complemented by a positive, potentiality-based ontology. After that a study of the Higgs boson and the hypothetical F-particle allows me to argue that real things resist being excluded from existence and persist in existing. At a perceptual level, the same view implies that real things resist disappearance by providing some effects and

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[1] On philosophical approaches, including their conceptual (or theoretical), normative, and reflexive dimension, see Radder (1996, chapter 8).

evidence, and they persist in appearing by making possible experience or evidence under appropriate conditions. Finally, the epistemological implications of the concepts of resistance and persistence are discussed. I conclude that "persistence" and "resistance" define the negative and positive meanings of the notion of "real" at different levels of discussion.

As I will explain, ontologically speaking "persistence" is more basic than "resistance" because it is the tendency to persist that explains the resistance to being annihilated or wiped out. Therefore, mentioning persistence first and then resistance is the more natural order: the former leads, or may lead, to the latter. At the same time, as I will further clarify, during actual investigations and before the thing under investigation is finally discovered/realized, we first encounter the thing's "resistance" to being excluded (or to be made to disappear); then, under the right conditions, the thing shows its "persistence". For this reason, I will sometimes use "resistance" before "persistence".

2.2 Reality as Consisting of Human-Independent Persistent Potentialities

According to Radder, the real is independent of the existence and/or knowledge of humans (see Radder 2012[1984/1988], p. 82 and pp. 169-170).[2] For this reason, he often employs the adjective

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[2] The "and/or" is explained by Radder as follows:

> The "or" applies to the scientific study of human beings, for instance in neuroscience or medical research" (2012[1984/1988], p. 170, n. 6). "If we deal with experiments on human beings, the notion of independence has to be slightly

“independent” or “human-independent” before the noun “reality” (see, e.g., 1996, chapter 4). Radder then defines the ontological and epistemological theses of realism as follows.

> Ontological thesis of realism: “The existence of the world (of nature) and its general structure is … independent of the existence of human beings and[/or] the process by which they acquire knowledge.”
>
> Epistemological thesis of realism: “concrete scientific propositions “are about” this human-independent reality” (2012[1984/1988], p. 82).

A critic might object that the latter thesis has a problem. Epistemology discusses knowledge, which may be non-propositional, while “concrete scientific propositions” only include explicit, propositional knowledge. After all, one may have “knowledge by acquaintance”, where the subject has non-propositional access to what is known. I think that our scientific knowledge of the world is conceptual, and thus it *can* be stated in propositions (see also section 6.6).[3] That said, to circumvent the critic’s objection, the thesis may be revised as follows.

> qualified to read: independence of the existence of the experimenters. (1996, p. 195, n. 3)

[3] I am generally sympathetic to John McDowell’s (1994) Kantian account that our world-disclosing experience is conceptual. This does not imply that perceptual beliefs are *already* propositional, but they *can* be stated in propositions. See also McDowell’s (2007a; 2007b) responses to Hubert Dreyfus (2005; 2007).

Epistemological thesis of realism (revised): scientific knowledge "is about" the human-independent reality.

A key concept here is the term "is about", which Radder uses in order to argue for a "referential realism", according to which a scientific term "is about" or "refers to" elements in a human-independent reality, provided that the experimental episode that is described by the term can be materially realized in a reproducible way (2012[1984/1988], section 4.4; 1996, p. 76). Apart from this, a noteworthy advantage of the term "is about" in the previous statement is that it appreciates that knowledge is intentional: knowledge is always *about* something.[4]

According to Radder, the notion of the real is independent of the existence and/or knowledge of human beings. This "independence" explains that, ontologically speaking, *(natural) reality would exist if no human being existed and/or if humans had not developed any knowledge of this reality*. The ontological thesis of realism implies that reality *does not* depend on humans. However, the thesis does not clarify the specific ontological features of this independent reality. Accordingly, we need a notion of reality that illuminates reality's *positive* nature. To fulfill this need, and in line with philosophers such as Roy Bhaskar (1978), Rom Harré (1986), and Nancy Cartwright (1989), Radder proposes an Aristotelian definition.

The Aristotelian notion of reality: the real consists of persistent potentialities or powers (or dispositions, tendencies, affordances, capacities, abilities).

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[4] My chapter 5 addresses the subject of intentionality.

To explain this notion, let us apply it to an ordinary object. A glass cup is real, but what does it imply that the cup consists of potentialities? It simply means that the cup possesses certain possibilities that can be realized in (specific) circumstances. The glass cup is break*able*, so it will be broken on condition that we put it under certain pressure. The cup enjoys certain potentialities, only some of which are already realized in specific conditions (e.g., its transparency in the presence of light). Its other potentialities are non-actualized (e.g., its breakability). And, the cup enjoys only *certain* potentialities. For instance, it is breakable but is not flexible like rubber. Similarly, scientific entities consist of potentialities, which we know via experimental activities that "realize" those potentialities. For example, we learn that salt is soluble by actually solving it in different solvents. The notion of "realization", next to "potentialities", is the second, basic ontological notion for Radder (more on realization, below).[5]

Radder does not employ the concept of "property" to explain potentialities. However, in contemporary metaphysics of science, scholars usually define potential capacities, dispositions, and powers in terms of properties (e.g., see Cartwright 1999; Mumford 2003; Bird 2007; Chakravartty 2007). For instance, Anjan Chakravartty argues that properties, such as masses, charges, densities, and acidities, confer on the things that have them certain abilities (2007, pp. 41; see also my section 4.2). Thus, a real thing enjoys properties that, under certain conditions, make

[5] Furthermore, in his 2006 book, Radder adds the ontological category of nonlocal meanings of extensible concepts. According to him, such concepts are abstract entities (see 2006, pp. 115 - 118).

it behave as it actually does. In the following, I presuppose that potentialities are indeed potential or dispositional “properties” of things.[6]

Radder argues that potentialities are human-independent. However, their socio-material realizations depend on conditions that are not, or not entirely, human-independent. He thus speaks of

> a *persistent potentiality of reality*, which as such is independent of the existence and[/or] knowledge of human beings. The *realization* of this potentiality, however, essentially requires human (material and theoretical) work. (1996, p. 79)

To realize an entity’s potentiality, scientists actualize that potentiality. In this sense, the term “realization” also implies a knowledge of the potentiality obtained through practical activities. The concepts “realization” and “creation” should not be confused. The former is “the mixture of discovery and production in scientific practice” (1996, p. 80). By realizing potentialities, humans do not create potentialities but humans actualize those potentialities to understand them. We learn that the glass cup is breakable by actually breaking it. In science, this kind of realization happens

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[6] Chapter 4 discusses how scientific properties are explored through empirical procedures. In addition, chapter 7 addresses the properties of objects according to special relativity.

in an experiment or an observation, both of which depend on skillful activities of humans.[7] Indeed, the realization of potentialities through an experimental or observational setting depends on human activities and the theoretical interpretations that guide these activities. However, this does not mean that the existence of potentialities is human-dependent. Potentialities exist in nature whether or not human beings realize them.

> The potentialities of nature persist independently of our existence and of our knowledge, even if they can be realized only if we cooperate and are able and willing to do the required material and theoretical work. (Radder 1996, p. 79)

According to Radder's Aristotelian notion of reality, "persistent" potentialities constitute reality. But could potentialities be non-persistent? I would think that, ontologically, potentialities are always persistent. In other words, entities that are not persistent are not real potentialities at all. Accordingly, the adjective "persistent" is used before the term "potentialities" in the Aristotelian ontology to emphasize that real things persist through changing human contexts (hence "human independent"). That is, persistence does not necessarily mean long-standing. Real things may appear in a very short period of time, yet they are real if they appear in *different contexts*. For instance, some elementary particles have short decay times, but they do have potentialities in particle interactions because they appear in different experimental contexts (I will further clarify this point by comparing neutrinos and so-called unidentified flying objects in section 2.5).

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[7] On experimentation, see Radder (2012[1984/1988], section 3.3) and (1996, chapter 2); on observation, see his (1996, section 4.7); see also his (2006, pp. 12-18) for a criticism of Bas van Fraassen's account of observability.

Historically, different theories may describe a certain reality differently. For instance, "mass" is defined differently from the Newtonian and Einsteinian perspectives. Likewise, the properties of an electron are different from Lorentz's and Dirac's perspectives (Radder 2012[1984/1988], pp. 83-84). Does this lead to epistemological relativism? Radder's "referential realism" suggests that the *persistence* of a potentiality secures a certain degree of *continuity* through different perspectives. According to Radder, this continuity is not conceptual-theoretical but material and formal, and therefore referential instead of representational. Successive terms, such as 'Lorentz-electron' and 'Dirac-electron', are not described by compatible concepts and theories, so there is a conceptual-theoretical discontinuity between theories in the history of science. In spite of this, successive terms can "corefer" to a certain element in reality (elements include entities, properties, processes, and the like, see Radder 2012[1984/1988], p. 93). In this regard, two conditions are necessary (see Radder 2012[1984/1988], section 4.4, specifically p. 98; see also 1996, p. 76). First, the terms refer to the same reproducible material realization, which implies that there is a numerical agreement of the values of the coreferring terms in the relevant empirical domain. Second, the terms should stand in a relation of the formal-mathematical correspondence. This is the case when the relevant mathematical equations of the terms formally correspond with each other in that domain (see 1996, chapter 3). Thus, coreference presupposes both that the separate terms refer and that they are theoretical interpretations of the same reproducible material realization.

Experimenters can express the descriptions of the material realization of an experiment in ordinary language such that laypersons can skillfully perform the experiment. Radder defines the material realization of an experiment thus:

> the whole of the experimental actions that are carried out by B [i.e., complete laypersons] in a correct way according to A [i.e., experimenters] and that can be

> described in A's instructions to B in the language in which A and B communicate with each other. (1996, p. 13; see also 2012[1984/1988], section 3.3, p. 94 and Postscript 2012, section 2)[8]

The laypersons' role shows that the sameness of the material realization does not depend on the theoretical concepts of the involved theories. Rather, it depends on the reproducibility of the experiment's material realization. If the experiment is not reproducible in principle, or if it is not reproduced in practice, the alleged experiment does not succeed in realizing a reproducible material realization (see 1996, pp. 16-17). Although the reproducibility of material realizations is not infallible (2012[1984/1988], p. 95), it (fallibly) helps recognize successful experiments and observations.

To sum up, Radder defends an ontological account of reality in terms of *human-independent potentialities*, whose *persistence* enables a *continuity* between successive theories, hence "referential realism". In chapter 6 of the dissertation, I will further address such intertheoretical "continuities". In the next section, I proceed the discussion of "what reality is" from the viewpoint of two constructivists.

2.3 Reality as Resistance: Constructivist Accounts

Bruno Latour and Andrew Pickering employ the concept of "resistance" to describe reality. In this section, I (critically) examine their views. To start with, Latour states that

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[8] Those who have watched the series *Breaking Bad* would agree that, in their laboratory work, Walter White is the experimenter and Jesse Pinkman the layperson!

> reality as the latin word *res* indicates, is what *resists*. What does it resist? *Trials of strength*. If, in a given situation, no dissenter is able to modify the shape of a new object, then that's it, it is reality, at least for as long as the trials of strength are not modified. (1987, p. 93)

Apparently, an analogy with politics underlies Latour's account of resistance. A political party exists insofar as it resists being wiped out through "trials of its strength" by other parties. The ruling party is in power for as long as it successfully resists attempts to destroy it. A new party, as a "dissenter", may enter the stage and gain power, and thus "modify" the balance of power. Similarly, in the world of science, as long as an entity is actively referred to by scientists, it is real, or more precisely, it is considered to be real in a (scientific) community. If a putative entity (e.g., the ether) does not resist being *replaced by* another entity (e.g., the electromagnetic field), the old one is not considered to be real any more. The resistance an entity can offer depends on its position in the network of human and non-human actors. This relational account is based on Latour's famous "actor-network theory". An entity successfully resists change when other actors of the network, including tools, graphs, experimenters, and policy makers, support it (or are its allies, so to speak).

Another concept, similar to resistance, that Latour uses to describe the reality of objects is "recalcitrance".

> Natural objects are naturally recalcitrant; the last thing that one scientist will say about them is that they are fully masterable. On the contrary, they always resist and make a shambles of our pretentions to control. … [M]icrobes, electrons, rock seams are utterly *uninterested* in what human scientists have to say about them. (Latour 2000, p. 116)

This implies a kind of realism, because the resistance or recalcitrance of entities, rather than the mere interests or expectations of scientists, has an effect on what we count as real in science.[9] On this view, an object is real if it resists being replaced (see 2000, p. 112).

In a similar manner, Pickering speaks of "resistance". He maintains that the outcome of scientific practice is

> a *dialectic of resistance and accommodation*, where resistance denotes the failure to achieve an intended capture of agency in practice, and accommodation an active human strategy of response to resistance, which can include revisions to goals and intentions as well as to the material form of the machine in question and to the human frame of gestures and social relations that surround it. (Pickering 1995, p. 22)

Accordingly, "resistance" implies a kind of "failure" of scientists to achieve what they want because it is not in agreement with reality. To illustrate this, Pickering puts forward the case of the physicist Giacomo Morpurgo, whose first observation of fractional electrical charge resisted to be in agreement with his expectations. Morpurgo then made several new "accommodations". That is, he altered the apparatus, revised his theoretical model of the apparatus, and modified his theory of the phenomenon. Finally, he observed what he expected. According to Pickering, such processes constitute a "dialectic" of resistance of the material reality and the accommodations to that world,

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[9] Francisco Salinas interprets Latour's view of science as "pragmatic realism", where "[r]esistance is what nourishes pragmata as the pillar of reality" (2014, p. 10). Matthew Watson (2015) confirms this interpretation. See also Latour (1999), where he depicts himself as a realist.

including changes of experimenters' intentions and expectations, of their theories, and of the instruments they employ in the experiment. Pickering dubs his view "pragmatic realism": It is realist because "*how the material world is* leaks into and infects our representations of it" (Pickering 1995, p. 183). However, it differs from "traditional philosophical realism" in that scientific representations should not be considered as correspondence links: "Pragmatic realism specifies nontrivial links between knowledge and the world that are quite independent of relations of correspondence" (Pickering 1995, p. 183).

Thus, these constructivists explain why reality, in many cases, resists conforming to scientists' expectations and interests. As such, this is definitely a valuable view. However, the main drawback of the notion of "resistance" is that it is a *negative* concept that needs to be complemented by a positive one. After all, many cases of the "failure" of scientists to achieve a theoretical aim or the fact that reality may be "uninterested" in what scientists say about it, should be explained by a positive feature of reality that (partly) causes the failure or dissatisfaction of the scientists. Furthermore, in quite a few cases, scientists are successful in offering novel predictions and explanations, and thus in these cases, reality "expresses an interest" in scientists' theories. A famous instance is Arthur Eddington's eclipse observation, as well as many further observational tests, that confirmed Albert Einstein's prediction about the gravitational deflection of starlight passing near the sun. Such examples imply that reality does not always resist scientists' expectations. A positive account of reality is needed to explain theories' conformity with reality. Radder's conception of reality, which has been developed as an alternative to the constructivists views (see 2012[1984/1988], Postscript 2012, section 3.5; see also 1996), can explain both the interest and the disinterest of reality in scientists' theorizing. According to the Aristotelean notion of reality, potentialities are *power*ful, so they do not simply "obey" what scientists expect or want.

Instead, scientists have to "pay careful attention to" reality (through what Pickering calls "accommodation") to understand them. In a less metaphorical language, the potentialities of (natural) reality exist in a human-independent way. Experimenters know them by actively realizing them through experimental processes. Humans cannot fabricate those powers. Even if all experimenters suppose that a certain power exists, it will not be real as long as it does not enjoy independent existence. The experimenters' supposition may have real social implications. However, this does not introduce a new power to already existent potentialities of (natural) reality. Indeed, the resistances of reality to scientists' expectations and interests rest on human-independent potentialities. In like manner, those predictions and explanations are successful that respect the persistent potentialities of reality.

Constructivists are often blamed for discounting the role played by natural reality. For instance, Allan Franklin and Slobodan Perovic (2019) assert that the natural world "never seems to be decisive in any of Pickering's case studies." Likewise, Ian Hacking claims that "[c]onstructivists think that the [theoretical and experimental] reasons are not decisive for the course of science" (Hacking 1999, p.91; cf. Radder 1996, pp. 86-89). The realist criticism of constructivism is, I think, mainly due to constructivists' lack of an explicitly positive, ontological view about reality. To avoid this criticism, the constructivist account of reality should consistently locate its ontological roots in a potentiality-based ontology, according to which reality consists of persistent powers or potentialities. Constructivists should indeed presuppose this ontological conception of reality to explain their account of resistance positively. That is, the resistance to be replaced by "dissenters" and to follow scientists' expectations and interests relies on the persistent potentialities or powers of things. This view acknowledges the indispensable role of a human-independent, persistent reality in determining knowledge of scientific entities and their properties.[10]

So far, I have argued that the constructivist account of resistance is a negative view, which should be complemented by a positive, potentiality-based account. The following section illustrates the ontological concepts of "persistence" and "resistance". Sections 2.5 and 2.6 discuss the relevance of these concepts to the perceptual and epistemological levels.

2.4 Persistence and Resistance: Higgs Bosons versus F-particles

Let me start this section with a conceptual analysis. "To persist" may literally mean "to continue existing". Real things persist in the sense that they continue to exist through changing human contexts. When a thing continues to exist, it "resists being excluded from existence" as well. After all, "persistence" and "resistance to be excluded from existence" are two sides of the same coin. Two related statements can be made at the ontological level thus: real things "resist" being excluded from existence, and real things "persist" in existing. The former provides a negative view of reality; the latter presents a positive one. Conceptually, both point to the same reality. However, in practice, the former is more suitable to describe a thing to be realized (or to be discovered), or

[10] This does not imply that scientific knowledge is unconditionally true. I will argue in the next chapter that scientific knowledge is always bounded by (instrumental and theoretical) perspectives. Thus, I agree with Pickering that there are "nontrivial links" between knowledge and reality different from the "relations of correspondence". The scientific representations are always perspectival.

in other words, a thing whose existence is under investigation. The latter, in contrast, better describes a thing that is already realized (or discovered) in the right conditions. My approach here is not limited to the final realization/discovery but also addresses the processes that precede these final results. Adding this process dimension (here and at various other places) is an important feature of my thesis.

Persistence is not unconditional. Real potentialities only demonstrate their persistence in the right conditions, where appropriate observational/experimental setups are prepared. The presence of the right conditions is contingent and may not always occur. Nor do observers/experimenters know from the beginning the characteristics of the right conditions. They make use of theories and models and of other observers/experimenters' practical experiences, and they also attempt to discover and provide appropriate observational/experimental setups by heuristic methods or sometimes even by trial and error. As a result of their attempts, a significant correlation may be established between observational/experimental instruments and the behavior of an entity, and therefore a property of the entity is being realized/discovered. In this process, precluding possibly disturbing external influences is a substantial part of the observational/experimental work (see Radder 2021, section 2). Accordingly, a lot of work needs to be done so that the right conditions, which are necessary for the display of "persistence", are prepared. That said, entities may resist being excluded from existence even when the right conditions are *not fully* prepared. This may be the case when these conditions are only partly prepared for different reasons: observational/experimental settings are not perfectly arranged, the conditions are not completely appropriate for that specific entity under investigation to be realized/discovered, not all disturbing influences are prevented, not all possibilities for the observation/detection of the entity are as yet investigated, or for other similar reasons. In these cases, the real entity may still resist being considered as non-existent, which accounts for why unsuccessful experimenters/observers are still reasonably hopeful that by providing *better* conditions they may observe/detect the supposed entity in their future attempts.

In the following, I study two experimental endeavors regarding the Higgs boson and the hypothetical F-particle (in which "F" is a Greek letter, which is pronounced "digamma" and whose shape is a little different from the English letter "F"). The Higgs boson is an elementary particle and plays a unique role in the Standard Model of particle physics. It explains why elementary particles such as W and Z bosons are massive, while photons and gluons are massless. This boson was predicted by François Englert, Robert Brout, Peter Higgs and others in the mid-1960s and it was finally discovered in 2012. In contrast, the F-particle was a hypothesis supposed to explain an unexpected anomaly in data collected in 2015 (at CERN in Geneva). However, further analysis and the data collected in 2016 combined with the previous data showed that the anomaly was a statistical fluctuation rather than the evidence of a new particle.

The central claim is that before its detection in 2012, the Higgs boson was resisting being excluded as non-existent, mainly because not all the masses it might have had been investigated. After its detection in 2012, it persisted in reproducible experiments. In contrast, the putative "F-particle" has not even resisted its exclusion. Its failed detection was not caused by imperfect experimental conditions; the probable reason was rather that the supposed particle did not exist. Please note that in explaining these cases my primary objective is to show the ontological features of real entities. Accordingly, my discussion concerns ontology rather than epistemology. I will explain the characteristics of a justification of realist knowledge briefly in section 2.6 and at length in the subsequent chapters. This section intends to clarify these two evident definitions of reality: 1- reality resists being excluded from existence and 2- reality persists in existing.

Let me start my discussion of the Higgs boson with an ordinary example. Suppose that one thinks that there is a beetle in one's home, and then one starts examining different rooms in search of the beetle. If it really exists in the home, and indeed because of its very "existence", the beetle resists being considered non-existent (despite its desire not to be found and killed!). Therefore, probably after one's active attempts to find it, the beetle will eventually be found in a room. In this case, the beetle resists being excluded from existence in this home. When it is trapped, one understands that it exists. Similarly, and providing that we disregard other complexities, there are different search probes into the Higgs boson. Each probe investigates whether the boson with a *specific mass* exists. Certain masses for the Higgs boson were excluded but the Higgs particles resisted being excluded from all possible probes. Finally, when the particles are realized/discovered, their persistent existence shows itself in experiments that reproduce the right conditions, as the consideration of the correct mass is a condition for its persistence in the experiment. Below, I further explain this example.

The Large Electron-Positron (LEP) Collider operated from 1989 to 2000 at CERN in order to measure the properties of Z and W bosons and also to search for the Higgs boson. Direct searches for the Higgs boson at the LEP set a lower bound on the mass of the Higgs boson: If there is such a particle, its mass must be more than 114 GeV/c^2 (Abbiendi et al. 2003). Until 2001, the Higgs boson hypothesis was neither confirmed nor rejected. The Tevatron, a proton-antiproton collider at the Fermilab in Chicago operating in the years 1986 to 2011, had probed masses of 100-200 GeV/c^2 in searching for the Higgs boson but couldn't find any evidence that could be a conclusive indication for a Higgs discovery (the evidence is an excess in mass (or energy) in the spectrum of the invariant mass of child particles to find a probable parent particle).[11] However, physicists couldn't completely reject the existence of the Higgs particle either. The Tevatron only excluded Higgs masses between 162-166 GeV/c^2 (Aaltonen et al. 2010; Buehler 2008). Although different searches might exclude the existence of the Higgs boson, it could also have some other mass. Up to this point, although the Higgs boson was not discovered, it resisted being considered non-existent.

The Large Hadron Collider (LHC) was subsequently designed at CERN to reach higher energies in order to further constrain the parameter space for the mass of the Higgs boson. Only if the total parameter space would have been probed and nothing would have been found, then physicists could conclude that the Higgs particle does not exist. All experiments before 2012 succeeded in excluding certain masses for the Higgs boson, and thus in those experiments the Higgs bosons were in fact excluded. However, the particle resisted being entirely excluded from

[11] When a parent particle decays into two child particles, the invariant mass of the two child particles is equal to the mass of the parent particle. Physicists search for an excess in the spectrum of the invariant mass of particles to find a probable parent particle. To search for an excess in a di-photon spectrum, i.e. the spectrum of the invariant mass of two photons, physicists first select events in the data where two photons exist in the final state of a collision. Then, they calculate the invariant mass of two photons and make a histogram of this variable. If the (parent) particle exists and can decay into two photons (child particles), then it will create an excess which is visible as a small peak in this histogram. The invariant mass where the excess happens is equal to the mass of the particle, which has decayed into the two photons.

existence. Finally, ATLAS and CMS collaborations at CERN using the combined LHC data, at the energies of 7 and 8 TeV, reported the discovery of the Higgs boson with a high statistical significance[12] (Aad et al. 2012; Chatrchyan et al. 2012). The proton-proton collisions at the LHC (with an energy of 13 TeV) were also in agreement with the previous results at 7 and 8 TeV (Tao 2018).

The Higgs experiments in proton-proton collisions at different collision energies, including 7, 8, and 13 TeV, confirm the reproducibility of the experiment. Since 2012, physicists have also collected further data at the LHC and measured the mass of the Higgs boson with unprecedented accuracy (Sirunyan et al. 2020). In sum, the Higgs boson has resisted being excluded while experimenters were probing it, and after its discovery in 2012 it manifested its persistent existence in reproducible experiments.

The so-called "F-particle" has had a different fate in comparison with that of the Higgs particle. In December 2015, ATLAS and CMS collaborations at the LHC reported an excess of mass in the di-photon spectrum (Aaboud et al. 2016; Khachatryan et al. 2016). In the first half of 2016, over 500 papers were written to examine the excess and offer possible explanations for its nature. The signal at first showed a rather high statistical significance (bigger than 3.4 sigma), which could be considered as an indicator of a possible, new particle, the so-called "F-particle" with a mass of about 750 GeV/c^2, which decays into two photons. However, accumulating data and further

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[12] A high significance level means more than 5σ deviation from the background, which implies that the null hypothesis (the background: no Higgs boson) could be rejected in favor of the alternative hypothesis (the signal: Higgs boson with mass around 125 GeV/c^2).

analysis excluded the possibility that the "F" particle exists. Unlike the Higgs boson, the putative F-particle has not put up enough resistance to being excluded. Nor has it entered into the class of entities showing persistence in reproducible experiments.

The present section has focused on the "ontological" feature of real things, which resist being excluded from existence and whose existence persists. This ontological view has a "perceptual" parallel, which the next section clarifies. The perceptual level addresses the experiential or evidential characteristics of real things.

2.5 Persistence and Resistance in Perceptual Processes

This section explains what "persistence" and "resistance" mean at the *perceptual* level. The main question is how we *perceive* reality if it ontologically consists of powers or potentialities that persist in existing and resist being excluded. To answer this question, I use the phenomenological account of perception, according to which the perceiving subject is both *active* and *passive*. The active dimension implies that things manifest themselves through *our bodily or instrumental engagements* with them. The passive (or receptive) side acknowledges the fact that perceptual "experience is shaped by the *insistence* of the world" (Gallagher and Zahavi 2012, p. 111, emphasis added).[13] I employ the concepts of "persistence" and "resistance" to explain the passive

[13] My view on perception is also akin to the account of the predictive mind, a much-discussed approach in modern theories and philosophy of perception. It says that the brain is a hypothesis-testing mechanism that minimizes the error of its predictions about its received input from the

feature of perceptual experience of reality. Let me first put forward the account and then clarify its terms and support it.

> The perceptual account of persistence and resistance: veridical experience persists in appearing and resists disappearing across several modes of our bodily or instrumental engagements.

The passive or receptive side of perception, the persistence and resistance of perceptual experience, and its active dimension, that perception is a kind of active human involvement through embodied or instrumental engagements, are both included in this account. “Appearance” includes the occurrence of mere appearances and of veridical experiences. The claim is that only veridical experiences persist and resist. Experience comprises ordinary perception (perceived by embodied organisms) and observational and experimental evidence (obtained through instrumental engagements). Thus, the terms “experience” and “perception” should be understood broadly to cover not only ordinary perception but also (the evidential results of) scientific observation and experiment.

The passive feature of perception ultimately relies on the *power*ful nature of reality. An experience persists in appearing and resists disappearing because the thing to which the experience belongs exists independently of the perceivers. One should note that the passive side of perception implies that *perceivers* are (partly) passive in perception. It does not mean that reality is passive.

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world. The theory implies that, although what we perceive originates in the world, its content depends on our action and attention. See Clark (2013) and Hohwy (2013).

A powerful reality is active in this respect, and for this very reason perceivers are and should be (partly) passive in their perceptual experience of real things.

Despite the connection between the ontological and perceptual levels, persistence and resistance have different implications at the two levels of discussion. Persistence and resistance are relations: A "persists in" B and "resists" B'. At the ontological level, A is a "thing", B is "existing" and B' is "being excluded from existence". Accordingly, a thing persists in existing and resists being excluded from existence. At the perceptual level, similarly, A "persists in" B and "resists" B', but A, B, and B' are defined differently. A is "the results of ordinary perception or observational/experimental processes", B is "appearing", and B' is "disappearance". Accordingly, the results of ordinary perception or observational/experimental processes persist in appearing (by making possible experience or evidence under appropriate conditions) and resist disappearance (by providing some signs or effects). In addition, the difference between "persisting in appearing" and "resisting disappearance" is similar to what I said about the ontological level. When we start engaging with a new thing, it may not be manifest persistently right from the beginning, while it may resist disappearing by displaying some signs, indications, or traces. After its manifestation under appropriate conditions, however, the thing's appearance (namely, either the results of ordinary perception or the results of scientific observational/experimental processes, the latter results being the "empirical evidence") persists in appearing.

"Engagement" here entails being significantly involved with something to *experience* it. Specifically, I- ordinary perception, II- scientific observation, and III- experimentation are cases of our engagement with things. The ordinary perception of an object or the empirical evidence of a scientific entity resists disappearing (before its realization/discovery) and persists appearing (after its realization/discovery). This is the case when we actively engage with an object by means of bodily interaction (and by using concepts that interpret the object) or with an entity by the mediation of technological instruments (and the theoretical interpretations that guide the observation/experiment).

Veridical experiences can appear across *several modes* of our engagements. That is, the engagement with a thing, in ordinary perception or scientific observation and experimentation, should be possible once again in a replicable way. A variety of different sensory capacities, such as seeing (from different angles), listening, smelling, touching, moving (approaching and taking a distance) constitute our modes of ordinary perception. A number of these modes should contribute to acquiring a veridical experience. Likewise, several processes of observation/experimentation should always be involved to be confident that scientific evidence is veridical. In the following, I first explain the veridicality of our ordinary perception, and then, I extend it to scientific observation and experimentation.

Human beings enjoy embodiment, so they can bodily engage with things to distinguish veridical perceptions from mere appearances, illusions, and hallucinations (see Merleau-Ponty 1962, pp. 296–297). For instance, we can approach a mirage and see it from different angles to become confident that its appearance is different from the veridical experience of an actual lake. My point here is that a veridical experience involves *several modes* of perception. Suppose that one thinks to see something in a dark area, but one is sure neither if there is something there nor what the properties of that supposed thing are. One can perform different actions to check if what is seen is a veridical experience. One can look at the thing from other positions, flash a torch on it, throw a small piece of grit to it to detect its reaction and to listen to the sound made. One can also collaborate with one's friends in order to share what is perceived. These and similar practices constitute several modes of engagement with the thing. If the experience of the thing itself first “resists disappearing” and finally “persists in appearing”, and different modes of our engagement with the thing are in agreement, then the experience of the thing is veridical. The experience of the thing’s *properties* may also resist and persist. For example, if there is a car in that dark area, first, one’s experience displays the signs of the car. Even if one initially supposes that the thing must be, say, a wild animal, that thing resists being experienced as anything but a car. And sooner or later, after several active engagements with it, it finally appears as a car. The more our experience is based on several modes of active engagement with it, the more veridical the experience of the thing and its properties are.

In similar ways, scientists devise several practical methods to engage with entities. These engagements, mainly through technological instruments, help scientists check if the evidence of a supposed entity (and its properties) is veridical (cf. Vallor 2009). Scientists can never be *completely* confident that observational/experimental results are veridical because it may always be the case that what has appeared to them is a stubborn, tenacious illusion or hallucination rather than a piece of veridical evidence. However, such a perfect level of confidence is not necessary; to the extent that their results rely on different processes of obtaining scientific evidence, they can be confident that the evidence is veridical.

In the remainder of the section, I clarify some further details of the perceptual account of persistence and resistance. The first point is that perception is a result of *collaborative* activities. In ordinary perception, perceivers share their experiences to recognize a veridical experience. Likewise, scientific observation and experimentation, which is made in a community of observers or experimenters, the empirical results should be confirmed by a broader scientific community. To emphasize this characteristic, I have stated in the perceptual account of persistence and resistance that "our" [in the plural] bodily or instrumental engagements enable us to have veridical experiences.

Another issue is that we may experience a thing only for a very short time. For example, soap bubbles quickly disappear upon most of our bodily engagements with them, but we do not doubt that they are real. Why? Because anytime we blow in soapy water, with a pipe or a straw, bubbles rise. Soap bubbles appear only for a short period. Similarly, in particle physics unstable particles undergo quick decays. However, (the evidence of) an unstable particle keeps reappearing in different experimental contexts, although within a very short time period. The fact that the duration of the appearance is short does not mean that it does not appear. Every time appropriate experimental conditions are replicated, the evidence of the particle appears persistently, even though shortly, because persistent appearance in experience is based on the persistence of human-independent potentialities that can be realized (even if shortly) under the right conditions. For example, although they are very elusive because they easily pass through ordinary matter, neutrinos are real, and thus their evidential effects persist every time appropriate experimental settings are prepared. Accordingly, "persistent" does not mean "long-lived". Real entities may appear in different experimental contexts even for a short time.

Consider, on the other hand, so-called unidentified flying objects (UFOs). One can hardly claim that they resist disappearing due to our engagement with them, because no specific condition to start engaging with these putative objects (through which we might experience even their sporadic signs) has so far been introduced. Even if one could claim that some effects of UFOs are somehow observed somewhere, no appropriate conditions for their persisting appearance have ever been determined. Thus, the alleged observations of UFOs are not veridical. Most pseudo-scientific posits are alike. Suppose that one would, by hook or by crook, claim that their effects resist disappearing (owing to the contention that, once in a while, people claim that these posits have manifested themselves to them), and that we charitably accept that claim. Even in this case, these posits do not persist in appearing through specific engagements with them, and therefore the experiences of these posits can hardly be veridical.[14]

A further remark is that there is no fundamental difference between our sensory capacities and experimental/observational instruments *with respect to the fact that* all are by their nature instrumental.[15] That is, they are instruments working according to the regularities of nature. It is true that instruments, such as eyes, ears, brain scanners, telescopes, or more complex apparatuses like those in CERN, are each sensitive to a specific aspect of reality. Still, all are similar in *mediating* our experience of reality (see also Radder 2006, pp. 87–90). All make it possible to engage with reality. At the same time, each engagement is bounded by the mediation structure that makes available the interaction with the world. In other words, we engage reality through our bodily apparatus and scientific instruments, both of which are perspectival. Thus, humans ordinarily experience the world from a colored perspective (Giere 2006a, chapter 2). Similarly, a scientific instrument is always conditional on what it is sensitive to. For example, a gamma telescope is responsive only to gamma rays and a CAT scan only shows the structure (rather than

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[14] I learned the example of UFOs and its contrast with the case of neutrinos from an interview with Frank Wilczek, an American theoretical physicist and a Nobel laureate, by Robert Lawrence Kuhn in an episode of the Closer to Truth series.

[15] There are of course differences between ordinary perception and instrumentally mediated observation, which I shall discuss in sections 5.4 and 5.5.

the function) of the brain (Giere 2006a, chapter 3). We can experience neither ordinary objects nor scientific entities without the mediation of embodied or instrumental perspectives. Moreover, our experience of things is always conceptual. In ordinary perception, our perception is concept-dependent. In science, observational and experimental results are interpreted by theoretical concepts. Therefore, in addition to embodied and instrumental perspectives, theoretical perspectives should also be considered. As a result, engaging with things without the use of any perspective, is beyond the human condition (see also my subsection 3.3.2). The resistance and persistence a thing shows are likewise perspectival in the sense that they are consistently demonstrated through embodied or instrumental perspectives and are interpreted by (theoretical) concepts. According to the account of perceptual persistence and resistance, we may rely on our experience when several modes of engagement successfully contribute to it. However, this does not imply that our experience can be non-perspectival or from no-where. That is, even in the case of "overlapping perspectives" (see section 3.4), engagements with a thing, and therefore the persistence and resistance that we experience from that thing, are not perspective-less. The perspectivism I am defending here concerns the perceptual level of human experience. Chapter 5 further discusses this level. Next chapters will also defend perspectivism at the epistemological level. Ontologically speaking, nevertheless, powers or potentialities are human-independent. Thus, my view is Aristotelian at the ontological level and it is Kantian at the perceptual and epistemological levels since we cannot experience or know things in themselves.

Let me finish this section by explaining that my usage of "resistance" in this and the previous sections has been in agreement with two points Martin Heidegger makes in *Being and Time* about Wilhelm Dilthey's and Max Scheler's notions of resistance. These two points pertain to both ontological and perceptual levels. Heidegger maintains that "*if* "Reality" gets defined as "the character of resisting", we must notice two things: first, that this is only *one* character of Reality among others; second, that the character of resisting presupposes necessarily a world which has already been disclosed” (1962[1927], p. 254). About the first point, I should say that my discussion in this chapter has not been restricted to “resistance”. In addition, reality’s ontological characteristics, such as its “human-independence”, its “being potential and powerful” and its “persistence” have been addressed and defended. Furthermore, I have addressed the notion of “persistence” and “resistance” at different levels, whose distinction clarifies the question of what reality means. Heidegger’s second point is also valid. Resistant reality is disclosed through our engagements, when we actively experience things in the world, that is, in our realm of possible, interrelated things. In Heidegger’s words, “*[t]he experiencing of resistance—that is, the discovery of what is resistant to one’s endeavors—is possible ontologically only by reason of the disclosedness of the world*. The character of resisting is one that belongs to entities with-the-world.” (1962[1927], pp. 253–254). I agree with this statement and would like to add that it is correct both about ordinary objects and about scientific entities whose resistance (and persistence) shows itself by means of embodied or instrumental perspectives and is interpreted by (theoretical) concepts in the “world” of scientists and experimenters.[16]

[16] I will not further discuss Martin Heidegger’s conception of “the world”. Nevertheless, section 5.2 addresses Edmund Husserl’s related concept of “the lifeworld”.

2.6 The Epistemology of Persistence and Resistance

At the epistemological level, "resistance" results in a kind of falsificationism in the sense that a theory is truthful insofar as it resists being falsified by refutations and counter-instances. A good theory is capable of withstanding all efforts to refute it. I should clarify in advance that I do not intend to discuss the methodological thesis of falsificationism. My previous discussions do not involve the "method" of science or what it should be.

The epistemological thesis of falsificationism is still a "negative" view. All it can say about current, successful theories is that they have resisted falsification up to now. 19th-century ether theorists would have claimed the same about the ether theory, but the fact that a theoretical posit has not been falsified so far is not enough to conclude that the posit refers to something real. The posit may be falsified in the near future. An extra reason is necessary to accept that the theoretical posit is really about something real and that the posit's description of that something is truthful. As explained in section 2.3, Latour's conception of resistance encounters the same problem. He presents a retrospective thesis that attributes reality to those entities that have so far resisted being replaced in "trials of strength". However, the (putative) entities may lose their power in the future when other actors of the network, including tools, experimenters or policymakers, no longer support them. Accordingly, a realist epistemology should (also) provide a criterion for prospectively determining what will probably remain a justified part of the scientific image of reality (more on the criterion for reality in chapter 4).

As the positive side of resistance, the concept of "persistence" is promising in providing a prospective criterion. At the epistemological level, persistence leads to the criterion that a thing may be taken to be real and our knowledge of it will probably remain in the future, if it is detectable/measurable in a variety of independent ways. A real thing is obtained by means of “replicable experiments or observations”, which is a specific case of “reproducible experiments and observations”. Replicability implies the reproducibility of empirical *results* using (possibly radically) different processes (Radder 1996, pp. 18 and 22-24). For instance, a gas and a mercury thermometer each measure an object’s temperature utilizing independent processes. For another example, consider the case of the Higgs boson, whose production mechanism at the LHC has always been the same. So far, physicists have been able to produce the Higgs particle only in reproducible proton-proton collisions. The boson has been detected in a “reproducible” but not in a replicable, way. Thus, our level of justification for the Higgs boson, as well as our knowledge of its properties, is still bounded by a specific set of experiments. But our justification in the boson will increase inasmuch as it is realized in “replicable” experiments, whose results are obtained through several independent processes. In fact, physicists are planning to design new colliders to study other properties of the Higgs boson with the aid of possibly different production mechanisms.[17] If they succeed, a higher level of justification will support the belief in the boson and its properties.

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[17] An International Linear Collider (ILC), a Compact Linear Collider (CLIC), a Circular Electron-Positron Collider (CEPC) and Future Circular Colliders (FCC) will all probe some properties of the Higgs particle with a precision which has not been accessible to the LHC (Abramowicz et al. 2017; De Blas et al. 2020). The European Strategy for Particle Physics stresses the importance of having a Higgs “factory” to measure the Higgs properties in order to understand it more fully (see Vachon 2019).

The concept of "replication" is one of the epistemological concepts that I will employ in the next chapters. In general, a real thing can manifest itself in "replicable" experiments or observations, is represented from "overlapping perspectives" (see chapter 3), is "robust" (chapters 3 and 4), provides "explorable" evidence (see chapter 5), and can be described by successive theories such that a later theory can restate the truthful descriptions of the preceding one (chapter 6).

2.7 Conclusion: What Reality Is

This chapter has been interdisciplinary. By building on the work of Radder, Giere, Latour and Pickering, I have bridged the gap between philosophy of science and STS debates on realism. Further, I have studied the experimental cases of the Higgs boson and the F-particle to analyze the ontological meanings of persistence and resistance. I have also indirectly reconciled the phenomenological and analytic analyses of science. (The discussion of this subject will be continued in chapter 5).

The chapter has argued that Latour's and Pickering's accounts of resistance are acceptable provided that their views rest on a positive, ontological account. The Aristotelian notion of reality can offer this positive account. Furthermore, the Higgs and F-particle cases enabled me to clarify the concepts of resistance (before the discovery/realization of the entity) and persistence (after its discovery/realization in the right conditions). In line with this ontological view, I have also developed an account of perceptual persistence and resistance. A real thing cannot resist manifesting its signs or effects during discovery/realization processes and persists in appearing under appropriate conditions after its discovery/realization. Finally, at the epistemological level, resistance and persistence imply respectively falsificationism and a realism based on replication, overlapping perspectives, robustness and explorability.

In conclusion, the chapter has explained what reality is. Three levels of discussion have been explored: ontological, perceptual, and epistemological. At each level, the real's meaning incorporates two interrelated dimensions: persistence and resistance. Table 2.1 summarizes the positive and negative sides at each level. The next chapters mainly address the epistemology of the positive level.

Level of discussion	*Positive side*	*Negative side*
Ontological	Persistent potentialities or powers	Resisting being excluded from existence
Perceptual	Persisting in appearing (by making possible experience or evidence under appropriate conditions)	Resisting disappearance (by providing signs or effects)
Epistemological	Persisting across several perspectives (prospective)	Resisting being falsified (retrospective)

Table 2.1

Chapter 3 Entity Realism Meets Perspectivism

3.1 Introduction

This chapter begins with discussing the views of Ronald Giere (1938-2020). The development of entity realism is indebted in part to his study of experimental practice. He has criticized antirealist versions of constructivism. Perspectivism is notably associated with him as well. Giere is also known as a promoter of the cognitive approach to the philosophy of science, but I focus on his views concerning the scientific realism debate.

This chapter argues that Giere's views on scientific realism provide a basis to reconcile entity realism and perspectivism. Thus, the chapter's aim is philosophical rather than historical or exegetic. It intends neither to present a new reading of the history of philosophy of science nor to provide a detailed exegesis of Giere's work. Instead, it primarily aims to show that contemporary perspectivists and entity realists can benefit from each other's valuable ideas for the purpose of acquiring a more comprehensive and more compelling view. For this purpose, Giere's concept of "overlapping perspectives" is of central importance. This concept, which surprisingly does not play an essential role in Giere's epistemological view, can ground the reality of entities. Moreover, it allows us to see a significant affinity between entity realism and perspectivism.

Thus far, Giere's versions of realism have not been studied in any detail. In this chapter, I fill this gap by discussing his entity realism, constructive realism, perspectival realism, and their mutual relations. This chapter is not restricted to explaining Giere's views. It also contrasts and compares them with the relevant literature. Specifically, it connects Giere's concept of "overlapping perspectives" with the notion of "robustness". Both concepts imply that a real thing can be presented from a variety of independent perspectives. Moreover, because the chapter concerns multiple experimental, instrumental, and measurement perspectives, it relates to New

Experimentalism, the philosophical movement that criticizes the theory-dominated approach to the philosophy of science and emphasizes the neglected role of experimentation and instrumentation in scientific practice (see Boon 2009, section 3; Chalmers 2013[1976], chapter 13). New Experimentalists include Ian Hacking (1983), Nancy Cartwright (1983), Hans Radder (2012[1984/1988]), Robert Ackermann (1985), Allan Franklin (1986), Peter Galison (1987), Ronald Giere (1988), Alan Chalmers (1990) and Deborah Mayo (1996), among others. This chapter does not discuss the views of all these scholars. It focuses on Giere, but it also considers Hacking, Cartwright, Radder, and Chalmers.[1]

Sections 3.2 and 3.3 provide an account of Giere's contribution to the scientific realism debate. Section 3.2 concerns entity realism. Section 3.3 explores constructive realism and its advanced version: perspectival realism. Sections 3.4 and 3.5 bridge the gap between the concepts "robustness" and "overlapping perspectives", arguing that perspectivism and entity realism are compatible and complementary.

3.2 Entity Realism

As portions of our successful theories turn out not to represent anything real, selective realists therefore strive to suggest criteria which separate the representational parts of scientific theories from the non-representational parts. An important form of selective realism is entity realism, implying that our scientific knowledge of entities, which results from appropriate experimental

[1] Chapter 4 discusses recent views of entity/*experimental* realism, which are akin to New Experimentalism.

uses of unobservable entities such as protons and genes, deserves realist commitment. Other scientific beliefs that merely rely on models, theories, or general laws are not worthy of realist commitment. Accordingly, entity realists "select" the former knowledge and not the latter kinds of scientific beliefs.[2]

Hacking first coined the term entity realism. He suggests that if one can causally manipulate an entity by intervening in other phenomena, one is justified to believe in the existence of that entity. Hacking's slogan is that when one can spray entities (such as electrons), they are real (see, e.g., Hacking 1983, p. 23). In the second half of his *Representing and Intervening* he develops a complex network of arguments in order to underpin this slogan. His main claim is that our knowledge of an entity's existence can survive even if the theoretical concepts that interpret the instruments or experimental results change. Hacking's argument for this claim is that experimenters can manipulate the entity under investigation to observe its effects. In his words, "entities that in principle cannot be 'observed' [by naked eyes] are regularly manipulated to produce new phenomena and to investigate other aspects of nature" (Hacking 1983, p. 262). Explicit examples of manipulable entities are those that can be used "as tools, as instruments of inquiry" (Hacking 1989, p. 578). In addition, the experimenters' belief in an entity increases when they can investigate the entity through different mechanisms, hence Hacking's "argument from coincidence". For instance, when different microscopes—whose working is based on other

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[2] About selective realism and a description of entity realism, see also Chakravartty 2017a, section 2.3.

mechanisms—detect a microscopic entity, one is justified to believe in the entity. (see Hacking 1983, p. 201; and 1985, pp. 146–147).[3]

Cartwright (1983, essay 5) is also known as an entity realist. She makes a distinction between theoretical and causal explanations. Theoretical explanations are based on laws, which can be explanatory without being true. Causal explanations, by contrast, cannot work without being true: "to accept the explanation is to admit the cause" (1983, p. 99). Accordingly, an entity can play its causal, explanatory role only if it actually exists. Accordingly, Cartwright "believe[s] in theoretical entities" (1983, p. 89; see also p. 92) whose causal roles can be established in "direct experimental testing" (1983, p. 98). In this regard, she agrees with Hacking that "we manipulate the cause and look to see if the effects change in the appropriate manner" (1983, p. 98).[4]

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[3] For an overview of Hacking's case for learning from experiments, see Franklin and Perovic (2019, section 1.2.1); see also Miller (2016) for different interpretations of Hacking's argument.

[4] Egg (2017; 2014, Chapter 2) also interprets Cartwright as an entity realist. See also Clarke (2001) for the difference between Hacking's and Cartwright's arguments for realism. For a criticism of Cartwright's entity realism, see Hitchcock (1992), Pierson and Reiner (2008), and Psillos (2009, chapter 6). In this dissertation I discuss Cartwright's views only as a part of the literature review on entity realism; I neither elaborate nor defend her (various) accounts of causation. This chapter focuses on Giere, the next one on recent entity realists. My argument in the next chapter is partly on the basis of Mathias Egg's "causal warrants", which is an alternative to Cartwright's "causal explanations". Cartwright's (later) views on causation, and their relations to my arguments, could be studied in a future work.

Giere has not described his views as a form of entity realism (at least, I have not found such an assertion in his writings). However, as the following quotation from Anjan Chakravartty shows, chapter 5 of his *Explaining Science: A Cognitive Approach* (1988) can be considered to be an early development of entity realism.

> Some early, canonical statements of this idea [i.e., entity realism] are found in Hacking 1982, 1983, Cartwright 1983, chapter 5, and Giere 1988, chapter 5. (Chakravartty 2017b, p. 29, n. 9; see also Chakravartty 2017a, section 2.3)

In *Explaining Science*, Giere calls his view "constructive realism". In *Science without Laws* (1999), he advances the case for constructive realism, and also suggests the notion of "perspectival realism", which is further developed in *Scientific Perspectivism* (2006a). One may ask what the relationship is between Giere's entity realism, his constructive realism, and his perspectival realism. In the remainder of this section and in the two next sections, I will explain Giere's versions of realism and their relations with each other.

In chapter 5 of *Explaining Science,* Giere studies the Indiana University Cyclotron Facility (IUCF), where scientists investigate "the structure of the nucleus by bombarding various nuclei with rapidly moving light nuclei, mainly protons, and seeing what comes out" (1988, p. 111). He argues that particles like protons are real entities because they are produced and used as research tools. Protons with roughly the properties that physicists attribute to them are employed as tools in other research. They are as real as other technologies employed in exploring the nuclear structure. Scientific knowledge of protons is indeed embodied in technologies that contemporary experimenters use to investigate other particles. If we take it that protons with their properties are not real, we should also be skeptical about the correctness of what is happening in other investigations which use protons as research tools.

> These nuclear physicists are producing protons with desired characteristics, such as energy, and then using them, together with other particles, to investigate the properties of various nuclei. To say that they are "producing" and "using" protons implies that protons exist. … The judgment about protons seems to me one whose correctness we should take as a basis for further explanations of what is going on in the laboratory. The judgment is not itself problematic. (1988, p. 125)

Giere rightly criticizes those authors who ignore or downplay the distinction physicists make between entities used as “research tools”, such as protons, and the “objects of current study”, the objects that are still under investigation. For instance, the details of nuclear structure, as an object of current study at IUCF, is investigated by means of protons, as research tools. We might not be justified in our consideration of the objects of current study as real and might count them as “theoretical”; however, the entities that are used as research tools are justifiably real. Ignoring this distinction, according to Giere (1988, p. 127), shows the “empirical failing of both empiricism and constructivism” in that they neglect *the distinction that is central to the actual scientific practice*. As a result, he (1988, p. 128) counts Bas van Fraassen’s (1980) account of science, in which this distinction is not taken seriously, as empirically inadequate. This distinction is so critical that scientific progress (at least partly) depends on the use of the objects of current study as established research tools in novel future investigations.

> The proton was once among the most theoretical of particles. Scientists had real questions about the reality of any such thing. Now the proton has been tamed and harnessed to the equipment used to investigate other particles and structures: quarks, gluons, and the shell model of the nucleus. Thus, some of what we learn

> today becomes embodied in the research tools of tomorrow. That is undeniable progress of a very different, and very important kind. (1988, p. 144)

Giere's entity realism is fairly similar to Hacking's (1983). Giere acknowledges this similarity, but says "I originally developed these ideas before I saw Hacking's book." (1988, p. 288, n. 2) He spent about three years, from 1983 to 1986, in a scientific laboratory at Indiana University (on his presence at IUCF, see 1988, p. 287-288, n. 1). Likewise, Hacking spent "a good deal of time" in laboratories such as the cell biology laboratory in the Department of Haematological Medicine in Cambridge University, and as a result he benefited from his experience in that laboratory to illustrate his view on truth in microscopy (see 1985, p. 144, n. 14). Hacking's and Giere's methodology are alike. Both empirically study the practice of experimenters in the laboratory, and accordingly develop relevant philosophical concepts and arguments. One should note that use of the same methodology *alone* cannot account for the similarity of Giere's and Hacking's views, because a similar methodology has been used by (social) constructivists such as Bruno Latour and Andrew Pickering, though to very different ends. Thus the empirical method of studying experimentation and instrumentation is a necessary but not a sufficient condition for understanding scientific practice. Social constructivists fall short of discerning the crucial distinction between manipulable entities and other theoretical posits, not because of their empirical method but due to their exclusively descriptive approach. This approach does not allow them to normatively distinguish between scientific knowledge that deserves realist commitment and other theoretical assumptions.[5]

3.3 Constructive and Perspectival Realism

3.3.1 Constructive realism

Giere's study of experimental work provides an *epistemological* criterion for establishing real entities. To show that his realist view is *conceptually* coherent, he develops "constructive realism" (see 1988, p. 92).[6] He describes constructive realism as a realist alternative to Van Fraassen's constructive empiricism (see Giere 1985; 1988, p. 93; 1999, p. 150). Constructive realism is in agreement with constructivism, the view that scientists (socially) construct scientific models. "Nature does not reveal to us directly how best to represent her" (1988, p. 93), but nature manifests itself through human-*made* models. The result is "constructive realism", a realist view that takes into account the fact that model building is a deliberate activity (1988, pp. 92-93). For instance, scientific categorization is a human process. At the same time, it may represent the real differences of things in nature.

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[5] For a defense of the normative approach to philosophy, see Radder (1996, section 8.3); see also his (1996, p. 180), where Radder addresses Giere's approach to philosophy, which is both naturalized (or empirical) and normative; on this approach, see also Giere (1999, chapter 8).

[6] Theo Kuipers (2000) has also developed a (different form of) "constructive realism".

> The categories we use are to some extent constructed by us. Nevertheless, scientists can sometimes legitimately claim genuine similarities between their logical constructs and aspects of reality. That makes me some kind of realist rather than a social constructivist. (Giere 1999, p. 150)

Giere's main point in his constructive realism is that scientific models are *both* constructed *and* representations of reality. A material model such as the metal model of DNA replication originally proposed by Watson and Crick is built in the same way as a material object such as a table is built. However, a table does not represent reality, but Watson and Crick's model does. Furthermore, compare abstract models (such as the Bohr model of the atom) with abstract social constructs such as money or the concept of currency. Money, despite the material existence of banknotes and coins, is not a concrete thing.[7] Many mathematical models used in physical or biological sciences are similarly abstract constructs. However, there are things in reality that are (at least claimed to be) represented by the elements of those abstract models, while money does not (or is not even claimed to) represent a real thing. Please note that there is no controversy over the fact that constructs are

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[7] My interpretation of money is compatible with the credit theory, which says that "coins and notes are merely tokens of something more abstract: money is a social construction rather than a physical commodity" (De Bruin et al. 2020, section 1.1). In particular, I agree with John Searle's (1995; 2010) view that the existence of money, as a specific social institution, depends on collective intentional attitudes and shared practices of treating certain papers and coins as money. The ontology of money has attracted quite some interest in recent years (see Epstein 2018, subsection 5.6).

themselves real since tables, concrete or abstract models, and money are all as real as other real denizens of the world such as elephants, because they all have real effects. The point is that some of these constructs, namely models, represent reality, but others do not.

Thus, scientific models are built actively, and at the same time they *represent* reality. According to Giere, the representational character of models supports realism. There is some degree of *similarity* between models and aspects of the objects of inquiry. Aspects of similarity exist not only between models and observable phenomena, but also between models and unobservable entities such as protons: "scientists legitimately make claims also about the hidden causal structure of the world. Thus, constructive *realism*" (Giere 1999, p. 150). The argument for this claim is the same as the one that supports *entity* realism. Those unobservable aspects of models that are used as research tools should be considered to be real in order for experimenters to rely on the results of their further investigations. In my view, a kind of transcendental argument is deployed here. That is, the condition of possibility for the reliability of the results of experimental investigations is that contributing research tools work well. An unobservable entity, as a research tool, cannot be effective in the laboratory if it is non-existent or merely mental.

More generally, Giere invokes the many successes of technologies against (social) constructivists' antirealism. Those scientific representations that are used significantly in the process of producing successful technologies manifest aspects of reality. For instance, engineers make use of scientific representations of electrical paths for manufacturing computers.

> The success of these endeavors, and the reliability of the product, prove beyond any reasonable doubt that there has got to be something right about these representations" (1999, p. 60).

This argument is similarly transcendental: the condition of the possibility of successfully manufacturing computers is that the representational character of electrical paths is to some extent right. In other words, working computers cannot be manufactured if there is nothing right about the representational character of electrical paths (provided that these representations genuinely contribute to manufacturing computers).

In line with Stephen Clarke's (2010), one might argue that a transcendental argument is valid if there is *only one (unacceptable) alternative* to the presupposition that we are compelled to accept in order to account for how the world is experienced. My response is that the *only* negation of the conditional statement "if A, then B", is the statement "A and not B". Accordingly, if the former should be presupposed to account for an unavoidable fact but the latter cannot explain that fact, the former is the only possible acceptable presupposition. After all, the *only* alternative to "if an entity is used as a research tool, then it exists" is the statement "an entity is used as a research tool and it does not exist". Likewise, the *only* alternative to "there is something right about the representational character of electrical paths if they are used in manufacturing computers" is "there is nothing right about the representational character of electrical paths and they are used in manufacturing computers". These alternatives are unacceptable, so their negations are the *only* possible presuppositions. Accordingly, the transcendental argument, at least in the way I have articulated it, is compelling.[8]

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[8] My logical point here only addresses Clarke's concern that a transcendental argument is valid if there is *only one* alternative to our presupposition. Still, antirealists might contend that the only negation of the realist presupposition may be acceptable. That is, they might claim that a putative entity may be used as a research tool, and at the same time it may not exist, or there may be nothing

Although I prefer to understand Giere's argument as transcendental, it can also be reconstructed as a kind of No-Miracle Argument, according to which realism is the only philosophical interpretation that can explain the successes of modern science (see section 6.1). Giere insists on the *technological successes* of scientific research. According to this interpretation, it would be miraculous if, say, scientific representations of electrical paths were not similar to aspects of reality but technologies based on these representations worked successfully nevertheless.[9]

3.3.2 Perspectival realism[10]

right about the representational character of, e.g., electrical paths, although they are used in manufacturing computers. For instance, Kyle Stanford's "predictive similarity" is an antirealist explanation of the successes of science. It will be discussed and refuted in section 6.5.

[9] Chapter 6 supports the no-miracle argument for realism.

[10] My discussion of perspectivism in this and the following chapters is mainly restricted to Giere's perspectivism, while similar views on perspectivism can also be found in the work of philosophers such as Gottfried Wilhelm Leibniz, Immanuel Kant, Friedrich Nietzsche, Nelson Goodman, Hilary Putnam, and in the phenomenological-hermeneutical literature and the American pragmatist tradition. See Giere (2006a, p. 117, n. 6, and references therein); on Kant, Nietzsche, the American Pragmatist tradition, and Putnam, see the first four chapters of Crețu and Massimi (2020); on Goodman's perspectivist view in his *Ways of Worldmaking* (1985), see Cohnitz and

Giere's perspectival and constructive realism are similar views which emphasize different points. The emphasis of the latter is on the *constructive* features of scientists' model-building, whereas the former stresses that *aspects* of the world are *represented* by constructed models.

> *Perspectival Realism*, finally, is a later development of constructive realism. The constructive element remains as before. The difference is the insistence that our theories do not ever capture the totality of reality, but provide us only with perspectives on limited aspects of reality. Scientific knowledge is not absolute, but perspectival (Giere 1999, p. 150).[11]

Scientific perspectivism also entails what is *unavailable* for human beings as a result of their scientific practice. It implies that the "totality of reality" is inaccessible, a "complete" image of the world that represents "all aspects" of real things is unattainable, an "ultimate" knowledge of reality that avoids any historical contingency is beyond human conditions, one cannot present things from

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Rossberg (2020, section 6); finally, on the phenomenological-hermeneutical approaches, see my chapter 5 and Berghofer (2020).

[11] At the beginning of *Scientific Perspectivism*, Giere again introduces perspectival realism as a development of his constructive realism:

> I began thinking about the possibility of a perspectival realism as a development of constructive realism in the mid-1990s. This possibility was announced in the conclusion of *Science without Laws* (1999). The present book is an attempt to fulfill the promise of that announcement. (2006a, p. ix; see also p. 118, n. 13)

“no perspective whatsoever”, scientific knowledge that is “independent of all perspectives” is unreachable. Still, perspectivism, or at least a *realist* version of perspectivism, concludes that it is reality that is presented from one or several perspectives. The point is that these representations are bounded by instrumental, theoretical, and historical conditions. In the following, I explain three interrelated types of perspectivism that are visible in Giere’s work: (1) instrumental perspectivism, (2) theoretical perspectivism, and (3) historical perspectivism.[12]

First, instrumental perspectivism maintains that human knowledge of reality depends on the mediation of bodily-sensory apparatuses and technological instruments. The way that human beings ordinarily experience the world is colored by specific perspectives. That is, the way we ordinarily perceive the world is bounded by the biological conditions of our eyes, which are sensitive to, e.g., three colors, and to the conditions of our neural system that contribute to analyzing input signals. If we possessed different apparatuses, we would experience the world differently. If human eyes evolved like a fish called Skate, whose eyes have no cone photoreceptors, we would see only in black and white. If human eyes had only two types of cones, we could distinguish colors in the way dogs and cats do. Our experience would also be different if our eyes had four types of cone cell. As a result, our ordinary vision *depends on* the instrumental

[12] In this section, my approach to Giere’s work is mainly expository. Subsection 7.2.2 will more critically review Giere’s conception of perspectivity and argue that it is preferable to understand “perspectivity” as “conditionality” than to conceive it as “partiality”. Accordingly, instrumental, theoretical, and historical perspectivism imply that our scientific knowledge is conditional, respectively, on instruments, on theoretical concepts, and on historical periods.

characteristics of our visual system. In like manner, the outputs of scientific instruments *depend on* their specific features, such as what they are sensitive to or how they analyze input data to produce outcomes. For example, a gamma-ray telescope is responsive only to gamma rays, or a CAT scan only shows the structure, rather than the function, of the brain. The analyzing processes of different instruments may also be different. For instance, the statistical analyses employed in producing neuroimages from noisy signals are different from the machine learning techniques employed in the analysis of astrophysical detector data.[13] Thus, since humans always experience the external world by means of (embodied or technological) instruments, they can experience neither ordinary objects nor scientific entities without the mediation of this instrumental perspective (Giere 1999, pp. 79-81; 2006a, chapters 2 and 3).

Second, according to theoretical perspectivism, scientific knowledge depends on qualified models. For this reason, a representation of all aspects of a real object is impossible. Scientific models are similar to maps. Different maps, for instance a subway map, a flat map, a neighborhood map or a geological map, are deliberately constructed, each from a different perspective, to represent specific aspects of a territory (1999, pp. 26, 81-82 and 214-215; 2006a, pp. 73 and 76-78). In a similar fashion, different models of an entity can be constructed to represent different aspects of a single entity. Reality is complex, and no model can fully represent all aspects of a real entity. Scientists create different theoretical models of entities, depending on the distinct problems

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[13] On statistical analysis in neuroimaging, see Klein (2009, section 2) and Roskies (2007, section 4). On the applications of machine learning techniques in astrophysical detection, see, e.g., Cuoco et al. (2021).

they encounter. Take water, for example. A molecular perspective, in which water consists of particles, is employed to account for its Brownian motion. However, water is a continuous fluid according to the hydrodynamic model, which accounts for the flowing behavior of water. These two inconsistent models represent two different behaviors of water from different perspectives. Each model fits specific aspects of water. Neither explains all behaviors of water. (Giere 2006b, 33-340).[14]

Finally, historical perspectivism is usually based on an interpretation of Thomas Kuhn's historicism (see Giere 2013). Michela Massimi describes this form of perspectivism as follows:

> our scientific knowledge is *historically situated*, that is, it is the inevitable product of the historical period to which those scientific representations, modeling practices, data gathering, and scientific theories belong. (Massimi 2018b, p. 164)[15]

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[14] The case of water is examined in Morrison (1999, pp. 53–60) and Teller (2001, pp. 401–402, 408–409). See also Massimi (2019) on perspectival disagreement in science.

[15] Moreover, Massimi states that "our scientific knowledge is [both *historically* and] *culturally situated*" (2018b, p. 164), the latter implies that science is also contemporaneously situated. In other words, there are *diachronic* and *synchronic* versions of perspectivism (see 2018b, p. 165). Hydrodynamics and statistical mechanics provide two synchronic perspectives to studying water. Similarly, the Copenhagen and the Bohmian interpretation of quantum mechanics suggest two synchronic perspectives to interpret quantum mechanics (Cushing 1994). The examples of

Giere acknowledges a realist version of historical perspectivism, according to which we should see neither the outputs of contemporary instruments nor scientific representations of current models as ultimate images of reality, even though scientific instruments and models do manifest aspects of reality. Thus, scientific knowledge is always bounded by historically available instruments and models, and future instruments and models augment our current understanding of reality.

Historical perspectivism can provide an answer to the pessimistic induction, an objection to realism according to which current theoretical concepts do not refer to real entities, just as abandoned theoretical concepts such as the ether have turned out to be non-referring (Laudan 1981). According to Giere, the fact that the ether does not exist does not imply that ether models are not, in some respects, similar to reality. “Whether the ether exists or not, there are many respects in which electromagnetic radiation is like a disturbance in an ether.” (1988, p. 107) Current perspectives are more advanced than past ones in manifesting aspects of reality. However, abandoned models are successful to the extent that they represent aspects of reality. (I will develop this view in chapter 6.)

Thus far I have explained different facets of Giere’s realism. He is realist about the scientific representations of entities employed in designing technologies, hence his entity realism. Giere’s realism is not extreme but modest, and it is consistent with constructivism inasmuch as scientific models are (socially) constructed by scientists. It is also compatible with perspectivism insofar as

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diachronic (theoretical) perspectives are the same as those addressed in my discussion of theory change and the so-called pessimistic induction in chapter 6.

the construction of scientific knowledge depends on historically available instrumental and theoretical conditions. Below I will further discuss entity realism and perspectival realism. As explained in subsection 3.3.2, perspectival realism is a development of constructive realism. Accordingly, a separate discussion of constructive realism is no longer necessary. Section 3.4 explains how the concept of "overlapping perspectives" helps avoid criticisms against both entity realism and perspectival realism, hence adding this concept contributes to a more resilient account of realism. A discussion of the relationship between entity realism and perspectival realism, in section 3.5, concludes the chapter.

3.4 Overlapping Perspectives

3.4.1 Realism and overlapping perspectives

A criticism of entity realism is that it is only realist about those entities that can be used as manipulable research tools. Thus, Hacking (1989) is antirealist about astronomical entities such as black holes that cannot be used as such tools. However, one can argue that to be realist in cosmology, biology, neuroscience, and so on, one does not need to presuppose that real entities can always be used as manipulable research tools (see Radder 1996, pp. 91-92). In general, there are real entities, which we have good reason to believe in, but some of these slip through the net of entity realists (Shapere 1993).

In response, I suggest that Giere's concept of "overlapping perspectives" supports the criterion of reality as *robustness*, a more inclusive epistemological criterion that can be applicable to claims about astronomical, biological, neurological, or other scientific entities. The same idea forms the basis of Hacking's argument from coincidence, although he does not develop this argument in order to further support realism, e.g., about astronomical entities. However, scholars such as William Wimsatt (2007) and Markus Eronen (2015) have significantly advanced the idea of robustness. According to Eronen, "X is robust in the relevant scientific community at a certain time insofar as X is detectable, measurable, derivable, producible, or explanatory in a variety of independent ways" (2015, p. 3967; see also 2017). X is a thing. It may be a scientific entity, a property, a phenomenon, or even an ordinary object.[16]

A fairly similar concept to robustness, which can be found in Giere's *scientific perspectivism*, is the notion of "overlapping perspectives." When several independent instruments manifest the same fact, "overlapping instrumental perspectives" are available (2006a, pp. 57-58). When assuming that a fact plays an explanatory role in several theories, "overlapping theoretical perspectives" are at work (2006a, p. 92). Giere considers the evidence achieved from overlapping perspectives as "good evidence", which is still perspectival.

> It is a commonplace that there can be many observational perspectives of the same objects. … Is this not good evidence that there is something "objectively" there? Indeed, this is good evidence that there is *something* there, but this need not be understood as knowledge in an "absolute objectivist" sense.
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> The simple but fundamental point is that to be an object detected in several different perspectives is not to be detected in no perspective whatsoever. All

[16] I will discuss Eronen's account in detail in the next chapter.

> observational claims made about the object are made in some perspective or other. (Giere 2006a, pp. 57-58)[17]

The role of robustness or overlapping perspectives is not essential to the *epistemology* of Giere's realism. He introduces overlapping perspectives as an "important *methodological* strategy" (2006a, p. 58; emphasis added). He discusses overlapping perspectives not to develop his epistemological thesis, but to argue against the claim that overlapping perspectives provide the condition of being viewed from nowhere.

I agree that humans cannot perceive or know independently of perspectives. In other words, to be perspective-independent, a thing should be observed or known independently of any instrument, (theoretical) concept, and historical contingencies. Human beings are embodied, so they cannot perceive without the mediation of any (embodied or technological) instrument. Their (scientific) knowledge is conceptual, so they cannot be free from (theoretical) concepts. And they are temporal beings, so they cannot be independent of historical contingencies. Thus, although overlapping perspectives together provide a broader perspective than each of their constituent members, they

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[17] Giere reiterates that

> the same object can often be observed from several different perspectives, such as a nearby galaxy observed by both optical and radio telescopes. This is indeed good evidence that there is "something" there, but that is scarcely knowledge in the objectivist sense. The knowledge we get comes from one perspective or another, not from no perspective at all. Multiplying perspectives does not eliminate perspectives. (2006a, p. 92)

still depend on some common instrumental, theoretical, and historical conditions of their constituent perspectives. In other words, $perspective_1$ and $perspective_2$ jointly provide a broader perspective which does not exclusively depend on the particular conditions of $perspective_1$ and $perspective_2$, but is still conditioned on the common features of these two perspectives.[18]

After all, the *validity scope* of several perspectives is still bounded. Let me explain this by the concept of "nonlocal patterns". Radder (1996, p. 84) argues that the observational/experimental results are not "universally" valid. The results may still be bounded by some conditions that are "nonlocal". This is similar to the view that overlapping perspectives do not present "non-perspectival" knowledge. Further, according to Radder, observational/experimental results are not local either, since the results of replicable experiments/observations transcend local circumstances (see 1996, section 5.4). In perspectivists' terms, replicable observations/experiments provide overlapping perspectives and not simply a single perspective. All in all, observational/experimental results are neither local nor universal. The fact that they are not universal implies that they do not provide a non-conditional or non-perspectival image of reality.

While our knowledge of an entity is always bounded by perspectives, the concept of "overlapping perspectives" can provide an epistemological criterion for our confidence in the reality of entities and their properties *in the nonlocal scope provided by overlapping perspectives*. This bounded criterion is a matter of degree. The degree of our confidence in the reality of the

[18] See also section 7.3, in which I argue that the constancy of the speed of light, as a robust property of light according to the special theory of relativity, is not independent of all perspectives whatsoever.

entity depends on the degree of robustness that different perspectives, from which the entity is detectable, measurable, derivable, producible, or explanatory, provide.[19] According to the criterion of overlapping perspectives (or robustness), one can show that entities in astronomy, biology, neuroscience, and so forth, can be real. It might be impractical to make use of all real entities to design and manufacture technologies, but it is highly practical to explore an entity from independent perspectives. Evidence obtained from independent perspectives can justifiably demonstrate whether or not an entity is real.

Furthermore, the criterion of robustness also allows us to answer the criticism that entity realism cannot provide a satisfactory criterion for establishing real entities. Axel Gelfert (2003) examines the example of "quasi-particles", that are manipulable in Hacking's sense, but which are also illusory. According to the criterion of robustness, we employ different perspectives to avoid considering illusory phenomena as real. Illusions depend on the particular conditions of a perspective. However, it seems unlikely that an entity investigated by independent perspectives will turn out to be illusory (see also section 2.5).

Another common objection to entity realism is that it is incoherent to believe in an entity without also believing in the theory describing the entity (Morrison 1990; Resnik 1994; Chakravartty 1998; Psillos 1999, pp. 247-249; Clarke 2001; Massimi 2004). Accordingly, one cannot separate knowledge of the existence of scientific entities from their theoretical descriptions,

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[19] "Overlapping perspectives", "robustness", and different meanings of "objectivity" (which I will define in the next subsection) are all *epistemological* terms in that they provide a criterion for our *confidence* in or a *justification* for the reality of things.

because the properties of scientific entities are always described by theoretical concepts. In response, Giere would respond that entities should be separated neither from their properties nor from the theoretical models describing them. For instance, protons possess mass, charge, and momentum (see Giere 1988, p. 124), which are described by the Standard Model of particle physics. This response is compelling if it also maintains the perspectival features of scientific knowledge. A robust entity or property is real inasmuch as nonlocal overlapping perspectives support it. Still, the theoretical descriptions of those entities and their properties depend on some theoretical perspective. The fact that an entity or its properties exist is obtained on the basis of overlapping perspectives. Nevertheless, the entity or its properties need to be described by some specific theoretical perspective.[20]

The idea of overlapping perspectives also allows perspectivists to preclude the criticism that perspectival realism tends to be reducible to epistemological relativism. Chakravartty poses this challenge to perspectivism as follows.

> Perspectivism, thus understood, is controversial because it engenders one or another form of relativism, and the prospect of relativism raises alarm among those, including most (but by no means only) scientific realists, who are attracted to the idea that there are non-perspectival facts about things, and that at their best, the sciences succeed in telling us what these non-perspectival facts are. A philosophically interesting perspectivism would appear to do away with these

[20] Chapter 6 argues for the intertheoretical continuity between the descriptions of overlapping theoretical perspectives.

> sorts of facts, and any sort of epistemic position defined in terms of them (Chakravartty 2010, p. 406; see also Lipton 2007).

According to the perspectivism that includes "overlapping perspectives" as a central concept, things are robust, and science succeeds in confirming the existence of robust entities and their properties according to the validity scope of overlapping perspectives. Thus, perspectivists can justifiably be a realist about robust entities when these are investigated by means of overlapping perspectives.[21] At the same time, our knowledge of robust things is not non-perspectival. This view is as relevant to science as the view that scientific knowledge is nonlocal rather than universal. Moreover, as chapter 7 argues, perspectivism is relevant because it can explain how knowledge of frame-dependent properties in the (special) theory of relativity is conditional on the choice of reference frame. These properties cannot be considered as non-perspectival.

Finally, there might be a question what is the relationship between the criterion that, in designing technologies, real entities should be employed and the criterion of robustness? I would think that the former criterion is a specific case of the latter. When an entity is produced and used in a technological facility, the entity is probably real. Our confidence in the reality of the entity increases if the entity is produced in a variety of independent ways. Giere asserts that using protons in the cyclotron facility should not be considered to be "a single isolated action," but that we should take account of

> the process of designing and performing hundreds of experiments, the process of designing, building, and maintaining a cyclotron facility over a period of

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[21] See Teller (2020) for another realist reading of perspectivism.

> years, and so on and on. As a result of this continuing and varied experience, contemporary nuclear physicists never even think about questioning the existence of protons or wonder whether they have adequate evidence for their beliefs about protons (1988, p. 129).

Accordingly, protons are real not merely because one can produce and use them in one specific kind of experiment, but their production is based on a variety of mechanisms provided by independent perspectives. In other words, protons are robust, so they are justifiably real.

3.4.2 Objectivism and overlapping perspectives

This subsection clarifies the relationship between "objectivity" and "overlapping perspectives". For this purpose, the different meanings of the notion "objective" should be distinguished. I call the first notion "objective$_1$", which implies being viewed from nowhere, or in other words: being universal in the sense of being independent of all local conditions. However, this kind of objective knowledge is unavailable forever, because our knowledge is always bounded by a range of instrumental, theoretical, and historical perspectives.

Objective$_1$ is quite different from Alan Chalmers's (1990, chapters 4 and 5) conception of objective, which I call "objective$_2$", according to which observation is *achieved* as a result of practical activities of observers, hence his term "observation objectified". Chalmers's objectified$_2$ observation either is achieved or not achieved. If objectifying procedures that help observers to purify their knowledge from subjective errors have been correctly employed, the observation is objectified$_2$. To be reliable, experimental/observational results should be objective$_2$. Chalmers maintains that objectified$_2$ results are fallible due to the theory-ladenness of observation, but this leads neither to subjectivism nor to relativism. It only shows the fallibility of objectivism$_2$. The requirement that experimental/observational results should be objectified$_2$ is compatible with Giere's rejection of "objectivism$_1$", because objectified$_2$ knowledge is still conditional on the validity of a number of presupposed theories and employed instruments.

I suggested that the concept of "overlapping perspectives" or "robustness" can be used to develop perspectivists' *epistemological* criterion for reality. In this regard, I introduce a third notion of objective, that is, "objective$_3$": "being dependent on a variety of different perspectives". Inasmuch as an observation transcends the particularities of single perspectives, the objectivity$_3$ of the observation, and accordingly our justification for knowledge of the thing observed, increases.

Although Giere does not speak of the epistemological criterion of "overlapping perspectives", textual evidence suggests that he *would* accept that additional perspectives can increase the objectivity$_3$ of an observation. About seeing a building from different angles and distances, he asserts: "Additional objectivity can be built into this example by imagining a series of photographs taken from different viewpoints" (1999, p. 80). Here, he is indeed ascribing objectivity$_3$ to *observations*. Objectivity$_3$ can also be ascribed to *experiments*; and subsequently to our *knowledge* of the things observed/detected in observations or experiments. Furthermore, when experiments or observations are performed in a variety of independent domains, the plausibility of *theories* that are tested also increases (see Radder 2012[1984/1988], p. 76).

Objectivity$_3$ is a matter of degree. The more our observational/experimental results transcend local conditions, the broader perspective the local perspectives provide, the more objective$_3$ our knowledge of the represented things is. In sum, although our knowledge of a thing cannot be "objective$_1$", it should be "objective$_2$", and it can (and should) satisfy a (high) degree of "objectivity$_3$". See the following table for three meanings of objective knowledge.

Objective$_1$ knowledge (which Giere denies)	Being views from nowhere or being independent of all local conditions
Objective$_2$ knowledge (which Chalmers supports)	Being purified from subjective errors and biases
Objective$_3$ knowledge (which I suggest based on "overlapping perspectives")	Being dependent on a variety of independent perspectives

Table 3.1

3.5 Entity Realism Meets Perspectivism

The affinities between robustness and overlapping perspectives help to understand entity realism and perspectival realism as two compatible and complementary theses. Entity realism specifies which aspects of our knowledge claims deserve realist commitment, whereas perspectivism sheds light on the role of theoretical, instrumental and historical conditions and contingencies in scientific discoveries. Further on, I explain the relationship between entity realism and perspectivism. Before that I would like to highlight a benefit of reconciling various brands of scientific realism that have thus far been proposed in the literature, including entity realism, structural realism, constructive realism, referential realism, transcendental realism, perspectival realism, and the like. The differences between these views are sometimes confusing for outsiders, or even for philosophers of science. One might argue that there is no consensus among realists about which scientific knowledge deserves realist commitment. This state of affairs makes a commitment to any realist view somewhat precarious. In response, realists can argue that versions of realism are in many cases compatible with each other. Thus, one of the tasks of the realist is to show the compatibilities that exist among a range of realist views. In this regard, for instance, Chakravartty (2007, chapter 2) argues that entity realism and structural realism can be compatible. Analogously (as I have discussed in subsection 3.3.2), Giere explains that his constructive realism and perspectival realism focus on two facets of one view, the latter being a developed version of the former. My arguments in this and the previous section entail that perspectival realism and entity realism are compatible and complementary.

To my knowledge, entity realists such as Ian Hacking (1983), Nancy Cartwright (1983), Matthias Egg (2012; 2014; 2016), Markus Eronen (2015; 2017; 2019), and Bence Nanay (2019) have not discussed perspectivism. Nor have perspectivists, other than Giere, employed the ideas of entity realism. Van Fraassen, who defends perspectivism in his *Scientific Representation: Paradoxes of Perspective* (2008), notoriously has issues with (entity) realism. Also, Michela Massimi, who defends perspectival realism in her recent work (e.g., 2012; 2018a), has criticized entity realism in her 2004 paper. Giere's philosophical views about science, however, allow us to harmonize entity realism with perspectivism.

Entity realists maintain that the experimental interaction with the world supports realism. They, or at least those entity realists who are among the New Experimentalists, also address how the experimental interaction with the world is possible. Thanks to the role that "overlapping perspectives" can play, perspectivism can be sympathetic to entity realism. Perspectivism can also further explain the conditions and boundaries of how humans interact with (physical) reality. Below, I explain how each type of perspectivism is compatible with and complementary to entity realism.

First of all, instrumental perspectivism explains that instruments provide perspectives to represent aspects of entities by observing, detecting, or producing their properties. If the observation/detection/production is made by means of independent perspectives, (the property of) the entity which is measured/observed is robust and thus real, even if the observational/experimental results are still bounded by nonlocal conditions of the relevant observation/detection/production.

About theoretical perspectivism, let me first make two statements in a Kantian vein: experimental interactions with entities are blind if the entities are not theoretically interpreted by concepts and models; conversely, theoretical concepts and models that are not, ultimately, connected to reality are empty. Theoretical perspectivism insists on the first statement that discovered entities are always described and interpreted by concepts and models. Entity realism and the idea of "overlapping perspectives" emphasize the second side: theoretical terms represent entities inasmuch as observers/experimenters rely on multiple interactions with reality.

Finally, both Giere, as a perspectivist, and Eronen, as a "robust entity realist", accept that experimental results and theoretical interpretations are bounded by historically available perspectives. Eronen's assertion that "[r]obustness is clearly a fallible criterion that is relative to a certain scientific community at a certain time" (2015, p. 3966) is explicitly sympathetic to the spirit of historical perspectivism. A relevant point is that historical perspectivism should not be understood in a relativist way, because according to the idea of "overlapping perspectives", there is always a certain degree of continuity or persistence among historical perspectives. Another point is that current perspectives are typically broader than past ones, hence scientific progress.[22] Therefore, if a scientific community at a given period of time lacks the technologies necessary to manifest an entity, or is bereft of the theoretical concepts necessary to describe it, the entity remains either unrecognized or unarticulated at that period. Future instruments may manifest that entity or its other properties. Future theories may also describe aspects of the entity differently and better than current ones. (Chapter 6 further discusses the ideas of this paragraph.)

According to our current scientific knowledge, robust entities will probably be acknowledged as real in future science. However, there is "no guarantee that things that are considered robust based on the current state of science are real, as arguments such as pessimistic induction purport to show" (Eronen 2015, p. 3966). Indeed, entity realism takes the sting out of the so-called pessimistic induction by restricting realist claims to those entities that satisfy the criterion of robustness.[23] Be this as it may, one should not expect that the justified warrant the criterion of robustness provides will be without any future defeaters. According to historical perspectivism, the strongest prospective claim an entity realist can make is that we are justified to think that robust entities may remain in the future of science. The least risky bet one can make on the future of science is betting on the reality of highly robust scientific entities, which have been explored through independent perspectives (see also section 2.6).

[22] Section 6.2 elaborates my view of progress. See also Davis Baird's and Thomas Faust's 1990 paper, where they argue that scientific progress relies on the advancement of technological instruments and the accumulation of instrumental techniques.

[23] Two pessimistic inductions are reviewed in chapter 6.

Another consequence of historical perspectivism is that the level of our confidence in an entity's existence differs in different periods. Since more technological facilities and theoretical resources are available today than in the past, current scientists are (and should be) more rigorous than past ones when recognizing the reality of entities. Still, it does not follow that current scientists can determine the reality of entities beyond any perspective, because, after all, current science is conditioned on current (and past) technological and conceptual resources.

In conclusion, understanding the relationship between the concepts of overlapping perspectives and robustness will help to bridge the gap between perspectivism and entity realism. The experimental exploration of an entity from different perspectives justifies the reality of a given entity. Entity realism and perspectivism are compatible and complementary. The former explains that (multiple) experimental interactions with entities and their properties support realism, and the latter emphasizes that experimental interactions are mediated by embodied or technological instruments, and are understood by means of theoretical concepts. Thus, scientific knowledge is bounded by historically contingent instruments and theories.

Chapter 4 A Dialogue among Recent Views of Entity Realism

4.1 Introduction

As I stated in section 3.2, some portions of our successful scientific theories have turned out in the past to be non-representational. Selective realists seek to determine which are the trustworthy parts of our scientific theories, i.e. those that probably represent real things. An example of selective realism is entity realism, according to which certain experimental interactions with unobservable entities, such as electrons and genes, can bring about knowledge that deserves realist commitment. Whether an entity is manipulable, is used as a research tool, or is the cause of an observable effect, are all determined through experimental practice. Therefore, *experimental realism* is sometimes used as another name for entity realism, thus insisting on the entity realists' analysis of the *methodology* of science, which relies on experimentation. In contrast, the term "entity realism" emphasizes the *epistemological* aspect, the fact that we can consider knowledge of entities to be true. Because my discussion is epistemological, I employ the term "entity realism" in the following.

Ian Hacking (1982; 1983), Nancy Cartwright (1983, essay 5), and Ronald Giere (1988, chapter 5) constitute the first generation of entity realists. Below, I discuss the views of Matthias Egg, Bence Nanay and Markus Eronen,[1] who belong to the current generation of entity realists. My

[1] I mention these authors neither alphabetically nor chronologically. I discuss them in a way that seems to be appropriate to create a dialogue between them.

definition of entity realism (i.e., that experimental interactions with unobservable entities can bring about knowledge deserving realist commitment) is inclusive enough to encompass all versions of entity realism. I have not included the concept of "manipulation" in the definition, although it is essential to some accounts of entity realism. A more general concept, such as "experimental interaction", which I have employed, takes into account the views of all entity realists, in particular Eronen, whose view does not primarily depend on manipulation.

Surely, there are other scholars who are somewhat sympathetic to entity realism as well: for instance, Anjan Chakravartty's semirealism (1998; 2007, chapter 2) and dispositional realism (2017b, section 4.2), Hans Radder's referential realism (2012[1984/1988], part II), and Shannon Vallor's (2009) phenomenological support for realism (see my subsection 5.3.2) all have some affinities with entity realism. Nonetheless, my focus in this chapter is on those scholars who explicitly consider their views as developments of entity realism. In his chapter on entity realism in *The Routledge Handbook of Scientific Realism* (2017), Egg describes his causal realism as "a modified version of Cartwright's [entity] realism." (2017, p. 129); Eronen asserts that "I formulate a new robustness-based version of entity realism" (2017, p. 2341); and Nanay also argues that "the only way entity realists can resist the pull of straight scientific realism about theories is by endorsing … [Nanay's] singularist semirealism" (2019, p. 499). I should finally note that I leave aside experimental views which are not realist, such as Mauricio Suárez's "experimental attitude" (2008, p. 139).

A merit of the current accounts of entity realism is that they begin from the promising but vague insights of early entity realists and make them precise by drawing conceptual distinctions, by discussing the implications of the claims, by providing further arguments, and by developing the theory in a way that avoids earlier objections. However, as we will see, a drawback of these recent accounts is that none of them have sufficiently discussed each other's views. Egg (2012; 2014; 2016) discusses neither Eronen nor Nanay, and only in a note (in Egg 2017, p. 130) says in passing that "[f]or another recent version of entity realism, see Eronen". Nanay (2013; 2019) refers neither to Egg nor to Eronen. Eronen (2015; 2017) briefly speaks about Egg's causal realism to distinguish his robust entity realism from causal accounts of entity realism. Thus, each of these entity realists has developed his view without paying enough attention to the attempts by others.

Entity realism appears to be a project, one that was initiated by a first generation and is now being developed by the present generation. I intend to contribute to this project's advancement by critically reviewing current accounts of entity realism and by bridging the gap between them. Thus, the objective of this chapter is to open a dialogue between current views of entity realism. Sections 4.2, 4.3 and 4.4 explain Egg's causal realism, Nanay's singularist semirealism, and Eronen's robust realism, respectively. Section 4.5 discusses the implications of Nanay's singularism for causal and robust realism. Section 4.6 addresses the relationship between robust and causal realism. Section 4.7 concludes the chapter by providing a criterion for the reality of property tokens. The criterion is based on the insights of recent entity realists and on the additional points I will make.

4.2 Causal Realism

Egg's (2012; 2014; 2016) "causal realism" is based on a distinction between causal and theoretical warrants. This distinction seems like Cartwright's distinction between causal and theoretical explanations. However, Egg argues that by "shifting from explanation to warrant," one can draw a distinction between causal and theoretical warrant, "even in the absence of a clear-cut distinction between causal and theoretical explanations" (Egg 2017, p. 127). According to Egg (2014, chapter

4), an explanation is *causally warranted* if and only if it fulfills the following three conditions. Each is a necessary condition, and the three together are sufficient to provide a causal warrant.

I- Non-redundancy: the hypothesis that causally explains the relevant empirical evidence is the *only* hypothesis that can account for the evidence, thus the hypothesis is "non-redundant". In other words, other hypotheses, if any, cannot provide a serious causal explanation for the empirical evidence (Egg 2012, section 4.1; 2014, pp. 50-53).[2]

II- Empirical adequacy: if the observable (in Van Fraassen's (1980, p. 16) sense) implications of the causal explanation are true, then the explanation is empirically adequate (Egg 2012, section 4.3; 2014, pp. 59-61).

III- Material inference.

Let me first examine Egg's presumptions about this condition, and then I will present his definition of material inference. The concept of "material" implies that a concrete matter of fact, rather than a mere theoretical law, should explain the phenomenon. To explain the concept of "concrete matter of fact", Egg draws on Chakravartty's (2007, pp. 41–42 and 107–111) notion of "causal properties", as the sources of capacities. Properties such as masses, charges, and temperatures confer on the entities that have them certain capacities, which manifest certain behaviors in certain circumstances.[3] Entities with "detectable" properties, that is, with causal

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[2] Egg's description of "non-redundancy" should not be conflated with other meanings of "non-redundancy" in the literature. This chapter follows Egg in the usage of this term: a non-redundant hypothesis provides the *only* explanation for the relevant empirical evidence.

[3] Chakravartty defines causal properties as follows:

properties that are detectable by scientific instruments, may deserve realist commitment. However, "auxiliary" properties, those attributed to entities only by theoretical models, are not trustworthy (Egg 2014, p. 57, n. 8; Chakravartty 2007, p. 47). Accordingly, a concrete matter of fact is a detectable property of a concrete entity.

Egg follows James Woodward in supporting a *manipulability* account of causation, according to which "for something to be a cause we must be able to say what it would be like to change or manipulate it" (Woodward 2003a, p. 112). Throughout this chapter, I also characterize causation in line with Woodward's view: if C is a cause of E, then C can be manipulated such that E is changed in turn. Thus, causal claims are tested by manipulating or modifying the presumed cause by intervening in it in order to see the change in the presumed effect.[4]

A characteristic of Woodward's account, which is crucial for my argument, is that it is not necessary that human agents manipulate a cause to see the effect. Thus, it avoids the

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> They confer dispositions for relations, and thus dispositions for behaviour on the particulars [such as entities] that have them. Why and how do particulars interact? It is in virtue of the fact that they have certain properties that they behave in the ways they do. Properties such as masses, charges, accelerations, volumes, and temperatures, all confer on the objects that have them certain abilities or capacities. These capacities are dispositions to behave in certain ways when in the presence or absence of other particulars and their properties. (2007, p. 41; on capacities, see also Cartwright 1999, chapter 3, section 4)

[4] On (other) manipulability accounts of causation, see Woodward 2016.

anthropomorphism of some manipulability accounts of causation, and allows for "natural experiments", that is, "the occurrence of processes in nature that have the characteristics of an intervention but do not involve human action or at least are not brought about by deliberate human design" (2003b, p. 94; see also 2003a, pp.103-104). Furthermore, his view allows for "hypothetical intervention":

> Because the notion of an intervention can be characterized without reference to human activities, it makes sense to speak of hypothetical interventions on X, of what would happen to Y under such interventions, and hence of X causing Y, even if manipulation of X is not within the technological abilities of human beings, and indeed even in circumstances in which human beings or other agents do not exist. (Woodward 2003a, p. 128)[5]

According to Woodward's view, it is only necessary to have a well-defined notion of what it would mean to manipulate the cause (Woodward 2003a, p. 128; see also Woodward 2016, section 12). That is, it should be clear what will happen to the cause if it were manipulated and if the claims about the presumed effect can be assessed. "All that is required is that we have some sort of basis for assessing the truth of claims about what would happen if an intervention *were* carried out" (2003a, p. 130). In sum, if actual experiments are not physically possible, it is still feasible to assess a causal claim so long as it can be understood as a counterfactual claim that can in some way be evaluated (see Egg 2014, p. 55).

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[5] On hypothetical experiments, see also Woodward (2003a, section 3.2; 2003b, p. 95).

One might argue that a counterfactual inference refers to a counterfactual fact, that is, a fact that did not concretely occur, so it cannot be a concrete matter of fact. However, a concrete matter of fact, in Egg's view, is defined in opposition to a general law. So it is concrete not in the sense that it should always actually occur, but in the sense that the fact is particular and not general (apart from its occurrence in an actual experiment or its counterfactual occurrence in a hypothetical experiment). In other words, the concrete fact is detect*able*, that is, either it has actually been detected or it is in principle detectable but has not been actually detected. In the latter case, the concrete fact is inferred according to a hypothetical experiment (this inference is causally warranted provided that the other conditions, that is, non-redundancy and empirical adequacy, are also fulfilled).

Egg accordingly defines a material inference as "one that results in ascribing to a concrete entity a property for which there is a well-defined notion of what it means to modify it" (Egg 2014, p. 58; see also 2016, p. 12; 2012, pp. 65–66). Yet, this definition needs clarification. It should be specified what is inferred from what. In material inference, a detectable property of a concrete entity is inferred from the result of an actual or a hypothetical manipulation. The result is used as an effect, which indicates the cause, i.e., the detectable property of the concrete entity. The adjusted definition can thus be stated as follows.

> Material inference: the inference from an actual or a hypothetical manipulation that results in ascribing to a concrete entity a property for which there is a well-defined notion of what it means to modify the property.

Please note that a property of an entity is not *real* only because it is materially inferred. To be justifiably considered real, the conditions of empirical adequacy and non-redundancy should also be fulfilled. That is, the observable implications of the hypothesis that the property exists should be true, and only that property should explain all the relevant empirical evidence.

4.3 Singularist Semirealism

As I stated in subsection 3.4.1, a criticism of entity realism is that it is incoherent to separate our causal knowledge of an entity from the theories that describe the entity. Scientists need theories to describe the properties of entities and their lawful relationships with each other. These theoretical descriptions are required to manipulate unobservable entities in experiments. Therefore, to be a realist about entities, one is also forced to be a realist about theories (see also Nanay 2019, section 3).

In response, entity realists often argue that being a realist about the detectable properties of entities does not entail that theories are totally true. According to causal realism, the detectable properties of entities may be trustworthy, while other parts of theories may not be true. Nanay (2013; 2019) employs nearly the same strategy. He argues that entity realists should be realist only about "singular representations," which attribute "property tokens" to particulars (that is, to entity tokens or property tokens). Concrete property tokens, rather than abstract property types, are manipulable in experiments,[6] and accordingly are known via singular representations. However,

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[6] According to Nanay:

> Experimenters manipulate property-tokens, not property-types. Of course, on the basis of the manipulation of property-tokens, science postulates models or

non-singular representations, which attribute “property types” to particulars or to other entity or property types, are not literally representational, because property types are only products of us humans; they do not exist out there, independently of us (2013, p. 380; 2019, p. 508).[7]

Nanay advocates a kind of nominalism,[8] according to which property types do not exist but are the products of grouping property tokens in a useful way. Property types should not be seen as real essences, but as pragmatically useful categorizations of property tokens. More useful categorizations provide better explanations and predictions. Laws (as general relationships between property types)[9] or models (which include non-singular representations) are derived

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> laws or general causal claims that are about property-types, but this process needs to start with the attribution of property-tokens. (Nanay 2019, p. 510)

[7] Nanay employs the concept of the “property token”, which is usually known in metaphysics as “trope”, the individual property of an object. Defenders of “trope nominalism” argue that tropes exist, but “universals”, or property types, are mere “resemblance classes of tropes”. Nanay states that singularist semirealism coincides with the version of trope nominalism that accepts that tropes (or property tokens) are logically prior to universals (or property types), and thus “only property-tokens (tropes) exist out there, independently of us; property-types (the resemblance classes of tropes) are the product of our grouping them in a certain way” (Nanay 2019, p. 511).

[8] See also Nanay (2010; 2011). He claims that his nominalism is compatible with Hacking’s (1991; 2007; 2015) later view of natural kinds.

[9] According to Nanay:

ultimately from singular representations.[10] Although property types, laws, and models are useful epistemic tools, they do not refer to real kinds or essences.

Experimenters are not required to be realists about property types, laws, or models used as epistemic tools to guide the manipulation of property tokens of entities. Experimenters learn how to perform an experiment from previous attempts:

> [T]here is no reason to think that the experimenter would need to be realist about any of the property types of the electron (or of the niobium ball or anything involved in this experiment). All she needs is some knowledge of previous attempts at spraying niobium balls with electron[s]. Some of these attempts were successful, some others, no doubt, unsuccessful. She should try to spray the

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> Singularist semirealism, for example, denies that we should be realist about laws—laws are about property-types (as David Lewis famously said, 'laws and natural properties [that is, natural property-types] get discovered together'). (Nanay 2019, p. 508)

[10] According to Nanay:

> Property-types are to be derived from property-tokens and non-singular representations are to be derived from singular ones—and the only way to derive them is by means of the pragmatic usefulness of the non-singular representations that are based on singular representations. (Nanay 2019, p. 512)

> niobium ball in a way that is more similar, in the relevant respects, to the previous (token) successful attempts. (Nanay 2019, p. 510)

Nanay calls his view singularist semirealism, which is realist only about property tokens, represented by singular representations. The view is not realist about property types, laws, or models. Singularist semirealism provides an answer to the criticism that entity realists are supposed to be realists about theories. The answer is claimed to be metaphysically more parsimonious than those endorsing essentialism about entity- or property types (see Nanay 2019, sections 4 and 6; 2013; cf. my response to this criticism in subsection 3.4.1). I do not agree that Nanay's view is necessarily more parsimonious. I will come back to this claim in section 4.5.

4.4 Robust Realism

Another current entity realist account is "robust realism", developed recently by Eronen (2015; 2017; 2019), who argues for the following criterion.

> Robustness: "X is robust in the relevant scientific community at a certain time insofar as X is detectable, measurable, derivable, producible, or explanatory in a variety of independent ways." (Eronen 2015, p. 3967; 2017, pp. 2345-2346)

In this criterion, Xs are primarily entities, properties, and phenomena, but they may include other things as well. Robustness provides justification for our ontological commitment.[11] Robustness is primarily an "epistemological" criterion, which provides *justification* for "ontological" commitments. This commitment is a matter of degree. More independent ways of accessibility indicate a higher level justification for our beliefs in entities and their properties (2015, p. 3970; see also 2012).[12]

Real entities are accessible by independent experimental mechanisms or can be derived from several independent models. Different means of detection, measurement, or production of an entity in experimental situations, or of deriving the entity from independent hypotheses, justify believing in the entity's reality. Unless an entity really exists, it is unlikely that the entity is accessible in multiple ways, none of which depends on the others.

We are justified in relying on the criterion of robustness, but this criterion is not infallible. "Robustness is clearly a fallible criterion that is relative to a certain scientific community at a

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[11] According to Eronen (2017, p. 2352), robustness provides a sufficient justification. Sections 4.6 and 4.7 argue that robustness is not sufficient for a realist commitment. An employment of a causal account and the condition of non-redundancy are also needed.

[12] For instance, "neurons and biological organisms are extremely robust, while some fundamental physical particles such as the Higgs boson are (as of yet) robust to a relatively low degree" (Eronen 2015, p. 3974). Of course, Eronen concedes that his view "does not mean that biological entities or properties are more real or more fundamental than physical ones, but simply that we sometimes have more robust evidence for them" (2017, p. 2351).

certain time" (2015, p. 3966). Prospective technological instruments or theoretical resources may demonstrate that a currently postulated robust entity is not really robust and real. Nevertheless, as long as there is no defeater for believing in a robust entity, we are justified to consider it to be real.

Thus far, I have briefly discussed recent entity realists, each of whom has developed his view independently of the others. Among these views "robust realism" is the nearest to the views I have developed in the previous chapter. Still, it can be improved by benefiting from the other recent views of entity realism. In the remainder of the chapter, I aim to advance the project of entity realism by suggesting some exchange of views between these recent entity realists. Section 4.5 discusses the implications of Nanay's distinction between property tokens and property types for material inference and for robustness. Section 4.6 discusses the relationship between causality and robustness. Section 4.7 concludes the chapter by suggesting a criterion for the reality of property tokens.

4.5 Implications of Singularism

Egg, like Nanay, argues that experimental knowledge relies on the manipulation of *concrete* entities, whose properties should explain an experimental phenomenon. According to Egg, "a concrete matter of fact" is "opposed to a law" (2014, p. 56). If we accept from Nanay that theoretical laws are relationships between property types, it follows that concrete properties which are examined in experiments are indeed property tokens rather than property types. Concrete entities are also entity tokens. Accordingly, the definition of material inference can be rewritten as follows.

> Material inference': the inference from an actual or a hypothetical manipulation that results in ascribing to an entity token a property token for which there is a well-defined notion of what it means to modify the property token.

By actually or hypothetically intervening in the property token, as the putative cause, an actual or a hypothetical change in its effect takes place. The change allows us to infer the property token from the effect.

There may be two conflicts between Egg's causal realism and Nanay's singularist semirealism. First, Nanay might argue that a mere hypothetical inference is not enough to infer a real property token. Only an actual manipulation in a real experimental situation brings about a knowledge of real property tokens. In response, Egg could argue that it is too restrictive to be realist about detect*ed* property tokens only. If the (hypothetical or counterfactual) claim that a property token is detect*able* is true and his other two conditions (non-redundancy and empirical adequacy) are satisfied, the property token is real.

The three conditions are, according to Egg, well suited to support a realist commitment. I agree with Egg that realism should not be restricted to actually manipulated properties. Nevertheless, in section 4.7, I shall provide a more sophisticated criterion for reality than the three conditions Egg suggests. According to that criterion, even if not *actually* manipulated, property tokens should be considered real if they can exclusively explain the evidence obtained in various independent ways of detection/measurement.

Second, Egg might argue that the fact that *property tokens* are being manipulated in actual experiments does not imply that a real property token does not belong to a real property type. Indeed, the realist can hold on to real property *types*, as long as these are restricted to the right kind of properties, namely detectable ones that satisfy the three conditions.

A reason for realism about property types concerns the fact that science is not interested in exclusively individual tokens. Experiments need to be reproducible, in some sense and to some extent. Realism about types takes account of the issues of reproducibility, by the original experimenters or by other experimenters. Even a *particular* detection/measurement process (apart from its replication through different processes) needs to be itself reproducible. Reproducing an experiment seems to presuppose that the tokens which are tested belong to the same type.[13]

The significance of reproducibility in scientific practice is clearly acknowledged by Eronen, whose view is that if there are several independent ways of manifesting an entity, the entity may justifiably be claimed to be real. In his view, replicating an experiment in which an entity is manifested is central to scientific practice. Reproducing an experiment in almost the same way as the original one brings about a low degree of robustness. Replicating the experiment in several different ways brings about a high degree of robustness.

Can Nanay's singularist semirealism take into account the issues of reproducibility? I think it can but in a complicated way. According to Nanay's view, an experiment is almost never reproducible *in the exact way* it is conducted the first time. The entity tokens (e.g., electrons, genes, black holes) examined in an experiment are not the entity tokens examined in the next, reproduced experiment. Nevertheless, the reproduction of the experiment helps to check if *similar* tokens, those being classified in a type, behave similarly in certain circumstances. A variety of independent

[13] About the role of reproducibility in experimental practice, see Radder (1996, pp. 11-26 and 78) and (2012[1984/1988], Chapter 3); on "reproducibility" and "replicability", see the previous sections 2.2 and 2.6.

experimental techniques assure scientists that *similar* property tokens (that is, members of a nominal property type) can be ascribed to *similar* entity tokens (that is, members of a nominal entity type). In other words, if a property token of an entity token is materially inferred in an experiment, the property token is real. If the experiment in which the property token is inferred is reproduced, the nominal property type is derived. The nominal property type can be ascribed to a nominal entity type in this sense: in reality, similar entity tokens have similar property tokens. That is, entity $token_1$ has property $token_1$, entity $token_2$ has property $token_2$, entity $token_3$ has property $token_3$ and so on, such that entity $token_1$, entity $token_2$, entity $token_3$ and so on are similar entity tokens, belonging to a nominal entity type, and property $token_1$, property $token_2$, property $token_3$ and so on are similar property tokens, belonging to a nominal property type. The entity type and the property type are *epistemic* tools that help replicate experiments in a useful and reproducible way. However, they are not real.

Accordingly, singularist semirealism can explain the replicability of scientific practice and, at the same time, argue that we are not justified to be realist about epistemic tools such as the types that are used in replicating experiments. We are justified to be realist only about property tokens which are manipulated in particular experiments.

Put differently, Nanay does not have to disagree with the view that the theoretical concepts abstracted from the material realization of replicable experiments have nonlocal meanings; their meanings do not depend on the particularities of each experiment. For Nanay, abstract theoretical concepts are derived from concrete tokens; however, their nonlocal meanings do not necessarily refer to *real* types. Instead, they are useful ways of grouping real tokens. Replicating experiments helps scientists in a useful way to eliminate arbitrary, subjective, or irrelevant factors from the process of categorizing similar tokens.

However, Nanay's nominalism has two drawbacks. In the first place, it is questionable that singular representations are first acquired, and then the non-singular representations of types are derived. This would imply that there is no theory-ladenness, which contradicts theoretical perspectivism (which I endorsed in chapter 3). Theoretical concepts structure the performance and understanding of experimental and observational processes (see Radder 2003, pp. 164-169), so they cannot be simply "derived" from singular representations. Theories include type statements, and thus it doesn't hold water to claim that singular representations are prior to non-singular ones.

What's more, Nanay's nominalism is not really parsimonious. Instead of the commitment to property types, it may need to postulate a brute relation of similarity between property tokens in order to account for successful generalizations from one experiment to the next. Consider the equation for Newtonian force, $F=m\times\mathbf{a}$. According to singularist semirealism, one should be realist only about the singular representations of the equation, e.g., about $F_1=m_1\times\mathbf{a_1}$, $F_2=m_2\times\mathbf{a_2}$, … $F_n=m_n\times\mathbf{a_n}$, which are all acquired from a series of experimental practices. To explain why the general equation $F=m\times\mathbf{a}$ is successful, the singularist semirealist should presuppose that some real similarity exists between F_1, F_2, ... F_n, between M_1, M_2, … M_n, and between a_1, a_1, … a_n, and so on and so on. In contrast, the realist about types simply presupposes that F, M, and a are real types, whose instances stand in a certain relationship with each other. Singularist semirealism is (partly) correct in that, in actual experiments, tokens are manipulated. However, this view cannot sufficiently motivate realists not to commit to the knowledge of types ontologically. The nominalist view is not the simpler, the more parsimonious one. At least, Nanay's argument does not convince us that it is.

Let us reconsider the general definition of entity realism: experimental interactions with unobservable entities can bring about knowledge that deserves realist commitment. What is implied by this definition for the question of the reality of types? Do unobservable entities manifest themselves or their properties as real types through experimental interactions? The answer to this question is not available a-priori. The point of nominalists that in experiments tokens are manipulated is correct. Still, the knowledge of types may reflect real categories of nature. An electron is a telling example of a real entity token that can be understood as a member of the identical tokens of a real type. According to basic quantum physics, electrons are strictly identical in the sense of being all of the same type. Non-identity would have substantial, empirically testable implications. Other examples should be examined case by case; nevertheless, my main point is that if the identity of similar tokens is necessary to explain experimental evidence, one is justified (and required) to consider those tokens to be instances of a real type. At the end of this section, let me redefine the criterion of robustness.

> Robustness': a property type X is robust in the relevant scientific community at a certain time insofar as the property tokens belonging to the property type are detectable, measurable, derivable, producible, or explanatory in a variety of independent ways.

In robustness', X is specified as a “property type.” It is a “type” because several ways of exploring (similar or identical) property tokens lead us to speak about the (nominal or real) type of those tokens. In this way, Robustness' is still neutral about the debate between nominalism and realism about types. A type is robust if its tokens (i.e., its members) are real, irrespective of whether the a-posteriori evidence obtained in a variety of independent ways requires that those tokens are essentially identical. According to the criterion of robustness, other concrete members of a robust property type, including not-yet-experimentally-examined property tokens that are expected to behave similarly/identically in certain circumstances, are justifiably considered to be further real property tokens of the robust property type.

X is a "property" because (as Chakravartty, Egg, and Nanay assume) entities are primarily detectable/measurable through their properties. That is, epistemologically speaking, an entity manifests itself through its properties. This is true both about ordinary objects and about scientific entities. For instance, we do not see the "table-ness" of a table, but we experience its shape, color, and so forth. Similarly, when scientists discover an entity, they indeed detect/measure one or more properties of the entity. If at least one property of an entity is accessed, the entity itself is also accessed.

The next section mainly aims to reconcile material inference' and robustness'. Section 4.7 further pursues this aim by using material inference', robustness' and non-redundancy to provide a sufficient criterion for reality. In sum, I employ Egg's three conditions but instead of his condition of material inference I use material inference' and instead of his condition of empirical adequacy I use robustness' (which provides a stricter version of empirical adequacy).

4.6 Causality and Robustness

The main idea of this section is that the existence of a cause is materially inferred, which means that the manipulation of the cause should be well-defined. Also, the cause is specified in practice on the basis of the empirical evidence obtained in a variety of independent ways. As a result, causality is defined according to the manipulability account of causation, but in practice the cause is inferred from the evidence obtained in a variety of independent ways. In what follows I first show the need for a causal account of the realist interpretation of robustness and then draw some further conclusions.

Eronen maintains that

> [causal] criteria are entirely compatible with the robustness approach. For example, one could argue that the ultimate metaphysical criterion for what is real is the causal criterion, but the source of justification for science-based ontological commitments is robustness. (2015, p. 3973, n. 15)

Elsewhere, nevertheless, he claims that the advantage of his view over causal realism is

> that it moves beyond causal motivations for realism, and is based on the notion of *robustness* instead. (2017, p. 2342)

Eronen is right that robustness is compatible with causal realism; however, it is not completely clear what he means by moving beyond causal motivations. If the criterion of robustness is employed in a realist view, Eronen should be explicitly committed to a causal account. After all, the properties of unobservable entities are not directly accessible. Empirical evidence of properties is obtained through technological instruments. In Chakravartty's words, trustworthy properties "are *causally* linked to the regular behaviours of our *detectors*" (2007, p. 47, my emphases).[14]

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[14] A critic might argue that the *causal* link is not necessary. A strong or full correlation between the object and the final state of the apparatus is enough for epistemological reasons (see Radder 2003, p. 153). This view may be correct. Nonetheless, my primary concern is to show that to establish a property, an *inference* from evidence (obtained by an apparatus) is necessary. Thus, I am sympathetic to the views of Chakravartty and Egg that this inference is based on a causal link. However, even if the inference is based not on a "causal" link but on a weaker kind of relationship,

Experimental results are detected by means of instruments; accordingly, property (tokens) of an unobservable entity, as the cause of the experimental results, is justifiably inferred (by material inference'). Even if evidence of the property is obtained in several independent ways, the *inference* from the evidence to the reality of the property is necessary for a realist. Due to the prevalence of technological instruments in scientific practice, knowledge of unobservable entities is mostly based on inferences from empirical evidence obtained by instruments, as the effect of the entities' causal properties.

To put it another way, an antirealist such as Bas van Fraassen (1980) would agree that evidence obtained from different, independent ways is empirically adequate but disagree that realism follows from this kind of evidence. What distinguishes robust realism from robust antirealism is the fact that empirical evidence is the effect caused by a real unobservable entity. The cause, or more precisely the unobservable entity's causal property, is *inferred* from the empirical evidence. Therefore, if Eronen is a realist, he cannot avoid explicitly employing a causal account.[15]

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it is still correct that the property is *inferred* from the evidence to which the property correlates by means of the apparatus.

[15] Eronen accepts that his account cannot satisfy a "staunch antirealist (along the lines of Van Fraassen 1980)". He adds that "my aim here is not to give a full-blown defense of scientific realism, but to show how robustness provides new ways of approaching the issue and new possible arguments" (2015, p. 3974). My assertion is that he could distinguish his robust *realism* from antirealism by subscribing to a causal account.

To further clarify my realist account, I will briefly discuss Nora Boyd's (2018) recent view of empirical evidence. She shares the antirealist commitment of Van Fraassen but employs "enriched" concepts such as "empirical results" or "empirical evidence" instead of "observables" or "experience". The former concepts are more faithful to the results gained by means of "intricate instruments and techniques prevalent in scientific research today" (2018, section 2). Her view that empirical evidence can accumulate, be merged, and constrain rival models allows us to understand current science, in which a large volume of data is gathered and analyzed to test scientific models. Boyd also speaks of "a causal story connecting the target of interest to the generation of the results". However, the role of the causal connection between empirical evidence made available by the instrument and "the target of interest" is not sufficiently developed in Boyd's view and it seems to conflict with her commitment to antirealism. The causal connections of scientific instruments with reality can distinguish evidence of real entities from illusory results and support a *realist* view. Empirical evidence is causally connected to properties of real entities. This connection cannot be found in what Van Fraassen calls a "public hallucination" such as the experience of a rainbow.[16] After all, it is not necessary to presuppose a colorful object in the sky

[16] Claims about non-existent entities may be wrong or even illusory, but calling them hallucinations seems inappropriate. In particular, calling a rainbow a (public) hallucination is questionable. A "hallucination" is usually defined as a sensory experience in the absence of an external stimulus; an "illusion" as a misperception of an external stimulus. Thus, a person considering the rainbow as a colorful object in the sky experiences an illusion rather than a

to explain the rainbow. In explaining a rainbow, we do assume the existence, composition and dispersion of (white) light rays in the presence of water drops. Therefore, we consider these rays to be real; however, we do not presuppose the existence of a colorful object. The explanation of the rainbow does not depend on a causal relationship between some colorful object and our experience. For another example, ions do need to exist to bring about the trace in the cloud chamber as their effect. Or astrophysical entities such as gravitational waves need to exist to bring about the evidence detected by instruments such as the Virgo and LIGO detectors, of course on the condition that the evidence is detected by *several* independent means.[17]

The measured/detected evidence is the effect of an unobservable cause on a technological instrument. Sometimes, the effects are actively *produced* and then detected/measured. For instance, in the experiment where Donald Eigler and Erhard Schweizer employed xenon atoms to inscribe "IBM" on a nickel surface, "our ability to *produce* such pictures testifies to our knowledge about atoms" (Egg 2014, p. 102, emphasis added). The pictures produced are indeed effects to be explained by the properties of atoms, as their causes. In this case, and more generally in experimental particle physics, detectable/measurable effects are deliberately produced by actually modifying their causes. Sometimes, however, the evidence is not produced by actually manipulating entities. For instance, in observational astrophysics, the evidence of gravitational

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hallucination. That said, in this and the following chapter when I discuss Van Fraassen I use his concept of the "public hallucination" to be faithful to his terms.

[17] Thus, my "causal" account here should be understood in line with my views of perception/evidence versus illusion/hallucination in sections 2.5 and 5.5.

waves or black holes is not produced by modifying the waves or holes. Still, it is *potentially available* on or near the Earth, and can be actively detected/measured by our observational apparatuses. Evidence is always evidence regarding a hypothesis, so in these cases suitable instruments are prepared to measure/detect the empirical results relevant to the hypothesis, and therefore can be used to acquire evidence for or against the hypothesis. We cannot actually modify the origin of black holes or gravitational waves in order to observe the changes in their effects. To be a realist about astronomical entities, it is enough to detect/measure their signals by multiple means of detection/measurement. In sum, although the role of the production of effects in controlled experiments should not be ignored, such productions do not play a part in all experimental/observational practices.

I have argued 1- that a *realist* view of robustness requires the active employment of a causal account; and 2- that the evidence of a real unobservable entity is not always produced as a result of an actual manipulation of the entity; it may be potentially available to be detected/measured by instruments experimenters actively prepare and use. These two points are consistent with each other, since according to the manipulability account of causation, an actual interference in the cause, and therefore the production of the evidential effects, is not necessary. Rather, there must be a well-defined notion of what it means for the cause to be manipulated. To consider the cause to be a *real* property token, other necessary conditions should also be fulfilled. According to Egg, the presupposed cause should also be non-redundant and empirically adequate. To establish highly empirically adequate hypotheses, I suggest that the criterion of robustness' is employed instead of that of empirical adequacy (more on this suggestion, in the next section).

Thus, suppose that only certain property tokens (all belonging to a property type) can explain the relevant experimental evidence, and that a variety of independent means of detection/measurement provides the evidence of the property tokens such that the evidence could not have been detected/measured by independent means unless the property tokens exist. In that case the property tokens are the real causes of the evidence obtained. In other words, the property tokens would be the *real causes* of detected/measured evidence, on the condition that *only* they explain the evidence and the evidence is obtained through *a variety of* independent means. In the following section, I will provide a more fine-grained version of this criterion for reality.

4.7 A Criterion for Reality

In this section, I intend to incorporate all the convincing aspects of the recent views of entity realism into a criterion for reality. Eronen suggests that a specific kind of evidence, associated with robustness, supports a realist commitment. However, as I have argued, for a realist view of robustness, it is necessary to *causally infer* reality from this kind of evidence. Egg's material inference, or more precisely, its refined version, i.e., material inference', could explain the causal part. While Egg employs the three conditions to single out the specific kind of evidence that justifies a realist commitment, it is preferable to substitute the condition "empirical adequacy" with the kind of evidence associated with "robustness". This allows realists to be committed, more cautiously, only to the empirical evidence gained from a number of independent ways/perspectives. Suppose evidence is gained only through one particular kind of instrument. In that case, a strict realist may be reluctant to place his or her ontological trust on the hypothesis confirmed by the evidence, even if the hypothesis satisfies the conditions of non-redundancy and material inference' as well.

What matters here is indeed the cogency of the criteria proposed both by Egg and by Eronen. It seems inappropriate to see either criterion as superior. Instead, these different insights of entity realists can reinforce each another to provide a more compelling criterion for reality. By taking into account these insights and the points I have made, I suggest the following criterion for the reality of property tokens.

> Insofar as only a type of property tokens can explain the evidence obtained in a variety of independent ways of detection/measurement, the property tokens may be taken as real in the relevant scientific community at a certain time.

In this criterion, Nanay's distinction between a property type and its concrete members, the property tokens, is respected. "Property tokens" play a justifiable explanatory role in concrete experiments, and consequently they can be real. In general, the criterion remains impartial regarding the nominalism versus realism debate about types. However, it accepts that a "type" of property tokens may be real if the evidence obtained in a variety of independent ways of detection/measurement requires that the property tokens are identical instances of a type, as in the case of electrons. Otherwise, the property tokens are merely categorized in a nominal "type" thanks to their similarities.

The criterion is primarily a criterion for the reality of property tokens. Nonetheless, it can also imply that a property type can be robust, in the very sense that its concrete members are real property tokens, regardless of whether or not a-posteriori evidence suggests that those robust property types are real essences.

Our confidence in the reality of property tokens is a matter of degree, hence my use of the qualifier "insofar as". Eronen's qualification that a thing is considered to be robust "in the relevant scientific community at a certain time" is also retained to alert us that the manifestation of property tokens depends on historically contingent technological instruments and conceptual theories provided by the scientific community. Experimental results are fallible and are conditioned on instrumental and theoretical resources. Because of these qualifications, the property tokens "may be taken as" real. One can hardly claim that they are $objectively_1$ real. These qualifications show that entity realism is compatible with the perspectivism I have developed in the previous chapters (see specifically my section 3.5). However, the reliance of the criterion for reality on scientific communities should not be understood in a relativist way. As chapter 2 argued, human-independent potentialities can be realized in different socio-material contexts, hence there is a certain degree of continuity or persistence among (theoretical or instrumental) perspectives. Chapter 6 will argue that the true descriptions of those (properties of) entities that are taken as real in a past scientific community can be restated from the broader perspectives of newer theories. Therefore, there is also a continuity between historical perspectives. In sum, although the property tokens are taken as real in the relevant scientific community at a certain time, their reality may be acknowledged by other communities as well. For this reason, my criterion for the reality of property tokens does not lead to relativism.

The criterion provides a sufficient (and not a necessary) condition for reality. There may be real property tokens that do not fulfill the criterion. However, if similar property tokens meet the criterion they are probably real.

The word "only" is added so that the criterion complies with Egg's condition that a causal hypothesis should not be non-redundant. If different types of property tokens can explain the evidence, the criterion is not satisfied.

A central concept employed in the criterion is the concept of evidence. Empirical evidence includes not only the effects of causal properties that our unaided organs can directly observe, but also the results gathered by scientific instruments. The latter kind of evidence is especially pervasive in science. It is the "detection/measurement" of evidence by instruments that provides our access to scientific entities. The evidence is "obtained" by making experimental efforts. The evidence may be produced in the experiments or it may consist of detectable/measurable signals, whose causes are not actually manipulated.[18]

The criterion concedes that causal properties are not directly detectable/measurable. They are "inferred" when only they can "explain" the evidence. Being explanatory is not an independent way of having access to a property apart from being detectable/measurable. If only the existence of a property token can explain the empirical evidence, the token exists. Our confidence in the existence of other property tokens, all of which belong to a property type, increases when a variety of independent ways of detection/measurement present evidence that is explainable only by the existence of those property tokens that belong to the property type of which the previously examined property tokens are members.

The causal inference from evidence to a real property token is reasonable when "a variety of independent ways" of detection/measurement agree that certain property tokens make the evidence happen. Thus, these property tokens (and not necessarily their type) are causally responsible for the evidence. It is virtually impossible to actually manipulate astrophysical objects. Still, we are justified to consider them to be the real causes of the evidence, provided that several independent means of detection/measurement confirm the inference from the evidence.

The emphasis of entity realists on the causal connection of property tokens (as causes) with empirical evidence (as the effects of the causes) distinguishes their realist view from antirealism. I have briefly discussed causality. My point was that entity realists are required to presuppose a

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[18] On the notion of evidence, see also sections 2.5, 5.4 and 5.5.

causal relationship. The manipulability account of causation (used in material inference') and the concept of robustness allowed me to explain the causal inference of property tokens from the evidence obtained in a variety of independent ways. The current chapter has aimed to open a dialogue among recent entity realists, which has culminated in the proposal of a criterion for the reality of property tokens.

Chapter 5 From Phenomenological-Hermeneutical Approaches to Realist Perspectivism

5.1 Introduction

During the last decades of the twentieth century, philosophers such as Patrick Heelan (1983; 1986; 1997), Joseph Kockelmans (1993), Theodore Kisiel (1970), Joseph Rouse (1987), Gary Gutting (1978), Robert Crease (1993) and Don Ihde (1991) have discussed scientific practice from phenomenological and hermeneutic viewpoints. In their analysis of science, they address ideas both from analytical history and philosophy of science and from works of Edmund Husserl, Martin Heidegger, Maurice Merleau-Ponty, Hans-Georg Gadamer, among others. I call these kinds of reflection on science the phenomenological-hermeneutical approaches to the philosophy of science.

In recent years, philosophers such as Shannon Vallor (2009), Don Ihde (2011; 2016), Harald Wiltsche (2012; 2017a; 2017b), Philipp Berghofer (2018a; 2020; see also Berghofer et al. 2020), and others (see Wiltsche and Berghofer 2020, chapter 1) have advanced these phenomenological-hermeneutical approaches. In this chapter, I would like to contribute to the debate among these philosophers. The focus is on disagreements between Vallor, Ihde, and Wiltsche. Due to the reliance of Ihde and Vallor on Heelan's account of instrumentation, Heelan's view is critically discussed as well. I also make use of ideas from Kockelmans to support my arguments. The scope of the chapter is confined to the scientific realism debate, in particular to the role of instrumentally-mediated empirical evidence.

I argue *against* both antirealism and the realist claim that scientific observation is the perception of unobservables, and *in favor of* the realist view that mediated access to scientific entities is possible by means of evidence provided by technological instruments and interpreted by theoretical concepts. I then argue that this realist view is perspectival. Several phenomenological-hermeneutical ideas entail a realist perspectivism. There is little work on the relationship between phenomenology-hermeneutics and perspectivism. Berghofer's 2020 paper is an exception. I agree with him that there are strong affinities between perspectivism and phenomenology. This chapter provides novel affinities, and argues that a realist perspectivism can help to resolve disagreements between a realist such as Ihde and an antirealist such as Wiltsche.

The argumentative surplus of this chapter consists in the novel affinities that it shows between the phenomenological-hermeneutical approaches and realist perspectivism. Thus, those approaches support realist perspectivism; conversely, realist perspectivism can be considered to be an attractive viewpoint for the phenomenological-hermeneutical approaches. The other surplus of this chapter concerns the explanation of the way realist perspectivism can settle certain disagreements among the advocates of the phenomenological-hermeneutical approaches to philosophy of science.

The thread that leads the reader through the discussion in this chapter is as follows. Sections 5.2 and 5.3 critically review the relevant literature regarding the scientific realism debate. Section 5.2 discusses the debate between Ihde and Wiltsche about Husserl's view of the lifeworld. Section 5.3 explains the phenomenological defense of realism. Vallor argues for the explorability of empirical evidence. Ihde and Heelan argue that instrumentally-mediated observations are perceptions of scientific entities. My more direct contribution to the debate starts in section 5.4, which questions the realist claim that scientific entities are perceivable by means of instruments. After that, I argue in section 5.5, with the help of the concept of "exploration" and the distinction between "manifestation" and "phenomenon", that scientific observation differs from what Bas van

Fraassen calls “public hallucination”. Although a scientific entity may not be perceived, it may be real inasmuch as *its evidence* is successfully explorable through several instrumentally-mediated processes. Explorability is necessary for a realist claim. In this regard, it is comparable with robustness: the evidence of a robust thing is explorable.[1] This supports realism, but as section 5.6 argues, based on phenomenological-hermeneutical ideas this realism should at the same time be perspectivist. The result is a realist perspectivism that acknowledges the role of theoretical concepts and inferences in science (as the hermeneutical character of science), agrees with Vallor’s analysis that scientific observation is horizonal, confirms Heelan’s and Ihde’s emphasis on the key role of instruments in science, denies “scientific objectivism” (which Husserl and Wiltsche attack), and is consistent with Kockelmans’s Heideggerian conception of truth. In concluding the chapter, I reevaluate the criticism of Ihde against Husserl.

5.2 Debate about the Lifeworld

In *The Crisis of European Sciences and Transcendental Phenomenology*, Husserl challenges an objectivist understanding of science. According to him, the main problem of this understanding is that it forgets that the origin of the scientific worldview is in the “lifeworld” – the world of our everyday views and experiences. The false assumption of the objectivist view is that the truly

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[1] Sections 4.6 and 4.7 explained why material inference' and non-redundancy should be added to robustness' to provide a sufficient criterion for reality. Like robustness', explorability is a necessary and not a sufficient condition for a realist claim.

objective image of reality is the one provided by science alone.[2] Husserl considers Galileo as the founding father of this view. Galileo, as Husserl claims, developed the metaphysical view that quantitative, measurable objects form the fabric of reality, so the only approach to truly understand the world is that of the mathematical sciences.

Husserl himself believes that what he calls the basic "fundament" of knowledge is formed by subjectively given experiences, on the basis of which "the mathematically substructed world of idealities" of scientific models is formed. The "only real world", as stated by Husserl, is "the one that is actually given through perception, that is ever experienced and experiencable—our everyday life-world" (Husserl 1970, pp. 48–49). Science without given perceptions is impossible. Science is constructed by idealization and mathematization of the complex perceptual experiences of the lifeworld. The main problem of the Galilean metaphysics, Husserl believes, is that if it is true, science loses its foundation. Following Wiltsche (2012; 2017a) I call this the problem of scientific objectivism.

It is necessary to see that "scientific objectivism" is not the position that scientific objects exist human-independently, which is uncontroversial (see chapter 2). In the context of the current chapter, scientific objectivism should be understood as the position that (only) science can deliver a purely objective view on the world. The view that the current scientific image is close to the ideal truth is a form of this objectivism. Against this, critics of scientific objectivism argue that scientific knowledge is bounded by specific conditions (or perspectives) and is not ultimate. In other words,

[2] Wilfrid Sellars echoes the objectivist view when he says that "science is the measure of all things, of what is that it is, and of what is not that it is not" (1963, p. 173).

scientific knowledge is not *objective*$_1$ (see subsection 3.4.2; see also sections 6.2 and 7.4). Scientific objectivism$_1$ is not a strawman because all those philosophers, scientists, science journalists and ordinary people who maintain that scientific knowledge is "approximately true", in the sense that it nearly corresponds to the world in itself, presuppose this kind of scientific objectivism.

Let us now review Ihde's view of science. He (2011; 2016, chapter 2) argues that Husserl gets both Galileo and science wrong because, according to Ihde, Husserl neglects the role of instruments in science in general, and in Galileo's work with the telescope in particular. Husserl, Ihde believes, focuses exclusively on the mathematical side of Galileo's work, while Galileo was also a "lens grinder", "the user of telescopes", "the fiddler with inclined planes", and "the dropper of weights from the Pisa Tower" (Ihde 2011, p. 78). This aspect of Galileo's practice makes a huge difference for Ihde, since "Galileo with a telescope is considerably more than a calculator or mathematician, Galileo with a telescope is also a perceiver and a practitioner within a now technologically mediated, enhanced world" (Ihde 2011, p. 80). Ihde claims that if we consider Galileo's work in practice (rather than only his words), we understand that he "never leaves the lifeworld", but "makes dimensions of the newly enhanced lifeworld open to perceptual-bodily experience" (Ihde 2011, p. 80). The reason is that the lifeworld is not limited to what is observed by bodily perception, but it also includes what is perceived by technological instruments. Instruments, according to Ihde, enrich human perception; Galileo's telescope, for instance, made previously invisible objects such as mountains on the moon intersubjectively visible. Instruments extend the bodily-sensory capacities of human beings (Ihde 1991, p. 75; 1979, pp. 35–39), and therefore their perceptual (life)world. All in all, Ihde contends that as a result of this instrumental embodiment of science, “science remains thoroughly immersed in the lifeworld” (2011, p. 69); see also Ihde (1991, pp. 102–103).

In his account, Wiltsche accepts that, from a historical point of view, Husserl’s discussion of Galileo does not include the “hands-on” character of Galileo’s work. He also concedes that the practical aspects of scientific activity “are largely absent in the *Crisis*” (2017a, p. 157). However, he complains about the one-sidedness of Ihde, who only focuses on Galileo’s work in astronomy and neglects such areas of science as mechanics and kinematics, in which *idealization* and *model building* play seminal roles. According to Wiltsche, if one scrutinizes the work of Galileo on straight, accelerated and projectile motion, one can recognize Galileo’s technique of idealization, by which he “impose[s] a geometrical grid” on “the complexity and messiness of the Lifeworld” in order to exclude what he assumes as irrelevant factors and to reduce the complex lifeworld we initially perceive to what the model represents. Accordingly, Galileo built his mechanics through idealization of what was perceivable by unaided senses. The objects of Galileo’s mechanics are, for instance, a frictionless plane or a point mass that moves along a perfect projectile trajectory. These are the idealized forms of the observable states of affairs. Thus, Wiltsche argues that, because of this idealization, Galileo’s mechanics is applicable only to objects of his ideal model rather than to the complex reality.

According to Wiltsche, considering the role of instrumentation in science does not preclude an analysis of Galileo’s idealizations (2017a, p. 165). What is observed through instruments is not simply used in scientific models. It is their idealized results that are used in models. For instance, Galileo did not simply deduce from his experimental results that the gravitational force is independent of features of bodies other than their masses and distances, but he employed interpolation and extrapolation techniques “in order to advance an *ideal limiting case*” (2017a, p.

169). Wiltsche concludes that studies of experiments and instruments or other practical aspects of scientific activity cannot ignore the role of idealization in constructing scientific models. This idealization is, according to him, the main factor that distinguishes the objectively understood scientific worldview from that of the human lifeworld.

Among philosophers of science, Nancy Cartwright (1983) argues that reality is messy and complex, and no idealized concept can perfectly describe a real thing. Also, natural laws are only true in controlled experimental situations and by considering some sorts of approximation. Thus, it may be concluded that scientific knowledge is conceptually imprecise. Yet, from the fact that science uses idealizations it does not follow that scientific knowledge is conceptually imprecise. The more tenable idea is that the complexity of the world in the face of our limited computational abilities is the reason why we need idealizations. But in contemporary physics, several appropriate physical corrections are routinely made to eliminate simplifying assumptions and idealizations (see my discussion of "a perfect vacuum" in section 7.3). Accordingly, it is fairly controversial to argue for the imprecision of scientific knowledge, and then for antirealism, merely on the basis of idealizations.[3]

Still, Wiltsche claims that "only" the antirealist "line of argument prevents the problem of [scientific] objectivism" (2017a, p. 172). To avoid misunderstanding, in this chapter my point is not to assess Wiltsche's interpretation of Husserl's view of science, but to argue against his defense

[3] Michael Weisberg (2007) discusses three kinds of idealization and concludes that they are compatible with a sophisticated definition of realism.

of antirealism. This defense is based on a reading of Husserl's principle of principles [PP], according to which

> every originary presentive intuition is a legitimizing source of cognition, that everything originarily (so to speak, in its 'personal' actuality) offered to us in 'intuition' is to be accepted simply as what it is presented as being, but also only within the limits in which it is presented there. (Husserl 1983, p. 44)

An item X is intuitively presented when the "intention towards X is fulfilled by the direct, immediate presence of X" (Wiltsche 2012, p. 108). With the term "originary" Husserl emphasizes that the presentive intuition of X is given directly to me, so it is different from a recollection of X or an imagination of X, in both of which I intend X indirectly (see Zahavi 2003, p. 95).

Wiltsche tries to clarify PP by referring to Husserl's definition of a physical object as "the possible object of a *straightforward* percept" that is "capable of being perceived" (in Wiltsche 2012, p. 110; and 2017b, p. 818). But might this "possibility" or "capability" not be a merely logical possibility that is applicable to any object of imagined worlds? Wiltsche does not think so. He believes that Husserl's concept of a "motivated possibility" can be an indicator of the possibility of a straightforward percept. An "assertion about an object" enjoys motivated possibility if "the object *could* become present in appropriate intentional acts" (Wiltsche 2017b, p. 821, emphasis added). Indeed, Wiltsche tries to understand PP in the sense of Van Fraassen's (1980, p. 16) principle of observability. That is, if there is a possible circumstance in which an object is at least *potentially* observable for an actual person, i.e., a "bodily situated subject" (Wiltsche 2017b, p. 818) rather than "an empty logical possibility" (Wiltsche 2012, p. 111), that object is observ*able*. For instance, there is no possible circumstance in which we as actual observers can see ions, and so they are unobservable. One may object that ionized particles can be observed with the aid of cloud chambers. Again following Van Fraassen (1980, p.17), Wiltsche responds that what is observable in this case are the silver-grey lines, whereas scientists *infer* that ions—as theoretical entities—are responsible for these lines. Thus, ions themselves are not observable; rather, their supposed effects are. Unlike Ihde, Wiltsche asserts that scientific instruments should not be considered as the extensions of our body, but they are the "engines that produce new observables for us to apprehend" phenomena through model building and theorizing (Wiltsche 2012, p. 117; 2017b, p. 823; cf. Van Fraassen 2001, p. 154; 2008, Chapter 4). I agree with Wiltsche and Van Fraassen that microscopic entities are not perceivable/observable, and therefore our knowledge of them is inferential. Furthermore, sections 5.4 and 5.5 argue that even if such entities are not perceivable, the empirical evidence obtained by instruments possesses perceptual characteristics, which makes it possible to acquire inferential knowledge of unobservable entities. In addition, section 5.6 argues that this view results in a perspectivist form of realism. But first I will further evaluate Wiltsche's view and critically review the realist motivations of philosophers such as Vallor, Ihde, and Heelan in section 5.3.

5.3 Motivations for Defending Realism

5.3.1 Inferential reasoning for unobservables

Wiltsche restricts his realist view to those objects whose existence is not justified by an act of inference. By so doing, he maintains his antirealist view about unobservables, those entities "merely given by means of inferential reasoning" (Wiltsche 2012, p. 114). However, this restriction is inconsistent with Husserl's foundationalism, according to which we are justified to believe both non-inferentially justified beliefs (i.e., basic beliefs such as perceptual beliefs) and the beliefs inferentially depending on them (i.e., non-basic beliefs, which are deductively, inductively or abductively inferred from basic beliefs). "[T]here is convincing textual evidence that Husserl needs to be interpreted as a moderate foundationalist" (Berghofer 2018b, p. 3).[4] Husserl's epistemology, accordingly, allows us to believe in objects whose existence is justifiably inferred. Thus, one need not restrict one's realist view to those objects whose existence is (claimed to be) non-inferentially justified.

Wiltsche might respond that only those objects whose existence *can be* non-inferentially justified may count as justifiably inferred. That is, in order for an object to be real, it should be possible that our knowledge of the object is a basic belief. This response is implausible, because it is unclear why justified beliefs need to be restricted to those that possess the possibility of being basic. A mathematical equation (say, $(a^2+b^2)(x^2+y^2) = (ax+by)^2+(ay-bx)^2$) may be justified, even if it cannot be a basic belief. Similarly, a belief about a microscopic entity need not have the possibility of being basic in order to be justified.

According to Husserl's foundationalism the knowledge of scientific entities *can be* inferentially justified. For instance, atoms are not experiencable but one can justifiably infer their knowledge, as a non-basic belief, from empirical evidence of atoms, as a basic belief. Along these lines, section

[4] According to *moderate* foundationalism, basic beliefs are adequately justified, but they are not infallible, indubitable, or incorrigible. They can be defeated by other justified beliefs. *Moderate* foundationalism is also compatible with the view that (basic or non-basic) beliefs can get extra justificatory support from their coherence with other justified beliefs (Berghofer 2018b, section 1.2).

5.5 argues that the knowledge of real entities can be inferentially justified, so we are justified to consider them real. But first, in subsections 5.3.1 and 5.3.2, I critically discuss the phenomenological views of philosophers such as Vallor, Ihde, and Heelan, who argue that the knowledge of scientific entities rests on a perceptual basis. The insightful, acceptable part of their views, as section 5.4 further clarifies, results in the claim that empirical evidence of unobservable entities (obtained by instruments) possesses perceptual characteristics, even if they are not perceivable by naked eyes.

5.3.2 Explorable perceptual horizons

Vallor's (2009) argues for a form of experimental realism. That is to say, she supports Hacking's experimental realism but also modifies his criterion for determining real entities. Hacking (1983) restricts his realist claims to the existence of the entities that can be (instrumentally) manipulated by being used as tools to interfere in other phenomena. However, this criterion leads to antirealism about entities that are not manipulable. This is why Hacking (1989) is antirealist about astronomical entities such as black holes; furthermore, according to Vallor, his criterion cannot sufficiently explain the engagement with microscopic entities that are perceptual in the phenomenological sense but not manipulable in Hacking's sense. Two examples of such engagements are the case of "Rutherford's encounter with the nucleus" and "Leeuwenhoek's encounters with microorganisms" (Vallor 2009, p. 19). In addition, Vallor maintains that her account can acknowledge that posits like Gelfert's (2003) "quasi-particles" are unreal, although they are manipulable in Hacking's sense (Vallor 2009, p. 20).[5]

Vallor's argument is mostly based on Husserl's (1960) view of the horizonal nature of perception, on Merleau-Ponty's (1968) view of "empirical pregnancy", and on Heelan's view that scientific instruments are extensions of human embodiment. Husserl argues that a real thing is perceived in a "horizon". The thing is surrounded by other things in a larger context. In addition, it possesses other aspects, or "profiles", which can be disclosed by further explorations. Accordingly, any feature of the present context of the thing and also all of its (other) possible profiles are included in the horizon of a thing (more on this horizonal account of observation below). Merleau-Ponty's idea that real things are "empirically pregnant", in a non-metaphorical sense, means that a real thing enjoys *possibilities*, which provide *actual* experiences when the perceiver actively engages with the thing. The engagement or encounter with the real thing takes place through our embodiment, or what Merleau-Ponty calls the "flesh". According to Heelan, this embodiment can be extended to instruments used in observations and experiments.

A central point of these accounts of perception is that objects cannot be observed through passive sensation. Instead, they are manifested through active explorations. Indeed, as a result of the "pregnancy" of a real thing, its perceptual horizon is explorable. That is, one can actively keep exploring different profiles of the thing.[6] According to this view, experimental experience of microscopic entities also has perceptual status. Profiles of a microscopic entity are fulfilled when

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[5] On Hacking's views, see also my sections 3.2 and 4.1.

[6] On the active and passive dimensions of perception, see section 2.5.

one starts, and proceeds with, the experimental exploration of the features and profiles of a microscopic entity by means of scientific instruments.

> When experimenters employ instruments in complex performances directly revealing empirical horizons pregnant with new features and profiles to be bodily explored, such performances are perceptions in every epistemically important sense of the term. (Vallor 2009, p. 16)

If one takes the active, engaged aspect of perceptual experience seriously, one admits that a scientific entity is real insofar as it is practical to experimentally engage with it in order to explore its various profiles. The sign or evidence of a microscopic entity "becomes manifest to the experimenter in a perceptual style" (Vallor 2009, p. 15). If the exploration of a scientific entity continues, insofar as the profiles of the entity are explorable through our different engagements, we may have reason to support realism:

> robust grounds for realism emerge only when the data are manifested in a perceptual style, that is, belonging to a horizon pregnant with kinaesthetic-sensory possibilities revealing the thing in an open but concordant series of explorable profiles. Cells, electrons and protons, unlike phlogiston, were revealed in experimental praxis as pregnant with such profiles (Vallor 2009, p.19).

The profiles of a microscopic entity are instrumentally explorable. The view that the perceptual horizons of things are explorable is insightful, and in section 5.5 I shall benefit from it in my argument for realism. Before that, it is necessary to discuss the claim that experimental instruments extend human embodiment.

5.3.3 Instrument use as extended embodiment

This subsection explains the view that the use of experimental instruments can be seen as an extension of embodiment. To do so, I first clarify the notion of embodiment. Phenomenologists argue that embodiment is the condition of the possibility for perceiving ordinary objects:

> spatial objects can only appear for and be constituted by embodied subjects. ... the body is a condition of the possibility for the perception of and interaction with spatial objects …. every worldly experience is mediated by and made possible by our embodiment. (Zahavi 2003, pp. 98–99)

Three important concepts are used in the quotation: "constituted by", "mediated by", and "made possible by". Each emphasizes an aspect of a single idea: the mediation of the body makes possible the constitution of the object. The concept of "constitution" can be further clarified as follows.

> Constitution must be understood as a process that allows for manifestation and signification, that is, it must be understood as a process that permits that which is constituted to appear, unfold, articulate, and show itself as what it is. … As Heidegger was to observe: "Constituting" does not mean producing in the sense of making and fabricating; it means *letting the entity be seen in its objectivity*. (Zahavi 2003, p. 73)

Perceptual experience is disclosed or brought to awareness through embodiment. That is, embodiment is a necessary condition for the manifestation of an object.

Heelan claims that embodiment is not limited to human embodied organs. Technological instruments can also make possible perceptual experiences: "The possibility of embodiment in readable[7] technologies … follow[s] from deep roots in *the conditions of possibility of* perception" (Heelan 1983, pp. 210-211, emphasis added). Merleau-Ponty's account of perception is usually employed to support the view that instruments are the extensions of human embodiment. A blind person's cane constitutes the person's perceptual experience (see Merleau-Ponty 1962, p. 143; Heelan 1983, p. 150). When the blind person is using the cane, or in Heidegger's terminology when the cane is a "ready-to-hand" tool (1927, section 15), it is not seen in an objective way, but considered as part of the people's embodiment that discloses their world. The so-called tactile-visual sensory substitution (TVSS) technology can similarly help a blind person to acquire a vision of the environment by sensing tactile stimuli.

> Once the subject is habituated to the tactile stimulation the technology itself ceases to be an object and is incorporated into the body in a way that discloses the world. Such technologies … become part of the body that we live. (Gallagher and Zahavi 2012, p. 157; see also Heelan 1983, pp. 200-201)

It is similarly argued that experimental instruments extend human embodiment, and therefore, make possible the manifestation of scientific entities that are not perceivable by human sensory organs alone. The heart of Ihde's "instrumental" realism is the idea that "what has previously been thought of as "theoretical" becomes replaced with the instrumentally "observable", and in differing degrees, this observability in turn becomes part of a new perceptual region" (Ihde 1991, p. 107). This basic idea of Ihde's realism can originally be found in Heelan's work.

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[7] In section 5.4, I will explain the meaning of this "readability".

> Now the position I have been defending is that theoretical states and entities are or become directly perceivable (alternatively, "observable," in the stipulated sense) because the measuring process can be or become a "readable technology," a new form of embodiment for the scientific observer. In this view, the term "observation" no longer means unaided perception. It implies that theoretical states and entities are real … because (and to the extent that) they are perceivable in the perceiver's new embodiment. It also implies that the nature and aim of scientific explanation is to make manifest the processes and structures of the real. (Heelan 1983, p. 203)

Heelan argues that a scientific instrument

> does not change the essential structure of the perceptual act, neither with respect to its phenomenological characteristics, (particularly directness), nor with respect to the physical and causal relationships between the embodied perceiver and the object. (1983, pp. 210-211)

Firstly, the physical aspect of the human observational apparatus is by its nature instrumental. Although it is specific to our species, and so it is different from that of other animals, it basically works by means of the same physical laws as the observational apparatuses of other animals do. Secondly, there is an intentional act that makes a difference between humans and scientific instruments. According to Heelan, this intentional act is embodied in human observational apparatuses. Adding new instruments to our inborn apparatuses provides an “extended embodiment”, in which new intentional acts are embodied. In unaided perception, the intentional act is mediated by our inborn apparatuses. In instrumentally-aided perception, the intentional act is mediated by inborn apparatuses joined with instruments.

It is correct that the physical condition of observation depends on the causal relationships between the perceiver and the object. However, Heelan's view that we have non-inferential knowledge of scientific entities is problematic. In my view, we can only have non-inferential knowledge of ordinary objects, but our knowledge of scientific entities and their properties are *inferred*, as I will explain in the next section.

5.4 Non-Inferential Knowledge of Unobservables?

According to Heelan, an experimental instrument is 'readable', in the sense that the response of the instrument is "in a position of 'text' to be 'read' in the 'context' of a scientific horizon" (1983, p. 206). Heelan's controversial claim is that instruments "make manifest to perception the constituent parts or hidden structures of the explanandum" (1983, p. 206). That is, the process of 'reading' is non-inferential.

> I now claim that this 'reading' is a perceptual process, since *it fulfills all the characteristics of perceptual knowledge*.
>
> Perceptual knowledge is (1) direct, not mediated by inferences, nor is it just knowledge of an "internal representation" or "model" constructed, perhaps, out of sensations, or in some other, perhaps, mathematical, way. (1983, p. 198)

For example, in the measurement of the temperature by a mercury thermometer, our knowledge of the thermodynamic temperature is non-inferential:

> the position of mercury on the scale functions as a 'text'; this 'text' has the character of $information_1$. Through a 'reading' of this 'text,' one gains knowledge of the current thermodynamic temperature [$information_2$]. The expression of this knowledge takes the form of a judgment, "The present ambient temperature is (say) 70°"; this judgment is empirical, direct [i.e., non-inferential and non-representational], and uses scientific terms descriptively of the World (1983, p. 198).

However, a 'reading' of the "position of mercury on the scale" is possible only by an act of inference. What is non-inferentially known, or "$information_1$" in Heelan's words, is not the thermodynamic temperature but the length of the column. We know that there is a (linear) relation between the length of the (mercury) column and the thermodynamic temperature, so the temperature, as "$information_2$", is justifiably *inferred* from the length observed. Therefore, Heelan's claim that the knowledge of thermodynamic temperature is "not mediated by inferences" is untenable.

More precisely, we should distinguish two levels of discussion: perceptual and epistemological. Heelan speaks of "knowledge" that takes the form of "judgment", so his discussion is epistemological. At this level, one's *knowledge* of the thermodynamic temperature relies on (usually unconscious) inferences. For this reason, if we ask whether one's judgment is correct, we need to address the relevant inferences. Thus, a correct analysis of the thermodynamic temperature example is as follows. We perceive the length of the column on the thermometer. This perception, which is conceptually interpreted by the terms "length", "column", etc., constitutes our basic belief that "the length of the column is (say) 3 centimeters". This belief is a perceptual, a basic one – it is non-inferentially justified (on basic beliefs, see subsection 5.3.1). But the belief/judgment that “the present ambient temperature is (say) 70°” is not basic anymore. If we are asked why we believe in it, we should justify the belief/judgment as follows:

(1) we have the perceptual, basic belief that the length of the column is (say) 3 centimeters.
(2) there is a (linear) relation between the length of the (mercury) column and the thermodynamic temperature, such that the length of 3 centimeters corresponds to the temperature of 70°
(3) it is inferred from (1) and (2) that the temperature is 70°.

Suppose that we are inside a warm house but the thermometer measures the temperature of the cold outside of the house. We perceive the warm inside by our embodied sensors, hence our non-inferential knowledge of the warmness. At the same time, we know the temperature of the outside inferentially. The fact that we have learnt to know the temperature of the outside through a thermometer without making conscious inferences does not entail that it is really non-inferential from an epistemological perspective.

Now consider Heelan’s assertion at the perceptual level of discussion: “theoretical states and entities are or become directly perceivable”. First, our observation of scientific entities is mediated through instruments and theoretical concepts.[8] For this reason, it may be confusing to assert that scientific entities are “directly” perceivable, and therefore I never use, in particular in the case of

[8] About the conceptuality of perceptual experience, see McDowell (1994); Brewer (1999); Noë (2004, Chapter 6); Radder (2006, Chapters 6 and 7). The conceptual interpretation of perception is not necessarily propositional (see Gallagher and Zahavi 2012, p. 121, n. 2) or conscious (see Radder 2006, p. 84).

science, expressions such as “direct perception” or “directly perceivable”. Second, Heelan seems to endorse a kind of direct realism, according to which our intentionality is directed to things and not to their mental representations or replica. I agree that we engage with things themselves and not with their mental representations, but this engagement is mediated through instruments and interpretations. Direct realism is compatible with (realist) perspectivism. The former is at odds with “(mental) representational realism” but not with perspectivism. In this regard, Giere rightly argues that the perspectivist interpretation of instrumentally-mediated observation is in line with direct realism.

> Direct realists argue that we perceive objects themselves. Representational realists argue that we experience not the object itself, but a mental representation of the object. For instruments, the direct realists are closer to the mark. Instruments clearly do not form representations of objects, which they then detect. Instruments interact directly with objects in the world. … Observation is thus always mediated; not, however, by a representation, but by the perceptual apparatus of the observer. (Giere 2006a, p. 126, n. 7)

In view of these two remarks, a preferable alternative to Heelan’s assertion at the perceptual level is that the conceptual interpretation of instrumentally-mediated empirical evidence directs our intentionality toward scientific entities themselves and not to their mental representations. (The role of “evidence” in this statement will be elaborated in section 5.5.)

Let us now return to the case of cloud chambers. Van Fraassen and Wiltsche are right that our knowledge of ions is inferential. A “trace” in the cloud chamber is perceived, and then it is interpreted that the trace is that of ions. The cloud chamber is thus the condition of the possibility for the perception of the “trace” that is interpreted as evidence of ions. The apparatus does not present the perception of the ions. Instead, it produces signs or empirical evidence of ions, and thus the knowledge of their existence and properties is inferred on the basis of the empirical evidence.

One might argue that the cloud chamber is not the only or the latest technology or practical technique scientists have invented to study ions. For instance, techniques of laser cooling and trapping have been devised since the 1980s. They also enable experimenters to make manifest ions and atoms. In this technique, ions or atoms are cooled and slowed down through collisions with laser light, and then they are "trapped" by electromagnetic fields (on the laser cooling of trapped ions see Eschner et al. 2003). By performing the relevant manipulations, the ions or atoms are "prepared" to be manifested by the mediation of a microscope such as a scanning tunneling electron microscope. Still, atoms or ions are not "perceived". Although the intentionality of the experimenter(s) is directed at the atoms or ions (and not at their mental representations), the intentionality is mediated by instruments and conceptual interpretations. Furthermore, to count our knowledge of the outcomes of the microscope as images of atoms, inferences are necessary. These inferences should be justified if they are to result in scientific knowledge of cooled and trapped ions.

As Joseph Pitt nicely illustrates, "seeing through a microscope is not the same as opening one's eyes and seeing a tree in front of me" (2005, p. 25). One should first of all be trained to see through any instrument. Observation is a skill and training is necessary to observe correctly. Michael Polanyi (1973, p. 101) illustrates this claim by explaining the perceptual experience of a medical student who is being trained to inspect an X-ray picture. During the processes of learning, the learner will gradually "see" the details of the picture. When the observer becomes skilled and experienced, his/her intentionality is promptly directed to the objects of inquiry, hence there is no need for conscious interpretation or inference. Still, this observation is made possible by the mediation of instruments and relevant conceptual interpretations. In the case of the scanning tunneling electron microscope, at the perceptual level the signals produced by *means* of the microscope need to be *interpreted* in order that the observer's intentionality is *directed* to the objects under inquiry. At the epistemological level, the justified results rely on valid *inferences*, even if observers do not usually make these inferences explicit.

Other more complex instruments, such as particle detectors at the LHC or the Virgo and LIGO detectors, should be studied carefully to determine what is really presupposed and detected by each of them. Nevertheless, a cursory examination of their operation suggests that various statistical and computational inferences are employed to provide evidence of the relevant scientific entities. Big data should be analyzed and interpreted to be used as evidence for the existence of complex scientific entities, such as gravitational waves or a binary black hole merger (see also subsection 3.3.2 and section 4.6).

Instruments extend our empirical evidence. The evidence provided by scientific instruments can change the epistemic status of scientific entities from a mere theoretical postulation to empirically justified entities. Our knowledge of the latter is still inferential. Again, the fact that the inference from available evidence to (the properties of) an entity is mostly made *unconsciously* does not mean that no inference is made.

Van Fraassen agrees that the use of instruments in experiments may extend our (perceptual) knowledge *of observables*, but he emphasizes that this extension (or "enlargement" in his words) should not be expressed in a metaphorical way:

> It will serve … to think of experimentation in terms of a literal *enlargement of the observable world*, by the creation of new observable phenomena, rather than a metaphorical *extension of our senses*. (2008, pp. 98-99)

I share Van Fraassen's concern that the metaphorical concept may bring about epistemological misunderstandings. Accordingly, I would prefer the non-metaphorical term "the extension of perceptual evidence by instruments" to the term "the extension of body/embodiment by instruments". The former term is consistent with Heelan's and Ihde's emphasis on the key role of instrumentation in constituting new perceptual horizons, without employing a metaphorical expression. The importance of instrumentation is such that scientific progress depends on the empirical evidence that technological instruments present, and in turn, on the availability of technological instruments, or what Isaac Record (2013) calls "technological possibility". The term "the extension of perceptual evidence by instruments" is also compatible with Vallor's Merleau-Pontian definition of "perception as an embodied engagement with an empirically pregnant horizon" (2009, p. 20). During process of instrumentally-mediated observation, the empirical evidence of a scientific entity is manifested in a perceptual style. Thus, the concepts "empirical" and "perceptual" can be used interchangeably. Perceptual or empirical evidence enables scientists to investigate a scientific entity. The next section explains my arguments for realism concerning unobservables with the aid of the concept of "exploration" and the distinction between "manifestation" and "phenomenon".

5.5 The Manifestation of Evidence

Based on Heidegger's phenomenological method (1962[1927], section 7), Kockelmans distinguishes between "manifestation" and "phenomenon". This distinction helps to better understand the epistemic status of empirical evidence generated by instruments. A phenomenon is observed, and then it is used as a sign or evidence of a scientific entity that is manifested through the phenomenon. The fire in the wood-burning stove, as a manifestation, announces itself by the smoke coming out of the chimney, as a phenomenon. Similarly, scientific instruments produce empirical evidence, which constitutes indications or signs of real entities.

> Let us call a *phenomenon* that which shows itself directly. Taken in the narrow sense, the set of phenomena is then the totality of all entities that are actually manifest to human beings without mediation through something else. Taken in the broad sense, the set of phenomena is the totality of all entities that *can* be actually manifest to human beings in the manner indicated. … An *appearance or manifestation* is something that does not manifest itself *directly*; rather it is something that *announces* itself without showing itself directly. What appears announces itself by means of something that shows itself immediately. In this case we often speak of indications, symptoms, signs, symbols, etc. (Kockelmans 1993, pp. 249-250)

Scientific entities announce themselves through what Kockelmans calls phenomena, indications, symptoms, signs, symbols, or in sum: empirical evidence.[9] A veridical perceptual

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[9] Empirical evidence is a common term in current scientific practice. See also section 2.5, where, I employed the concepts of signs, indications, traces and effects for those cases in which the persistence of the empirical evidence is still being questioned, but in which our experience of

experience is not independent of what the experience is evidence *of*. It is epistemologically different from the experience of an illusion. We know that perceiving a rainbow does not provide evidence of some colorful object in the sky. The rainbow phenomenon can be explored from the lifeworld and scientific perspectives, both of which can establish that no colorful object exists in the sky. One (for instance, an uneducated child) might first be under the illusion that there is a colorful object in the sky when he/she experiences the rainbow phenomenon by his/her ordinary perception, but one can easily change the positions and angles of observation to finally learn that there is no colorful object in the sky. Also, from a scientific perspective we know that perceiving a rainbow provides evidence *of* the refraction of light rays of different frequencies and not of some colorful object in the sky. Similarly, in the case of the cloud chamber the trace perceived is evidence *of* ions, as entities. As section 2.5 explained, it may be the case that what has appeared to us (ordinary people or scientists) is an unknown illusion or hallucination rather than a veridical experience or evidence. However, mostly and to the extent that our experience or evidence relies on *several modes* of our bodily or instrumental engagements, the experience or evidence is veridical. In the case of ions, we are justified to claim that ions refer to real things, because multiple processes of obtaining scientific evidence suggest that this evidence is veridical and thus the ions are real. In the case of the rainbow, multiple ways of encountering the phenomenon, that is, its ordinary perception from different angles and positions or its investigations through different scientific methods, make it obvious that there is no tangible colorful object in the sky.

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the thing resists disappearance since some (possibly sporadic) signs, effects, traces or effects of the thing are available.

As embodied subjects we are not brains in vats. We can use our different modes of engagement with things and perform multiple exploratory actions to distinguish evidence *of* real things from illusions and hallucinations, which do not indicate real things.[10] Unlike real entities, to which evidence or signs testify, the supposed objects of illusions and hallucinations will sooner or later be dispelled after multiple and active bodily or instrumentally explorations. We can actually approach a mirage and see it from different angles to become confident that its manifestation is different from the veridical experience of an actual lake. Similarly, scientists devise various practical methods to distinguish "real evidence" from merely artificially created illusions and hallucinations.

Vallor's criterion for reality is applicable in differentiating empirical evidence of real things from illusions and hallucinations: A real entity is explorable indirectly through its evidence or signs.[11] My usage of the term "explorable" implies that scientific entities are explorable through

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[10] On the role of "exploratory action" in determining illusion see Merleau-Ponty (1962, pp. 296–297). Exploratory actions can also be used in scientific practice to determine illusory and hallucinatory experiences. See below and also section 2.5.

[11] Vallor employ's Heelan's concept of 'reading'. However, she does not discuss whether 'reading' is inferential or not. Heelan explicitly states that the direct perception of the hidden structures of reality is possible by means of instruments but without any act of inference. However, it would seem that for Vallor only the *signature* or *evidence* of a real entity (rather than the entity itself) is perceivable: "the pregnant signature of that particle becomes manifest to the experimenter in a perceptual style" (Vallor 2009, p. 15).

several independent ways of obtaining evidence. This usage is in line with the view of robustness explained in previous chapters. Detecting, measuring, deriving, and (re)producing are different modes of "exploring". When the empirical evidence of a thing is explorable in several independent ways, one is usually justified to reject the claim that the thing is a mere artefact of the experimental instruments. The replication of an experiment in different conditions justifies the conclusion that the results of the experiment do not depend on the specific circumstances of the instruments. In contrast, the supposed object to which an illusory or hallucinatory phenomenon seems to refer does not provide evidence that is explorable through several different modes of investigation.

The view I am defending is different from Van Fraassen's, which does not take seriously the fact that empirical evidence indicates something else. He does not differentiate between veridical and hallucinatory "observables", equating an image made by a microscope with a public hallucination.

> It is accurate to say of what we see in the microscope that we are ''seeing an image'' (like ''seeing a reflection'', ''seeing a rainbow''), and that the image could be *either* a copy of a real thing not visible to the naked eye or a mere public hallucination. I suggest that it is moreover accurate and in fact more illuminating to keep neutrality in this respect and just think of the images themselves as a public hallucination. … [W]hat are the practical implications? To keep neutrality in this respect does not prevent us from gathering empirically attestable information. (2008, p. 109)

> [W]e can report on our sightings made by means of a microscope in the same way as we report our rainbow-observations. (2008, p. 110)

However, scientists do (and should) not stay neutral about empirical evidence and hallucinatory experience. Multiple methods enable scientists to determine evidence *of* real entities. On the other hand, although illusory results or mere artefacts of instruments can teach experimenters something about the possible problems of the instruments, they are not evidence of entities inasmuch as multiple processes demonstrate that they are illusory results or mere artefacts.

The view that explorable empirical evidence indicates scientific entities supports realism. Nevertheless, this realism is not objectivist$_1$. We humans, as situated, bounded beings, cannot acquire knowledge of reality independently of our instrumental and conceptual conditions. Empirical evidence is always provided by instruments whose validity scopes are qualified. Also, theoretical and interpretive inferences, as the hermeneutical side of scientific knowledge, are necessary. In the next section, I discuss the perspectival nature of scientific observation and knowledge in order to support a realist perspectivism.

5.6 Realist Perspectivism

On the basis of the explorability of perceptual horizons, Vallor supports experimental realism (see subsection 5.3.2). The shortcoming of experimental realism, and its phenomenological defense, is that it does not properly take into account the theoretical/mathematical side of physical sciences. This sometimes results in not acknowledging the crucial theory-dependence of scientific observation (see Hacking 1985, p. 137). The underestimation of the role of theorizing in science is also a problem of Ihde's instrumental realism, in which one hardly find a discussion of model building or the formal features of physical science.

As chapter 3 argued, experimental or entity realism and perspectivism augment each other, and hence result in realist perspectivism. Consistent with what I claimed there, this section argues for a realist perspectivism that is compatible with but not limited to experimental realism. My realist perspectivism, as we shall see, takes into account both the practical/experimental aspects and the mathematical/theoretical sides of scientific practice. Chapter 3 defended realist perspectivism on the basis of Giere's views, while here I support realist perspectivism with the aid of phenomenological-hermeneutical ideas.

Husserl's view that physical objects manifest themselves *perspectivally* is an initial, insightful idea to argue for perspectivism. Husserl maintains that when we perceive an object, we always see it from a perspective. Only one profile[12] of the object is actually presented; its other profiles are co-given in a horizon: "a *core of* '*what is actually presented*' is apprehended as being surrounded by a horizon of '*co-givenness*'" (Husserl 1983, p. 94). He argues that

> there belongs to every external perception its reference from the 'genuinely perceived' sides of the object of perception to the sides 'also meant'—not yet perceived, but only anticipated …[T]he perception has horizons made up of other possibilities of perception, as perceptions that we *could* have, if we *actively directed* the course of perception otherwise. (Husserl 1960, p. 44; see also Smith 2016, section 3.2; and Zahavi 2003, pp. 95–97)

[12] Perception is always a mixture of presence and absence. The present parts are called profiles. Some scholars distinguish the concepts of "side", "aspect", and "profile"; see, e.g., Sokolowski (2000, p. 19). I, nevertheless, use these concepts as roughly equivalent. Further on, I will expand the notion of a profile to include the perspectival dimension of empirical science.

In criticizing Van Fraassen's (1985) view that observation is only about actual objects, Vallor supports Husserl's perspectival account of perception, and admits that perception is always based on an object's "profiles", and an "anticipatory horizon" is always needed to bring about a complete perception (Vallor 2009, pp. 4–5). Perception is always about a whole, only a part of which is presented in the actually perceived profile of the object; other parts are added in an anticipatory horizon: "the most epistemically significant component of a perception is the projected horizon of the *non*-actual" (Vallor 2009, p. 7). This horizon helps to provide a complete understanding of the object from a limited number of perceptual profiles. Non-actual parts of perception are anticipated, based on a limited, discrete set of actual profiles.

Vallor's view needs further clarification. I have argued in the previous section that a scientific entity manifests itself through empirical evidence, and vice versa, the evidence or sign indicates the scientific entity. Accordingly, empirical evidence is immediately perceivable; and then it indicates an entity. With this in mind, it can be suggested that profiles of an entity consist of empirical evidence obtained by means of the relevant instruments. The evidence which has thus far been collected constitutes the a*ctual* profiles of the entity. All prospective evidence of the entity constitutes its *non-actual* profiles. Actual profiles have been perceived, but there should always be an *anticipatory horizon*, which helps to complete the currently actual profiles of the entity by anticipating non-actual profiles. The role of the completion of the horizon rests on the shoulders of scientific theories. We can understand this role better with the help of *realist perspectivism*.

In phenomenological terms, instruments provide us with the perceptual profiles of an entity and we actively complete the anticipatory horizon with the aid of theoretical assumptions that rely on scientific theories to provide a complete image of the thing. For example, the main assumption of the PET scan is that neural brain activities can be understood in terms of the blood flow (or the metabolic changes) of a particular area of the brain. More generally, the proper interpretation of neuro-images is always theory-dependent. In addition, statistical analyses are always necessary to produce a “complete” outcome from signals. Thus, a scanned brain image is not simply a photograph of the real brain (Klein 2009; Roskies 2007), but it consists of signals obtained from the brain that are completed with the aid of anticipatory assumptions. As a result, our access to the brain substantially depends on how brain scanners work and how their results are interpreted. “One has images *as produced by CAT or MRI or so forth*. One cannot detach the description of the image from the perspective from which it was produced” (Giere 2006a, p. 56).[13] According to this view, the perspective is provided by the specific sensitivity of the instrument and by the theoretical assumptions that help us actively complete the initial profiles we gain by means of the instrument.

One might argue that in the case of ordinary perception, we can easily “direct the course of perception otherwise” to check if “we could have” the actual perception of the other aspects of the object, those aspects that were previously anticipated, while it is hardly possible to check if the theoretical assumptions that anticipatorily constitute the result of an instrumentally-mediated observation are correct. In response, even if we accept that it is often hard *in practice* to check the validity of the anticipatory assumptions, there is no *in principle* hindrance to conduct experiments in order to investigate other evidence of the entity, the evidence that was previously anticipated merely theoretically. The evidence obtained by means of instruments determines if the previous

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[13] This does not contradict my previous point that replicated experimental results are reliable. Robust or replicated results achieved from “overlapping perspectives” are not non-perspectival. See subsection 3.4.2.

theoretical assumptions were acceptable. In this way, non-actual, anticipatory profiles of the entity can become actual.

One should also take into account that a scientific experiment is not limited to the practice of an individual experimenter. The justification of experimental results is a *collective* enterprise. An experimenter may rectify the empirical evidences provided by other experimenters. Also, an experimenter may reproduce the experiment in a different way to provide new signs of an entity which is under investigation. Experimenters in a scientific community revise and complete evidence collected by the experimental processes of one another, which makes the empirical results reasonably reliable.[14]

Let’s return to perspectivism. According to perspectivism (see my section 3.3.2), all aspects of a real thing cannot be represented. Scientific models make available a perspective within which a scientific model fits the world. This view respects pluralism in science. Different models can be devised to describe different aspects of the same thing. For example, hydrodynamics and statistical mechanics provide two models for studying water. Although neither of these two idealized, imprecise models is believed to describe water as it really is, each of them is a perspective that expands human knowledge about water. Different models of a thing may imply *inconsistent* claims about reality, e.g., that water is a continuous substance, according to the hydrodynamic model, is inconsistent with that it consists of discrete elements, according to the statistical-mechanical model. These inconsistent models coexist with each other. Each model manifests an aspect of water from a perspective, and neither of them manifests water as it is in itself.

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[14] Cf. De Boer et al. (2018).

The result of this discussion is realist perspectivism, which is in sharp contrast with objectivism$_1$. Accordingly, it does not lead to the problem of scientific objectivism, as the major motivation of Wiltsche to defend antirealism. If one wants to preserve realist intuitions while rejecting objectivism, realist perspectivism is a convincing alternative. It also takes into consideration the role of scientific instruments in providing perceptual evidence, which is sympathetic to Ihde's argument for realism.

The perspectivist view that the strongest true claim to be made should be qualified is also akin to Kockelmans's statement that "it is very difficult to subscribe to the view that scientific theories are true without further qualification" (Kockelmans 1993, pp. 135). In general, Kockelmans's (1993, chapter 3, §2, 6) Heideggerian view of truth is in harmony with a perspectivism, according to which scientific knowledge is always made possible within conditions. Depending on certain contexts and purposes, scientists construct models that represent the object qualifiedly.

Heidegger's (1962[1927], section 44) account of truth is based on his reading of the Greek word "aletheia" as the antonym of lethe, meaning concealment. Accordingly, aletheia means un-concealment, and truth is un-concealedness (Heidegger's word is *Unverborgenheit*). When a thing becomes un-concealed, some aspect of it in a context becomes evident. The process of un-concealment begins and continues in a background or context of meaning. A truthful statement reveals some aspects of the thing that can be un-concealed in that context, and at the same time, the statement conceals other aspects of the thing.

Similarly, according to perspectivism, the manifestation of an entity always concerns profiles of the entity in a theoretical and technological context. The process of discovery occurs in the historically situated background of theoretical concepts and models as well as that of available technological instruments. In a specific context, some profiles of the entity are discovered through empirical evidence, while some other profiles remain covered. Discovered profiles may be interpreted differently in the future, and covered profiles may become uncovered in the future. Nonetheless, even in the future, uncovered profiles will be conceptually interpreted – they are not theory-free. Also, there may still be further uncovered profiles – the entity will hardly be discovered from all possible perspectives. Accordingly, scientific knowledge is always *qualifiedly* true. It is

> impossible for us to claim that in our judgments we state how things are "in themselves," comprehensively, exhaustively, definitively, and absolutely. We can claim only that our judgments state how things are as seen from some limited context of meaning or, in the final analysis, from the perspective of the whole of meaning of which we can conceive. Thus, every form of revealment implies for us also some form of concealment. (Kockelmans 1993, pp. 145)

5.7 Scientific Pluralism

Let us examine Ihde's criticism of Husserl once more. Ihde argued that Husserl's distinction between the world of science and that of the ordinary lifeworld is unjustified, because both are provided on the basis of perception. Ihde, however, does not take into account that different *perspectives* disclose different *aspects* of reality. Thus, he (1991, pp. 11–44) employs the concepts of "paradigm", "episteme", and "macroperception" (suggested respectively by Thomas Kuhn, Michel Foucault, and himself) to explain the way we see the world. These concepts, however, do not allow for *synchronous* ways of discovering an object. There is more than one available perspective not only across history but also in specific periods of time.[15]

Husserl is right that science and ordinary perception present different images of the world, but this difference is not restricted to the two images Husserl mentions. There are also synchronous perspectives in science itself which present different images of a particular object. Therefore, there may be *more than two* images of objects. About water, for example, there are at least three images: that of ordinary perception, that of hydrodynamics, and the one statistical mechanics provides. Each of these images is presented from a specific perspective.

In many cases, our perspectival knowledge concerns different aspects of the same object. For instance, in her discussion of cancer theories, Anya Plutynski (2020) argues that each cancer theory provides bounded knowledge about cancer. For another example, Margaret Morrison (2011, section 2) holds that turbulence models provide complementary understanding about different features of one phenomenon. In these and similar cases, it is unproblematic to claim that none of

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[15] Another reason why I prefer the concept of "perspective" to, particularly, "paradigm" is that the origin of the word *perspective* implies that we basically *look*, but this looking is bounded by the instrumental and theoretical means *through* which the looking has been possible for us as human beings. Thus, perspectives are our basic means for discovering reality, although they are bounded by their contingent conditions. A paradigm, on the other hand, only helps us to solve puzzles. However, there is no implication that we discover reality by solving problems with the aid of a paradigm's possibilities. For other comparisons between the notion of paradigm and perspective, see Giere (2006a, p. 82; 2013) and Massimi (2015).

the models is more original than the others; each complements our understanding of the same thing by presenting a different aspect of the thing (see also Rueger 2005).

However, there are cases of theories/models that do imply inconsistent claims about reality. For example, that water is a continuous substance (hydrodynamic model) is inconsistent with that it consists of discrete elements (the statistical-mechanical model). For another instance, it seems inconsistent to believe that light consists of waves and particles at the same time. Morrison (2011, section 3) provides a further example: there are more than 30 different models of the nucleus, many of which attribute incompatible assumptions to the same thing, thereby challenging the commitment to any realist claims, including Giere's perspectival realism. According to Morrison, perspectivism

> isn't a satisfactory option because none of these "perspectives" can be claimed to "represent" the nucleus in even a quasi-realistic way since they all contradict each other on fundamental assumptions about dynamics and structure. In this case perspectivism is simply a re-branded version of instrumentalism. Given that we assume there is an object called "the atomic nucleus" that has a particular structure and dynamics it becomes difficult to see how to interpret any of these models realistically since each is successful in accounting only for particular kinds of experimental evidence and provides very little in the way of theoretical understanding. (2011, p. 350)

However, in my view, Morrison's assumption that "there is an object … that has a particular structure and dynamics" seems problematic. According to the ontological view I endorsed in chapter 2, the properties of a real thing are not already realized in a "particular" way. Rather, a real thing consists of persistent potentialities that can be realized differently in different conditions. As a result, the realizations of the potentialities of a thing in different conditions may result in attributing inconsistent properties to the thing.

Thus, I (partly) agree with Anjan Chakravartty (2010, section 3) that inconsistent models can be explained on the basis of a dispositional ontology. For instance, depending on the circumstances (such as temperature) salt may dissolve in water or it may not. Similarly, light behaves both wave-like and particle-like, depending on which experimental settings it is subjected to. The difference of my view and Chakravartty's is that he defends dispositional ontology as a rival to perspectivism. However, I think that human-independent potentialities are realized in instrumental and theoretical perspectives, hence a realist ontology about potentialities and a perspectivist epistemology regrading instruments and models/theories (see my chapter 2). On this basis, it is questionable that "scientific investigation [into the nature of light] has revealed perspective-transcendent facts about how light behaves in different conditions" (Chakravartty 2010, p. 410). If the facts are perspective-transcendent they should be true independently of any condition. But light behaves wave-like in certain conditions and thus our knowledge that light behaves like waves is subject to these specific conditions. Our knowledge that light behaves particle-like is not correct under these conditions. Accordingly, the conditions in which our knowledge is valid should always be stated, and in this sense our knowledge is conditional and perspective-dependent. A critic might argue that the dispositional knowledge that light *can* behave like waves (or like particles) is perspective-transcendent. In response, we should distinguish between epistemological and ontological issues. When it comes to the latter I do agree that human-independent potentialities exist, e.g. light *exists* and consists of the *potentialities* of behaving like waves and particles. At this level, a mere dispositional account might be adequate. However, at the epistemological level there are several problems that certainly are not addressed unless the concept of perspectives, or similar concepts such as conditions or contexts, are used. In the first place, our truthful beliefs are not *limited* to dispositional facts. The scope of the validity of an (observational/experimental) statement that is expressed in a non-dispositional language (e.g., light behaves like waves) depends on (instrumental) perspectives. That is, without considering the perspectives in which our statements are truthful, we cannot address the epistemological question of what constitutes the *boundaries* of our knowledge. Furthermore, as Michela Massimi rightly argues, the *justification* of our knowledge always depends on perspectives. For instance, the problem "*under what conditions* we are justified to believe that electrons have electric charge" cannot be solved if we do not use perspectives (2012, p. 41). Moreover, our knowledge of entities and their potentialities are described from theoretical perspectives. Accordingly, the next chapter shows that to solve the problem of theory change we should employ the concept of diachronic perspectives and argue that a persistent potentiality may be interpreted differently from historically different theoretical perspectives.[16]

In this chapter I have connected phenomenological-hermeneutical approaches with work on perspectivism. The latter is realist inasmuch as the evidence gathered by multiple ways of instrumental exploration suggests that the entity is real, rather than illusory or hallucinatory. This view is perspectival inasmuch scientific knowledge is bounded by instrumental and conceptual conditions. This perspectivism coheres with a number of phenomenological-hermeneutical ideas

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[16] See also my criticism of Chakravartty's objection to perspectivism in subsection 3.4.1 and section 7.4.

and can, as a middle ground, reconcile the disagreements among an antirealist such as Wiltsche and a realist such as Ihde.

Chapter 6 Diachronic Theoretical Perspectives

6.1 Introduction

Scientific realism implies having a positive epistemic attitude toward both the observable and unobservable contents of current successful theories. A major argument for scientific realism is what is known as the No-Miracle Argument [**NMA**], according to which the successes of modern science—including novel predictions and intricate explanations—are the result of their truthful descriptions of the world. In this regard, Hilary Putnam (1975, p. 75) maintained that realism is the only philosophy that explains why science is successful.

The pessimistic induction adduces the actual history of science as evidence against this realist attitude. There is more than one version of the pessimistic induction (see Mizrahi 2013; Psillos 2018; Wray 2018, part 5). In this chapter, the focus is on two of its versions. Section 6.2 concerns the version that casts doubt on the realist view that the theoretical terms of current theories are referring and thus current theories are truthful. This section also argues for scientific progress and against the conception that current science is ultimate. Section 6.3 reconstructs the version of the so-called pessimistic induction against NMA. Sections 6.4 to 6.7 discuss my response to this version of the argument. My claim is that the descriptions of a past theory that contribute to successful explanations and predictions are truthful, although the real thing these descriptions are about may be better described by later theories. To support this claim, I develop a "restatement strategy" in sections 6.4 and 6.5, according to which the successes of a past theory can be restated from the perspective of its currently accepted successor. Thus, the theoretical descriptions of the past theory can be overlapped with certain theoretical descriptions of a current theory, thereby constituting diachronically overlapping theoretical perspectives, which deserve realist commitment. By employing a realist version of Ramseyfication in section 6.6, I also examine the logical side of the restatement strategy. I explain that a theory can contain truthful descriptions even if some of the descriptions its central terms entail are false. Finally, section 6.7 provides a detailed case study about Ptolemaic astronomy to support the philosophical arguments.

In this chapter, the concepts of "truthful" and "approximately true" are used more or less interchangeably. "Approximately true" is a more common term in debates on scientific realism, meaning "to some extent true". Yet, this concept is also understood as "close to *the truth*". However, "the truth" may be only a regulative ideal, which is in principle beyond our access. Therefore, we are not able to compare an actual theory with it. For this reason, the notion of truthfulness is preferable. "Truthful" theories contain a number of true descriptions about the world and hence are to some extent true. A truthful description is always made from a (theoretical) perspective. According to that perspective, the description is that of a real entity. As I will explain, truthful descriptions are perspectival and at the same time they can be restated by other successful theoretical perspectives.[1]

6.2 The Pessimistic Induction against Scientific Realism

The first version of a pessimistic induction runs as follows:[2]

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[1] See also my criticism of the concept of "partial truth" in subsection 7.2.2.

[2] This argument is known as "Putnam's pessimistic induction". Hilary Putnam presents an early formulation of this argument, while he does not endorse it (see Putnam 1978, pp. 24-25; see also Wray 2018, pp. 69-74).

(1) Most central terms of past successful theories turned out to be non-referring (examples of these terms are phlogiston, ether, and caloric.).

(2) In this respect, there is no essential difference between current and past theories.

(3) Therefore, most central terms of our current successful theories are probably non-referring.

If one accepts that a theory whose central terms do not refer is false (on a critical analysis of this assumption, see section 6.3), one can infer from (1) and (3) the following statements, respectively.

(1)' Past successful theories turned out to be false.

(3)' Our current successful theories are probably false.

I will argue in the next sections that (1) and (1)' are questionable. The present section is sympathetic to realists, who question (2). These realists emphasize the characteristics that differentiate contemporary theories from past ones. They argue that current theories enjoy higher standards of explanatory success (Doppelt 2007, pp. 111–112), that they are the result of more serious scientific work (Fahrbach 2011), or that they are formed on the basis of broader observational data from various fields (Park 2011, pp. 79–82). If these judgments about current theories are true, there would be essential differences between current and past theories, so the pessimists cannot legitimately claim that current and previous theories have a similar fate.

The distinctive characteristics of current theories justify us in believing that present theories provide *broader perspectives* than their predecessors in discovering reality. Still, these characteristics are unable to justify us in believing that our current theories are unchangeable. Thus, one might advocate what Marry Hesse calls the "*principle of no-privilege*, according to which our own scientific theories are held to be as much subject to radical conceptual change as past theories are seen to be" (1980, pp. 143-144). In my view, however, the principle of no-privilege is misleading, because current (non-)mathematical concepts, technological instruments, and experimental techniques are clearly superior to their past predecessors. Current theoretical concepts, including mathematical ones, are considerably more nuanced than past concepts. Present technologies and techniques are also more powerful in providing observational and experimental evidence than past ones do. Moreover, current scientists, observers, and experimenters have access to past theoretical concepts, instruments and techniques; but it goes without saying that the converse is not true. Accordingly, current perspectives, constituted by currently accepted theoretical resources, by instruments and detectors, and by techniques of experimentation, are *broader* than past perspectives, in the sense that current science is in a better position now to discover reality.[3]

Still, the preferable perspectives of current science cannot result in the view that it provides the ultimate image of reality. Progress in science is foreseeable and future theorists may describe reality much better. Thus, instead of "the principle of no-privilege", I assume "the principle of no-ultimacy", according to which an ultimate image of reality is always unattainable. The fact that current theoretical and instrumental perspectives are broader than past ones should not prevent us from conceding that our current (and even future) perspectives are qualified. Theoretical concepts only provide a grip on limited aspects of reality. Further, theorists' capacities to conceive (alternative) theories are limited (see Stanford 2006). Nor can technological instruments and

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[3] I argue for the preferable perspectives of current science by comparison with past ones. I do not discuss whether individual, present-day scientists are epistemically privileged. For criticism of the latter discussion, see Wray (2018, chapter 4).

techniques provide all evidence necessary to confirm theories (e.g., in cosmology). The "boundedness" of present science (and future science) pushes us to employ a modest account regarding the boundaries and possibilities of science.

Moreover, were we to believe that present science possessed ultimate truth, we would foreclose possible progress in science. Seeing current science as ultimate ignores that by adjusting current theories for their anomalies, or by proposing new theories tuned to new locations and scales, we may significantly develop or change our present theories (Alai 2017, sec. 5). On top of that, this conception is dangerous for scientific practice itself. Suppose that policy-makers and funding agencies take the view that current theories are the best and therefore not or hardly improvable: it would then be unreasonable to allocate further finance to fundamental research in science. Model buildings, computer simulations, experiments, and observations regarding current fundamental theories would not be fruitful anymore. When an ultimate true image of reality is already at hand, it would not be reasonable to check that image any further, let alone improve it. This would inhibit any further enquiries and development in current theories. As a result, science would freeze.

In this regard, Kyle Stanford (2015a; 2015b) argues against increasing "theoretical conservativeness" in scientific communities, where contemporary scientists prefer "safe" research that presumes currently established beliefs. As a result of this theoretical conservatism, making a scientific breakthrough becomes unlikely. Stanford claims that antirealists are more prepared than realists to encourage attempts to develop radically alternative theories because, for antirealists, current science is far from settled. On the other hand, Finnur Dellsen (2019) criticizes this claim, arguing that realists place a lower probability on a successful search for alternative theories; however, they have even better reasons to have a high opinion of both successful and failed attempts to develop radically alternative theories. Failed attempts are precious because, on the basis of the so-called “no alternatives argument” (see Dawid, Hartmann, and Sprenger 2015), failing to find an alternative to a currently available theory may provide *some* evidence for the current theory to be (approximately) true. Successful attempts are also valuable since a radically distinct theory that is superior to current ones is, according to realists, in many respects truer than current theories. Dellsen is right that realists can avoid theoretical conservativeness and welcome attempts at presenting radical alternatives. However, in doing so, realists should develop a sophisticated version of realism, in which the no-ultimacy principle is explicitly acknowledged. Dellsen presupposes this principle when he argues that the attempts at presenting *radical* alternatives may be successful.

In this regard, I suggest that realists should distinguish two ontological commitments. 1- A *permanent* commitment is justifiably made about the referents of those theoretical terms whose descriptions are all true. This commitment can only be made to the referents of the terms of an *ultimate* true theory. 2- A *provisional* commitment can be made to the referent of a theoretical term whose core descriptions are true according to our best available knowledge. According to NMA, we are now justified (though provisionally) to assume that the terms of current successful theories are referring, although permanent commitments to their referents are unjustified, because it may be concluded in the future that a number of the core descriptions the terms entail are false. (In the next sections, I explain and defend this version of NMA in more detail).

To be sure, one can imagine an ultimate theory, whose descriptions are all true about real things and whose terms are all referring. Such an ultimate theory is perhaps a theory of everything, which is unqualifiedly valid. However, the ultimate theory may only play a regulative role to determine an ideal, unreachable aim for scientific theorizing. In practice, all theories can be improved and an ultimate theory is always beyond human bounded capacities. Scientific discovery is indeed an ongoing process whose final step is out of sight. The realist view I am defending here can be broadly considered within the recent trend in moderate realist approaches that emphasize more the progressive aspect of science in discovering reality than the conception that scientific theories are approaching the ultimate truth.[4] Successful theories are truthful, even though they do not provide an ultimate image of reality. We cannot know in a well-defined way how far current successful theories are true, because an ultimate image of reality to which we could compare our current successful theories is unavailable. Nevertheless, what is knowable is that successful theories contain descriptions that are true under certain conditions and that current theories contain more truthful descriptions than their less successful predecessors.

This realist account is also compatible with the *selective* view that the trustworthy parts of scientific theories are preserved in later science. This selectivism does not need to be in conflict with the principle of no-ultimacy, because even the trustworthy parts of theories are not ultimate. Future theories may still better describe the real things to which these trustworthy theoretical parts pertain. After all, there is a spectrum from provisional commitment to permanent commitment. Although the latter side of the spectrum is never available, we can justifiably be more committed to those theoretical terms whose knowledge is more trustworthy. In line with recent views of entity

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[4] For other representatives of this trend, see Ilkka Niiniluoto (2017), Juha Saatsi (2016; 2017), Hans Radder (2012[1984/1988], section 4.4), Ronald Giere (2006a, p. 94; 2015, p. 5). In contrast, authors such as Gerald Doppelt (2011; 2013; 2014), as a philosopher, and John Horgan (1996), as a science journalist, seem to defend the view that current best theories are ultimate. Mario Alai (2017) convincingly criticizes this view.

realism, I implemented a selective strategy in chapter 4, where I suggested a criterion for the reality (of property-tokens). The heart of that criterion (and also of the perspectivism chapter 3 has developed) is that *several* independent ways of detection/measurement may provide knowledge of real things. That criterion mostly concerns the experimental dimension of science. A similar claim about the theoretical dimension of science can also be made: *overlapping theoretical perspectives* may present trustworthy knowledge. In other words, something may be real if overlapping theoretical perspectives presuppose the existence of the same thing (see also section 3.4). Overlapping theoretical perspectives are either synchronic or diachronic. The former is the case when (rival/independent) contemporaneous models/theories agree on theoretical descriptions. My focus in the following is on the latter, that is, on historical overlapping theoretical perspectives. When two (or more) successive theories/models, developed in different historical periods, assume the existence of *the same* thing, the thing may really exist.

There might be a question which terms of successive theories refer to the same entity. In other words, why should O (a theoretical term of an older theory) in a number of descriptions refer to the same entity to which N (as a term of a newer theory) refers? In response, first, there is no explicit rule to establish coreferring terms; this process is heuristic. In this regard, some intertheoretical explanations can make it sensible that the terms corefer. For instance, the facts that Einsteinian mass and Newtonian mass are both employed to attribute comparable properties such as inertia to objects and that there are similarities between their mathematical equations in their respective theories suggest that these two terms may corefer. Furthermore, a necessary condition that helps to know coreferring terms is that there should be empirical domains to which O and N are both successfully applicable. For instance, the dephlogisticated air may refer to the entity to which oxygen refers, because there are empirical domains to which both dephlogisticated air (from the perspective of the phlogiston theory) and oxygen (from the perspective of current theoretical chemistry) successfully apply. This condition helps to know which terms are candidates for "coreference" to the same entity.

Hans Radder (2012 [1984/1988], pp. 102-105) then argues that when there is a numerical agreement of the values of the coreferring terms in the same empirical domain (for instance, between phlogiston and oxygen), the terms refer to the same reproducible material realization, which regards the fact that the theoretical terms of successive theories can *successfully* provide (experimental) explanations and predictions (see also my section 2.2). Before we can reach this conclusion, however, another condition is necessary: the terms O and N should stand in a relation of the formal-mathematical correspondence. This is the case when the terms' mathematical equations formally correspond with each other in that domain. In this regard, Radder's view is similar to structural realism (while there are differences too; see 2012[1984/1988], Postscript 2012, section 3.6). I am generally sympathetic to Radder's view of coreference. Nevertheless, two observations distinguish my view from his. To begin with, endorsing the no-privilege principle, he claims that

> one should not assign a privileged place to current scientific theories in philosophical arguments, and in particular not in arguments concerning scientific realism. We should not suppose that, just at this very moment, the development of science has been completed or that, exactly from now on, this development will be of a different character. (2012[1984/1988], pp. 89-90)

In this quotation, two different claims are made in turn: the no-privilege and the no-ultimacy principle. These two should not be confused. I have argued against the former and for the latter.

Current perspectives are reasonably preferable to past ones. We shall see that the preferability of current perspectives is essential to my support of NMA.

My other observation is that Radder's account of coreference faces the same problem structural realists do. Their views are mostly suitable to mathematical sciences. In this regard, Mark Newman maintains that structural realism "is limited to only the mathematical sciences" (2005, p. 1377). Dana Tulodziecki (2016) similarly argues that structural realism, at least in its current forms, seems to have problems dealing with such theories as the miasma theory of disease, which are outside physics. Therefore, a more inclusive account of coreference is necessary to explain why past non-mathematical theories, e.g., in medicine, were successful. Below, I will suggest a "restatement strategy", whose primary purpose is to support NMA, while is sympathetic to but more inclusive than Radder's account of coreference.

6.3 The Pessimistic Induction against the No-Miracle Argument

Larry Laudan (1981) maintains that it is not miraculous if a false theory produces successful empirical results, because many theories have been successful, even if they turned out to be false because their central terms failed to refer. Laudan's argument has been reconstructed by scholars such as Stathis Psillos (1996) and Peter Lewis (2001). For instance, Psillos (1996) formulates it as follows.

> "(A) Current successful theories are approximately true."
>
> "(B) If current successful theories are truthlike, then past theories cannot be."
>
> "(C) These characteristically false theories were, nonetheless, empirically successful.

> So, empirical success is not connected with truthlikeness and truthlikeness cannot explain success: the realist's potential warrant for (A) is defeated." (Psillos 1996, p. 307)

The merit of this formulation is that it is simple. It helps Psillos makes his criticism of Laudan's argument straightforwardly by questioning premise (B). Nevertheless, this reconstruction is not strictly what is set out in Laudan's 1981 paper. Laudan presents a list including the examples of theories whose central terms are, according to him, non-referring. He claims that this list can be "extended ad nauseam" (1981, p.33). The list indeed provides empirical evidence against NMA. This kind of argumentation is common in naturalistic epistemology, on which Laudan starts the discussion in his paper. According to the naturalistic approach to epistemology, (counter)examples can meaningfully provide (counter)evidence regarding an epistemological doctrine. Moreover, an explicit presupposition of Laudan's argument is that "*a realist would never want to say that a theory was approximately true if its central theoretical terms failed to refer*" (Laudan 1981, p. 33, emphasis in original),[5] while this is not plainly expressed in Psillos's (and Lewis's) reconstruction, so their reformulations are not accurate. I try to formulate Laudan's argument precisely according to the content of his 1981 *A Confutation of Convergent Realism*. (Laudan's sentences from which each premise is extracted are mentioned in the footnotes.)

[5] Alan Musgrave (1988, pp. 236–237) nicely analyzes the role of this sentence in Laudan's argument.

(1) Epistemic doctrines including NMA are empirically testable,[6] so NMA can be refuted by (a list of) counterexamples.

(2) A large number of past theories whose central terms fail to refer were successful.[7]

(3) If the central terms of a theory fail to refer, then the theory cannot be (approximately) true.[8]

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[6] "Once one concedes that epistemic doctrines are to be tested in the court of experience, it is possible that one's favorite epistemic theories may be refuted rather than confirmed" (1981, p. 19).

[7] "What the history of science offers us is a plethora of theories which were both successful and (so far as we can judge) non-referential with respect to many of their central explanatory concepts" (1981, p. 34).

"This list, which could be extended ad nauseam, involves in every case a theory which was once successful and well-confirmed, but which contained central terms which (we now believe) were non-referential" (1981, p. 33);

"a realist would presumably insist that many of the central terms of the theories enumerated above do not genuinely refer" (1981, p. 35).

"The realist's claim that he can explain why science is successful is false at least insofar as a part of the historical success of science has been success exhibited by theories whose central terms did not refer" (1981, p. 27).

[8]"I take it that a realist would never want to say that a theory was approximately true if its central theoretical terms failed to refer" (1981, p. 33).

(4) There are a large number of successful theories which are not (approximately) true.[9] (2,3)

(5) There are a large number of counterexamples against NMA; therefore, NMA is not empirically acceptable.[10] (1,4)

Since (4) and (5) are inferences based on other premises, they cannot be the direct targets of the realists' counterarguments. Thus, to refute Laudan's argument, realists need to cast doubt on at least one of the first three premises. Generally speaking, realists tend to call into question the second premise, asserting either that the size of Laudan's list is not large or that the central terms of past successful theories did somehow refer. To conclude that relevant terms of past theories may refer, s*elective* realists distinguish the true from the untrue parts of past theories, asserting that only those parts that are really responsible for the successes of a theory are true and will survive. Philip Kitcher's (1993) distinction between working posits and presuppositional ones, Psillos's "divide et impera" strategy, which distinguishes between essentially contributing

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"A necessary condition—especially for a scientific realist—for a theory being close to the truth is that its central explanatory terms genuinely refer" (1981, p. 33).

[9] "…it follows that none of those theories could be approximately true" (1981, p. 35).

[10] "Accordingly, cases of this kind cast very grave doubts on the plausibility of (T2), i.e., the claim that nothing succeeds like approximate truth" (1981, p. 35).

"…is there any plausibility to the suggestion of (T2) that explanatory success can be taken as a rational warrant for a judgment of approximate truth. The answer seems to be "no"" (1981, pp. 32–33).

constituents and idle ones (1996), Peter Vickers's distinction between what merits the realist's commitment and what does not (2017), the structural realist distinction between structure and nature, and entity realists' distinction between the existence of an entity and its theoretical descriptions can all be understood as examples of the selective strategy.

A selective realist can also cast doubt on the third premise. Laudan himself does not provide an argument for this premise. He simply "take[s] it that *a realist*" should typically accept it. Prima facie this thesis seems obvious. But some selective realists argue that a theory with non-referring central terms *can* contain *other* referring terms which are responsible for the successes of the theory. Thus, it may be that certain claims of the theory are approximately true even if its central terms do not refer, because they do not essentially involve those terms. For instance, Psillos argues that the caloric theory of heat included enough true claims to foster Laplace's prediction of the speed of sound, and those claims were true because they did not involve the existence of caloric (1999, chapter 6; see also Alai 2014).

More interestingly, some realists argue that some roles of a wrongly reified entity may indeed be played by currently accepted entities. E.g., Clyde L. Hardin and Alexander Rosenberg have proposed that "gene" or "phenotype"—as the central terms of the Mendelian genetics—no longer refer, although Mendelian genetics is still approximately true. Mendelian phenotypes have been replaced by "the immediate polypeptide products of DNA expressions", and the role of genes is "parceled out to other entities" (1982, pp. 606–607). Likewise, Juha Saatsi (2005) suggests that "we must respond [to the pessimist] by questioning the link she draws between approximate truth and reference": while "ether" is not referring, Fresnel's theory had true theoretical properties that brought about explanatory successes. Furthermore, Pierre Cruse and David Papineau (2002) and

Papineau (2010) extensively develop the idea of "truth without reference" (section 6.6 will discuss the views of Papineau).

Let me now explain my view about NMA and Laudan's argument against it. First of all, I endorse the highly intuitive idea that successes of science rest on the fact that it describes reality. Therefore, NMA is supportable and can still be employed to explain the successes of current theories. But to explain the successes of past theories, I suggest that NMA needs to be implemented jointly with what I call the "restatement strategy", according to which current theories explain the successes of past theories. Current theories, which are truthful according to NMA, explain why and how past theories bring about successful explanations and predictions.

The restatement strategy enables realists to claim that some of the terms in the theoretical descriptions of a past theory may (partially and in some contexts) refer to a real entity, which is better explained by a current theory. Hence, the second premise of Laudan's argument is problematic. Although there may not exist a real entity to which all descriptions of a past theory apply, there is still some thing which is better described by current theories and about which some descriptions of the past theory are truthful. In this sense, these theoretical terms of truthful descriptions may corefer to some thing to which a current theoretical term also refers. Here, I am generally sympathetic to selective realists. What I add is that they should presuppose the restatement strategy, because it is needed for explaining the successes of past theories. In practice, realists often presuppose the restatement strategy, even if they do not declare it explicitly. In general, my impression is that realists always favor current theories, hence they distinguish selected parts of past theories on the basis of the current image of reality. This assumption is acceptable, because the present standpoint is rationally preferable to past ones thanks to the fact that current theories, instruments, and experimental techniques constitute broader perspectives to discover reality than past ones do.

Moreover, Laudan's third premise is confusing. The premise would have been fully acceptable if it had been expressed as follows: "if the entities to which the terms of a theory refer do not exist, then the theory cannot be (approximately) true". This statement is obviously correct, because a true theory cannot be about non-existing entities. However, expressed in the way Laudan does (If the central terms of a theory fail to refer, then the theory cannot be (approximately) true), the premise is so equivocal that by some revisions and adding some qualifications its contradiction is supportable: if the central term of a theory fails to *perfectly describe* an existent entity, but if in (a number of) theoretical descriptions the term (*partially*) refers (in some *contexts*), the theory is truthful. The truthful descriptions of the past theory can be explained from the perspective of a later theory.[11]

To refer to a thing, it is not necessary that all our descriptions of the thing are correct, hence no need for a "perfect" description. A limited number of truthful descriptions of a thing are enough to direct us to it. The false conception of someone who thinks that the sun does not influence starlight does not prevent one from referring to the sun. Even in our reference to ordinary objects, we do not need to enjoy a perfect image of the object. For example, one can refer to my pen, even if one (and even all other people in the world) wrongly think that the pen is breakable. When they know that the pen is not breakable (because it is constructed from some non-breakable material),

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[11] The qualifications added by the terms "partially" and "contexts" will be clarified in my discussion of the views of Hartry Field and Philip Kitcher in section 6.5.

it does not mean that from then on they refer to another thing when they speak of the pen. They still refer to the same thing, even if they now know that some of their previous descriptions of the thing have been false.

6.4 The Restatement Strategy

This section first explains how the restatement strategy complements NMA, then elaborates my account of reference, and finally discusses whether all past successes are explainable from current perspectives. Section 6.5 compares the restatement strategy with similar views. Section 6.6 provides a logical framework to explain how the truthful descriptions of past theories can be recognized. The study of the Ptolemaic model in section 6.7 illustrates the restatement strategy.

6.4.1 The restatement strategy complements NMA

The main idea of this section is that NMA is reasonable, but to answer the question why and how abandoned theories were still successful, a restatement strategy should be added to it. According to **the restatement strategy [RS]**: *The truthful content of a past theory can be restated from the perspective of the later theory, which then enables us to account for the successes of the predecessor*. This strategy denies the no-privilege principle but it accepts the no-ultimacy principle, in that if our current best theories turn out to be false in the future, the favored perspective of *their* successors, with more explanatory power, can account for the successes of our current theories.[12] In this case, the successes of current theories are still explainable by their truthfulness. However, to explain the successes of past theories, RS should be added to NMA. That is,

use NMA to explain the successes of present theories, and

use NMA and RS to explain the successes of past theories

According to the first instruction, NMA is necessary to explain the success of current theories. In the second instruction, firstly, RS is needed to show that the success of a past theory is explainable by a current theory; secondly, NMA is also needed to account for why the current theory is itself truthful. Accordingly, both instructions are based on NMA, so the whole strategy is realist. Indeed, RS is used only in cooperation with NMA, so to explain the successes of theories one always needs NMA.

The novelty of this suggestion is that NMA does not directly apply to past theories. It only applies to our current most successful theories, but RS explains the successes of past theories. As a result, the truthfulness of current theories can account both for their own successes and for the successes of past theories. The successes of past theories are explicable in light of current theories, demonstrating that those successes were not miraculous. This approach also implies that *the successes* of successive theories are consistent with each other, and accordingly, it assures us that successive theories are on the right track in their progress.

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[12] Thomas Kuhn famously argues that successive theories are incommensurable. Incommensurability does not mean incomparability, however (Hoyningen-Huene 1993, p. 218ff.). Comparability of a current theory and its predecessor is enough to "restate" the successful explanations or predictions of the predecessor in the terms of the successor.

Furthermore, RS helps to show that there is intertheoretical continuity between successive theories. This continuity holds between diachronic theoretical perspectives, showing that they are overlapping. Suppose that a past theoretical perspective posits descriptions, which contribute to a successful explanation or prediction, and whose successful role can be explained from the perspective of a current theory by restating their role with the aid of mathematical or non-mathematical terms of the current theory. In this case, the theoretical terms of those descriptions may refer to some things referred by the terms of the current theory. This continuity is similar to Radder's account of coreference, but it does not presuppose the no-privilege principle. Also, it permits the non-mathematical explanations of the successes of past theories.

In practice, some core descriptions of an abandoned term are false. This justifies that current theories do not preserve the terms of their predecessors. Of course, this is not bad news for science, but rather a positive sign because new theories can describe the same reality with better theoretical terms, whose descriptions are preferable according to our current perspective. The emergence of new terms shows that science is active and developing. Thus, realists need not worry if the central terms of past theories turn out to be useless, because at the same time theories containing new terms are being developed to provide better descriptions of reality, and to explain the successes of their predecessors.

That a current theoretical term is better than a past one in bringing about successful explanations and predictions does not mean that the current theoretical term is *ultimate*; or that, in other words, *all* the core descriptions of that term are true. Some theoretical assumptions presupposed by the descriptions of the current theoretical term *may* be false. That is, the true descriptions that the term entails (Ts) denote something real, but the term may also entail descriptions (Fs) that are inapplicable to that "something". Therefore, the Ts $\wedge$ Fs together are untrue about the "something".

The “something” may be better described by a future theoretical term. However, that future term may be similarly imperfect, as some of its core descriptions may turn out to be false. Accordingly, theoretical terms are always *provisional*. They may be replaced with terms that better describe reality.

6.4.2 The reference of theoretical terms

Let me explain how an abandoned theoretical term that contributes to a successful explanation or prediction *refers*. In line with what I said, *the theoretical term existentially refers to some thing, which may better be described by later theories.* That some thing existentially refers means that *some thing exists* in the fabric of reality, even if the referent may always be described better by future (non)mathematical, theoretical terms. Why should there be some thing in reality to which the term refers? Because of overlapping theoretical perspectives. When there is a continuity between two or more successive theoretical perspectives, those theoretical descriptions that are in (non)mathematical correspondence with each other may be about the same real thing. RS helps to show this correspondence.

With the aid of Ramseyfication subsection 6.6.2 will clarify how *some thing* may be existentially referred to, while its descriptions are not definite. To this end, I will employ existential quantifiers that hint at their referents but cannot determine the unique descriptions of those referents. This is (partly) in line with Putnam’s (1980; see also 1981, chapter 2) claim that first order logic does not *uniquely* determine an ontology. Although the intended interpretation of one’s theory of the world cannot be fixed uniquely, RS suggests that a theoretical interpretation can be intertheoretically connected with another theoretical interpretation. The result is both a referential continuity in successive theoretical terms and a conceptual plurality of the descriptions of the referent.[13]

My view of reference is in line with the positive sides and avoids the drawbacks of the two common accounts of reference: causal-historical and descriptive (on these theories, see Psillos 2012). According to the former, the link between a term and its referent is direct, unmediated by descriptions. That is, although the act of naming is conventional, it attributes to the bearer (or the referent) a name that is in a causal contact with the bearer. The historical chain also links current uses of a term to its initial use. In this account, the nature of the "causal" link between the name and its referent is rather unclear. But if it means that the name refers to the *existing* referent, and not necessarily to its descriptions, it holds water. According to this interpretation, the human-

[13] I agree with Putnam about the rejection of "metaphysical realism" in that a single true and complete description of the world is unavailable. In late 1970s and 1980s, he advocated "internal realism" but he abandoned this position after 1990. One component of internal realism is "conceptual relativism", which Putnam maintained even in his later thoughts. I do not advocate internal realism, mainly because of the existence of intertheoretical continuities between successive conceptual descriptions, while my endorsement of the conceptual plurality of these descriptions is in general agreement with Putnam's arguments for conceptual relativism. Nevertheless, thanks to intertheoretical continuities between theoretical perspectives, I prefer the expression "conceptual plurality" to "conceptual relativity". On Putnam's conceptual relativism, see Button (2013, section 9.1 and chapter 18). See also De Caro (2020, section 4.6), who argues that Putnam's eventual view on realism agrees with (Massimi's version of) perspectival realism.

independent world specifies the real entities to which our theoretical terms refer. This helps to show that intertheoretical continuity between successive theories rests on the persistent existence of real entities (on persistence, see chapter 2). Two main criticisms of the causal-historical theory of reference say that, first, it underestimates the theory-dependence of theoretical terms; and second, it may face the risk of overgeneration, implying that all past theoretical terms refer arbitrarily to some real entities in nature (see Papineau 2010, pp. 376–377; Psillos 2012, part. 3). In contrast, the positive side of the descriptive theory is that it takes seriously the theory-dependence of theoretical terms. But it makes referential success of the abandoned terms of past theories rarely possible, because some thing to which most theoretical descriptions of an abandoned term apply can hardly be found in reality. Therefore, this theory may be inadequate in explaining the realist intuition that some terms of past theories manage to express true things about reality (see McLeish 2005, p. 668; cf. Psillos 1997, p. 270; 2012, p. 218).

My account of reference takes seriously the independence of reality, whose persistence manifests itself in intertheoretical continuity between successive theories. In agreement with what I argue in previous chapters, diachronic overlapping perspectives may refer to this independent reality. But real things are always described by (theoretical) concepts, so our knowledge of reality is always theory-dependent. In other words, our knowledge of reality always depends on theoretical perspectives. RS helps to show that these perspectives are overlapping in many historical cases, where the successful explanations and predictions of past theories can be restated by current theoretical terms. In these cases, overlapping theoretical descriptions are about the same persistent thing. (Please note that the adjective "overlapping" can apply to both perspectives and descriptions. The latter is the case when a truthful description of a past theory can be restated from the perspective of currently accepted descriptions, and thus the past and current descriptions are both about the same thing).

This account of reference lacks the drawbacks of the two common theories of reference. The theory-dependence of theoretical terms is taken seriously due to their perspectival interpretation. Neither theory-independent nor ultimate knowledge of reality is available. Overlapping perspectives of current and past theories do not present non-perspectival knowledge (see subsection 3.4.2). Nor do current theories provide an ultimate image of reality, although they are preferable to past ones. Further, the referential successes of the abandoned terms of past theories neither face the risk of overgeneration nor are they rarely possible. If the theoretical descriptions of a past theory contribute to a successful explanation or prediction, and if those descriptions can be restated on the basis of current theoretical terms, then the relevant terms of those descriptions may (partially and in some contexts) refer to a thing better described by a current theoretical term. These conditions are fulfilled in quite a few historical cases, so the referential successes of past theories are not rare. They are not overgenerated either, because only those theoretical descriptions that contribute to a successful explanation or prediction and whose successes can be restated from our perspective are truthful. Thus, one is not justified to say, in an unqualified way, that all past terms are referring or that their descriptions are true.

6.4.3 Kuhn loss

A phenomenon relevant to my current discussion is the so-called Kuhn loss, in which a later theory lacks the (theoretical) resources to explain the successes of a preceding theory (see Kuhn 1970[1962], chapter 9). Kuhn maintains that a new paradigm usually enjoys a higher problem-solving power than its predecessors. Nevertheless, there are a small number of historical cases in which the next paradigm does not possess the explanatory successes of its predecessor (1970[1962], p. 169). Scholars such as Heinz Post (1993[1971], pp. 229-230) have questioned the claim that there are genuine cases of this phenomenon, however. In this regard, a defensible account is that current theories can *in principle* explain most successes of past theories, although in practice it may be easier to employ past theories to solve problems. For instance, classical physics is practically superior to the theory of relativity in solving the problems related to the mechanics of ordinary objects. However, the theory of relativity can in principle explain why classical physics is successful in ordinary domains. In general, new theories are not quite satisfactory unless they can, at least in principle, account for earlier explanations and predictions. When a new theory is developing, it may ignore some formerly explained or predicted phenomena for a period of time. However, when the theory is well-developed, it is expected to explain previously known phenomena. Thus, theoreticians are typically pushed to advance their theory in a way that includes the already known explanations and predictions. For this reason, the examples of Kuhn loss in the history of science are few, if any.

Apart from this, the purported cases of the Kuhn loss should be examined case by case to see if they pose a serious threat to RS. In this regard, Ioannis Votsis (2011) explores historical examples and concludes that none of them have been empirically successful, except one:

> Despite all the commotion surrounding Kuhn loss, finding examples that satisfy this stronger notion [that is, those historical examples that are empirically successful] is not an easy task. Radder (ibid. [1996], p. 63) puts forth Poiseuille's law as one such example – *the only one it seems*. … Crucially, and according to Radder, it is impossible to reproduce this law from quantum mechanical accounts of fluids. (Votsis 2011, p. 113, emphasis added)

Concerning the case of Poiseuille's law, also known as the Hagen–Poiseuille law, and its relevance to RS, there are two points. First, the central terms of this law are the viscosity of the flow, the dimensions of a flow tube, and the pressure difference between the two ends of the tube. None of these terms are currently considered as non-referring. Therefore, the successful explanations of this law cannot be used as an example in Laudan's argument against NMA, and accordingly RS is not needed to be employed in this case. Second, Radder's claim about this and similar examples is that "quantum physics is *practically* useless and classical physics obviously superior" (1996, p. 63, emphasis added). This is correct because quantum theory cannot *directly* apply to the fluid molecules and it is not useful in practice. Still, it is reasonable to maintain that this and similar cases are *in principle* explainable by current theories. The Hagen–Poiseuille law is derived from the Navier–Stokes equations, which arose from the application of Newton's second law to fluid motion (and in addition to the assumption that the stress in the fluid is the sum of a viscous and a pressure term). Newton's second law is also obtainable from the Schrödinger equation in the limit of Planck's constant becoming zero (see Radder 1996, p. 58). As a result, quantum theory together with some assumptions indirectly results in the Hagen–Poiseuille law and thus can in principle explain the successes of this law.

Let us examine another proposed example. According to Hasok Chang (2012, subsection 1.2.4.1; 2011a, p. 422), a clear example of Kuhn loss concerns the phlogistonists' explanation of the common properties of metals on the basis of their richness in phlogiston. But Chang (2012, chapter 1) also explains in detail that the concept of phlogiston has historical affinities with potential energy and electricity. He intriguingly concludes thus:

> When [William] Odling and others saw phlogiston as the predecessor of chemical potential energy, and when [Gilbert] Lewis saw phlogiston as the

> predecessor of electrons … their insights provide a sufficient answer as to why I am not going to try to bring phlogiston back to modern chemistry—it is already here! (2012, p. 65)

My claim is similar: many successful explanations and predictions of past theories are "already here". We can restate them by the light of current theories. Specifically, in the case of the common properties of metals the lack of an explanation was a "loss" for about one hundred years (see Hoyningen-Huene 2008, p. 110). However, the phlogistonists' account of the common properties of metals can now be explained by the modern theory of electricity. Thus, RS is not threatened, and it is even supported by this example. I also agree with Chang (2011b, p. 323) that some forgotten historical cases (of experiments) provide an opportunity for the recovery of scientific knowledge, in addition to their opportunity for pedagogic purposes. But this is consistent with the contention that those historical cases are in principle explainable from current perspectives.

6.5 The Restatement Strategy and Similar Views

This section further explains RS, comparing it with similar ideas in the literature and defending it against criticisms. I start with discussing Wilfred Sellars's similar view. In section 6.4 of his article, Laudan questions the possibility of explaining the successes of past theories, arguing against Sellars's belief that a new theory should account for why its predecessors were to some degree successful (Laudan 1981, pp. 43–44).[14] Laudan contends that the requirement that a theory should explain why its predecessors were somewhat successful "is neither a necessary nor a sufficient

[14] For a relatively recent defense of the Sellarsian view see Rosenberg (2007, Chapter 4).

condition for saying that it is better than its rival". According to him, only "more confirmed consequences (and greater conceptual simplicity)" are decisive for scientists to accept a theory. However, Laudan's view is questionable because unless scientists clarify why their theory can account for the success of past theories, their theory won't be considered completely satisfactory. Apart from this, I want to emphasize that RS pertains to the history and philosophy of science. That is, even if we would charitably accept that scientists only engage with the "confirmed consequences" and "conceptual simplicity" of current theories, this does not mean that historians and philosophers of science should not or cannot restate the success of past theories by currently accepted theoretical terms. What matters is whether past successes can or cannot be explained by current theories. It is irrelevant whether scientists perform this task or historians and philosophers do. In any event, the application of current theories to the successes of past theories, apart from its role in defending NMA, can actualize some potential explanatory power of current theories, which is valuable even from a purely scientific point of view.

Another criticism Laudan makes is that for realists to explain the successes of past theories they need a "robust" sense of explanation, according to which the truthfulness of a new theory and the partly overlapping results of the theory and its predecessor jointly explain why the predecessor was successful. According to Laudan, however, such an explanation is unavailable since a (new) theory cannot even explain its own success, let alone the success of another theory (1981, p. 44). However, this counterargument is circular: Laudan first claims that NMA is incorrect since the success of past theories cannot be explained (by RS or other approaches); then he says that RS is unavailable since the success of current theories cannot be explained by their own truth, because NMA is incorrect. This can be rephrased as follows:

(1) NMA isn't tenable since strategies like RS are unavailable

(2) RS is not available because NMA is untenable

However, the claim that NMA is untenable can be refuted by the availability of RS, so it is not correct to deny the possibility of RS itself by appeal to the claim that NMA is untenable. As I argued, not only can current theories account for the success of their predecessors, but they are even better placed than their predecessors themselves to account for these successes.

A view similar to RS is what Stanford (2000) calls "predictive similarity", as an explanation for the successes of science. Stanford suggests this view to object to those who believe that NMA is the *only* explanation of scientific successes. According to him,

> The success of a given false theory in a particular domain is explained by the fact that its predictions are (sufficiently) close to those made by the true theoretical account of the relevant domain (Stanford 2000, p. 275).

> We do indeed explain the success of the (revised) Ptolemaic system of epicycles by pointing out how closely its predictions approximate those of the true Copernican hypothesis. Let us call this relationship the predictive similarity (Stanford 2000, p. 273).

However, it is questionable that predictive similarity can explain the success of abandoned theories. As it has already been pointed out in the literature, the main criticism of this claim by Stanford is that the successes of a discarded theory consist just in its correct predictions, i.e. in its "predictive similarity". Therefore, Stanford proposes to explain one phenomenon by the very same phenomenon, which is not an explanation.

If we interpret Stanford's suggestion in a way that is consistent with RS, it implies that the predictions of past theories can be explained by current theories. However, this does not provide an explanation of the successes of *current* theories since future theories are not available now to provide us with their predictive similarity with current theories. If one restricts oneself to predictive similarity, the successes of the latest theories are always unexplained. This problem can be settled if we assume that current theories are true (in the same way as Stanford does when he uses "true" for the "Copernican hypothesis" and for the "theoretical account of the relevant domain" in the previous quotations); Realists are legitimized to take successful theories as truthful because they accept NMA. But how can Stanford consider them to be true? This seems to be inconsistent with his criticism of NMA. Unlike predictive similarity, RS is a realist view, since as I said earlier, it should be combined with NMA to explain the successes of past theories.

Let me now examine the views of Hartry Field (1974), who argues that scientific terms may be referentially indeterminate. He objects to Kuhn's view that before relativity theory the term "mass" did not refer to the same thing to which it refers today. Instead, he argues that the term "mass" was referentially indeterminate:

> Newton's word 'mass' partially denoted proper mass and partially denoted relativistic mass; since it *partially* denoted *each* of them; it didn't *fully* (or *determinately*) denote *either*. (Field 1974, p. 474)[15]

Field's view implies that what Newton really denotes in each usage of the term 'mass' is clarified from a later perspective. Only in light of Einstein's theory of relativity and its theoretical terms one can "refine" Newton's term mass. The theoretical terms of Einstein's theory include "proper mass" and "relativistic mass". The former is equal to the non-kinetic energy divided by

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[15] See also Frost-Arnold (2014, p. 540; 2008).

the constant c^2; thus, it is independent of the motion of the body. The latter is equal to the total energy divided by the constant c^2 and depends on the motion of the body. According to its use in a specific statement, Newton's term "mass" may denote the proper mass or the relativistic mass (see below). Similarly, Future theories may refine the terms of current theories. To quote Field, "future scientists may very well refine many of our current scientific terms" (1974, p. 480). According to both RS and Field, the principle of no-privilege is questionable, and thus *current* theories clarify the denotations of past terms.

Field's primary aim is to refine the denotations of past terms, which can be used *as one step* in the application of RS to the references of past theoretical terms in order to show their referential successes. How can "refinements" be used in RS? Let me clarify. Earlier, I explained that the specification of the coreferring terms O and N is a heuristic process and the terms O and N may be candidates for coreference to the same thing if there are empirical domains to which O and N are both successfully applicable. Field teaches us that the older term O may partially denote N_1 and partially denote N_2 (N_1 and N_2 are two terms of the newer theory). As a result, O partially corefers to the referent of N_1 and partially to the referent of N_2. For example, for velocities much smaller than the velocity of light, the Newtonian mass (O) nearly equals that of either the relativistic mass (N_1) or the proper mass (N_2). Thus, Field's analysis helps to show that for low speeds the Newtonian mass (O) partially denotes the relativistic mass (N_1) and partially the proper mass (N_1), and therefore to illustrate the role of RS in explaining the truthful content of descriptions of the Newtonian mass. For instance, three relevant statements about the Newtonian mass of an object are:

(S1) (the quantity of) the mass of an object is equal to its momentum divided by its velocity (measured in a chosen frame of reference), (S2) the mass is independent of (the velocity of) the frame of reference, and

(S3) the mass is proportional to the amount of matter.

The “relativistic mass” should be used instead of “the mass” in S1, the “proper mass” should be used instead of “the mass” in S2, and either “the proper mass” or “the relativistic mass” can be used instead of “the mass” in S3. RS thus helps to *restate* the truthful content of the theoretical descriptions of the Newtonian mass. The truthful descriptions, which are restated from the perspective of the relativistic theory, explain why the term “Newtonian mass” may be successfully used to provide explanations and predictions.

Another useful concept that can be taken into account while using RS is Philip Kitcher’s notion of “context”. He suggests that a term of a past theory has a “reference potential”. A term (such as phlogiston) may refer in some contexts and not refer in others. It may also refer to different things in different contexts. Consider the case of ‘dephlogisticated air’. In some tokens it refers to the referent of the term oxygen, and in other tokens it does not refer (1993, pp. 101–102). But how can we distinguish referring from non-referring tokens of a term? Kitcher’s response is that this is possible by applying the “principle of humanity”, according to which, we can attribute to past scientists a “cognitive equipment that is similar to our own, and using what we know about the experiences they had” to distinguish between the referring and non-referring tokens of a term. In other words, if these past scientists would live now, they would agree with us (Kitcher 1978, p. 142; 1993, p. 101). Stathis Psillos (1997) and Christina McLeish (2005; 2006) convincingly argue against Philip Kitcher’s approach. The main thrust of their arguments is that historical facts in a specific context are not sufficient to single out a specific referent among potential referents. Psillos believes that “the principle of humanity makes referential continuity too easily available”, such that it doesn’t prevent one to claim that, for instance, by using the phrase “seeking its natural place”, at least in some tokens, Aristotle intended to refer to “motion-along-a-geodesic” (Psillos 1997, p. 269). McLeish similarly states that no facts are available “in distinguishing Priestley’s referential tokens from those that failed to denote anything” (McLeish 2005, p. 684). I agree with these authors that it is impossible to demonstrate that past scientists intended to refer to what the terms of current theories refer, but we don’t need this kind of fact at all as long as our aim is to speak of the truthfulness of past theories or to explain their success. We indeed judge the truthfulness of past theories from our current standpoint. This is exactly what is the point of RS: current theories help us to know why past theories were to some extent successful.

But is RS only a retrospective thesis? No. I finish this section by adding further clarification about the retrospective and prospective dimensions of RS. First of all, I agree with Stanford (2006, p. 166) that realists would beg the question if they consider the features of a past theory that survive in contemporary science as the success-fueling parts of that theory. Indeed, the theoretical terms in the successful explanations or predictions of a past theory may be absent in current science. Therefore, realists should characterize the terms that play a role in the successes of a past theory not on the basis of the present views, but according to the role of the terms in the past theory’s explanations. This is compatible with RS, as one should first characterize the essential terms of a past theory from its own perspective; only after that, one can start to restate its successes on the basis of current theories’ terms. For instance, “epicycle” is a contributing term in the successful predictions of Ptolemaic theory about the path of the outer planets. This fact is recognizable whether or not “epicycle” is an acceptable term in current science; it only depends on its role in the explanations of Ptolemaic astronomy. Only after acknowledging the epicycle’s role in Ptolemaic astronomy one may start to restate its truthful content by currently acceptable terms (see section 6.7). As a result, the contributing parts of past theories are not recognized retrospectively but from their own perspectives.

Nevertheless, in applications to past theories RS explicitly plays a "retrospective" role, whose purpose is to highlight consistence and continuity in the history of science. Be that as it may, we can also justifiably argue that some aspects of a theory that are overlapping with past theories may survive in the future (see also section 2.6). It is true that, with the aid of future technological instruments and experimental techniques, creative scientists may offer novel theories with radically different central terms. For instance, it does not seem unlikely that any future theory reconciling general relativity theory and quantum mechanics may deploy novel concepts that enable a better grip on reality. However, what we can expect, which is also the "prospective" aspect of RS, is that the theory-to-be will account for the successes of current theories, just as current theories clarify why abandoned theories were successful. The terms of those descriptions of past theories that are (partially and in some contexts) overlapped by current theoretical terms are existentially referring and may be described even better by the terms of future theories.

Finally, RS is not a mere promissory note; it sticks out its neck because it is certainly possible that the successes of a past theory could not be explained by a current theory. What is more, future scientists may devise a theory that is more explanatorily powerful, and yet cannot account for the successes of current theories. Hence, RS is fallible.

6.6 Ramseyfication

This section presents a logical framework that further elaborates the claim that abandoned terms of past theories may entail truthful descriptions of real things, so that the relevant terms of those truthful descriptions can (co)refer to things to which current theoretical terms refer. This framework is based on the Ramsey-sentence approach to scientific theories, according to which a *Ramsey-sentence* is defined as an existential sentence that is equivalent to a scientific theory. *Ramseyfication* refers to the procedure of writing down that Ramsey-sentence.[16] In the following, I first review other applications of the Ramsey-sentence approach by logical positivists and structural realists; then, I explain my own specific use of this approach.

6.6.1 Ramseyfication and structural realism

Frank Ramsey's (1903-1930) notable approach to scientific theories appears in his *Theories*, an essay written in 1929 and published posthumously in 1931 in the collection of his papers, edited by Richard Braithwaite. This essay attracted attention only in the 1950s and thereafter. Braithwaite discussed *Theories* in chapter 3 of his *Scientific Explanation*, published in 1953. Carl Hempel coined the term "Ramsey-sentence" and utilizes it in his *The Theoretician's Dilemma* (1958). Rudolf Carnap also employed the Ramsey-sentence approach, in the late 1950s and the 1960s, in his work on the nature of scientific theories and in his neutral position in the debate between instrumentalism and realism (see Carnap 1963, pp. 958-966).[17] David Lewis also employed the Ramsey-sentence approach to define theoretical terms in his 1970 paper (which will be further

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[16] Both "Ramseyfication" and "Ramsification" have occurred in the literature. I prefer the former, in which Frank Ramsey's surname appears correctly.

[17] On the history of Ramsey-sentence, and specifically, on Carnap's "re-invention" of the Ramsey-sentence approach to scientific theories and its shortcomings, see Psillos (1999, Chapter 3).

discussed below) and to develop his other philosophical views, e.g., on the nature of the mind (see, e.g., Lewis 1972).[18]

Grover Maxwell (1962; 1968; 1970a; 1970b), who first coined the expression "structural realism", employs the Ramsey-sentence approach to specify the realist commitment to the *structure* of the world. He claims that the Ramsey-sentence approach is not associated with instrumentalism.[19] According to him, the Ramsey-sentence of a theory captures the observable knowledge of the theory as well as the structural knowledge of the unobservable world. Other structural realists such as John Worrall and Elie Zahar (2001) also endorse the Ramsey-sentence approach to theories.

Structural realists such as Maxwell obtain the Ramsey-sentence of a theory by replacing the theoretical terms of the theory with certain variables (employing the same variable for the same theoretical term, and different variables for different theoretical terms), and then by binding the variables with existential quantifiers. Suppose that the theory T is represented as T (t_1, t_2, …, t_n; o_1, o_2, …, o_m), in which t_1, t_2, …, t_n are theoretical terms and o_1, o_2, …, o_m are observational terms. Then, the Ramsey-sentence ^{R}T of the theory T is defined as follows:

$$\exists x_1\, \exists x_2 \ldots \exists x_n\, T(x_1, x_2, \ldots, x_n; o_1, o_2, \ldots, o_m),$$

[18] On Lewis's uses of Ramseyfication, see Weatherson (2021, section 4.1) and Nolan (2005, pp. 213-227).

[19] Carnap accepts this claim of Maxwell. On these historical details, see Salmon (1994, p. 282) and Psillos (1999, pp. 58-59).

where x_1, x_2, …, x_n are different variables that, together with observational terms, are used in several sentences that are conjoined to each other to constitute the single sentence ^{R}T. Structural realists claim that there is no need to existential commitments to unobservable entities anymore, because theoretical terms are removed in ^{R}T and x_1, x_2, …, x_n are employed instead of them. These variables are not theoretical or observational terms. They are merely variables that stand in certain relations with one another, thereby constituting a formal structure. Thus, according to structural realists, our knowledge of unobservables, (1), includes only the knowledge of their structural (or higher-order) properties, and (2), does not include that of the intrinsic (or first-order) properties of unobservable entities.

However, structural realists run into grave difficulties. The simplest and most fundamental one is how structures, as relations, can be defined without specifying any entities, as relata. Even in ^{R}T, n different entities (such as e_1, e_2, …, e_n) should *exist* to fulfill the n different variables with existential quantifiers. Therefore, our knowledge of unobservables also includes that of the existence of the n unobservable entities, and therefore (1) is not acceptable. These n real unobservable entities cannot be identical, because each fulfills a *different* variable. Therefore, each entity should have its specific properties and (2) is also incorrect. (1) and (2) are untenable; hence, a realism about entities and their properties will be the corollary of structural realism.

Max Newman (1928) raised a similar objection to structural realism. The objection was first made against Bertrand Russell's (1927) structuralism, according to which the structural, or the logical-mathematical, properties of the external world can be inferred from the structure of observable phenomena, whereas the intrinsic properties of the world are unknowable. Newman's major problem with any (exclusively) structuralist view is that structure alone is not enough to uniquely specify any relation in the external world. In his words,

> the doctrine that *only* structure is known involves the doctrine that *nothing* can be known that is not logically deducible from the mere fact of existence, except ('theoretically') the number of constituting objects" (Newman 1928, p. 144; see also Demopoulos and Friedman 1985).

In other words, if structural realists refuse to accept that their structural knowledge of the world also includes the knowledge of entities and their properties, their knowledge will be restricted to that of "the number of constituting objects" of the external structure. Suppose that four distinct objects whose properties are *unspecified*, {a, b, c, d}, are in the relation R {<a, a>, <a, b>, <b, c>, <c, d>, <d, a>, <d, d>}. About this structure, we know nothing except that the number of the constituting objects of the structure is four. Every four-object structure can have the relation R, *unless* one specifies the properties of the four objects so that each object has specific relations *only* with the other objects. According to set theory, four objects instantiate $2^4 - 1$ non-empty one-place relations (one of which is, for example, R': {<a>, <b>, <d>}), $2^{4\times 4} - 1$ non-empty two-place relations (one of which is R), $2^{4\times 4\times 4} - 1$ non-empty three-place relations (one of which is, for instance, R":{<a, a, a>, <a, b, c>, <a, c, d>, <c, a, d>}), and so forth. A four-object structure can have many relations like R', R, and R". Therefore, the definition of relations is not helpful, unless (the properties of) the four objects are specified in a way that restricts the actual relations (see Ainsworth 2009, subsection 3.1; see also his 2009, sections 4-6, where he argues convincingly against different responses to Newman's objection).

Altogether, the Ramsey-sentence approach does not help structural realists to limit commitment to the structure of the world. Any purely structural account of reality faces problems such as the Newman objection. Thus, James Ladyman (1998) argues that structural realism does not improve traditional scientific realism if the former "is understood as merely an epistemological refinement of" the latter, and therefore he argues that structural realism "ought to be developed as a metaphysical position" (1998, p. 411). Early structural realists such as Bertrand Russell, Grover Maxwell, and John Worrall are ambiguous on whether they make epistemological or metaphysical claims. However, in recent years, "ontic" structural realism has explicitly been defended by James Ladyman and Don Ross (2007) and by Steven French (2014), among others. Motivations for this metaphysical account derive from the interpretations and problems of quantum (field) theory and general relativity. Still, it is problematic how precisely real relations are possible without real relata (see Psillos 2009, chapter 8). Moreover, there is no consensus on the reasons why relations are "more fundamental" than relata (seven different proposals are summarized in Ladyman 2020, section 4). Furthermore, *if* ontic structural realism describes reality as consisting of *actual* structures, it clashes with my arguments for the account of reality in terms of potentialities in chapter 2 (otherwise, the two views may be compatible). An issue that, at this stage, makes it difficult to compare ontic structuralism and the account of reality as consisting of potentialities is that structural realists have rarely, if ever, discussed the experimental, practical dimension of science, while the motivation behind the account of reality in terms of potentialities (to some extent) rests on this dimension.

Anyway, ontic structural realism and its relationship with the account of reality as potentialities should be discussed in a separate work. The focus of this chapter is on the problem of theory change, to which only the epistemological claims of structural realists such as Worrall (1989) pertain. In my view, epistemic structural realism is helpful in addressing the problem of theory change inasmuch as it is not construed as an extreme doctrine that non-structural properties are unreal or unknowable, but as an account that highlights the importance of the logical-mathematical structures of theories. When put like this, structural realism is compatible with RS in that the mathematical formulas of current theories can help to restate past theories' mathematical formulas that bring about successful explanations and predictions. Bohr's correspondence principle entails that quantum mechanical equations can reduce to classical equations when the number of particles is large or the Planck constant is arbitrarily reduced to zero. Other examples of similarity in the mathematical structure of successive theories can be found between classical and quantum mechanics and between special relativity theory and classical mechanics. In all these cases, the mathematical formulas of a newer theory allow us to account for the successful explanations and predictions that (the mathematical formulas of) its predecessor brought about.

6.6.2 Ramseyfication and restatement strategy

This subsection explains my specific use of Ramseyfication, as the logical framework for RS. I should in advance insist that I do not use it to support instrumentalism or structural realism. My use of Ramseyfication concerns the problem of theory change; so in this respect, it is similar to Papineau's (2010) realist employment of Ramseyfication to cast doubt on Laudan's pessimistic induction. Nevertheless, I disagree with Papineau's idea of truth without reference (see Papineau 2010, section 3.1; see also Cruse and Papineau 2002; for its criticism, see Newman 2005, section 4). This idea relies on the descriptive theory of reference, which I do not endorse (see subsection 6.4.2). A Ramsey-sentence clearly says that there *exists* some thing with the properties the original theory roughly describes. Therefore, although the *existing referent* may not be perfectly described by the (abandoned) theoretical term, the Ramsey-sentence of the (abandoned) theoretical term may refer to that existent referent. Thus, the utilization of Ramseyfication to defend "realism without reference" is questionable. My use of Ramseyfication is in line with my endorsement of the combination of entity realism and perspectival realism in previous chapters: the existential quantifiers of the Ramsey-sentence of a theoretical term may successfully refer to an entity, which exists even if its descriptions depend on (historical) perspectives.

As another preliminary point, my use of Ramseyfication does not presuppose that theories should be reconstructed as formal axiomatic systems. It does not even need the presupposition that theories are actual sets of propositions. The only relevant presupposition is that theories can be learnt and described. Inasmuch as theoretical terms are learn*able*, and thus describ*able*, our knowledge of them can be stated in sentences. If one can state the sentences concerning the contributions of an abandoned term to successful explanations and predictions, one will be able to employ Ramseyfication to implement RS. For this purpose, it is unnecessary to presuppose a specific interpretation of scientific theories such as the syntactic or semantic view of theories.

Like Papineau (2010), I pursue Lewis's (1970) suggestion that, unlike what logical positivists such as Hempel and Carnap and structural realists such as Maxwell maintained, theoretical terms should not be conceived in opposition with "observational" terms, but in opposition with "old" terms (according to Lewis), or "antecedently understood" terms (according to Cruse and Papineau 2002, section 5), or according to my preferable terminology: "independently-understood" terms. The meaning of a theoretical term is obtained from its place in the theory under discussion; by contrast, the meaning of an independently-understood term, which may still be theory-laden, is fixed independently of its place in *that* theory. In other words, we know the meaning of independently-understood (or "old") terms before we become familiar with that theory. On the other hand, "theoretical" terms are "new" for us when we first meet that theory. The following example by Daniel Nolan vividly illustrates this point.

> Suppose I am given some very basic lessons in atomic chemistry. I am told that there are three sorts of fundamental particles: electrons, protons and neutrons. I

am told that electrons are much smaller than protons and neutrons, and that in atoms, protons and neutrons are clumped together in the centre in a nucleus, while electrons whiz around at some distance from the nucleus. (Nolan 2005, p. 214)

The terms such as “electron”, “proton”, “neutron”, “atom”, and “nucleus” are the theoretical terms of these lessons. The meanings of other terms do not depend on their place in this theory. The Ramsey-sentence of the theory taught in these lessons is roughly as follows.

There are some things, Vs, Ws, Xs, Ys, and Zs, and the Xs, Ys and Zs are three sorts of fundamental particles, and the Xs are much smaller than the Ys, and in the Ws, the Ys and Zs are clumped together in the centre in a V, while the Xs whiz around at some distance from the V. (Nolan 2005, p. 215)

V, W, C, Y, and Z are variables, employed instead of the theoretical terms of the theory. At the beginning of the sentence, it is stated that some things should exist that satisfy the variables V, W, C, Y, and Z. Other terms used in this Ramsey-sentence are its independently-understood ones.

In general, the Ramsey-sentence ^{R}T of the theory T is $\exists x_1 \exists x_2 \ldots \exists x_n\, T(x_1, x_2, \ldots, x_n; i_1, i_2, \ldots, i_m)$, where $i_1, i_2, \ldots, i_m$ are independently-understood terms, $x_1, x_2, \ldots, x_n$ are variables, and $T(x_1, x_2, \ldots, x_n; i_1, i_2, \ldots, i_m)$ is a single sentence that describes the theory by means of the variables and independently-understood terms in a number of sentences that are conjoined to one another. The existential quantifiers employed before the variables stipulate that n entities exist and should realize the descriptions of $x_1, x_2, \ldots, x_n$ in the Ramsey-sentence. This “realization” does not need to be perfect. According to Lewis, a “*near*-realization”, in which “an n-tuple [is used] that does not realize the original theory, but does realize some theory obtained from it by a slight weakening or a slight correction" is still adequate (1970, p. 432). Lewis states that theoretical terms refer only if there is a *unique* (near) realization of T, that is, if variables are filled uniquely by the entities e_1, e_2 …, e_n. In Nolan's example, V, W, X, Y, and Z should uniquely be filled by "electron", "protons" and so on. If there is more than one class of real entities, the theoretical terms lack denotation (Lewis 1970 p. 433). I agree, but the unique realization of variables should not be interpreted *statically*. That is, in line with what I argued in subsection 6.4.2, a variable can existentially refer to some unique thing, which may be described differently by historically different theoretical perspectives. There may be referential continuity between two theoretical terms in that a past theoretical term can (partially and in some contexts) refer to some unique thing that is also referred to by a new theoretical term. As a result, the existential realization of a variable is unique, but the unique referent may be described by multiple sets of descriptions.[20]

The theory and its Ramsey-sentence are equivalent because the theory, inasmuch as it is learnable, is stated in its Ramsey-sentence. The existential quantifiers used at the beginning of the sentence emphasize that the truth value of the theory depends on the existence of entities, hence "semantic realism": theoretical terms should be treated on the basis of standard referential

[20] Psillos (2009, section 9.8) concludes from the Ramsey-sentence approach that a theory is a "growing existential statement". According to him, "[i]n writing the theory we commit ourselves to the existence of things" but "we don't turn our growing existential statement into a definite description" (2009, p. 168). Psillos's "Ramseyan humility" is compatible with my view that different historical descriptions of some thing are possible, and future theories may provide better descriptions of the thing.

semantics. Ramsey-sentences, according to my preferable interpretation, state that theoretical terms directly refer to some external entities and should not be reduced to logical, observational, or structural terms. The variables employed instead of theoretical terms also highlight the point that even if the entities that satisfy the definite descriptions of the theory do not exist, entities may exist that nearly realize the variables. These entities are usually better described by newer theories.

To get the full Ramsey-sentence of a theory, the specific Ramsey-sentences for all the theoretical terms of the theory are conjoined in a single sentence (see the descriptions conjoined by several conjunctions "and" in Nolan's example of a Ramsey-sentence). For this reason, if one conjunct of the full Ramsey-sentence is false, the whole Ramsey-sentence is false. Still, a valid Ramsey-sentence can apply to the truthful part of a theory. For instance, Nolan's "lessons" might be part of a larger theory whose other parts would be false. Furthermore, to form the Ramsey-sentence of the whole theory, sufficient independently-understood terms should be available. Nevertheless, to defend the view that the descriptions of a past theory about its abandoned *theoretical terms* may be truthful, it is not necessary to form the full Ramsey-sentence of *the theory*, and in turn introduce a sufficient number of independently-understood terms. It is enough to form the Ramsey-sentences of the *abandoned terms* of the theory by the using its non-abandoned terms, including its independently-understood terms. Therefore, in what follows I focus on the Ramsey-sentences of abandoned theoretical terms and not on those of a whole theory. A Ramsey-sentence always consists of conjunct sentences, each of which describes a supposed property of the referent.

To obtain the Ramsey-sentence of a theoretical term "f", first, a set of its core descriptions D(f) should be stated in a single conjoined sentence, that is, in $D_1(f) \wedge D_2(f) \wedge \ldots D_n(f)$, where $D_1(f)$, $D_2(f)$, …, $D_n(f)$ are necessary descriptions for learning the characteristics of the theoretical term.

In other words, they describe the properties that are ascribed by the theoretical term to its referent. Second, the term “f” in each description should be replaced with the variable x. The single conjoined sentence will become $D_1(x) \wedge D_2(x) \wedge \ldots D_n(x)$. Third, an existential quantifier is employed in front of the sentence. As a result, the Ramsey-sentence of the term is: “$\exists x\ (D_1(x) \wedge D_2(x) \wedge \ldots D_n(x))$”, or for short, “$\exists x\ D(x)$”, which says that there is some thing such that the descriptions D(x) are true of it. In addition to independently-understood terms, other theoretical terms of the theory may also be used in the Ramsey-sentence of “f”, by including them in D(f), in order to explain their (mathematical/causal) relations with “f”. If those other theoretical terms are considered abandoned, they may also be replaced with their Ramsey-sentences

According to the descriptive theory of reference, most abandoned terms are “non-referring” because a number of their descriptions are false. That’s why Papineau, presupposing the descriptive theory, speaks of non-referring terms. Instead, I prefer this expression: abandoned but possibly referring terms. As I explained earlier, to refer to a thing, it is not necessary that our knowledge of the thing is perfect. It is enough that *there is some thing (∃x) such that a number of its descriptions are truthful.* My use of Ramseyfication is based on this *italic* statement, which is in line with RS. The “some thing” to which the abandoned term in those truthful descriptions refers may be better described by a currently acceptable theory. Therefore, a number of the sentences in the Ramsey-sentence of an abandoned term may be truthful about the referent as *re*stated by a currently accepted theoretical term.[21] A current theoretical term and the Ramseyfied abandoned theoretical term can thus (partially and in some contexts of use) corefer to the same persistent thing in reality.

Now let me illustrate this use of Ramseyfication with the case of the ether. For (a number of) the sentences in the Ramsey-sentence of the ether hypothesis to be truthful, it is enough that there exists a real thing to which (a number of) the descriptions of the ether are applicable. So, even if the entity *fully* satisfying all core descriptions of the ether does not exist, an entity may exist that satisfies only a number of those descriptions. In this regard, "ether" is an abandoned term, and our truthful knowledge of the electromagnetic field is included in our current theories, which can be used to show that a number of the sentences in the Ramsey-sentence of the ether are truthful. More specifically, the three relevant descriptions of ether are as follows: D_1(ether): the ether is made of an elastic solid, D_2(ether): the ether is the medium for the propagation of light, and D_3(ether): Maxwell's equations hold for the ether. Accordingly, three sentences, which are equivalent to the three descriptions of the ether, are as follows:

$D_1(x)$, i.e., x is made of an elastic solid,

$D_2(x)$, i.e., x is the medium for the propagation of light,

$D_3(x)$, i.e., Maxwell's equations hold for x.

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[21] A point that should be emphasized is that the descriptions of the referents of past theoretical terms are *re*-stated from the perspective of a later theory. Hence, this account contrasts with older positivist ideas about the *reduction* of theoretical terms to observational ones.

Then, the Ramsey-sentence of the ether hypothesis is $\exists x\ (D_1(x) \land D_2(x) \land D_3(x))$, which is false because there is nothing that is made of an elastic solid *and* that is the medium for the propagation of light *and* for which Maxwell's equations hold, so $\neg\exists x\ (D_1(x) \land D_2(x) \land D_3(x))$. However, the following conjunction is still true: $\exists x$: $(D_2(x) \land D_3(x))$, because there is something that is the medium for the propagation of light *and* in which Maxwell's equations hold. This something, according to our current perspective, is better described by the electromagnetic field.[22]

Finally, I would like to underscore two points. First, a specific sentence in the Ramsey-sentence of a theoretical term may itself contain another Ramseyfiable term. For instance, "∃x such that x has mass" is one sentence in the Ramsey-sentence of the term Lorentz-electron, whose mass is nonrelativistic.[23] Based on our current knowledge, specifically according to the Dirac equation, the electron has a relativistic mass. Thus, that sentence is truthful provided that the Ramseyfied nonrelativistic mass is considered truthful (perhaps partially and in some contexts). Second, one true sentence alone, such as, "∃x such that x has mass", is practically empty, since it applies to many things. Therefore, in order to provide a substantive claim, e.g., about the truthful content of Lorentz-electron, at least some other property must be specified beside having mass. To be significant, the conjunction of a number of sentences should be truthful. The conjunction of those

[22] Other historical examples can be similarly discussed. For instance, although Putnam's causal theory of reference is problematic, his (1978, p. 24) discussion of Bohr-Rutherford descriptions of an electron can be nicely illustrated by means of Ramseyfication.

[23] At least in its earlier version (see Radder 2012[1984/1988], p. 84).

sentences that are true according to our current knowledge is indeed the truthful content of the past theory's term.

6.7 The Case of Ptolemaic Astronomy

Some realists, such as Hardin and Rosenberg (1982, p. 610), claim that scientific realism can be applied only to "mature science". Therefore, realists need not be concerned about immature theories like Ptolemaic astronomy. However, these realists do not take into account that a basic aspect of scientific realism is the intuition that successful theories are truthful. Thus, the novel successes of false immature theories are as miraculous as (or even more amazing than) those of false mature ones. Accordingly, the successes of immature sciences, insofar as they are achieved not haphazardly but as a result of systematic theories, should be explained by current theories (according to RS). Apart from this, Christián Carman and José Díez (2015) argue on the basis of the novel predictions of the Ptolemaic model that "any non-question-begging notion of maturity qualifies Ptolemy's astronomy as mature science" (2015, p. 32; see also Wray 2018, pp. 177-181). I agree that Ptolemaic astronomy enjoys the characteristics of mature sciences, and thus, the Ptolemaic model, as a mature science, deserves a philosophical interpretation of its successes. My contention is that the successes of this model can be restated from the later perspective of Kepler's model. Therefore, nothing miraculous happens. An implication of this analysis is that an antirealist intepretation of the Ptolemaic model, e.g. that of Brad Wray (see 2018, p. 13 and section 11), is questionable.

In what follows, I account for the particular success of the Ptolemaic model regarding the prediction of the position of the (then known) outer planets, that is, Mars, Jupiter, and Saturn. I first introduce the model; then, I restate its successful explanations in terms of Kepler's laws of planetary motion (as a currently acceptable theory, or taken to be a theory that itself can similarly be restated in terms of current theories). I also argue that the descriptions of Ptolemaic astronomy about epicycles are truthful, even if some descriptions of epicycles are false.

According to the Ptolemaic astronomy, all celestial objects (including the stars, moon, sun, and planets) orbit the earth, which is located at the center of the universe. Figure 6.1 shows a planet on its *epicycle* (the smaller circle) orbiting around point C, which itself moves on the *deferent* (the larger circle). The center of the deferent is the *eccentric point* (X), and the earth is located at the center of the universe (at point O). Another concept of the Ptolemaic astronomy is the *equant* (the point E in the figure), whose distance from point X is equal to the distance between the earth and the eccentric point (EX = XO). If an observer would be located at an equant point, the center of the planet's epicycle (point C) appears to move at a uniform speed, while other viewers (including ones located on the earth) observe point C moving at a non-uniform speed. By using the equant point, Ptolemy tried to keep the motion of planets both uniform and circular.

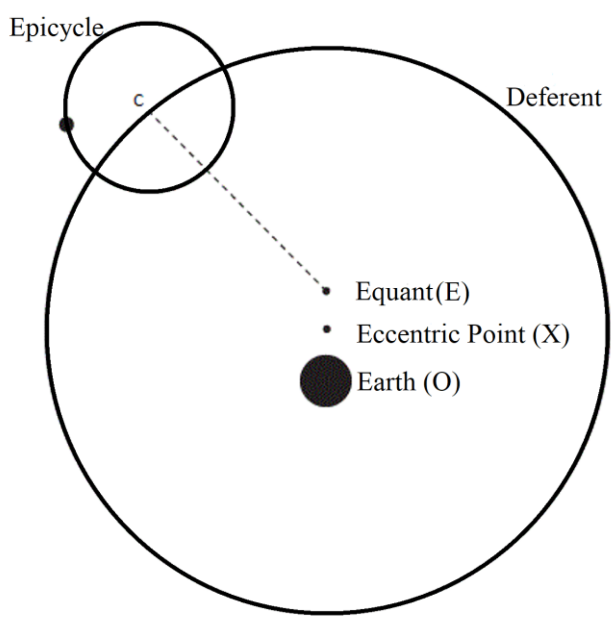

Figure 6.1: The Ptolemaic Model

Now, my aim is to restate the Ptolemaic description of the movement of the outer planets with the help of Kepler's laws of planetary motion, as a currently acceptable theory. According to these laws, the earth and other planets move around the sun, their orbits are ellipses with the sun at one of the two foci. In the following figure, vector **r** indicates the earth's position from the sun, and vector **R** indicates the position of an outer planet. The observer on the earth sees this planet in the direction of vector **-r+R**. Because the eccentricity of the orbit of the earth is very small, i.e., 0.0167, the orbit of the earth can be regarded as a circle rather than an ellipse (see figure 6.2).

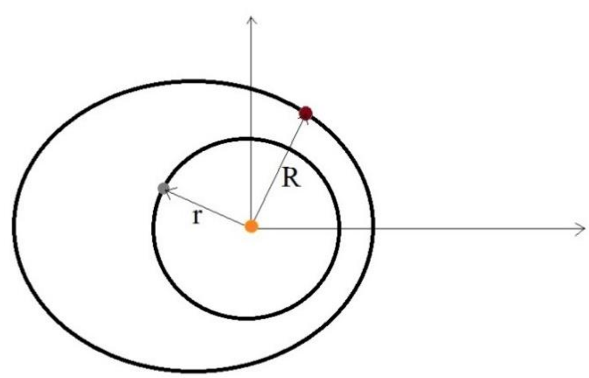

Figure 6.2: Kepler's Model

The reference point in figure 6.2 is the sun. This point can be changed to the position of the earth (figure 6.3). We know that there is indeed no preferred reference point, but by choosing the position of the earth as the reference point, we take a step toward the Ptolemaic perspective that the earth is located at the center of the universe. On the basis of figure 6.3, the sun moves around the earth through the (small) circle, while the outer planet moves around the sun through the (large) ellipse. Accordingly, the observations in figure 6.3 are the same as those on the basis of figure 6.2. The observer still sees the planet in the direction of vector **–r+R**.

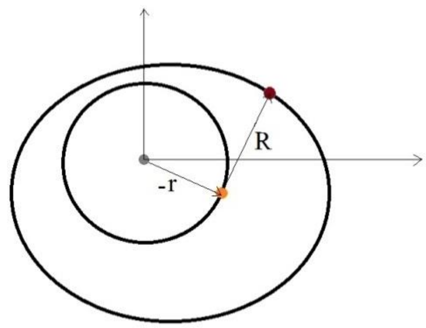

Figure 6.3

Now, in figure 6.3 the order of **r** and **R** can be changed to **R**+(–**r**). Because of the commutativity of vector addition, from the same reference point (i.e., the position of the earth) both –**r**+**R** and **R**+(–**r**) arrive at the same point (i.e., the position of the planet). Thus, by this transformation what is observed will not change, but it allows us to take another step toward the Ptolemaic perspective. In figure 6.4, the (small) circle is similar to the epicycle of the planet, and the (large) ellipse is similar to its deferent.

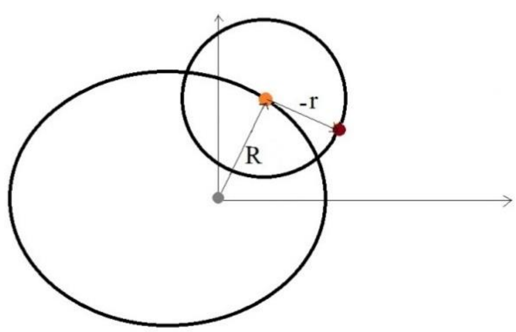

Figure 6.4

Now it can be demonstrated that if we approximate the elliptical orbit of the planet to the first order of its eccentricity, the equation of the planet in an elliptical orbit (whose semi-major axis is "a"; whose eccentricity is "e"; and with one of the foci at the origin of the coordinate system) is equal to the equation of a circle. Indeed, the equation of an ellipse in the polar coordinate system is $R = \frac{a(1-e^2)}{1+eCos(\theta)}$. When e is small, one can neglect e^2 and conclude that $\frac{1}{1-eCos(\theta)}$ is approximately equal to $1 + eCos(\theta)$. Accordingly, $R = a - aeCos(\theta)$, which is approximately equal to the equation of a circle whose radius is "a" and whose center is shifted a distance equal to "ae" from the origin of the coordinate system (see the distance XO in figure 6.5).

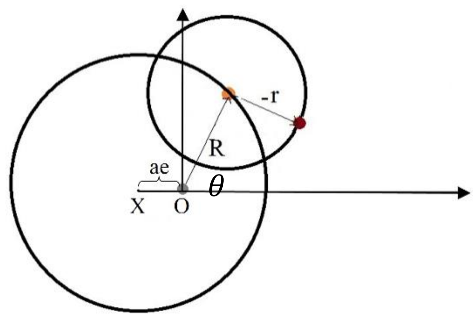

Figure 6.5

By comparing figures 6.5 and 6.1, we can conclude that the value of "a" (the semi-major axis of the earth's elliptical orbit) is equal to the value of the radius of the deferent in the Ptolemaic model. Furthermore, the value of "ae" is equal to the distance between the eccentric point in and the earth in the Ptolemaic model (XO). The small circle is the epicycle of the planet and the large one is its deferent. Hence, to a first-order approximation, Kepler's model equip us to restate the Ptolemaic description of the position of the outer planets.

Ramseyfication can further clarify how the concept of, e.g., an epicycle contributes to the successful explanations and predictions regarding the outer planets. Let me write the Ramsey-sentence of the epicycle of an outer planet. The sentence consists of a set of the descriptions of the epicycle.

> There is an x such that x is a circle, whose radius is equal to "r" ∧ the center of x moves around the circle "y", whose radius is equal to "R" ∧ the outer planet moves in the circumference of x ∧ the observation of the planet from the earth

depends on x and y ∧ the planet is observed according to the vector **R**–**r** (figure 6.5) ∧ other relevant sentences.

In this Ramsey-sentence, y is used instead of the deferent of the planet because that is another abandoned term which should also be Ramseyfied. Even though the single Ramsey-sentence of the epicycle is false, the following sentence can be nearly true,

> x is a circle, whose radius is "r" ∧ the observation of the planet from the earth depends on x and y ∧ the planet is observed according to the vector **R**–**r** (figure 6.5).

If the actual orbit of the earth is considered to be x and that of the outer planet is considered as y,

> the actual orbit of the earth is a circle, whose radius is "r" ∧ the observation of the planet from the earth depends on the actual orbit of the earth and the actual orbit of the outer planet ∧ the outer planet is observed according to the vector **R**–**r** (figure 6.2).

This sentence is nearly true as my earlier analysis demonstrated. I do not think that Ptolemaic astronomers intended to state this sentence, which is a *re*statement of, and not the exact expression of, a part of their beliefs. However, from our perspective, because they stated something like this sentence, their explanations and predictions regarding the outer planet were to some extent successful. It is also important to emphasize that I have only provided an explanation of why the Ptolemaic model regarding the path of the outer planets is successful. Thus, I have not "reduced" the whole Ptolemaic model to Kepler's model. I do not even claim that my treatment of this specific case can be exactly applied to other successes of Ptolemaic model. They should be examined case by case.

With the aid of a currently acceptable theory, I have explained why Ptolemaic astronomy, in the case of the path of the outer planets, had explanatory and predictive power, so that its success was not miraculous. This confirms RS: the success of a past theory can be accounted for by restating the truthful content of the theory.

The case study of Ptolemaic astronomy shows how RS can be applied. This restatement requires mathematical descriptions. But RS can also be applied to non-mathematical cases of past successes. Shahabi et al. (2008) employ an experimental approach in order to restate the successful explanations of humoral medicine in terms of hot and cold natures by using concepts from modern medicine, such as the sympathetic nervous system, its adrenal sympathetic, adrenal corticosteroid and parasympathetic activities, and its deviation from the immune system. Their work helps to understand why humoral medicine was (and still is) successful to a certain extent. For another case, Changizi Ashtiyani et al. (2012) explain why the successful "findings of Rhazes about treatments of gout were consonant with modern medical theories".[24] Showing such a consonance is compatible with RS, although there is no mathematical structure in Rhazes's or modern medicine.

[24] Rhazes is the Latinized name for Muhammad Zakariyya Razi, a Persian physician, (al)chemist, and an important figure in the history of medicine and pharmacology. He lived in the ninth and tenth centuries AD.

Chapter 7 Realist Perspectivism and Special Relativity Theory[1]

7.1 Introduction

In the previous chapters, I have engaged with several accounts, such as the ontology of potentialities, constructivism, perspectivism, entity realism, phenomenological-hermeneutical views and the Ramsey-sentence approach, in order to provide a comprehensive account of science and reality. In this way, I have adopted a *constructive* procedure so as to incorporate other thinkers' valuable observations in my arguments. The belief that different philosophical views in the scientific realism debate are in many cases compatible and complementary has been a major characteristic of the entire dissertation.

The case of Higgs bosons and hypothetical F-particles and that of the Ptolemaic and Kepler model have been studied, respectively, in sections 2.4 and 6.7 to support the specific claims of chapters 2 and 6. The current chapter aims to apply realist perspectivism, as the general view of the dissertation, to the case of special relativity theory. This new case clarifies how the central concepts of the dissertation, including potentialities, robustness, and perspectives, relate to science. In this way, several themes of the dissertation are employed and summarized in this case study.

[1] This chapter is a modified version of an article published in the *Theory of Science* journal (Publisher: Centre for Science, Technology and Society Studies, Institute of Philosophy of the Czech Academy of Sciences).

This chapter also bridges the gap between perspectivism and special relativity theory. Although reference frames are genuine examples of perspectives, in the context of the scientific realism debate a well-developed perspectival reading of the theory of (special) relativity is still lacking. The theory of relativity is mentioned, but only in passing, by Massimi (2019, p. 8), by Berghofer (2020, pp. 12–13) and by Evans (2020, pp. 19–20). Nonetheless, considering "reference frames" as perspectives is not an unprecedented idea. For instance, Bas van Fraassen (2008, pp. 69–72) does not object to using "perspective" in describing events in reference frames.[2] Jenann Ismael (2015) also uses reference frames to refine and develop Huw Price's (2007) perspectivist account of causation. In addition, in the context of the metaphysics of spacetime, perspectival readings of the special theory of relativity have recently been suggested. I will address them in subsection 7.2.3. My discussion concerns the scientific realism debate. I present a perspectival reading of frame-dependent and frame-independent properties in special relativity theory. The frame-dependent properties that I address are length, time duration, and simultaneity, while the frame-

[2] Van Fraassen advises "caution against [loose] talk of 'perspective' when discussing coordinates and frames of reference" (2008, p. 69). However, he concludes his discussion thus: "the use of 'perspective' and 'perspectival' in connection with depictions of events in varying frames of reference cannot be banished completely." Van Fraassen agrees with Hermann Weyl who "refers to coordinate systems as 'the unavoidable residuum of the ego's annihilation'" (2008, p. 71; here Van Fraassen refers to a discussion of Weyl in Ryckman 2005, p. 134). Weyl's interpretation of reference frames as a 'residuum of the ego' is too subjective, however. It goes against the perspectival *realist* understanding of reference frames I will support in this chapter.

independent property that will be discussed is the constancy of the speed of light. According to the special theory of relativity, the constancy of this speed is a property of light. My focus is on the frame-independence of the speed of light, but the frame-independence of the laws of physics may also be discussed in a similar vein.[3]

The previous chapters presupposed the interpretation of perspectivity as conditionality, but this chapter explicitly develops this interpretation. I argue that it does not make sense to say that our knowledge of frame-dependent properties in (special) relativity theory is “partial”. On the other hand, they are obviously “conditional” on the choice of a reference frame. Furthermore, in the case of experimental methods, whose partiality consists in a kind of qualification that defines a perspective, “partiality” reduces to the more inclusive concept of “conditionality”. Moreover, experimental results are bounded by conditions, but it is confusing to call them “partially true”, as they are completely true in the qualified condition in which the results are obtained. All these considerations lead me to conceive perspectivity as conditionality and not as partiality.

More specifically, the chapter explains that the experimental measurement of the speed of light and the measurement of a frame-dependent property such as the length of an object are both perspectival in this general sense: *their measurements are conditional on specific instruments*. This general sense entails a particular meaning in different cases. First, in the measurement of the speed of light it means that our knowledge of the constancy of this speed is conditional on the validity scope of the results of certain instruments. Second, in the measurement of a frame-dependent

[3] On frame-dependent (or “relative”) and frame-independent (or “absolute”) properties in special relativity theory, see Kosso (1998, chapter 3).

property, such as the length of an object, the result directly depends on the frame of the measuring instrument.

Furthermore, frame-independent and frame-dependent properties both relate to the concept of robustness. From the perspective of special relativity theory, the constancy of the speed of light is a robust property of light because it is measurable by means of different instruments and techniques. A frame-dependent property such as the length of an object is also a "robust" property in this (obvious) sense that "having a length", apart from the specific quantity of the length, can be observed from different reference frames. The account of reality in terms of potentialities makes it clear that objects possess the persistent potentiality to manifest differently realized lengths in different reference frames.

Section 7.2 argues that inertial reference frames can be considered as perspectives provided that "perspectival" implies "being conditional" rather than "being partial". The term "conditionality" is preferable to "partiality" and, moreover, is essential to a perspectival account of truth. I also explain that frame-dependent properties are not mere appearances; thus, statements about them can be true. Section 7.3 addresses the constancy of the speed of light: this constancy is claimed to be conditional on factors such as the idealized definition of the speed of light and the procedure of Einstein synchronization. While reference to these factors is not enough to provide convincing arguments for perspectivism, I argue that the constancy of this speed, as a robust property of light, is conditional on certain experimental setups, and this conditionality can support perspectivism. Section 7.4 summarizes and discusses the motivations for a perspectival reading of special relativity theory. I conclude that reference frames are genuine cases of perspectives and the role of reference frames in the (special) theory of relativity helps to clarify how perspectivism in experimental science works. The validity of our knowledge of the constancy of light speeds is conditional on certain experimental conditions in a similar way as the validity of our knowledge of frame-dependent properties is conditional on the choice of reference frames. Although the chapter concentrates on special relativity theory, some comments about the general theory of relativity are made in section 7.5.

7.2 Frame-Dependent Properties

7.2.1 The basic principles of special relativity theory

To articulate the special theory of relativity, I follow Albert Einstein's (1905) "principle" approach. I should first explain that Einstein (1919) makes a distinction between two kinds of theories. 1- principle theories, such as thermodynamics, which are formed when one raises empirical regularities about certain phenomena to the level of principles; 2- constructive theories, such as the kinetic theory of gases, which explain certain phenomena by building up a picture of the reality underpinning the phenomena. Special relativity theory, according to Einstein, is a case of the former class of theories.[4] The two basic principles of the special theory of relativity are: 1-

[4] Thus, a constructive theory is needed if we want to explain the underlying grounds of special relativity theory. Two "constructive" approaches to the (special) theory of relativity have been proposed: the geometrical approach and the dynamical approach. According the former, the symmetry of Minkowski spacetime, which constitutes the constructive grounds of the theory, explains the Lorentz invariance of the dynamical laws. Lorentz transformations ultimately depend on the Minkowski geometry of spacetime, and accordingly, the geometric structure of spacetime accounts for special-relativistic phenomena such as time dilation and length contraction (see

The principle of relativity: the laws of physics are invariant in all inertial frames of reference (i.e. in unaccelerated reference frames).[5] 2- The principle of the constancy of the speed of light: the speed of light in vacuum is constant and independent of the motion of the light source.[6]

Lorentz transformations transform the coordinates (x, y, z, t) of a point or an event from one reference frame to another, in such a way that the constancy of the speed of light is preserved and the laws of physics remain invariant. These transformations have certain counterintuitive implications, such as the relativity of simultaneity, length contraction, and time dilation. While

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Janssen 2009; Maudlin forthcoming; see also Friedman 1983 for a classic defense of the geometrical approach). According to the dynamical approach, the Lorentz invariance of the dynamical laws provides the underpinning reality, and thus it governs Minkowski spacetime. Time dilation and length contraction are at root due to Lorentz-invariant dynamics. Proponents of this approach "consider absolute space-time structure as a codification of certain key aspects of the [dynamical] behaviour of particles (and/or fields)" (Brown 2005, p. 25; see also Brown and Read forthcoming). In this chapter I am neutral about the geometrical versus dynamical approach. It can be argued elsewhere that even if these approaches do not necessitate perspectivism, both *are* compatible with the perspectival understanding of special relativity theory.

[5] According to Einstein, "the same laws of electrodynamics and optics will be valid for all frames of reference for which the equations of mechanics hold good." (1952[1905], pp. 37–38)

[6] According to Einstein, "light is always propagated in empty space with a definite velocity c which is independent of the state of motion of the emitting body." (1952[1905], p. 38)

according to classical physics and common sense knowledge the values of position and speed are dependent on the reference frame of the observer, in special relativity theory this holds true as well for properties such as simultaneity, length, and time duration.

For instance, when $event_1$ and $event_2$ are not causally connected, it *may* be that in $frame_1$, $event_1$ occurs before $event_2$; in $frame_2$, $event_1$ occurs after $event_2$; and in $frame_3$, $event_1$ and $event_2$ occur simultaneously. For causally connected events, when $event_1$ occurs before $event_2$ in $frame_1$, this ordering cannot be changed in other reference frames, because in the physical world, without particles that travel faster than light (i.e., superluminal tachyons), it is impossible to change the ordering of events that lie within the light cones of each other.[7] Accordingly, simultaneity is *conditional on* the choice of reference frame. Or rather, this entails that the *classical* concept of simultaneity loses its meaning in special relativity theory.

7.2.2 Reference frames as perspectives

The concept of a "reference frame" is central to (special) relativity theory. A reference frame is defined as a state in space, time, and motion from which an observation/measurement is made. Indeed, Einstein's main breakthrough in 1905 was to make explicit that a physical observation depends not only on the space- and time-state of the observer (which is trivially true), but also on the observer's relative speed with respect to the observed object. That is, in the definition of the reference frame, its motion should always be considered. Accordingly, a reference frame is

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[7] I would like to thank one of the reviewers for *Theory of Science* for prompting me to be more precise on this subject.

described as an (ideal) observational state of position, time, and motion.[8] The state of observation is either inertial or undergoing acceleration. Special relativity theory exclusively discusses inertial reference frames. My question is whether an inertial reference frame is an example of a "perspective" in its philosophical sense. To answer the question, I focus on Giere's conception of perspectives. He starts his argument concerning perspectivism by asserting that human vision is perspectival, in the sense of being *partial*.

> For my purposes, maybe the most important feature of perspectives is that they are always partial. When looking out at a scene, a typical human trichromat is visually affected by only a narrow range of all the electromagnetic radiation available. (2006a, p. 35)

Then he extends his argument, asserting that scientific observations are also *partial*: that is, each observational instrument or detector responds to a specific feature of reality. For instance, a radio telescope receives only radio waves, and a gamma-ray telescope is sensitive only to gamma rays. Giere then proceeds still further and argues that scientific models are also *partial*. Only some features of a phenomenon are represented in a model, and others are ignored or eliminated. In this regard, models are similar to maps. "Maps are *partial*. Only some features of the territory in question are represented" (2006a, p. 73; see also pp. 76–78).

The other meaning of "perspectival" for Giere is dependence on a *condition*.

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[8] Valuable comments by both of the reviewers for *Theory of Science* helped me to define reference frames with greater precision.

> For a *perspectival* realist, the strongest claims a scientist can legitimately make are of a qualified, *conditional* form: "According to this highly confirmed theory (or reliable instruments), the world seems to be roughly such and such." There is no way legitimately to take the further objectivist step and declare *unconditionally*: "This theory (or instrument) provides us with a complete and literally correct picture of the world itself." (Giere 2006a, pp. 5–6, emphases added)

According to this and the previous quotation, for Giere "perspectival" implies both "being partial" and "being conditional". Let us suppose that the notion of being partial is directly related to that of being conditional. That is, instruments form "perspectives", from which reality is represented "partially". Accordingly, instruments constitute perspectives in either of these two equivalent senses: our observational knowledge is "conditional" on specific instruments, *or* because of their specificity instruments "partially" represent reality. However, in what follows I argue that the supposition that partiality is always linked to conditionality is problematic and the description of perspectivity as conditionality is preferable.

To begin with, I do not use the term "partiality" to describe the perspectivity of reference frames. The measurements of frame-dependent properties are conditional on reference frames, but it is incorrect to say that frame-dependent properties are the "partial" representations of an object. In other words, one can naturally say: *on the condition that* one observes from frame_1, the length of a rod is L_1; *on the condition that* one observes from frame_2, the length of the rod is L_2, and so on. Moreover, no observation/measurement of its length can be made *unconditionally*, from nowhere. Therefore, if being perspectival means being conditional, a reference frame is a perspective. However, it is quite implausible, or indeed even meaningless to say, that L_1 and L_2

represent the rod “partially”, or that each represents an “aspect” or a “part” of the rod. If an observer measures the length of an object from another frame, a further aspect of the object is not measured. The same aspect (length) is measured from a different frame.

Moreover, conditionality (rather than partiality) of scientific knowledge is essential to the perspectival account of *truth*. “For a perspectivist, truth claims are always relative to a perspective” (Giere 2006a, p. 81). I understand the term “relative to” in Giere’s statement as “conditional on”. A truth claim in science is conditional on the instrumental or theoretical perspectives from which the claim is made. Regarding special relativity, frame-dependent properties such as length, time duration, and simultaneity are measured from the perspective of specific reference frames. Those measurements are *completely* true given the perspective from which the measurement is made. It is not the case that the descriptions of the frame-dependent properties are partially true in the perspective. Nor are those properties measurable unconditionally, from nowhere.

In the case of different experimental measurements, I agree that partiality is a feature of perspectives. When we see an astronomical object through different telescopes, each of them provides us with an aspect of the object. These aspects augment our understanding of the object (see also my discussion of profiles in subsection 5.3.2 and section 5.6). That said, I prefer to describe the perspectivity of instruments as conditionality. In the first place, the use of “partial” refers to a type of constraint that defines a perspective, and when put like this, it is a subspecies of a perspective being “conditional”. That is, possible concerns about “partiality” can collapse into the more inclusive notion of perspectives as being “conditional”. What’s more, “partial truth” is a misleading term to explain the truthfulness of observations through instruments. For instance, when one appropriately observes an astronomical object through a gamma-ray telescope, what is observed is completely[9] true *given* the perspective of the gamma-ray telescope. It is not the case that what is observed is partially true "*from nowhere"* because, according to perspectivism, we have no (complete or partial) access to "nowhere" (or to "reality from *no perspective whatsoever"*). There is a substantial difference between "partial truth from nowhere" and "truth according to conditions". The term "partial truth" may mean the former, which is unavailable (if not nonsense). For this reason, I prefer not to use the term "partial truth" and "partiality". The expression "truth according to conditions" is clearly preferable. It implies that a statement may be true according to the bounded conditions of a specific perspective.

It is also questionable that what is observed (by the gamma telescope) is considered as "partially true" because of the fact that some aspect of the observational results (say, the relative distances of objects) can also be observed by other instruments (such as a radio telescope, an X-ray telescope). "Robustness" (rather than "partiality") is the suitable concept to explain the agreement of observations/measurements made by independent instruments.

In sum, not only is "conditionality" more inclusive than "partiality", it is also essential to a perspectival account of truth. In particular, the truth of the measurements of a frame-dependent property is conditional on the choice of a frame. In the following I argue that frame-dependent properties are not mere appearances, so the statements about them can be true.

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[9] "Completely" here is in contrast with "partially" rather than "approximately" (meaning imprecise). In making astronomical or microscopic observations, data analysts may employ statistical approximations or approximate computations.

7.2.3 Frame-dependent properties are not mere appearances

Mere appearances are unreal. Thus, statements about them may encounter the same problems as apply to sentences about so-called nonexistent objects, such as Pegasus, the present King of France, and the round square. For instance, it is a matter of philosophical debate whether the sentence “Pegasus is a flying horse” is true, false, or truthless, or how the sentence “Pegasus does not exist” can be true when its subject is non-referring (on nonexistent objects, see Reicher 2019). The view that frame-dependent properties are mere appearances, illusory, or merely subjective has the same consequences, such that all statements about frame-dependent properties, even those about the length of ordinary objects, the time duration of everyday events, or even the position and speed of ordinary objects, as described by the special theory of relativity, face the same problems as encountered by sentences about nonexistent objects.

However, it is certainly correct to say that statements about the position, speed, and length of ordinary objects or the time of ordinary events are either true or false. Ordinary objects and events exist, and therefore statements about them should not face the same problems as sentences about nonexistent objects. In general, it seems correct to say that statements about frame-dependent properties are not of the same kind as statements about nonexistent objects. Perspectivism supports this intuition satisfactorily, by explaining that and why frame-dependent properties *are real from the perspective of* a specific reference frame. Therefore, statements about frame-dependent properties *are either true or false given that perspective*. Furthermore, according to special relativity theory, a well-formed statement about frame-dependent properties should include the reference frame from which the statement is made. Without specifying the frame, the statement is still incomplete, not yet constituted, and it is therefore impossible to determine whether it is true or false. Once the frame has been specified and the statement is properly constituted, it is either true or false. This can be explained by perspectivism, according to which scientific claims are always made from perspectives, or given certain conditions. They cannot be unconditionally true, as knowledge from "nowhere" is unattainable.

In this regard, Martin Lipman (2020) has recently suggested a perspectival ontology for special relativity that underscores the reality of frame-dependent properties. Based on Kit Fine's (2005) fragmentalism, according to which reality is ontologically perspectival, Lipman argues that the instantiation of each frame-dependent property is a real fact in a "fragment" of reality. The difference between a "fragment" and Giere's "part" is that the former is an ontological term, while the latter is employed in an epistemological doctrine. According to fragmentalism, the accounts from all reference frames about frame-dependent properties are equally true because these accounts refer to real but different fragments of the world. As a result, frame-dependent properties are all real and not mere appearances; thus, statements about these real properties can be truthful.

One might not agree with *ontological* perspectivism and still support the view that different accounts from different frames are equally true, as Matias Slavov (2020) does. He employs perspectival realism to argue that what makes statements about the present time true is the choice of a reference frame, as a perspective. The problem is that, according to theory of relativity, there is no frame-independent present moment; therefore, truthmaking in statements about the present time should depend on the choice of the perspective from which events are represented. To solve the problem Slavov assumes that events are frame-independent, but representations of them constitute perspectival phenomena. Perspectival phenomena, then, are neither mere appearances nor frame-independent facts. They are representations of frame-independent events. Therefore, frame-dependent properties are not mere appearances, and statements about them can be true. Because of his assumption of the frame-independence of events, Slavov's view need not buy into ontological pluralism; thus, his proposed perspectivism is metaphysically more parsimonious than Fine's and Lipman's fragmentalism. Slavov only presupposes the existence of frame-independent events and not of the whole frame-independent fragments of reality (see Slavov 2020, section 3 for his criticism of fragmentalism).

When a rod is accelerated from one constant speed to another, the frame that co-moves with the rod in its initial state and the other frame that co-moves with the rode in its final state present two different accounts of the length of the rod. According to perspectivism, the accounts of the two frames are equally true. Lipman relates the two accounts to existing but different facts in reality. Slavov interprets them as two representations of one real event. Both views are perspectival. Yet, it seems to me that the latter view is preferable, because it is more metaphysically parsimonious than the former. Moreover, fragmentalism seems to presuppose the *actual* instantiation of each frame-dependent property in a fragment of reality, which is not in harmony with my account of reality as consisting of potentialities in chapter 2. As a result of that account, frame-dependent properties are potential, and only after they are measured by a measuring instrument in a reference frames, they become actual.

So far, I have argued that reference frames are perspectives, and that frame-dependent properties are perspectival. I will argue in the next section that the constancy of the speed of light can also be interpreted perspectivally ("perspectival" in the sense of being conditional).

7.3 The Speed of Light

Conceptualizing reference frames as perspectives might seem problematic because, according to the principles of special relativity theory, the laws of physics and the speed of light in vacuum are not dependent on the reference frame of the observer. They are frame-independent.

However, a perspectivist can argue that the two principles contain perspectival factors that contribute to constituting the theory. On the *condition* that those factors are assumed, the physical world is the way that the theory of special relativity describes it. Thus, when an observer measures a frame-dependent property such as length, two kinds of perspectives are employed. First, the world is seen from the theoretical perspective that the two principles of the theory of special relativity (containing perspectival factors) provide. Second, the observational perspective of that frame of reference is at work. Accordingly, the frame-independence of these principles should not be understood as being *independent of all* perspectives (or *from no* perspective whatsoever); rather, they are independent only of the perspectives understood in terms of reference frames.

One might claim that something is independent of all perspectives when it is conditional on any perspective whatsoever; for example, regardless of which perspective one takes, one will get the constant speed c for light. This claim, however, is of little help, because human beings have access only to historically available instrumental and theoretical perspectives, and not to “any perspective whatsoever”. Accordingly, inasmuch as we are bounded human beings, we can never determine whether something is conditional on *any* perspective whatsoever. There may always be some theoretical or instrumental conditions that have not been taken into account. In this sense, therefore, it makes little sense to claim that something is perspective-independent. Moreover, regarding the example of the speed of light, whose constancy plays a central role in the framework of special relativity theory, we already know that the speed of light is not constant in “any perspective whatsoever”. In general relativity theory, as I will briefly discuss in section 7.5, the speed of light is not the constant c. In the presence of gravity, i.e., in curved spacetime, measurement demonstrates that the speed of light varies. As a result, the speed of light is not *universally* (or: from any perspective whatsoever) constant.

Let us now examine possible perspectival factors in the framework of special relativity theory. One of these perspectival factors is the idealized concept of a perfect vacuum, by which the speed of light is defined and understood. According to Giere, the speed of light is defined in an idealized form, in a perfect vacuum – a condition unavailable in our messy world. He argues that

> the definition [of the speed of light] is theoretical, the speed of light in a perfect vacuum. It is a constant in an idealized model. Our best theories tell us that there are no perfect vacuums to be found anywhere in the universe. So-called empty space is full of all kinds of "space dust." If it were not, we could see a lot further with optical telescopes than we can in fact see. (Giere 2006a, p. 92)

This quotation raises two questions. 1- Can one infer from the theoretical definition of the speed of light that the principle of the constancy of the speed of light is merely a theoretical perspective? 2- Does "idealization" unavoidably contribute to the measurement of (the constancy of) the speed of light? My answers to both questions are negative.

First, different earth-based or astronomical setups have thus far measured the speed of light (and of other electromagnetic waves). These experimental measurements demonstrate that the constancy of its speed is a "robust" property of light. Accordingly, the principle of the constancy of the speed of light is an empirical fact, which is described by theoretical concepts but which is not merely a theoretical perspective.

Second, the idea of "a perfect vacuum", as an "idealized" concept, is rarely used in contemporary physics. If a given measurement requires it, the appropriate physical *corrections* are routinely made. That is, the interaction of light with atoms can be calculated based on the known physical laws of optics, atomic theory, and fundamental quantum theory. Modern physical theories can take into account the impact of interstellar magnetic fields, particles and plasma on the propagation of light through a very sparse medium, thereby *correcting* the vacuum speed of light (as well as other parameters such as amplitude, polarization and dispersion). Even in a "pure vacuum state", when the light propagates through space without any ordinary matter, quantum field theory can calculate very subtle corrections arising from the mutual interaction of photons with virtual particles such as positrons and other photons. Accordingly, several appropriate physical corrections are routinely made to correct, and thus avoid, idealizations. Experimental progress (partly) depends on these corrections.

Nevertheless, a perspectivist can argue that the accepted theoretical models used for such corrections are themselves conditional on certain theoretical assumptions and experimental results, none of which is ultimate or perspective-free. I agree with this new argument. But it does not rely on idealization anymore because, at least in the case of the measurement of the speed of light, the distorting assumptions can mostly be de-idealized and removed. This new argument, instead, highlights the bounded validity of theoretical assumptions and experimental results. Regarding the speed of light, the speeds measured are always *conditional on* the validity scope of certain experimental setups, and this conditionality supports perspectivism. It is always possible that in a domain unexamined so far, it may turn out that the speed of light is not constant, as we actually know that in the presence of gravity light speeds vary (see section 7.5). Accordingly, the constancy of light speeds should not be read as being objective$_1$: it should not be understood from an unqualified, unconditional viewpoint (see subsection 3.4.2). In general, future investigations may show the bounded validity of currently accepted findings.

Let me now consider another possible perspectival motivation. It bears upon the fact that what is measured in experiments is the two-way speed of light from the emitter to the detector and back again. In order to measure the one-way speed of light the *Einstein synchronization convention*, according to which the speed of light is equal in different directions, must be employed. Therefore, an a priori convention is still needed to measure the (one-way) speed of light. Einstein established this convention in his 1905 paper on special relativity thus:

> We have so far defined only an "A time" and a "B time". We have not defined a common "time" for A and B, for the latter cannot be defined at all unless we establish *by definition* that the "time" required by light to travel from A to B equals the "time" it requires to travel from B to A. (Einstein 1952[1905], p. 40, emphasis in original)

If two clocks are used to measure the one-way speed of light (one clock at the starting point, where the emitter is, and the other where the detector is), the two clocks must be synchronized with each other. The synchronization of the clocks is equivalent to the Einstein synchronization convention. Wesley Salmon (1977) argues that a conventional factor is always involved in the experiments that have historically been performed for the measurement of the speed of light.[10]

One should note that the conventionality thesis relies on a traditional interpretation of special relativity theory. In addition to Albert Einstein (1952[1905]), Hans Reichenbach (1958) and Adolf Grünbaum (1973) argued that the standard synchronization procedure has a conventional status. However, David Malament (1977) has cast doubt on this claim, arguing that the Einstein convention is the only definable simultaneity relation. Malament's objection is in turn criticized

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[10] Comparatively recently, Greaves, Rodríguezb, and Ruiz-Camacho (2009) have claimed that the one-way speed of light is experimentally measurable, but Finkelstein's (2010) comment shows that they actually have measured the two-way speed of light.

by Sahotra Sarkar and John Stachel (1999) as well as by Adolf Grünbaum (2010). The criticism of Sarkar and Stachel is in turn questioned by Robert Rynasiewicz (2001), who himself vigorously defends the conventionality thesis (Rynasiewicz 2012). In this regard, Allen Janis (2018) maintains that "[t]he debate about conventionality of simultaneity seems far from settled". I cannot review this debate in detail here, because it would detract too much from the main subject of this chapter. It is worth mentioning, though, that (future) arguments in favor of the conventionality thesis would support the perspectival reading of the speed of light.

7.4 Motivations for the Perspectivism of Special Relativity

Thus far, I have supported two motivations for a perspectival interpretation of special relativity theory. The first motivation concerns frame-dependent properties and is a motivation specifically applying to perspectivism in special relativity theory. The second one concerns the speed of light and relies on a general motivation for perspectivism in science, including the special theory of relativity.

> M1: Frame-dependent properties such as length, time duration, and simultaneity are conditional on the reference frame of the observer.

> M2: The robustness of the constancy of light speeds is conditional on certain experimental setups and their theoretical interpretation, which are valid only within the scope of special relativity theory.

Critics might question the connection between the perspectivism of frame-dependent properties and that of the speed of light. They might even object that subsuming the relativity to reference frames and the dependence on experimental setups under a single notion produces a heterogeneous brand of "perspectivism". I agree that the role reference frames play in measuring frame-dependent properties is different from the role of experimental setups in measuring the speed of light. Be that as it may, M1 and M2 are the same in expressing the *conditionality of scientific knowledge*, which realist perspectivism emphasizes. M2 is similar to M1 with regard to a general feature: both are similar in expressing the *conditionality* of scientific knowledge within the special theory of relativity. My point is that highlighting this similarity is useful because what we know about the way reference frames work in special relativity theory can be used to clarify how perspectivism in experimental science should work. After all, the validity of our knowledge concerning the constancy of the speed of light is conditional on certain experimental conditions in a basically similar way as the validity of our knowledge of frame-dependent properties is conditional on the choice of frame. The validity scope of each of these two kinds of knowledge is *qualified*.

M1 and M2 enjoy different degrees of cogency and plausibility. M1 is the most evident reason for a perspectival reading of special relativity theory. Even a staunch critic of perspectivism cannot deny that the length of an object and the time duration of an event in special relativity theory depend on the choice of reference frame. Therefore, the perspectival character of frame-dependent properties suffices to accept that there is a genuine example of perspectivism in science. M2, on the other hand, is plausible *if* perspectivism is correct in general. It is as compelling as other examples of perspectival knowledge discussed in the literature on perspectivism. That said, thanks to the fact that M1 is more evident than M2, the similarity between M1 and M2 can shed some light on the conditionality expressed in M2. Both frame-dependent properties and the experimental results concerning the constancy of the speed of light are *valid in bounded conditions*. In the remainder of this section, I defend M2 and M1 by setting aside some misunderstandings and by blocking some criticisms.

M2 rests on a general motivation for perspectivism applicable to all experimental results. According to this motivation, one can never claim that the results of currently valid experiments may not be untrue in novel, uninvestigated domains. Prospective theoretical/mathematical concepts or future observational/experimental instruments may alter our currently established knowledge in unconceived ways or may show that our current knowledge has a bounded validity. Thus, perspectivism invites us to exercise epistemological modesty, implying that scientific knowledge is not unconditionally true, but *according to* the scope and validity of our current theories and instruments.

But is M2 not a trivial view? That is, why would someone deny that what we take to be true is true according to currently accepted theories and instruments? The main issue is whether or not we can unconditionally state that current theories and instruments provide us with the ultimate picture of reality. Perspectivists make an effort to show that we cannot. (Realist) perspectivism is not entirely obvious. A number of scientists and philosophers presuppose that reality is exactly or approximately alike to what science already describes. For instance, Giere (2006a, pp. 4–6) discusses several objectivist expressions made by Steven Weinberg, a leading physicist. For another example, among philosophers, Gerald Doppelt (2011; 2014) seems to maintain that the current best theories are ultimate and will not be replaced by better alternatives (for a cogent criticism of his view, see Alai 2017).

One should note that M2 does not imply that experimental results are unreliable. According to the criterion of robustness, our belief in the truth of experimental results may well be justified. The perspectivist's point is that realist claims should be expressed in a conditional form. Accordingly, constancy of speed is a real property of light *conditional on* the experimental setups in the contexts of inertial reference frames. Nor does M2 imply that experimental results are subjective. As I explained in subsection 3.4.2, perspectivism is at odds with $objectivism_1$. However, perspectivism does not underplay the role of objectifying procedures that help observers/experimenters to purify scientific knowledge from subjective errors. For this reason, perspectivism is consistent with the fallible $objectivism_2$ of Alan Chalmers, which emphasizes the capability of observational/experimental methods to eliminate subjective errors.

Anjan Chakravartty (2010, section 3) argues that the conditional nature of detection can be accepted by realists who accept the "dispositional" view of natural properties, and therefore there is no need for perspectivism. If Chakravartty's aim is to be defending $objectivism_2$, then his position is hardly controversial. However, if he also supports $objectivism_1$, his view is questionable. Particularly in special relativity, due to M1, properties such as simultaneity cannot be understood as being $objective_1$ (they are not, and cannot be, measured/observed from nowhere).

A relevant question is whether a dispositional account of properties can explain the frame-dependent properties of special relativity. I think it can, but not in a way that is incompatible with perspectivism. Chakravartty describes dispositions thus:

> Dispositions are often manifested differently depending on the ambient circumstances, and while such manifestations can vary, they may be manifestations of one and the same property nonetheless. (2010, p. 409)

Let us examine the length of a rod, understood as a property of the rod. The length is "disposed" to be manifested differently in different reference frames. However, this is just another way of expressing the perspectival account. Since one cannot disregard reference frames in specifying the length of the rod, one has to accept the role of reference frames as perspectives. Consider Chakravartty's definition of a non-perspectival fact: "a proposition that is true, independently of any particular perspective one may take with respect to it" (2010, p. 407). As I argued earlier, one cannot make a statement (or proposition) about the length of the rod independently of any reference frame. Therefore, there is no non-perspectival fact about the length of the rod.

One may claim that "having a length" (or, in the dispositional language, "the capacity to have some length") is a reference-independent property of the rod, as the rod always has *some* length in any reference frame. This claim is trivial, and for this very reason the dispositional account needs perspectivism to jointly express that the capacity to have a length is a dispositional or potential property of the rod *and* that the realized length of the rod depends on the choice of frame. Similarly, concerning simultaneity, it is trivially correct that $event_1$ is disposed to happen at the same time, before, or after $event_2$ (provided it is not causally connected to $event_1$). However, to specify which option is actually true, the reference frame should play its role to determine the perspective from which the events are observed. As a result, the dispositional account of properties can explain frame-dependent properties only jointly with perspectivism.

The dispositional account of properties is not in conflict with M2 either. The main point of realist perspectivism about experimental results is that they should not be expressed in an unconditional, universal way. Prospective theories or future technological instruments may demonstrate that some of the presuppositions underlying our currently accepted (dispositional) facts are only valid in certain domains. Several cases from the history of science show that experimental results once considered to be unconditionally true have later turned out to be valid only under certain conditions. In this regard, a dispositional view that is compatible with the perspectivist's concern about experimental results is the Aristotelian account of "potentialities" (see section 2.2), according to which the existence of independent potentialities or dispositions (in part) accounts for the results of replicable experiments. But our knowledge of the results of these potentialities or dispositions is not "universally valid". The results may still be bounded by "nonlocal" conditions. As I explained in subsection 3.4.2, this is similar to the view that overlapping perspectives do not present "non-perspectival" facts.

In sum, a perspectival reading of the special theory of relativity is well motivated, and should be interesting for philosophers discussing scientific realism. Perspectivists should take the case of (special) relativity theory more seriously. The primary focus of this chapter has been on the theory of special relativity. An independent study of the general theory of relativity might analyze its relationship with perspectivism more precisely. However, let me finish the chapter by adding some comments on general relativity, which may bring a new perspective into the debate on perspectivism.

7.5 The Perspective of General Relativity Theory[11]

The special theory of relativity is restricted to *inertial* reference frames, whereas general relativity theory discusses all frames, including non-inertial ones. From a more general perspective, the latter theory provides formulas for physical measurements from any arbitrary reference frame, that is, from any state of motion, position, and time (with any speed and acceleration, anywhere and anytime in the universe). Interestingly, general relativity theory is also a theory of gravity, since according to its "principle of equivalence" the effects produced by a non-inertial reference frame and those by gravity are indistinguishable.

[11] I am grateful to one of the reviewers for *Theory of Science*, whose comments are incorporated in this section.

In general relativity theory, the "principle of relativity" is generalized to all reference frames, and thereby the "principle of covariance" is obtained: the laws of physics are the same in *all* frames of reference. However, from the perspective of the general theory of relativity, the speed of light is only constant c *locally*, where spacetime is flat. Measurements carried out in curved spacetime (that is, in the presence of gravity) vary from one event to another. Indeed, the coordinate speed of light (that is, the speed of light measured in a reference frame, using its local coordinates) is not the universal constant c. Thus, the coordinate speeds of light measured in different reference frames are different and can be much bigger than c. There is of course no contradiction, since general relativity theory provides the mathematical formula that transforms the coordinate speed of light from one frame of reference to another. Locally the propagation of light defines a unique light cone, which defines the local causal structure of the world.

According to the general theory of relativity, the speed of light can be straightforwardly understood as perspectival. When measured in a non-inertial frame, the speed of light is conditional on the proper acceleration of the frame (which is defined as the acceleration of the frame relative to an inertial observer). The broader perspective of general relativity theory enables us to see clearly the perspectival nature of the speed of light in terms of its dependence on non-inertial frames. Accordingly, we know that the validity of special relativity theory is *bounded* by conditions. In Einstein's terms,

> the special theory of relativity *cannot claim an unlimited domain of validity*; its results hold only so long as we are able to disregard the influences of gravitational fields on the phenomena (e.g. of light). (1952[1916], p. 98, emphasis added)

Laws of physics are still frame-independent in general relativity. Also, there are invariants such as the Ricci scalar (or the scalar curvature) that have the same value in all reference frames. These invariants, which are typically the specific combinations of frame-dependent quantities, are frame-independent. Note, though, that they are independent of all *reference frames*, rather than of all perspectives whatsoever. If perspectivism is right, laws and invariant quantities cannot be considered as "non-perspectival". They either are robust (in this case, they are not objectively$_1$ true but are "nonlocally" true according to "overlapping perspectives") or are conditional on the a priori assumptions of the general theory of relativity.

General relativity theory has demonstrated the perspectivism of light speeds in terms of its frame-dependency. Similarly, prospective theories may provide broader perspectives, revealing some perspectival features or bounded validity of the general theory of relativity. In Giere's words,

> the specific contingencies that sustained the earlier perspective became evident only from the vantage point of the later perspective. Often it is only from a new perspective that one can see, relative to that new perspective, where the earlier perspective was lacking (2006a, p. 94).

A relevant debate concerns the possibility of a "theory of everything". Nowadays, many physicists devote their time to develop "quantum gravity", a theory that aims to reconcile general relativity, which only describes the gravitational force, and quantum theory, which can explain the other three forces of nature: strong, weak, and electromagnetic. A quantum gravity theory is needed to describe phenomena, e.g., in the vicinity of black holes, where gravitational and quantum mechanical effects are at work at the same time. Thanks to the progressive feature of science, we could be optimistic about the possibility of a well-developed quantum gravity theory. Nevertheless, a prediction on the basis of realist perspectivism is that the validity scope of the theoretical assumptions and experimental support of even a theoretically consistent and experimentally successful theory of quantum gravity will be qualified. Furthermore, the theory will be valid only in fundamental physics. It is highly questionable whether all chemical, biological, psychological, ethical, social, political, religious, and other phenomena are explainable by that theory.

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Summary

This dissertation is about human knowledge of reality. In particular, it argues that scientific knowledge is bounded by historically available instruments and theories; nevertheless, the use of several independent instruments and theories can provide access to the persistent potentialities of reality. The replicability of scientific observations and experiments allows us to obtain explorable evidence of robust entities and properties. The dissertation includes seven chapters. It also studies three cases – namely, Higgs bosons and hypothetical F-particles (section 2.4), the Ptolemaic and Kepler model of the planets (section 6.7), and the special theory of relativity (chapter 7).

Chapter 1 gives an introduction to the main problem, concepts, and approach of the dissertation. The problem concerns the scientific realism debate. Concepts such as "perspectives", "persistent and resistant" potentialities, "overlapping perspectives", "replicable" observations and experiments, and "explorable" evidence are employed to advance a realist yet perspectivist view about knowledge of reality. Different philosophical methods, which are used in the analytical and phenomenological-hermeneutical traditions, collaborate to support the claims of the dissertation.

Chapter 2 clarifies what is the meaning of the notion of "real" (in science). The answer is that "resistance" and "persistence" explain the negative and positive meanings of the real at three levels of discussion: ontological, perceptual and epistemological. The chapter starts with Hans Radder's ontological view that reality consists of human-independent, "persistent" potentialities or powers. It then discusses the constructivist account of "resistance", which is a negative view about reality but can be suitably complemented by a positive, potentiality-based ontology. A study of the cases of the Higgs boson and the hypothetical F-particle helps to argue that real things persist in existing and resist being excluded from existence. At the perceptual level, the same view implies that veridical perceptions persist in appearing by making possible experience or evidence under

appropriate conditions, and they may resist disappearance by presenting some signs or effects (before persistently appearing under appropriate conditions). Finally, the epistemological implications of the concepts of persistence and resistance are realist perspectivism and falsificationism, the former being extensively explored in the following chapters.

A line of argument the dissertation pursues is that entity realism and perspectivism are complementary. The former explains why we should have a positive attitude toward the existence of (the properties of) unobservable entities. The latter emphasizes that scientific knowledge is bounded by theoretical and instrumental perspectives. Chapter 3 advances this argument by discussing Ronald Giere's versions of realism: entity realism, constructive realism, and perspectival realism. According to him, scientific models are constructed to represent aspects of real entities from different perspectives. On the basis of the idea of "overlapping perspectives", this chapter develops a realist perspectivism that reconciles entity realism and perspectivism but avoids their criticisms.

Chapter 4 advances the entity realist dimension of the thesis of the dissertation, and therefore provides a criterion for reality. Having been started with the work of Ian Hacking, Nancy Cartwright and Ronald Giere, the project of entity realism has recently been developed by Matthias Egg, Markus Eronen and Bence Nanay. This chapter opens a dialogue among these recent views on entity realism, and develops the project by consistently combining them. The result is a criterion for the reality of property tokens: insofar as only a type of property tokens can explain the evidence obtained in a variety of independent ways of detection or measurement, the property tokens may be taken as real in the relevant scientific community at a certain time.

Inspired by the phenomenological-hermeneutical approaches to the philosophy of science, chapter 5 presents further support for realist perspectivism. Philosophers such as Patrick Heelan, Joseph Kockelmans, Don Ihde, Shannon Vallor, and Harald Wiltsche have discussed philosophical issues concerning scientific practice from phenomenological-hermeneutical points of view. Among them, Vallor has already defended experimental/entity realism. Furthermore, there are a number of ideas in the phenomenological-hermeneutical approaches that support perspectivism. The chapter, first, employs the distinction between "manifestation" and "phenomenon" and the view that the evidence of a real thing is "explorable" in order to argue that a scientific entity manifests itself through instrumentally-mediated perceptual evidence. Then, it underpins the phenomenological notion of the horizonal nature of scientific observation with realist perspectivism. The result agrees with Edmund Husserl's (and Wiltsche's) attack on scientific objectivism, with Heelan's and Ihde's emphasis on the importance of instrumentation in scientific practice, with an acknowledgement of the hermeneutical characteristics of science, and with Kockelmans's Heideggerian conception of truth.

Variant forms of the so-called pessimistic induction adduce the actual history of science as evidence against realist attitudes. Chapter 6 focuses on two versions of the pessimistic induction. The first version calls into question the realist view that current theories, containing referring theoretical terms, are truthful. The second one disputes the no-miracle argument, according to which the successes of science would be miraculous without a realist interpretation. In response to these two arguments, the chapter first develops a view of scientific progress, according to which current theories are not ultimate but provide broader perspectives than preceding theories do. Then, it contends that the theoretical descriptions of an abandoned term that contribute to successful explanations and predictions are truthful. To support this claim, the chapter suggests a "restatement strategy", according to which the successes of a past theory can be restated from the perspective of its currently acceptable successor. As a result, the theoretical descriptions of a past theory may overlap with those of its succeeding theory, which brings about diachronic overlapping theoretical perspectives that deserve realist commitment. By employing a realist interpretation of Ramseyfication the logical side of the restatement strategy is clarified. What's more, the chapter studies the relationship between the Ptolemaic and Kepler's model, and concludes that the former's successful explanations regarding the paths of the outer planets can be restated from the perspective of the latter.

The final chapter of the dissertation serves as its conclusion by applying themes such as potentialities, robustness, and perspectives to the case of the special relativity theory. Chapter 7 distinguishes between two related meanings of "perspectival", and argues that reference frames are perspectives, provided that perspectival means "being conditional" rather than "being partial". Frame-dependent properties such as length, time duration, and simultaneity, are not partially measured in a reference frame, but their measurements are conditional on the choice of frame. Indeed, objects possess potentialities to manifest differently realized properties such as lengths in different reference frames. The chapter also critically discusses whether the constancy of the speed of light depends on perspectival factors such as the idealized definition of the speed of light in a perfect vacuum and the Einstein synchronization convention. Furthermore, the chapter defends the view that the constancy of its speed is a robust property of light according to the conditions of currently acceptable experimental setups pertaining to special relativity, and concludes that this view supports perspectivism. Overall, relativity theory holds significant interest for scientific perspectivists.