

**Science and Metaphysics**  
*A Methodological Investigation*

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*I dedicate this to my wife, Alida, who has encouraged and supported me since the beginning of my study at IU, a year after our marriage; who has willingly and happily walked with me all the way through good as well as difficult times.*

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## **ABSTRACT**

**ZAINAL ABIDIN**

### **Science and Metaphysics: A Methodological Investigation**

Traditionally, knowledge of nature *as it really is* belonged to the realm of metaphysics. With the rise of empirical sciences in the seventeenth century, and Logical Positivism in the twentieth century, metaphysics as an *a priori* knowledge had gradually lost its credibility, to be replaced by the sciences. Recently, however, there are ideas of constructing metaphysics that is somehow grounded on science (e.g. experimental metaphysics, metaphysical naturalism, and certain forms of constructive theology as discussed in recent science and religion discourse). The main question addressed in this dissertation concerns the possibility of such metaphysics.

Science can be relevant to metaphysics only if one believes it to have significant things to say about the world; in other words, only if one holds some form of scientific realism. In this dissertation the author argues for the metaphysical ambiguity of scientific theories. This claim can be divided into two sub-claims: 1) That a modest version of scientific realism can be defended, which justifies the belief that scientific theories speak about the world, but 2) what precisely the world is like as presented by the theories is not fully-determined. A metaphysical system can be

constructed only through the process of interpretation, in which the function of the theories is more in the direction of putting constraints on possible metaphysical interpretation of them. Another related conclusion is that some theories are more readily interpretable compared to others.

In the final part of the dissertation an illustration is given, which is drawn from recent discourse on science and theology. This illustration of an attempt to ground a (philosophical) theology on science shows how the above conclusions apply to this case.

***Keywords:*** scientific realism, metaphysics, interpretation, theology, Bas van Fraassen, Ernan McMullin

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## **Chapter 1**

### **Introduction:**

### **“Science” and “Metaphysics”**

#### **A. Background**

In contemporary discussions about the relation between science and metaphysics, there are two related issues. The first issue concerns the endeavor to ground metaphysics/worldview on the results of science, while the second poses a question as to whether science presupposes a certain metaphysics or worldview.<sup>1</sup>

#### **1. Grounding metaphysics on science?**

Along with the progress of empirical science since the beginning of its modern history, the legitimacy of metaphysics as an intellectual undertaking has been

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<sup>1</sup> “Metaphysics” is perhaps not the best term for the kind of knowledge referred to here. An alternative I shall consider later in this chapter is “worldview”. In philosophy of science we find this term is recently used by Abner Shimony (1989) in his essay, “Search for a worldview which can accommodate our knowledge of microphysics”. For him, a worldview consists of (i) a metaphysics, (ii) an epistemology, (iii) a theory of language and (iv) a theory of ethical and aesthetic values. (*ibid.*, 62) He concentrates on the first two; it is through the meshing of these two enterprises that he hopes to attain his aim of “the closing of the circle”, following the tradition of systematic philosophers such as Aristotle and Kant. Schlick also uses the term “worldview” in a similar sense to Shimony’s, though, it seems, without the third element. Different philosophers use the term to refer to different kinds of knowledge—sometimes contradicting each other. In any case, I shall clarify the term “metaphysics” later in this chapter.

questioned by philosophers such that being “metaphysical” is a vice in today’s philosophy. Indeed, after the 17<sup>th</sup> century metaphysics fared much worse when compared to science. One of the main reasons was that agreements among its practitioners were notoriously very difficult, if not impossible, to achieve. One running theme in the modern rejection of metaphysics, which has started since Kant, is its inability to progress. It is not hard to see in history that in science, unlike in metaphysics, there are always relatively broad agreements. A contrast popularly drawn by philosophers and scientists between metaphysics and science only makes the impotence of metaphysics more apparent. Kant’s contention in his *Critique of Pure Reason* and *Prolegomena to Any Future Metaphysics*, which contrasted the random groping of metaphysics in the face of the secure path of science, has become an influential and almost standard argument to reject metaphysics in later times. For Kant, the human mind, with its categories of thought, is simply not suitable to deal with metaphysical issues. As such there does not seem to be any way to decide metaphysical disputes.

Nevertheless, oftentimes the same philosophers who reject metaphysics also propose a different kind of metaphysics. Kant, for example, while maintaining that traditional metaphysics is epistemically impossible, proposed a new enterprise, a “transcendental metaphysics” of the *Critique of Pure Reason*, and a special metaphysics (of nature in general) of the *Metaphysical Foundations of Natural Science*. Very broadly speaking, in many cases it is not metaphysics *per se* that is rejected, but the way metaphysicians define its scope, or handle its problems.

In the twentieth century, the most prominent rejection of traditional metaphysics, motivated by similar observations as Kant's, was the one attempted by Rudolf Carnap and his colleagues in the Vienna Circle. In his "Elimination of Metaphysics", taking a few examples of metaphysical propositions, he sets out to show the futility of metaphysical enterprise in general. But even here Carnap is open to the possibility of a different kind of metaphysics, viz. one that '*endeavors towards a synthesis and generalization of the results of the various sciences.*' (Carnap 1932; cf. Friedman 1996)

Carnap's idea points to an interesting trend in the 20<sup>th</sup> century philosophy of science, in which philosophers who take science as the best kind of knowledge try to develop an idea of a metaphysics that is well-informed by current scientific theories. This is quite attractive since it promises to provide knowledge beyond the scope of experience in an epistemologically legitimate way. The pessimism as to what metaphysics, left to itself, can do is accompanied by a great optimism with respect to science's capability to answer important questions about the world. We can discern this trend in the beginning of the 20<sup>th</sup> century, but also in its end.

In 1915, in his writings on Einstein's relativity theory, Schlick began to develop his idea of how philosophy in general could, and should, follow the guidelines suggested by scientific theories in solving its questions, and, more specifically, how science could decide between competing epistemological and metaphysical claims. Relativity theory shows many ways science comes into contact with philosophy. In a sense this theory unavoidably has important philosophical

bearings, since it directly speaks about fundamental concepts that had been regarded as weighty metaphysical problems, such as those of space, time, and substance, and therefore enters the philosophical domain. (Schlick 1922, 343) Moreover, the insights suggested by the theory are regarded as superior to any mere speculation since it can be verified empirically. Schlick contends that scientific discoveries, especially in cases where new principles such as the principle of relativity are revealed, should give philosophers ‘a sort of criterion for the soundness of philosophy.’ (Schlick 1915, 154) Science, or physics more specifically, can supply us with a sort of “confirmation by experience”, or “an objective criterion of truth” for philosophical systems.<sup>2</sup> (Schlick 1929, 96)

At the other end of the history of philosophy of science, this conviction still reverberates. Michael Redhead, for example, struggles with quite similar issues as Schlick did. He asks, for example, “Is it possible to bring experiments to bear on metaphysical theses such as realism?” (Redhead 1995, 41) He uses the term “metaphysics” here in its traditional sense, as an examination into the nature of reality. (*Ibid.* xi, xii), And, like Schlick, he wants to bring in the results of latest empirical investigations done by physicists to answer general issues concerning the nature of reality that used to belong to the domain of traditional, *a priori* metaphysics. In his small book he states his project with a bold statement: “to consider the

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<sup>2</sup> Schlick’s concern was mostly to critically “test” the adequacy of neo-Kantianism and Machian positivism against the development in relativity theory; this appears most clearly in Schlick (1921), in which he reviews Cassirer’s and Reichenbach’s books on relativity, but also in other essays related to the theory.

relevance of developments in theoretical physics to metaphysical questions.”  
(Redhead 1995, xi)

Another example of such an endeavor is carried out by Shimony. As exemplified by the title of one of his essays (1989), Shimony’s project is a search for a worldview which can accommodate our knowledge of the natural world by way of natural sciences. He cites several examples from recent physical theories, such as relativity theory and quantum mechanics, but also from cosmology and molecular biology, that show how scientific theories might have substantial bearings on metaphysics. With this in view, Shimony sees that it is “legitimate to speak of the enterprise of *experimental metaphysics*”. (Shimony 1989, 64)<sup>3</sup>

It is interesting to note that Kant’s rejection of metaphysics is based on his transcendental investigation which concludes that human cognitive make-up is simply not capable to capture the realm of *noumena*, and as such this realm was forever exiled from the realm of possible *knowledge* of any kind. Yet now the metaphysical enterprise is opened up again. Of course, this does not mean that there is a revival of traditional metaphysics. As a matter of fact, in epistemological terms, the situation now is less fortunate since the notion of *a priori* knowledge, which plays a significant role in Kant’s very limited metaphysics, has become suspect. What makes the case for metaphysics seems to be different now mostly concerns the

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<sup>3</sup> As I shall discuss later in Section B of this chapter, David Papineau (1996) divides issues discussed in philosophy into two groups: one concerns the epistemology of science, another one concerns the metaphysics of science, which is called “scientific metaphysics” by Worrall (2000). The latter consists of issues such as the general structure of reality, the nature of causation, the nature of probability, determinism, how spacetime should be understood, reductionism, etc.

method of pursuing it, and how it is situated in its relation to empirical sciences. A comparison with similar projects may help us to see the scope and method of this new enterprise. Below is a brief comparison with contemporary naturalism and the empiricists' idea of scientific philosophy.

In contemporary philosophical discourse, part of the discussions about this issue is carried out under the rubric of "naturalism". As admitted by many observers of the naturalistic trend in philosophy, there are many strands that go under this banner. But one of the few things which unite them is the belief, in different degrees, in *the continuity of philosophy with empirical science*. This is probably best shown by Quine's move in his naturalized epistemology, which in many ways can be seen as a response to Carnap's idea of *aufbau*. After the elimination of the distinction between the analytic and the synthetic, and rejection of the idea that there is some part of knowledge which is attained through *a priori* ways, for Quine there is no more ground for the distinction between philosophy and empirical science. Papineau's naturalism, however, is more moderate than Quine's in that he still sees that philosophical issues are different from the scientific ones. The former are characterized by a special kind of difficulty that, unlike the latter, cannot be solved by the uncovering of further empirical evidence. But, even so, 'the task of the philosophers is to bring coherence and order to the total set of assumptions we use to explain the empirical world.' (Papineau 1993, 3) 'All that naturalist claims is that this investigation, like any other philosophical investigation, is best conducted within the framework of our empirical knowledge of the world.' (*Ibid.* 5) A more lucid, slightly

different, way to express this commitment is given by Kornblith: ‘the task of the naturalistic metaphysician ... is simply to draw out the metaphysical implications of contemporary science. ... There simply is no extrascientific route to metaphysical understanding.’ (Kornblith 1994, 40) An important element in all this is (a certain degree of) “scientism”. (Cf. Rosenberg 1996, 4, on four key axioms of naturalism)

Helmholtz’s brand of scientific philosophy is similar to Quine’s naturalized epistemology in that it addresses exclusively epistemological issues by using the method of empirical sciences, such that philosophy and science become indistinguishable. This is different from the Logical Positivists’ idea of scientific philosophy. Dispensing completely with the use of intuition by traditional metaphysicians, their scientific philosophy made great use of the then newly built logical tools of analysis. (Carnap’s elimination of metaphysics is achieved through logical analysis, as shown by the title of his famous work.) Like Kant, the two enterprises are still fully demarcated, yet closely related. Unlike Kant, philosophy is not conceived as needed to ground empirical science. On the contrary, results of science should inform philosophy, whose task is limited to logical clarification of scientific concepts.

“Experimental metaphysics”, on the other hand, addresses issues of *traditional* metaphysics, but *not* through a fully a priori investigation; this metaphysics is built squarely on the results of current science—at least, that’s the



aspiration. A distinction between science and philosophy is still maintained.<sup>4</sup> With this, the term “metaphysics” seems to have been used not merely in pejorative sense in philosophy of science.

The traditional *issues* included in the scope of this new metaphysics might not be restricted to those directly related to the subject matter of physical sciences. Shimony himself actually conceives his project in the broader context of worldview, of which metaphysics and epistemology are its most important parts. (See fn. 1) Papineau mentions that “other less specialist philosophical questions”, such as the existence of free will, would be relevant to a naturalistic metaphysics, since they might arise because of some empirical scientific assumptions. (Papineau 1993) Redhead, in several of his review articles even indicates an approval to bring theological issues in metaphysical discussions about current scientific theories. Despite the need to give independent evidence for a Divine Creator, he sees that invoking God as a causal factor in current cosmological discussions may be thought to be genuinely explanatory. (Redhead 1996) In this direction, we also see theological issues debated in Adolf Grunbaum-Craig-Smith exchanges on the Big Bang cosmology. In the context of science in general, there is also an exchange between Plantinga, van Fraassen, and Ernan McMullin, which I will mention at the end of this paper, as to whether the metaphysics of science necessarily excludes theistic beliefs.

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<sup>4</sup> As I will mention below, the activity of the “experimental metaphysician” mostly consists of giving interpretation to physical theories. Interpretation does not add any empirical content to the theories. (Sklar 2000,)

In a way, we can see the motivation behind the endeavor to ground metaphysics on science as quite natural. Metaphysics of any kind has to start somewhere. In its history, metaphysics always has knowledge about the natural world as one of its sources. Now, natural science is (almost) unanimously taken as the best available knowledge, so, as a start, it is only natural to make use of it in our investigation of reality. The relevance of natural science to metaphysics then can be expressed as simple as indicated in Schlick's criticism of Hegel: 'We can put no trust in a philosophy which deduces from concepts that the number of planets in the solar system must be seven (Hegel), at the very moment when the eighth was being discovered.' (Schlick 1915, 153)

Even Duhem, who is regarded as an instrumentalist, sees that metaphysics shouldn't be indifferent to the results of science. Duhem unequivocally believes that metaphysics should not and does not play role in the formulation of scientific theories. Science does not presuppose metaphysics and does not give us metaphysics. But from this it does not follow that the metaphysician 'can pursue the construction of his cosmological system without any concern for the set of mathematical formulas by means of which the physicist succeeds in representing and classifying the set of experimental laws.' (Duhem 1962, 291)

Nevertheless, the venture to create an experimental or scientific metaphysics might also seem odd: On the one hand, this attempt is surely based on the belief that bringing science into what Kant calls "the battlefield of endless controversies" would make a difference; on the other hand, recent discussions in philosophy of science—

especially concerning whether science is capable of giving us true theories about the world—have arrived at such modest conclusions about many aspects of science that such an expectation might be out of place. Among contemporary philosophers of science, Bas van Fraassen is the most prominent in his skepticism toward this kind of project. Earlier I mention that one of the few things which unite different versions of this project is the belief, that comes in different degrees, in the continuity of philosophy with empirical science. In general van Fraassen tries to defend the metaphysical neutrality as much as possible (for example, see his (1996)). He attacks the “philosophical conception [that] makes physics continuous with metaphysics.’ (van Fraassen 1991, 374) This position, he believes, is a straightforward scientism; ‘that is not science—it is superstition, no matter how scientific it is made to sound.’ (van Fraassen 1994, 133)

## **2. Is science grounded on metaphysics?**

Another facet in the issue of the relation between science and metaphysics, which in a sense is the reverse of the aim of the project discussed above, tries to answer the question of whether science has metaphysical presuppositions. If science is said to presuppose a certain metaphysics or worldview, then its results would surely be compatible with that metaphysics/worldview. Duhem considers this question too.

As mentioned earlier, for him the aim of science should not be to give (metaphysical) explanations, since if that’s the case, then there needs to be a commitment to a particular metaphysics. In which case, we would be expecting a

similar lack of agreement among its practitioners as has happened in metaphysics. An objective science, for Duhem, can't probe into the nature of things, so that to answer the questions about the nature of things, the theory needs additional elements which can only be supplied by a metaphysics. 'Therefore, *if the aim of physical theories is to explain experimental laws, theoretical physics is not an autonomous science; it is subordinate to metaphysics.*' (Duhem 1914, 10; italics from Duhem) In his famous "Physics of a Believer", answering accusations that his physics is based on certain beliefs, Duhem insists that his science is not grounded on any metaphysics. In the history of science, unresolvable disputes emerged in the case of Copernicus, Descartes and Newton when they tried to give explanations, which betray their metaphysical commitments. In a sense, we can see Duhem as giving a choice between two ways of doing science: i) his style of non-metaphysical science; this science is incapable of giving explanations, but the results is an autonomous body of knowledge free from unending metaphysical disputes; ii) a science that have loftier aims of giving explanations, with the risk of losing consensus among scientists.

Recently, philosopher Alvin Plantinga takes up this issue again, when he poses the question, "is science metaphysically-neutral?" He finds that science, as exemplified in evolutionary biology, astrophysics, and evolutionary psychology<sup>5</sup>, has indeed made grand metaphysical claims, mostly because they start from the belief in methodological naturalism (MN), which excludes any reference to "un-natural" or

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<sup>5</sup> Indeed, the samples of what he calls "science" here can be questioned whether, strictly speaking, they are science. But Plantinga sees that for all practical purposes, these enterprises are nowadays commonly regarded as scientific. (Plantinga 1996)

supernatural agents. Instead of being neutral, MN is a kind of metaphysics. He admits that there is a choice such as suggested by Duhem—which he calls a choice between Duhemian science and Augustinian science—and sees that there is nothing wrong in adopting an Augustinian science. He accepts its consequence, as has already been indicated by Duhem, that this choice means that people with different metaphysical commitments would each have their own scientific system. Cooperation and consensus can be attained only up to the limit of Duhemian science, from which point scientists of different groups would go on their separate metaphysical ways.<sup>6</sup>

At the first glance, the idea of Augustinian science, insofar as it acknowledges the legitimacy of metaphysics in scientific activity, reminds one of discussions about the role of metaphysics in post-positivist philosophers of science, such as Kuhn, Feyerabend, Popper, and Lakatos. For Feyerabend, proliferation of metaphysical views is encouraged for the sake of increasing the likelihood of scientists hitting upon the truth. This suggestion is, indeed, backed up by impressive evidence from the history of science, as shown in his and Kuhn's works. Kuhn's notion of incommensurable paradigms is taken by some philosophers to be a kind of metaphysics too. For Lakatos and Popper, in spite of his project of demarcating metaphysics from science, metaphysics, as a research programme, might be useful

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<sup>6</sup> Plantinga suspects that science in general is not metaphysically-neutral in that it excludes theistic beliefs. In his widely-discussed *Warrant and Proper Function*, he proposes a naturalistic epistemology, especially a theory of warrant, which, he says, works best in a theistic framework. (Cf. Kvanvig 1996)

methodologically as the ground to explore new possibilities to solve scientific problems.

In his response to Plantinga's views, van Fraassen, while noticing the similarity of the proposal to include metaphysics in scientific practice with Feyerabend's proliferation strategy, tries to differentiate between metaphysical theses that have empirical implications and those that don't. He argues that for the first class, when the theses are admitted, their implied empirical hypotheses will "forget their origins". The theses play some role only, as it were, in the context of discovery. Taking materialism as his example of metaphysical thesis, he argues that for the latter class, either they will not make a difference in scientific practice, or, if they make a difference, they act as "false consciousness", a kind of heuristics.

What is at stake here? If science is indeed *effectively* grounded on some sort of metaphysics or worldview, this will undercut an expectation for scientific theories (or their interpretations) to meaningfully provide us with some kind of "scientific metaphysics/worldview", since its results would surely be compatible with the metaphysics/worldview initially presupposed. Laying out a metaphysics—probably as a sort of foundation—in the pre-scientific activity would simply mean that the metaphysics is there in an *a priori* way. Not only this would take us back to where we start—rejection of *a priori* metaphysics—it would even put science in a worse situation of potential unresolvable disputes as feared by Duhem.

### 3. Pierre Duhem on the relation between science and metaphysics

I shall discuss Duhem's idea about science and metaphysics at some length here for several reasons. In Duhem we find an unusual attention—relative to later philosophers of science—given to the subject of science and metaphysics in not a totally negative way, which is the main reason why I need to devote a special section on him here. His ideas will also be helpful later when I try to delimit what is meant by metaphysics. Also, interestingly many philosophers of science today advance views quite similar to Duhem. He addresses the two questions about the relation between science and metaphysics that I raise earlier.

Duhem, a member of the first generation of philosophers of science, devotes considerable space in his work for demarcating science from metaphysics and affirming the *logical* priority of science over metaphysics. Duhem was not as hostile to metaphysics as later philosophers, such as the Logical Positivists. One of the cores of his ideas in this regard can be summarized by a subheading in his famous “Physics and Metaphysics”: ‘Physical theories are independent of metaphysics and *vice versa*.’ (Duhem 1893, 35)

There he starts with some clarification of the terms. In the peripatetic sense, *physics* is the study of the motion of material things, or the changes undergone by the essence of things. But in modern time, *physics* is the experimental study of inanimate things, comprising observation of facts, discovery of (empirical) laws and construction of theories. These activities do *not* include investigation of the essence of things as causes of physical phenomena. This last activity is a subdivision of

*metaphysics*, which in its general, peripatetic sense includes the study of the essence of things. For Duhem, “metaphysics” in the contemporary use of the term include both the peripatetic metaphysics and physics. The peripatetic division is made on the basis of subject matter. But Duhem’s differentiation is made mainly on an epistemological basis. What is the peripatetic term for modern physics as described above? It is interesting to note that Duhem couldn’t locate a distinctive, pre-modern body of knowledge that parallels modern physics, which, he wants to argue, studies only the appearance of things (with no purposes of achieving knowledge about reality, which is assumed to lie behind the appearance). The best he can come up with is astronomy, which he understands as consistent in maintaining the tradition of “saving the phenomena” throughout the whole of its long history.<sup>7</sup> This can be summarized as follows:

<i>Peripatetic</i>	<i>Duhem</i>	<i>Subject Matter</i>
Metaphysics	-	Essence of things (*)
Physics	Cosmology	Causes of motion as found in the essence of things (**)
Metaphysics + physics	Metaphysics	(*) + (**)

The very fact that there is no pre-modern parallel to modern physics might suggest that physics is an unprecedented discipline. If physics is the study of

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<sup>7</sup> Duhem’s suggestion that astronomy had always had the “saving the phenomena” tradition has been questioned. There were non-physical reasons given by Ptolemy as well as Copernicus in advancing their models. For example, in choosing only perfect circles to “save the phenomena”—with the consequence of significantly complicating his model—Ptolemy’s reasons are non-physical. But this issue is not directly relevant to my discussion here.



phenomena, then it would not even qualify as real knowledge (*episteme*) in the Platonic as well as Aristotelian sense. *Episteme* is study of real things, real relations between things, and not merely appearances. If a discipline is not called *episteme*, then it might be *techne* (craft). But this division can also be construed as made not (only) on the basis of the subject matter but the aim of the enterprise. A discipline is called *episteme* if it aims at knowing the real nature of things and the real causes of events, *techne* if its aim is wholly practical. As I will show in the next chapter, Duhem is a realist of some sort, despite many statements of his to the contrary effect. As such we can say that his physics is a new enterprise in the sense that it studies the appearance but with the ultimate aim of getting, up to a certain level, at the reality behind it.

But, to repeat an earlier point, Duhem's division of physics and metaphysics is done mainly on an epistemological basis, though at some point it's undoubtedly related to the concerns about reality. Thus when he argues for the independence of physics and metaphysics, his argument is mainly epistemological as well.

It starts with the acknowledgement that human beings do not have direct access to the essence of things, but only to the phenomena that arise from them. Knowledge of phenomena gives the physicist hints about the things themselves, since they are the efficient cause of the phenomena. But our knowledge of the things themselves (which sometimes are also called substance) is limited insofar as they function as the causes of phenomena. A complete knowledge of the external world is what is aimed at by metaphysics. So physics studies 'phenomena arising from brute

matter and of the laws that govern these phenomena’, while metaphysics studies the nature of the things that are the subject matter of physics. (1893, 31) At this point, Duhem reminds his reader that the distinction between physics and metaphysics is an epistemological one: ‘It does not follow from the nature of the things it studies, but only from the nature of our intellects.’ (1893, 31) To conclude, two features of metaphysics as opposed to science are (1) *epistemological* (metaphysics is not empirical and the object it studies are inaccessible to direct access) and (2) *ontological* (it speaks about the world in more general terms than science does).

Duhem’s strict division of physics and metaphysics seems to be motivated mainly by the need to make science a common endeavor. His first philosophical statements on the characters of scientific theory (1892) already mention what he sees as a disturbing trend in physics. That is, physicists attempted to give metaphysical explanation of their theories, and as such their physics leads to ‘the incessant upheavals that theoretical physics has suffered, and, consequently, the discredit into which this science has fallen in the mind of many physicists.’ (Duhem 1892, 11) In this specific case he refers to the mechanical theories, as contrasted with his metaphysics-free energetics. But in principle this criticism applies to any attempt at giving (metaphysical) explanation (i.e. explanation with regard to the causes of the phenomena represented in the theories). ‘[Such physicists] believe they are contemplating the very structure of the world but will have before their eyes only a fragile construction soon to be destroyed to make room for another.’ (*Ibid.*, 15) If science is to be associated with metaphysics, the incessant rise and fall of

metaphysics will always accompany science too. Thus, instead of being an autonomous discipline, physics becomes dependent on metaphysics. ‘In that way, far from giving it a form to which the greatest number of minds can give their assent, we limit its acceptance to those who acknowledge the philosophy it insists on.’ (Duhem 1962, 19) He finds his ideal model of universal consent in mathematics.

Duhem defines explanation as an attempt ‘to strip reality of the appearances covering it like a veil, in order to see the bare reality itself.’ (Duhem 1962, 7) By this definition, then, every attempt at explanation is bound to be metaphysical, since speaking about objective reality (things in themselves) means speaking metaphysically.

What actually is physics capable of? For Duhem, physical theories are only about appearance. He explains this by laying out the structure of physical theory. At the first level, human mind encounters the domain of *facts*, the domain of events directly perceivable by the senses. Each observation, each experiment, presents a new fact. Next, through induction, the mind arrives at *experimental laws*.<sup>8</sup> So, from the observation that different bodies—for instance, a piece of amber, or glass or copper—when similarly rubbed, can attract a pith ball suspended on a silk thread, the mind concludes that under the same conditions this kind of event can happen to *all* bodies. This is the experimental law, which can be expressed as, ‘Through suitable rubbing, all bodies are electrified’. We can see here laws have substantially richer contents

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<sup>8</sup> Duhem is aware of the need to explain further and justify the mechanism of the inductive procedure, but he says it’s the job of philosophers. (1892, 2)

compared to the facts given by observation, yet they do not speak about the things themselves. They do not explain why all bodies act in that way.

The next level in physical theory is *theoretical level*. Theories are merely classification of (experimental) laws of similar kind, and this classification may be completely artificial. The purpose of theorizing is to help scientists to retain in memory a variety of isolated laws as a small number of propositions and definitions stated in the language of mathematics. (1892, 2) Duhem thus defines physical theory as ‘a system of mathematical propositions, deduced from a small number of principles, which aim to represent as simply, as completely, and as exactly as possible a set of experimental laws.’ (Duhem 1962, 19) In this way, theories have no more content compared to laws.

Therefore, in ontological terms (which corresponds to the richness of content), physical theory is as “deep” as empirical laws in its penetration of the world. That is, ultimately physical theory is about appearance.

If that’s the case, if theory is not about description or representation of reality, what is its use? First of all, a theory is useful as a representation of a number of experimental laws, just as an experimental law is useful as a representation of diverse facts. Following Mach, Duhem regards theory as an economy of thought. (1962, 21; Cf. his review of Mach’s *Mechanics*). Secondly, closely-related to the first, theory is also a classification of the experimental laws. It groups laws of similar kind. (1962, 23-25ff)

In his earlier works, Duhem takes this classification as mostly artificial, not as it is in nature. However, in his later work (1962), he goes further. When such a classification seems to work so well, it incites in the scientists aesthetic feelings, which in turn persuades him to believe that theory contains *natural* classification. In some cases, a classification seems so ordered, each experimental law finds its place in the classification in a very neat way, that the scientist finds it hard to not believe that the classification reflects the way the real order of the world, despite his inability to prove it. At this stage the scientist believes that she has reached a natural classification. However, even here Duhem insists that this is only a matter of representation, not explanation.

‘[P]hysical theory never gives us the explanation of experimental laws; it never reveals realities hiding under the sensible appearances; but the more complete it becomes, the more we apprehend that the logical order in which theory orders experimental laws is the reflection of an ontological order, the more we suspect that the relations it establishes among the data of observation correspond to real relations among things, and the more we feel that theory tends to be a natural classification. (Duhem 1962, 26-27)

So here we see that the properties of things in themselves can be inferred from knowledge of appearance, when it reaches the stages of natural classification, but it is still limited that it can't reach explanation. Physics eventually *can* lead to natural, not merely artificial, classification. But, strictly speaking, it is still not metaphysics. It

does not give an explanation of how things are the way they are. But only that the classification is natural, is *in* the nature, not only in the mind of the scientists.

To return to an earlier point, the task of physicist is not to provide *explanation*, understood as an attempt to get at things in themselves and provide the underlying causes in the essence of things. To provide an explanation the scientist is demanded to fill in the gap with her metaphysics. If science is not independent from metaphysics, then it cannot be a common endeavor, since it has gone too far beyond the available evidence.<sup>9</sup> Again, the main reason here is that we don't have direct access to the causes in the essence of things due to the limitation of our intellect. (Duhem 1893, 31) The farthest we can reach is natural classification.

However, while physics and metaphysics are (should be) mutually independent; metaphysics should be informed by physics—as a matter of fact, metaphysics starts with physics but then it steps further than physics to induce the properties of the substances as causes. Though, as mentioned earlier, metaphysics precedes physics in the order of excellence concerning their subject matter, physics precedes metaphysics in the order of logic. 'Any metaphysical investigation concerning brute matter cannot be made logically before one has acquired some understanding of physics.' (Duhem 1893, 32) Because of the subtlety of physical theories, 'the metaphysician should know physical theory in order not to make an

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<sup>9</sup> In his "Physics of a Believer", where he rejects the accusation leveled against him that his physics is influenced by his faith (due to, among other things, his distrust that scientific explanation is never complete by itself), he emphasizes this point again: that nothing in his physics is unacceptable to anyone who does not share his metaphysical convictions or religious beliefs. In short, his metaphysics is irrelevant to the physics. (Duhem 1962, 273, 279)

illegitimate use of it in his speculations.’ (Duhem 1962, 291) This substantiates my statement earlier that despite his insistence that physics should be independent from metaphysics, Duhem is not hostile to metaphysics. What he opposes is a priori metaphysics from which physical theories are supposedly to be deduced. On the contrary, metaphysics should be informed by physics. For different reasons, I shall argue in the last chapter that a scientifically informed metaphysics is possible.

But first, I have to clarify my key term, “metaphysics”, which so far I have used very loosely, interchangeably with “worldview”, “worldpicture”, even “philosophy”. In the following I will first justify my use of the term “metaphysics” and how it will be used in this thesis, formulate the main question I shall try to answer in this dissertation and give an outline of the arguments and chapters of this thesis. Duhem’s discussion will inform me later when I try to define metaphysics.

## **B. Formulation of the Problem**

### **1. Preliminary formulation of the problem: on the aim of science**

To formulate the problem in more familiar terms in philosophy of science, I may say that it is about the issue of the aim of science. There has been a long history of debates about the aim of science. This problem can be construed as a normative problem of what science should (ideally) attain. It may as well be formulated as an epistemological question about what kind of knowledge of the world science is capable of providing us. Difficulties in specifying “the aim of science” have

prompted some philosophers to even state that there is no agreed upon aim of science, but scientists have their own different—and probably personal—aims in doing their scientific work. In this last construal *the* aim of science ceases to be a problem in philosophy of science.

The main question I address in this dissertation can be stated in most general terms as follows: *whether science provides us with increasingly better knowledge or description of what the world is like.* (One may add an unnecessary emphasis here—‘what the world is *really* like’—to make the point that the aim is not merely one about “(saving) the phenomena”.) Later in the next section I will try to refine it. There are difficult questions about how to characterize “increasingly better knowledge” in more specific terms—e.g. in terms of the notion of truth, or higher predictive accuracy, or the scope of explanation—but I shall not discuss this here. One thing we may quickly note is that this question does not assume equal urgency among philosophers of science across the spectrum of views about the aim of science, which I shall show in the course of the chapters here. Nevertheless, despite all the difficulty to come into an agreement on this issue, it is hard to deny that in itself a question about the aim of an activity constitutes an important problem in a discussion about that activity.

To emphasize, in this dissertation I shall not elaborate different views about the aim of science, but my question is neutral relative to views about aim of science or independent of that discourse. The question formulated above asks about actual state of affairs of modern science; the way I try to answer it here is by looking into



the epistemological warrant given by science: epistemically, what is science capable of giving us? What kind of knowledge about the world, or how “deep” into the world can it penetrate?<sup>10</sup>

For philosophers who do not agree with the statement that science aims at “a description of what the world is really like”, the outcome of the efforts to answer this question is sometimes called “metaphysics”, used mostly in pejorative sense as something unfounded in empirical evidence; in other words, speculation. The appearance of the forbidding term in the title of this dissertation, therefore, needs some explanation.

## **2. On “science”, “worldview”, and “metaphysics”,**

Given the aim of science as describing the world as it really is, then if it can be achieved we would not need the other terms, since whatever is described in those terms would be subsumed under “science”. However, such an aim of science can hardly be taken as something given. In general, despite the notorious difficulty of defining what science is there seems to be a very wide agreement among scientists about what they do. Yet, attempts by philosophers of science to define, or demarcate, science almost always ended up in quandaries. One of the main problems here seems to be the fact that through their work the scientists themselves constantly use a variety of available methods, widen its scope, and explore new territories.

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<sup>10</sup> The language of “depth” refers to my discussion in Chapter 2 about whether the knowledge science gives us is limited to phenomena, or noumena (things as they really are), or probably there are levels in between the two—for example, “structure”.

Nevertheless, there seems to be an agreement. In terms of its method, anything is possible as long as it is empirically verifiable/falsifiable in most general terms (directly or indirectly); in terms of end-product, science is what appears in academic, peer-reviewed journals. The last criterion of course is too contemporary; it applies mostly to the twentieth century history of science. But what is important here is agreement among scientific communities—though probably tacitly—about whether such works are scientific. In terms of history of science, in retrospect, in most cases—say Galileo Galilei, Isaac Newton, or Robert Boyle, Lamarck, Darwin—there would not seem to be serious doubts to call a work as scientific or the person as scientist, despite the fact that they refer to what we now understand with “science” with different terms, like philosophy (e.g. natural philosophy, or mechanical philosophy).

Such a loose definition of science surely would not be adequate for borderline cases. But attempts to specify what science is further than this would drag us into philosophical quandaries, which today’s philosophers of science are very familiar with. The very emergence of philosophy of science as a discipline in the end of the 19<sup>th</sup> century, could actually be attributed to this attempt at construing science in a systematic way, either by defining (or demarcating) science with reference to the so-called “scientific method”, or “logic of discovery”, or by attempts to reconstruct science by giving it some foundation and formalize its theories. Yet, it seems fair to say that the main lesson from this early episode in philosophy of science concerns the futility of such attempt. In its history scientists themselves have constantly tried to

define or re-define what they do. The subject matter of philosophy of science is not static but constantly changes.

As such it wouldn't be inappropriate to say that "science" is always fluid, and changed by the scientists constantly; that what counts as science is constantly negotiated by scientists (and philosophers of science)—as long as one does not read too much into this kind of statement to the level of saying, for example, that scientific theories are nothing but the result of social construction. For my purpose here it suffices to characterize science in very general terms as that which is widely agreed by community of scientists of a time, with the recognition that its boundaries need not always be well-defined. My main question, then, remains: can science provide us with a picture of the world? For naïve realists, this is a trivial question. But the fact that there are disagreements about this since the earliest days of modern science shows that it is not.

To capture this question some would use "scientific worldview" to refer to that view of the world provided by science; others use "metaphysics" to indicate that such a view of the world is beyond empirical ("physical") science as such but might be based on science.<sup>11</sup> "Worldview" would seem to be the most natural term since literally it means view of the world; "scientific worldview", then, simply means scientific view of the world; or a view of the world based on scientific theories.

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<sup>11</sup> Two other candidates are "ontology" and "cosmology". But "ontology" is too restrictive, since it deals mostly only with questions of existence. What I aim to do, on the other hand, is broader than that. In its 19<sup>th</sup> century usage, as used by Pierre Duhem, for example, "cosmology" almost overlaps with metaphysics; but to use this term now for my purpose is quite problematic, since it has gained significance as a branch of science about the universe (its age, its formation, its dynamics, etc.) So I shall not consider these two terms in the following discussion.

However, this expression has gained a significance which is too rich to represent what I want to convey with the question. It is a translation of the German “Weltanschauung”, which connotes, in the words of *The American Heritage Dictionary of English Language*, ‘the overall perspective from which one sees and interprets the world’; or ‘a collection of beliefs about life and the universe held by an individual or a group.’ Besides, this expression has also been widely used as a technical term in sociology and anthropology to connote a more comprehensive meaning. *The Blackwell Dictionary of Sociology*, for example, has this entry which explains what worldview is :

Within a culture, a worldview is a general way of looking upon the universe and our relation to it, a general set of assumptions about the meaning of life, about what is important, and about how things work. ... A worldview is typically associated with a group or society....

*Weltanschauung* is also a concept with which the German philosopher and historian Wilhelm Dilthey is most closely associated. In Dilthey, this term is actually developed to distinguish human sciences from natural sciences. In contrast to the latter, the categories of the former are value, purpose, and meaning, which are better expressed by “worldview”. In human sciences, this expression is intended to refer to reflection on the ultimate sense of our existence. ‘A worldview constitutes an overall perspective on life that sums up what we know about the world, how we evaluate it emotionally, and how we respond to it volitionally.’ (*The Cambridge Dictionary of Philosophy*, Cambridge University Press, 1999) But what I want to capture with this

question, and discuss in this dissertation mostly concerns cognitive claims about the world. The rich content of “worldview” as understood very broadly above would not do justice to this, and may as well mislead us, since it contains much more than our knowledge of the world. Even in some works of philosophy of science, worldview sometimes gets this very broad meaning. For example, Feyerabend, in an article titled “Quantum Theory and Our View of the World”, define “worldview as follows: ‘a collection of beliefs, attitudes and assumptions that involves the whole person, not only the intellect, has some kind of coherence and universality and imposes itself with a power far greater than the power of facts and fact-related theories.’ (Feyerabend 1994, 152) As such, “worldview” is not quite appropriate to represent what I aim at in this dissertation.

Another candidate expression is “metaphysics”. At the outset, this does not seem an attractive option, since being “metaphysical” seems a vice in today’s philosophy. “Metaphysics”, which in its original Aristotelian understanding denotes the highest—in logical, epistemological as well as ontological orders—kind of knowledge now at best is regarded as unnecessary excess baggage of a theoretical account of the world. In philosophy of science, especially, metaphysics—understood simply, broadly, and vaguely as claims ungrounded in empirical evidence—is something that most philosophers want to avoid in their account of scientific activities. As “science” has oftentimes become an honorific term, “metaphysics” has been used by philosophers of science mostly in a pejorative way, and understood as some ungrounded intellectual speculation. As opposed to science, metaphysics has

come to characterize a discipline where agreements among its practitioners are very difficult, if not impossible, to achieve. Despite all these, there are good reasons I shall give later to use this word.

In general the dictionaries admit that metaphysics has meant many different things. Among other things, it has traditionally been understood as an attempt to explore the realm of the world beyond experience, or to compile an inventory of what sort of things ultimately there are, or to establish indubitable first principles as a foundation for all other knowledge.<sup>12</sup> Aristotle's metaphysics is understood mostly in this sense. In its relation to more specific sciences, it may be seen as providing the ontological and epistemological foundation of them. In later historical period, these lofty aims had engendered centuries of disagreements and ultimately criticisms since they are regarded as in principle unattainable.

This understanding is what many dictionaries refer to, and in some modern dictionaries the negative sense attached to this word is also conveyed in their entry on "metaphysics". To take one dictionary definition (*The American Heritage Dictionary of English Language*), metaphysics is described as synonymous to philosophy, but also 'the branch of philosophy that examines the nature of reality'; 'the theoretical or first principles of a particular discipline'; and, interestingly, also one that indicates the low regard given to this discipline: 'A priori speculation upon questions that are unanswerable to scientific observation, analysis, or experiment', undertaken by 'excessively subtle or recondite reasoning.'

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<sup>12</sup> *A Dictionary of Philosophy*, Laurence Urdeng Associates Ltd, 1979

However, more positive meanings are given in philosophical dictionaries and encyclopedias. For example, metaphysics may also be understood as similar to natural science in terms of its attempt to characterize reality, but in a wider scope: whereas natural sciences deal with specific parts of reality, it deals with reality (or existence) as a whole. The one which most closely capture the question I raise earlier is described in *The Cambridge Dictionary of Philosophy* (Cambridge University Press, 1999). Here it is understood as ‘the philosophical investigation of the nature, constitution, and structure of reality’, with two characteristics that distinguish it from science: *first*, it is broader in scope than particular sciences; and, *second*, ‘it is also more fundamental, since it investigates questions science does not address but the answers to which it presupposes.’

As such, metaphysics seems a better choice. It is true that, as a legacy of Logical Positivism, its use in philosophy of science is mostly pejorative, denoting something based on speculation, not evidence; or it is conceived as an enterprise producing claims about what the world is really like that are independent from or indifferent to contemporary science. However, there are arguments to use it in more neutral terms. *First*, compared to “worldview”, it does not have the association of a totality of one’s views and attitude. Furthermore, “worldview” includes myths and narratives based completely on faith. But metaphysics is understood as a rational enterprise, the results of deliberation (observation, reflection, argumentation). *Second*, traditionally, it precisely is about knowledge about the world though in a wider scope. The main objection is not so much about its aim, but mostly about its “content” or

theories (how the constituents of the world is understood in terms of substance, essence, accident, etc.) and, most importantly, its alleged certainty. But some philosophers of science now speak more positively about metaphysics and in general they do not have this presumption of certainty. As an example, one can mention Abner Shimony who tries to build what he calls “experimental metaphysics”, which is a scientifically credible metaphysics (or “based on” scientific theories).

One final note on the use of “metaphysics”: in the last chapter of this dissertation, when giving an illustration of how a metaphysics can be based on science, as a matter of fact my example has to do more with theology. In the broad sense of the term, metaphysics may include theology. My own justification for this is that what can be said here may apply equally well to metaphysics of other sorts. Besides, as I shall show later, in theology the methodological problems of grounding a metaphysical enterprise on science appear more vividly, as such it is more effective to serve as an illustration. Not least important, the literature on theology and science has increased quite significantly, so some of my problems have also been treated in some ways there.

### **3. Formulation of the Problem and Methodology**

My initial question is *whether science provides us with increasingly better knowledge or description of what the world is really like*. In the remaining of this dissertation I shall use the term “metaphysics” to substitute “knowledge or description of what the world is really like”. As I mention earlier, a naïve realist



would probably equate the two. By not doing so, my intention is not to reject naïve realism as a possible position right away. But we need a starting point where there is an agreement among different philosophical orientations (realism, instrumentalism, anti-realism, etc.); and the starting point is some minimum understanding of what science is, as stated in the rough definition earlier, which does not specifically mention what the aim of science is. This is necessary before I take the next steps, as elaborated in the next chapters.

Though the main problem has to do with metaphysics, my question is about the epistemology of science. Some recent work on philosophy of science divides the area of study in philosophy of science into two: *epistemology of science* and *metaphysics of science*. An example is David Papineau in his *The Philosophy of Science* (1996). For him epistemology of science deals with the justification of claims to scientific knowledge, while metaphysics of science investigates philosophically puzzling features of the world described by science. The book itself concerns exclusively with the epistemology; and the articles contained in this anthology are almost exclusively on scientific realism debate.

The same division is used by James Worrall in an encyclopedia entry on philosophy of science<sup>13</sup> as well as James Ladyman in his *Understanding Philosophy of Science* (2002). He gives examples of questions in the epistemology of science:

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<sup>13</sup> In his encyclopedia article, Worrall divides philosophy of science into three areas: theory of scientific knowledge (which is about epistemology of science); “scientific metaphysics” (the questions here are similar with what Papineau and Ladyman call “metaphysics of science”; and foundational issues from current science (which contain both epistemological and metaphysical questions arising from particular sciences). (Worrall 1998)

What is the scientific method? How does evidence support a theory? Is theory change in science a rational process? Can we really be said to know that scientific theories are true? (Ladyman 2002, 7) While the metaphysics of science tries to answer questions related to what scientific theories tell us about how the world is. There are general questions such as ones concerning the notions of necessity, natural laws, or determinism, but also what science is telling us about the overall structure of the universe.<sup>14</sup>

In this dissertation I reserve the term “metaphysics” for issues concerning mostly the existence and the structure of the world. Even though ultimately my goal is to talk about ways in which metaphysics can be said to be based on science, my discussion almost wholly falls on the area of epistemology. My question does not concern what science is telling us about the world, but whether science is capable of doing so.

This construal is also similar with the framework used by John Worrall in his *The Ontology of Science* (1994). Worrall divides this anthology into two parts. The first is about the general issue of scientific realism—‘the general issue of reasonable attitude to take towards the entities postulated by currently accepted theories in developed sciences.’ (Worrall 1994, xi) This part contains arguments for and against scientific realism, and also some articles which try to specify kinds of realism. The second part, about “the ontology of science” (or, metaphysics of science), assumes a

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<sup>14</sup> An example of such question, and one which is popular, is: should we believe in the existence of unobservable entities? However, even this question is partly (or even ultimately) an epistemological question, since it asks about how one’s belief and what may justify it.

realist attitude towards successful scientific theories. Given such a realist attitude, there are questions about what ontological commitments a realist attitude towards a particular theory entails: What exactly does ‘quantum realism’ amount to? Suppose quantum theory is true; what does that tell us about the furniture of the world? What is involved in being a realist about ‘genes’? (Worrall 1994, xii, xxvi)

At this point I can specify my initial question as one about whether science is capable of providing us with a metaphysics, or, in other words, whether it is possible to have a metaphysics which in some way can be said to be based on science. With this I shall not ask about the possibility of metaphysics, but *given a certain (i.e. “modest”) realist view of science, to what extent can a metaphysics be based on scientific theories? What are the limitations of this enterprise?* In terms of the two problem I mention earlier (“can science give us a worldview/ metaphysics?” and “is science grounded on metaphysics?”), I will deal only with the first question. In general I shall not deal with specific projects mentioned earlier, such as scientific philosophy, naturalism, or experimental metaphysics. Instead, I will mostly look at whether science has an epistemic capability to provide us with a worldview/metaphysics. To phrase it in McMullin’s expression (see Chapter 3): whether the reach of evidence of scientific theory extends or can be extended into the realm of metaphysics. Only in the last chapter shall I give an illustration of one such project.

In this dissertation I try to answer this question by focusing mostly at relevant discussions in recent philosophy of science. Two main issues that stand out concern

scientific realism and interpretation. The first is about justification of belief in the content of scientific theories as saying something about the world, while the second is about its interpretation.<sup>15</sup> I shall briefly explain the two issues by way of introduction.

*a. Realism debate and the aim of science*

Metaphysics addresses the ultimate questions about the nature of reality. An “experimental” or “scientific” metaphysics, then, could sit well only with a certain aim of science, i.e. one that takes science as aiming, roughly speaking, to produce true theories about the world as opposed to, for example, the view that the aim of science is simply to produce useful instruments of predictions. In the face of the prevalent skepticism as to whether science has anything to do with reality at all, the first issue that would arise in grounding metaphysics on science is surely the very capability of science to provide such a ground. When a scientist makes a statement about the natural world, what is it exactly that he claims? Do scientific statements represent the world literally? And how should such claims be evaluated? This kind of questions falls in the rubric of realism-anti realism debate. At first it seems that to make use of scientific theories as the ground of a metaphysics, one should be a realist of some sort, and be able to provide some justification for it.<sup>16</sup>

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<sup>15</sup> Cf. Redhead (1995, 16-20), on four issues related to issues in realism need attending to as some sort of presuppositions of the experimental metaphysics.

<sup>16</sup> Some naturalists reject the demand that they need to establish an independent warrant for the epistemic role they grant to science before embarking in this enterprise. This is a concern widely shared by critics of naturalism, but one which has also been for long objected to by naturalists such as Ernest Nagel. (Rosenberg 1996, 1-2) In his survey of naturalism in philosophy of science, Rosenberg

The realism debate has occupied the center stage of recent philosophy of science. Some philosophers of science see that this debate has passed its useful and interesting phase, and should be just abandoned, and other philosophers, like Arthur Fine, have even declared that ‘realism is well and truly dead.’ However, it can be also clearly seen that the debate keeps on gaining new energy and fresh perspectives to deal with the old problems. In his recent survey article of the debate, Psillos (2000) even notices that ‘the realism debate is currently going through its renaissance period.’ (Part of the vitality of the issue, it seems, owes to the “turmoil” created in the Science Wars by claims that science has nothing to do with reality but is simply the result of social construction. However, a good portion of the realism debate today proceeds independently of this concern.)

With the disappearance of the extremes in the debate—naïve realism or naïve anti-realism—the main question for many participants of the debate now is what kind of realism is possible. One main question in this direction is, How far into reality can science probe? The possibility question here is related mainly to the question of what science is capable of giving us epistemically; or, what conclusions about the world

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notes that Laudan’s naturalism does not demand realism. But in the end he couldn’t keep silent about the epistemic goal of science in order to distinguish himself from relativists. Despite his criticisms of scientific realism in general, he is much more opposed to social constructivists’ relativism and van Fraassen’s constructive empiricism. As such he needs to specify this new goal more precisely. As for van Fraassen, he regards the interpretative question of “how could the world possibly be the way the theory says it is?” (see the next section) makes equal sense to the realist as well as (anti-realist) empiricist. “We may grant that theories are the sort of thing which can be true or false, that they say something about what the world is like; it does not follow that we must be scientific realists.’ (van Fraassen 1991, 3-4)

can we draw from certain scientific theories as warranted by the evidence that supports the theories.

***b. Interpretation***

Philosophers who try to develop an “experimental metaphysics” do perceive their job as providing interpretation of the highly technical formalisms of contemporary physics with the view of answering issues about the nature of reality. (Redhead 1995, Shimony 1992, French 1999) Interpretation is inevitable in any attempt to ground metaphysics on science, if only for the fact that the two are separate disciplines (we can’t expect a discussion about the nature of reality in physics journals), indicating, among other things, that the scope of their subject matter is different. One common way to distinguish the two is to say that science is about appearance, while metaphysics is about the reality behind it. Interpretation is what bridges the two.

Interpretation is commonly understood as an answer to the question of what the world would be like if the theory is true. This term has been used to refer to at least two different “levels”: interpretation of formalism and metaphysical interpretation. Van Fraassen uses the term “interpretation “in two senses: Scientific theories are not merely *representations* of the world, but interpretations of it, and, secondly, the metaphysics arising from them is a result of interpretation of theories. (van Fraassen 1991, 8)

One field in which interpretation plays a prominent role is surely quantum mechanics. But as a matter of fact even in classical physics the two senses of

interpretation come into play. One implication of this acknowledgement is that the result of interpretative work is hardly unique. To take an example in quantum mechanics, it is now accepted that formalism does not *uniquely determines* its interpretation. In other words, there is the issue of underdetermination. (Cushing 1994, 114) This problem is unavoidable in any interpretative undertaking, including in classical physics and in relativity theory. Despite the urgency of this problem of interpretation Sklar notes that general discussion of this problem (as opposed to discussions of interpretation of particular theories) has not been conducted systematically. “An intensive, extended and systematic methodological study of its aims and methods is long overdue.” (Sklar 2000, 742)

In relation to the issue discussed here, if even scientific theories are underdetermined by available empirical evidence, the same predicament could be expected to apply, *a fortiori*, to the case of metaphysical views grounded on them. But this point poses a serious question to any attempt at grounding metaphysics on science. Most of the rejections of metaphysics by philosophers, at least since Hume, are motivated by the impossibility of resolving metaphysical issues, as shown in its history. Now, in light of the potential acute problem of underdetermination, would a metaphysics that is grounded in science fare better than the traditional metaphysics in terms of resolving its disputes? Doesn't the problem of underdetermination undermine the belief in the continuity of empirical science with metaphysics? How should we see the role of interpretation in our attempt to know the world?

### **C. Outline of the Chapters**

In Chapter 2, “Qualifying Realism”, the main question I shall start dealing with concerns what kind of realism is possible in the face of antirealist many criticisms. After considering several versions of realism, I will argue that after imposing many qualifications to the thesis of realism we can still have a position that can be genuinely called realism and distinguish it from the anti-realist position. I will first recapitulate important arguments in the debate, and then propose several ways to qualify the realist thesis.

In Chapter 3, “Inference to the Best Explanation and Its Limitations”, I discuss one of the most important positive arguments on the realist part, which concerns Inference to the Best Explanation, or abduction/retroduction. It is the reasoning used to infer from empirical data to the unobserved and unobservable entities or processes as part of the causal explanation of the phenomena involved. The result of such an inference includes ontological claims. This chapter will investigate when and under what conditions it works and does not work. It will turn out that realists have to make more “sacrifices” by further qualifying their realism to maintain their beliefs. Nevertheless, I would argue that the resulting position is still worth-calling realism, and still contains important claims that differentiate them from the antirealists, though it might not be sufficient to support a full-fledged “experimental metaphysics”.



In Chapter 4, “Realism, Interpretation, and Metaphysics”, I shall first suggest that the first move toward a tenable realist position is to disengage scientific realism from metaphysical commitments of a global scope or a particular view of reality. But ultimately realists want to assert some particular view of reality, or some version of metaphysical realism. Here I defend Searle’s external realism, which is a very modest/minimal metaphysical realism, from criticisms by its opponents. External realism states that there is a way that things are, without specifying a particular view of ‘the way that things are’. Ontologically it is almost empty, but is sufficient to challenge idealists as well as social constructivists. Finally, as mentioned earlier, I need to deal with the problem of interpretation as a way to draw out some “metaphysics” from scientific theories. Interpretation is what bridges scientific evidence and theory and some particular view of reality. Due to the problem of underdetermination, the evidence itself might not logically imply certain ontological view, but at least it suggests and puts some constraints on tenable views. I will end this chapter with some conclusions of the discussions up to this chapter. I shall also mention, based on the foregoing discussions, the possibility as well as the constraints of and limitations on any attempt to construct a “metaphysics” supposedly based on science.

Only in the last chapter (Chapter 5) shall I give an illustration of one such project, namely the attempts to draw out theological implications of scientific theories. Part of my justification for this is that it is here that the attempt to somehow “ground” metaphysics (understood in the broader sense) on science is taken quite

seriously, and the field of science and theology<sup>17</sup> has grown very fast in recent decades and produced an enormous literature both in the academic as well as popular context. On the other hand, projects like “experimental metaphysics” are undertaken by very few philosophers, with the consequence that the literature is also limited. I shall provide further, more philosophical, justifications for this choice in the beginning of Chapter 5. \*\*\*

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<sup>17</sup> In this context, theology is understood more as philosophical or constructive, not dogmatic, theology.

## **Chapter 2:**

### **Qualifying Realism**

As indicated in the previous chapter, the question about science's capability to afford us with knowledge about the world has been discussed in contemporary philosophy of science under the rubric of realism-anti realism. The issues of the debate have been around since the late 19<sup>th</sup> century. In the past few decades it has occupied center stage, and so far it seems there is no indisputable winner. Some philosophers of science see that this debate has passed its useful and interesting phase, and should be just abandoned, while others, like Arthur Fine, have even declared with strong words that realism is well and truly dead. (Fine 1984, 21) Besides the recurrent problems that beset the realist position, the critics of realism find that problems in the interpretation of quantum theory have contributed significantly to its death. Besides Fine, van Fraassen definitely has said so. More specifically, he takes the experimental violation of the Bell inequality as going decisively against realism. (van Fraassen 1982, ) Writing almost two decades ago, Fine saw the efforts to save realism at that time as the first stage in the process of mourning and denial.

Yet, judging from the vigorous debate in the last decade, that death announcement now seems quite premature. In his recent survey article on the debate, Psillos even notices that ‘the realism debate is currently going through its renaissance period.’ (Psillos 2000, 706) If nothing else, the appearance of many books with “scientific realism” in their title indicate this phenomenon. Undoubtedly, the defenders of realism have learned many lessons from its critics, and they are now defending different versions of realism as their response to these criticisms. But, as noted by Psillos, they can be seen as participating in a common project: ‘that of characterising carefully the main features of the realism debate and offering new ways of either exploring old arguments or thinking in novel terms about the debate itself.’ (Psillos 2000. 706)

An interesting feature of the recent debate is the fact that the two opposing positions are moving closer to each other. In the face of criticisms the realists have modified their position on many issues and made it more and more modest, while the anti-realists have also conceded many points to their opponents. Thus many differences that at first seem fundamentally irreconcilable tend to dissolve as each side makes concessions.

For example, van Fraassen, one of the strongest opponents of realism, has changed his position during recent years. In *The Scientific Image* (1980), his constructive empiricism departed quite significantly from earlier empiricists. For example, he accepted that claims about theoretical entities are not reducible to observational statements. This position thus differs from realism in its insistence that

(empirical) evidence never warrants us in believing in a theory more than its being empirically adequate. He thus makes the distinction between belief in and acceptance of a theory, and acceptance of a theory involves as belief only that the theory is empirically adequate; empirical adequacy is truth with respect to observable phenomena. In his *Laws and Symmetry* (1989) he goes further by admitting that his empiricist arguments might not be sufficient to rationally persuade his opponents. Neither realist nor anti-realist positions are irrational, but both equally lack objective resources or grounds to persuade the proponents of the other position. The concessions are more telling in his *Quantum Mechanics* (1991), when he expresses that empiricists are equally interested in the *content* of a theory (what it says the world is like). Thus he finds the interpretation of theory (which tries to answer the question about what kind of world might be projected by the theory) as a common project which interests the proponents of both realism and anti-realism (though with quite different motivations). As I shall discuss, the same tendency of conceding more and more to the opponent also shows up in the realist camp. Of course, this is not to say that realists and anti-realists have reached agreement as to how to answer all the questions that arise in the debate. Ultimately, they still differ on the crux of the issue as to whether science has given us better and better knowledge about the real world, or whether it has the capability to do so, and to what degree.

It is interesting to note here two recent books on scientific realism that end up with an acknowledgment of the strength of their opponent's criticisms and the difficulty in judging which position has stronger arguments, i.e. Kukla's (1998) and

Psillos's (1999). To be more precise, the negative arguments of each of the realist and anti-realist positions against their opponents are much more successful than the positive arguments for their own positions, such that 'They've swept all the positive arguments away, leaving an empty field.' (Kukla 1998, 152) Though in that work he seems to lean more toward realism, Kukla's conclusion is that the debate is irreconcilable *in principle!* Psillos still wants to hold on to his realism, and he sees that by learning from its opponents' arguments, it gets stronger. But this allegedly stronger realism has departed significantly from the optimism of earlier (prior to the 1980s) scientific realism. What he is able to assert with confidence is that 'realism is not an all-or-nothing doctrine.' (Psillos 1999, xxiv, 113, 161) This is a realism which has qualified the claims of earlier scientific realists and made them much more modest, but, expectedly, can still be genuinely called realism and distinguished from the anti-realist position. Of course, characterizing a position as "realist" or "anti-realist" is not merely a semantic issue. What we want finally is a reasoned affirmation of science's capability to provide us with knowledge about the world.

In the following I am not so much interested in recapitulating the arguments from both sides, as in seeing what kind of a realist position—however minimal or modest it is—is tenable, after considering the constraints on any tenable realist position as revealed mostly by anti-realist criticisms. Such a position will be evaluated mainly from the point of view of the warrant that can be given by evidence. Undoubtedly I will touch on a few issues that have occupied the center stage of the realism debate, but I will leave a side certain issues which I see not directly relevant

to my interest (these are issues like observation-theory distinction, correspondence theory of truth, etc). First, I shall discuss some issues and standard arguments for and against scientific realism. The anti-realist criticisms will be seen mostly as constraints within which a tenable realist position (i.e. if it's still worth the label "realist") can be construed. I shall also see, starting with this chapter and continued in the next two chapters, the consequences of holding such minimal realism for our ability to claim knowledge about the world as based on science.

## **1. For and Against Realism**

By way of introduction, scientific realism could be described as a thesis asserting that scientific theories describe the world and are true or false in virtue of how the world is independent of our awareness of it. This thesis can be seen to have two main components: metaphysical and epistemological.

The *metaphysical* component asserts the existence of certain entities or structures postulated by science, independent of the awareness of the scientists individually or as a social community. It holds that objects of scientific knowledge do exist, to which theoretical terms used such as "true" or "acceptable" theories genuinely refer. Acceptance of a theory here means at the same time acceptance of the existence of the entities it postulates in the world. The world, then, is believed to be populated by entities postulated in the accepted (or mature) theories, such as electrons, quarks, lines of force, curved space-time, genes, etc. As I will discuss later,

however, the commitment to realism can be construed as independent from the commitment to those specifics of the world. Anti-realists, on the other hand, include not only the idealists, who deny the existence of a mind-independent world, but also empiricists, whose claims are limited only to the realm of phenomena (and sometimes conjoined with the view that statements of unobservables are reducible to that of the observables), as well as social constructivists, who regard those entities and the so-called natural laws that describe them as social constructions, or the products of human creation agreed upon by the community of scientists.

As suggested by many realists, scientific realism—in the limited sense of the term, as discussed in this and the next chapters—needs to be disengaged from metaphysical commitments of a global scope as well as to a particular view of reality. Of course, by saying that science can give us a view about the world, realists have an interest in formulating a certain view of reality. But this is a separate issue posterior to the main question of realism, which is epistemological. I shall discuss this issue more elaborately in Chapter 4, when discussing the relation between realism and metaphysics. At this point suffice it to say that the difficulty in formulating a particular view of reality (like the most prominent case of quantum mechanics) does not necessarily refute realism, as Fine would suggest.<sup>18</sup>

The *epistemological* component asserts that we can know entities or structures mentioned above that inhabit the world and the laws that govern them. A variation of

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<sup>18</sup> The death of realism was ‘hastened by the debates over the interpretation of quantum theory, where Bohr’s non-realist philosophy was seen to win out over Einstein’s passionate realism.’ (Fine 1984, 21)



this component includes an assertion regarding the aims of science and actual scientific practice. It is, for example, claimed by the realists that the aim of science is to find out true theories about the world. Others argue that mature and successful theories are approximately true of the world. An anti-realist might dispute our capability of probing into the reality beyond mere appearance. She might believe that the aim of science is limited to making predictions, that theories (along with the entities they postulate) are mere fictions created to be useful tools of prediction (instrumentalism), and that successful theories only warrant belief in the empirical adequacy of the theories, and do not indicate their truth (constructive empiricism). In this and next chapters we shall be concerned mainly with the epistemological component. In the fourth chapter I will discuss a minimal version of metaphysical realism.

There are a few standard arguments to argue for or against realism, some of the most important of which I shall describe shortly. The foremost argument for realism is the non-miracle argument (NMA), which was suggested by J. J. C. Smart, Grover Maxwell, and Putnam.<sup>19</sup> ‘The positive argument for realism is that it is the only philosophy that does not make the success of science a miracle.’ (Putnam 1975, 73) As regards the theses of realism (that terms in mature theories genuinely refer,

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<sup>19</sup> The three philosophers’ argument is commonly grouped into NMA. (e.g. see Fine 1998, ). But Psillos sees that actually Smart and Maxwell’s cosmic coincidence argument is different from Putnam’s non-miracle argument in that the former can’t quite be construed as an inference to the best explanation. As such, the heavy criticisms directed to the latter do not necessarily apply to the former. (Psillos 1999, 72-76) For Psillos, the latter could be viewed as a plausibility argument, so the charge that IBE is circular does not apply.

that such theories are approximately true), Putnam views them as ‘part of the only scientific explanation of the success of science, and hence as part of any adequate description of science and its relations to its objects.’ (Putnam 1975, 73) Since Putnam advances this argument, it has been subjected to many criticisms, and has been modified and refined, but the main points of contention remain.

The first premise in this argument is the *success* of science. Since this premise is supposed to be shared by anti-realists<sup>20</sup>, we should understand “success” as instrumental success, i.e. success in terms of predictions. However, instrumental success only licenses acceptance of the empirical adequacy of the theories. Between the empirical and theoretical level there is a significant gap. The realists make a jump from the observational to the theoretical level, while the anti-realists could simply say that from the premise which is about the empirical level, the only legitimate conclusion that can be drawn is also limited to that level. It is just not clear what can justify the jump from the empirical to the theoretical level. (In the next chapter I discuss inference to the best explanation [IBE] as a means of such justification.)

In a different way, we can construe statements about the aim of science as an explanation of the success of science; that the success is best understood or explained by saying that the aim of science is, indeed, true theories about the natural world. But from the same evidence of success, the anti-realists might also say that the aim of science is precisely to produce good instruments of empirical prediction. Now since

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<sup>20</sup> Questioning the very notion of “success” is actually a line of reasoning open for anti-realists, yet not pursued by many, except some social constructivists. Cf. Kukla 1998, 13-14.

the evidence for both views are the same, i.e. the success of science, while the content of the realists' conclusion is much richer, the anti-realists' argument is consequently stronger.<sup>21</sup> In other words, we can make good sense of scientific practice instrumentally, without having to argue for the stronger (i.e. harder to defend), realist claim. Instrumentalist explanation of the success of science, that it achieves those instrumental success precisely because scientists aimed to achieve just that, might not sound as plausible 200 years ago, when natural philosophy is expected to supply us with a worldview that is better than the earlier Medieval one; but today it is relatively easier to believe this.

Still another criticisms of NMA is that it is a species of inference to the best explanation (IBE). Realists' use of this kind of inference is regarded as circular and question-begging, since it (the use of IBE) is a form of IBE itself,<sup>22</sup> thus assumes the reliability of IBE. Besides, as asserted by van Fraassen, the best might not be good enough; it might be the best of a set of bad explanations. In other words the question of best explanation is separate from the question of truth. (van Fraassen 1980, ) Furthermore, just as in the above case, the instrumentalists, as an anti-realist position, can co-opt this inference to produce their own conclusion: that instrumentalist explanation is a better alternative than the realists'. Instrumentalism, which does not

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<sup>21</sup> A similar version of this argument is proposed by Boyd, who modifies this argument by saying that the success of science is best explained by the fact that the method of science, however flawed it is, aims at truth, which is believed only by realists. But anti-realists can say that the success is surely better explained if we say that the method is crafted to winnow out instrumentally reliable information. (Fine, 1998)

<sup>22</sup> So in NMA what is used is second-order IBE. I will discuss below, when discussing the underdetermination thesis, the first-order IBE as allegedly used by scientists.

require anything more than instrumental success explains the instrumental success of science better than realists' demand that scientific theories be (approximately) true.

In general, as shown by Fine (1986), realist explanations can always be transformed into antirealist explanation. He expresses this in a metatheorem: 'If the phenomena to be explained are not realist-laden, then to every good realist explanation there corresponds a better instrumentalist one.' (Fine 1986, 154) He proves this by giving a sort of algorithm of how to produce an antirealist explanation from a realist explanation of a phenomenon. Strictly speaking, this algorithm might not work in every case (for a criticism see Kukla 1998, 32-34; Psillos, 90-93). But even without such an algorithm we can see that realists almost always have more burden of proof to their claims than antirealists do. Van Fraassen, too, claims that constructive empiricism can account for any scientific practice that realism can account for. In addition, it is a significantly weaker theory. While this is not a reason to reject realism, this at least shows that anti-realists have a stronger alternative, and that, at least, it is as rational to be an anti-realist as to be a realist.<sup>23</sup>

While NMA could be considered as realists' most popular, if not the ultimate, argument, the underdetermination thesis is considered by the anti-realists as theirs. This thesis shows that theories which save the same set of phenomena or entail exactly the same observational consequences, thus observationally indistinguishable,

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<sup>23</sup> Of course, as I briefly mentioned above, this stronger theory has poorer content. But van Fraassen can easily say the the richness of the realist theory is merely illusion, since the additional content of the realist claim is mere opinion, not additionally vulnerable to empirical tests. 'It is but empty strutting and posturing, this display of courage not under fire and avowal of additional resources that cannot feel the pinch of misfortune any earlier.' (van Fraassen, 1985, 255)

are at the same time epistemically indistinguishable. Furthermore, in principle it can be shown that any theory has many such observationally indistinguishable rivals. Does it mean that belief in a theory is arbitrary? One of the consequences of this thesis is that we ought not believe in the theoretical entities or structures posited by the theory. The fact of underdetermination of theory by observational evidence is accepted by anti-realists as well as realists. Anti-realists would readily espouse agnosticism, at the very least, as their response. For them this attitude can be formulated quite simply: believe only what is warranted by evidence.

As for the realists, faced by the fact that empirical evidence cannot conclusively eliminate all except one theory, they resort to non-empirical grounds. The most popular criteria to select one of the empirically equivalent theories, in whose ontology the realist would believe, is in terms of explanatory virtues, such as simplicity, fertility, breadth of scope, or unifying power. Historically, it can be shown that inference to the best explanation is indeed a common practice in science, i.e. it has worked. (A good history of IBE, which will be discussed in the next chapter, is given by McMullin 1992.) However, there are legitimate reasons to be sceptical of this kind of inference as a global strategy for science.

For one, from the philosophical point of view, there is a question about the link between explanatory virtues and truth. What justifies the forging of this link? Van Fraassen's objection to the second-order IBE above works as well against this argument. *On logical, objective grounds* it is thus impossible to select the best from a set of empirically equivalent theories.

Secondly, historically, anti-realists can also show that although IBE is commonly used by scientists, it might not be a good inference. Just as realists can cite impressive cases in the history of science to show that it works, anti-realists can also cite examples of cases where the use of this inference has misled scientists. The so-called explanatory virtues were responsible for the appearance in history of science of Laudan's famous list of fictitious entities like phlogiston, optical ether, crystalline sphere, the effluvial theory of static electricity, the caloric theory of heat, etc. (Laudan 1981, 121-122)

Laudan's argument has more far-reaching consequences since it amounts to what is called the pessimistic meta-induction from the history of science. Anti-realists emphasize the fact that the history of science is a history of theory-change, a history of repeated overthrow of theories once considered as "successful" or "mature". In some striking cases, the change involves a radical change of ontology. So there is no theoretical or ontological continuity. If we have to draw some lesson from such a history, then it is not that science moves in the direction of truth, as realists would like us believe, but that we can inductively conclude that most probably in the future the currently successful science will be overthrown, and the underlying ontology will be revised too. It is difficult to anchor the notion of truth into this unstable body of knowledge. If there is a continuity at all, it is only at the empirical level; that the empirical content of the older theory is carried over to the new ones. Some realists respond to this by proposing "structural realism", a variant of realism designed partly to avoid this particular problem. They argue that there is continuity at the level above

mere empirical, i.e. at the structural level. Though this proposal seems promising initially, it is plagued by some difficult problems, as I will later discuss.

The next chapters in the history of realism debate mainly consists of refinements and modifications of the realist arguments by considering the criticisms, but also of attempts to revise scientific realist claims. Thus there is, for example, entity-realism, which believes in the entity postulated by mature scientific theories but not the theories themselves (Ian Hacking); there is also theory-realism, which holds the contrary; another strategy would use IBE *not* to infer to the truth of theory, but only to the causes of phenomena (Nancy Cartwright).

In general, with the disappearance of the not so modest claims in the debate, the main question for realist participants of the debate now is to specify what kind of realism is tenable. There are a few ways for realists to make refinements of their thesis to avoid the problems as pointed out by their critics. In what follows, and continued in the next chapter, I will discuss some ways out of this quandary, not by proposing a new version of realism, but by setting the limits of tenable realism. The result might be too minimal a realism, but I hope it still enables us to say that, in most cases, we have come to understand more and more about the objective world, though probably this knowledge does not warrant us to construct some kind of “naturalistic world-view” or any full-fledged “scientific metaphysics”.

## 2. Differentiation between the “good” and the “bad” parts of theories

One of the strongest antirealist arguments concerns the ontological rupture which, in anti-realists' view, have taken place frequently in many cases of radical theory changes. Responding to this problem, David Papineau suggests that a promising line of argument to uphold scientific realism, in the face of revolutionary change in science, is to make a principled differentiation between the “good” and the “bad” parts of theories, those that endure theory changes and are carried over to the new theory, and those abandoned. If such an identification is possible, we may defend the idea that in cases of theory change somehow there is still some important continuity despite the apparent ontological discontinuity. In these terms, then, realists can speak of explanatory progress in science. I will discuss two possible ways to do this.

### *(a). Structural realism*

The first of such an attempt was tried out by John Worrall through the idea of structural realism (SR). Though now it seems that the idea is not workable, it is quite instructive in many ways. The intuition behind SR had been around for sometime. Worrall himself does not claim originality to the idea, but ascribes it to Poincare and Piere Duhem. (Worrall 1989, 140) More recently, Bertrand Russell and Grover Maxwell seem to advance similar ideas, though from a different path. (Psillos 2000b)



Before discussing Worrall's idea in some detail, I will briefly review the other proposal.

The two kinds of SR, as referred to by Psillos, represent different paths. The first, proposed by Bertrand Russell, is the upward path. It begins from empiricist premises and moves upward to reach a realist position. For him what can be known is not limited to appearances, but it also does not reach the "objective counterparts" of the appearance (i.e. the things-in-themselves). Instead, scientists can know the "structure", i.e. the totality of the formal, logico-mathematical properties of the external world, which can be "inferred" from the structure of the perceived phenomena. So, structure is in between noumena (nature) and phenomena. The idea is to stay within the empiricist confines, yet also claim that the structure of the unobservable world can be known. This idea was later developed by Grover Maxwell. Russell argues that the unperceivable world is isomorphic to the structure of the phenomena, while Maxwell argues for his structural realism by taking (descriptions of) the phenomena as embedded in logico-mathematical structures.

Without reference to Russell and Maxwell, Worrall develops his idea based mainly on Poincaré's. In contrast to the the two, Worrall's structural realism takes the downward path. That is, he starts with realist premises, then constructs a weaker realist position to save them from some important antirealist objections, while trying to claim more than the antirealist claims. Worrall construes his SR as the best of two worlds, the worlds of realism and anti-realism. The realist's main argument is the

ultimate or ‘no miracle’ argument, as discussed earlier. That is, it would be a kind of cosmic coincidence or a miracle if a mature scientific theory has been successful in making many astonishingly predictions, yet what the theory says is not true. However, this is precisely what has happened in scientific revolutions, when an accepted scientific theory is refuted by another theory, such as in the case of Newton’s theory—which was able to make astonishing, accurate predictions—when it was falsified by Einstein’s. The two theories are just logically inconsistent, and the ontological change was so radical that we cannot speak of cumulative development of scientific theories. (Worrall 1989, 143) This is the starting point of anti-realism.

On the other hand, if—as van Fraassen’s variant of anti-realism holds—one is committed only to the empirical adequacy of a theory—however successful it is—without trying to relate it with its truth, such radical change would not affect his belief. One of anti-realism’s stand sees that in such a case, though the theory is refuted its successful empirical content is in general carried over to the new theory. So, while the theoretical claims are abandoned, the empirical content is retained. The ‘safe’ way to construe a theory, therefore, is to regard it as making no claims about unobservable entities beyond its direct empirical consequences. (Worrall 1989, 149) But anti-realists lose many things (of course, from a realist point of view) in arguing for their case: scientific theories have nothing to say about reality. Another way to express the issue is to say that when there is a radical change affecting a scientific theory, while there is discontinuity (between the old and the new theories) at the level of description of unobservable entities, there is continuity at its empirical level.

Worrall agrees that at the theoretical level there is discontinuity, but he wants to have continuity at more than mere empirical level.

He sees that in between the empirical and theoretical levels there is another level, which he calls *structure*, and in which there is still a continuity. The structure of a once accepted theory is more or less retained in the new theory, either as it was, or as a limiting case of the new theory. As rightly mentioned by Psillos, Worrall wants to establish a philosophical position which saves realism as much as possible, given the existence of radical changes in scientific history. (Psillos 1995, 17) Structural realism, then, is a realism that is committed to a belief in the structure, but not the nature, of unobservable entities postulated by successful theories. Its objective is to have the best of both realist and antirealist worlds: to account for the empirical success of theories in terms of the truth about some ontological level (i.e the “structure”) deeper than the empirical level (the realist no-miracle argument) without denying historical facts about ontological discontinuity in theory change (the antirealist strong point). It is in this sense that Worrall’s idea becomes very close to Poincare, whose motivation is very similar to Worrall’s, i.e. resisting the pessimistic meta-induction, as expressed in the following:

The laity are struck to see how ephemeral scientific theories are. After some years of prosperity, they see them successively abandoned; they see ruins accumulate upon ruins; they foresee that the theories fashionable today will shortly succumb in their turn, and hence conclude that these are absolutely idle. This is what they call the *bankruptcy of science*. (Poincare 1905, 140.)

His defense against such a charge is also by restricting the scope of realist belief in the truth of scientific theories:

... If we look more closely, we see that what thus succumb are the theories properly so called, those which pretend to teach us what things are. But there is in them something which usually survives. If one of them taught us a true relation, this relation is definitely acquired, and it will be found again under new disguise in the other theories which will successively come to reign in place of the old. (Poincare 1905, 351)

The true relations survive, because they ‘preserve their reality... only this something formerly we called *motion*; we now call it *electric current*. But these appellations were only images substituted for the real objects which Nature will eternally hide from us.’ (Poincare 1905, 140-141)

In another sense, we also see that Worrall’s SR is quite close to Russell’s. However, unlike Russell who argues for his case by way of logic (mainly the notion of isomorphism), Worrall suggests his SR by looking at cases in the history of science. Worrall, however, never elaborates on the notion of “structure” explicitly. From the examples he gives, we can see that it is not merely phenomena, but relations between phenomena, which are usually expressed in the formal mathematical equations of the theory. But what precisely is this?

Worrall’s paradigmatic case is Fresnel’s theory of light. This theory should count as a mature scientific theory; it proved to be very successful in its predictions, similar to the success of Newton’s theory. Yet this theory was later overthrown by Maxwell’s theory, thus the entity that plays a major role in the theory, the elastic

solid ether, was shown not to exist as well and replaced by a disembodied electromagnetic field. However, the important thing is that the mathematical equations were still completely preserved in Maxwell's theory.

Responding to this kind of radical change in science, Worrall writes, a hard-headed realist position such as Hardin and Rosenberg's would deny that there is even an ontological discontinuity. So they would construe Fresnel's position as if he has been talking about an electromagnetic field all along, but named it differently. As implausible as it may seem, this is actually their position. Anti-realists, on the other hand, would point to this case as empirical evidence for their claim that we should not talk about the truth of a theory, or be committed to believing in unobservable entities. If one's account of Fresnel's success is only limited to, say, its empirical adequacy, then this part is still well preserved in Maxwell's theory.

The structural realist, on the other hand, would not approve the hard-headed realist move. This case is too striking to be misunderstood. But he wants to go much further than claiming that what has been preserved is merely the observable part of the theory. He wants to save realism as much as possible by showing that not only in terms of the observable effects is Fresnel's theory identical with Maxwell's, but also in terms of the structure, which is expressed by the mathematical equations. That is, these equations are directly entailed by Maxwell's.

So here in Worrall's construal there are three levels of the theories. *First*, in the theoretical level, related to the *nature* of light, Fresnel was quite wrong in conceiving of an elastic solid ether. For the structural realist and anti-realist, this part

of the theory could not be preserved at all and was replaced by Maxwell’s distinct disembodied electromagnetic field. The *second* level is what Worrall calls the structural level. It is the formal law as expressed by mathematical equations. They state that those optical phenomena depend on the oscillations of something at right angles to the light. The *third* level is the optical phenomena or the observable effects. The realist would believe that all the three levels were preserved; the structural realist the second and third level, while for the antirealist, according to Worrall, only the third level was preserved. We may put this in the following table:

	<b>Realism</b>	<b>Structural realism</b>	<b>Antirealism</b>
<i>What is affirmed:</i>	<ul style="list-style-type: none"> <li>◆ The nature of light (<u>noumena</u>);</li> <li>◆ Mathematical equations (<u>structure</u>);</li> <li>◆ Empirical observations (<u>optical phenomena</u>)</li> </ul>	<ul style="list-style-type: none"> <li>◆ Mathematical equations (<u>structure</u>);</li> <li>◆ Empirical observations (<u>optical phenomena</u>)</li> </ul>	<ul style="list-style-type: none"> <li>◆ Empirical observations (<u>optical phenomena</u>)</li> </ul>
<i>What is preserved in the new theory</i>	<ul style="list-style-type: none"> <li>◆ What oscillates;</li> <li>◆ The oscillations—on which the phenomena depend—at right angles to the light</li> <li>◆ The observable effects</li> </ul>	<ul style="list-style-type: none"> <li>◆ The oscillations—on which the phenomena depend—at right angles to the light</li> <li>◆ The observable effects</li> </ul>	<ul style="list-style-type: none"> <li>◆ The observable effects</li> </ul>

Worrall argues that this case is representative of theory change in mature (successfully predictive) science in general. (Worrall 1989, 160) He observes that in the case of theory change, especially in the history of physics, the mathematical

equations of the old theory re-emerge as limiting cases of those in the new theory. The new tends to the old as some quantity tends to some limit. But Fresnel's is a special case: his equations are taken over intact into Maxwell's. This clearly shows that there is a continuity, despite the total rejection of the elastic solid ether. In another sense, this also implies that there is accumulation in science *structurally*. In this way, Worrall really gets very close to being a realist, without committing himself fully to the no-miracle argument, though ultimately at some point he has to invoke a weaker form of it.

We can see Worrall's position as mainly involving an epistemic thesis. That is, he puts an epistemic constraint on realism. This position makes him share some realist as well as antirealist presuppositions. In a way, as analyzed by Psillos, SR is a species of scepticism about the possibility of scientific knowledge (Psillos 1995, 23). In that way, it is very close to some strand of antirealism, notably van Fraassen's constructive empiricism. Scientific theory may have truth values in virtue of an independently existing reality, yet we can not know whether a particular theory is true (i.e. correctly representing that reality), the most important reason being the underdetermination of theory by evidence. The realist twist to SR surfaces in the claim that we can know the structure of that reality correctly, in which the structure is part of the unobservable world, "underlying" the observable world. Thus, to sustain his argument, or to maintain his in-between position, Worrall needs to justify and substantiate the crucial distinction between the observable level, the structural level, and the level of the nature of reality. This is an instance of the principled

differentiation suggested by Papineau. However, failure to do so will make his position collapse to either a realist or an antirealist position.

For Psillos, Worrall is too close to being a full-fledged realist. In his argument, the nature of a scientific entity is construed as something over and above its structure; the physical content/entity referred to by the mathematical symbols are somehow above the equations. But in fact, ‘when scientists talk about the nature of an entity, what is normally understood is a bunch of basic properties and a set of equations, expressing laws, which describe the behaviour of this entity.’ (Psillos 1995, 31) Nature, then, is knowable only through the mediation of structure. Furthermore, it may be the case that knowing what laws an entity obeys does not exhaust the knowledge of what the entity *is*, but the idea that structure is the necessary epistemic mediator suggests that the nature of the entity may not be over and above the structure. Instead, they form a continuum, such that knowing what an entity is involves further knowledge of the laws it obeys. ‘To say what an entity *is* is to show *how this entity is structured*: what are its properties, in what relations it stands to other objects, etc.’ (Psillos 1999, 156) Thus he concludes, ‘the “nature” of an entity is not over and above its “structure” and that knowing the one involves and entails knowing the other.’ (Psillos 1995, 32) If epistemically and ontologically nature cannot be separated from structure, then Worrall in fact claims nothing less than realists’. When Fresnel discovered his theory of light, then, what he has correctly discovered—from the vantage-point of Maxwell’s theory—was not only its structure, but also a huge portion of its nature. In this particular case, the continuity is not only



at the structural level, but ‘fundamentally correct theoretical principles about the propagation of light and some features of the carrier of light waves were also carried over.’ (Psillos 1995, 34).

Now, while in a realist understanding structure cannot be strictly separated from nature (such that it is difficult to distinguish SR from the scientific realism that is vulnerable to PMI), in another understanding it is difficult to distinguish it from antirealism. This is due to its scepticism about knowing the full content of scientific theories. What differentiates the two positions is Worrall’s realism about knowing their structure, *and* his contention that this structure, though not identical with the reality itself, is representative of the underlying reality.<sup>24</sup> But to do that there is still a missing argument, that is, the one to the effect that the formal mathematical equations represent the so-called “structure” of the real world, which is knowable, though the entities themselves are not knowable.

Since the structure of phenomena goes beyond the level of the observable, van Fraassen’s arguments against scientific realism apply as well to SR. One possible constructive empiricist objection is that underdetermination by evidence applies as well to the mathematical formalism. Suppose we accept that structure is something above the observable. From the empirical level to the structural level, then, ampliative inference is used. This inference would not be like inductive generalization, which empiricist would accept, but more like the inference to a theory. Whereas in inductive

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<sup>24</sup> Psillos 1995, 24. According to Psillos the realist gloss is seen clearer in Worrall’s later article. Here he refers to Worrall’s unpublished manuscript, “Scientific Realism and the Luminiferous Ether: Resisting the ‘Pessimistic Meta-Induction’” (1990).

generalizations there is no “ontological jump”, i.e. both the particular observations and the generalization stay in the same observable level, in the second case there is an inference from the observable to the unobservable (i.e. the structure). As such this kind of inference is closer to the realist inference (abduction, or retrodution), which the empiricists regard as not reliable.<sup>25</sup>

Another empiricist objection could focus on precisely the vagueness of the idea of “structure” of the world, which Psillos, as realist, sees epistemically undistinguishable from the nature. An empiricist could accept Worrall’s idea that empirical evidence only warrants knowledge of the structure, but he wants to excise any ontological significance given to the structure. That is, structure is not something above (in the ontological hierarchy) or underlying the phenomena. For him, structure expresses nothing other than empirical adequacy. This is precisely van Fraassen’s objection.

Van Fraassen sees Worrall as making three claims. *First*, scientific knowledge is cumulative in some important respect. *Second*, the knowledge accumulated is of features of the world that transcend the empirical world, or features of the reality behind the phenomena. *Third*, these features constitute “structure” rather than “quality” (i.e. features of things in themselves). The way van Fraassen sees it,

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<sup>25</sup> In the next chapter I will discuss retrodution more elaborately. As I mention there, an empiricist would reject the division of ampliative inference into induction and retrodution (or abduction); for them both are induction. But whereas the first kind of induction does not commit one to the postulation of unobservable entities or processes, the second one does. Empiricists do not accept this kind of inference; they regard it as unreliable beyond pragmatic purposes—as shown by the case where entities once postulated in old theories turn out to not exist. Again, realists surely have a different take on this, which I will discuss in the next chapter.

however, the support given for the first claim ‘is explicitly and admittedly concerned only with an accumulation of empirical knowledge.’ (van Fraassen’s structure., 37) Van Fraassen is a bit unfair here<sup>26</sup>, since Worrall does give arguments that suggest that what is retained in theory change or what is cumulative is something more than the empirical contents of the theories. In any case, van Fraassen’s main contention has to do with the no-miracle argument employed by Worrall, though it is of a weaker form. Just like the case of underdetermination, as discussed earlier, which applies to the structure of the theory as something beyond the empirical level, his arguments against the no-miracle argument would apply to any claims that are above empirical adequacy, whether it is about truth (as in the full-blown realist) or truth about structure (as in SR). As to the third claim, van Fraassen makes a demand similar to Psillos’s; that is, the ontological division of structure and noumena needs more justification than what Worrall has given. For van Fraassen, this ontological division can be made only by introducing more metaphysical presuppositions, which is against his overall empiricist project. For van Fraassen, there are only two “worlds”: the observable and the unobservable. If something does not fall under the first category, it belongs to the unobservable. Thus Worrall’s supposedly in-between world, “the world of structure”, as he himself perceives it, falls squarely under the second category. An appropriate or rational attitude for the empiricist is to remain agnostic about that world.

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<sup>26</sup> Here he quotes from Worrall’s paper (1989, 147-149). There Worrall shows several possible positions that can be taken in seeing the cumulativeness in science, one of them being an antirealist position, which sees accumulation only of empirical contents.

For van Fraassen, the division between structure and content of theories is not a principled difference. At least as expressed in Worrall, the division seems arbitrary. ‘The structure discovered is identifiable only in retrospect—it is the part retained through scientific theory change .... The atoms are still there at some level, so that was structure. The ether is no longer there, at any level, so that was a mistake in content ....’ (van Fraassen, 34)<sup>27</sup> He does not deny that there is an accumulation of knowledge about nature in science, and this is what makes any attempt to specify in what the accumulation is always has genuine appeal. His disagreement with Worrall is precisely on this issue. The differentiation between content and structure might not represent an objective difference in nature, but merely our representations of nature. For van Fraassen there are two realms of scientific investigation: the phenomena, which are investigated, and models, which are abstract structures studied in mathematics and advanced by the theory as representation of the phenomena. Further, this representation, i.e. the structure of theory, is always context-dependent; it is always partial and selective. It is created to model certain phenomena for certain purposes, thus it involves selection of things which are considered relevant for the purpose. For example, human beings can be modeled by certain mechanical models, and however good that model is, there are many important things about human beings which are deliberately omitted. Structure, then, is not something that truly represents some underlying reality. ‘[B]y becoming comfortable with this context-dependence

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<sup>27</sup> From “Structure: Its Shadow and Substance”: <http://webware.princeton.edu/vanfraas/abstract/index.htm>. (Date unknown)

we make sense of the intuition that science presents us with the structure, and that it is knowledge of the structure of the empirical phenomena which is accumulated. ... Science represents the empirical phenomena solely as embeddable in certain abstract structures (theoretical models), ...' ("Structure", 43) So there has indeed been accumulation in terms of "knowledge of structure", but this simply means an accumulation of empirical content as expressed in the formal structure of the theory. Structure does not represent some ontological level higher than the observable world, but is simply about empirical adequacy.

With this an empiricist can have an explanation of the success of science which does not relate it to either truth about nature or truth about structure. The success of science is not a miracle, because in any theoretical change the past empirical success is retained. An older accepted theory ought to have important empirical success—otherwise it wouldn't be accepted in the first place—when it is replaced by a new theory. The new theory is better than the old one *at least* due to its capability to retain the empirical successes of the old one and shows us why the old one was successful. So both the past empirical success retained and the new empirical success are needed as credentials for acceptance. (van Fraassen "Structure" , 39)

Thus we can see that in a sense SR tends to realism, yet in another to anti-realism. The source of this is the ambivalence exhibited in the vague notion of "structure" as employed by Worrall, epistemically as well as ontologically. Though at this stage we may say that the idea has failed, the story of its failure is quite

instructive, since, among other things, it exhibits the main arguments of realism and anti-realism, and how Worrall tries to navigate between between them by weakening the realist claims, to find some position which is still worth calling ‘realism’. This is a very good example of Papineau’s suggestion, as mentioned earlier, that realists should make a principled differentiation between good and bad parts of theories. In other words, there should be differentiated (realist) commitments to theories. Acceptance or rejection to theories need not be made indiscriminately. Below I will continue the discussion a bit further in this spirit.

***(b) Contesting Laudan’s meta-induction***

Worrall’s structural realism, as a strategy to restrict realist claims, mainly aims at avoiding the pessimistic meta-induction in its most famous form as advanced by Laudan (1981). This argument has been considered by many as a fatal blow to scientific realism. Laudan shows, by way of many cases in the history of science, that so many scientific theories considered successful in the past were nevertheless shown to be false with regard to the ontological claims they made as well as with regard to the theoretical terms they employ.<sup>28</sup> From these many cases, Laudan wants to rebut some important arguments for realism, by asserting (i) that empirically successful

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<sup>28</sup> Laudan discusses specifically a family of theories, i.e. the subtle fluids and ethers of eighteenth- and nineteenth-century physics and chemistry (Laudan 1984, 114-115). His long list includes ‘a few more prominent examples’: the crystalline spheres of ancient and medieval astronomy; the humoral theory of medicine; the effluvial theory of static electricity; ‘catastrophist’ geology, with its commitment to a universal (Noachian) deluge; the phlogiston theory of chemistry; the caloric theory of heat; the vibratory theory of heat; the vital force theories of physiology; the electromagnetic ether; the optical ether; the theory of circular inertia; theories of spontaneous generation. (Laudan 1984, 121-122)

theories do not provide warrant for believing the claim that such theories are (approximately) true about the world; and (ii) that there is discontinuity at the theoretical level in the succession of theories (in ontological terms as well as in terms of referents of those terms—even worse, some terms turned out not to refer).

One realist response to such an attack may take the form of asserting the differentiated commitments such as shown by Worrall. Related to this, it is also important to look at Laudan's list more closely, and examine the examples one by one. In that way, some realists found that the long list of once successful but false theories can be shortened considerably. Since that list of theories functions as part of the premise for Laudan's (pessimistic) induction, shortening the list means weakening the basis for the inductive generalization.

One challenge which can be posed immediately to Laudan concerns his premise that all the theories in his list were empirically successful—yet Laudan's reasoning goes, the entities and processes they posited turned out to be rejected by later scientists, and dropped out of the theoretical description of the world. Recent empirically successful theories, therefore, shouldn't be claimed to describe the world. The question is: what is his criteria of "empirically successful" theories? Were all those theories really empirically successful in their period? Many realists doubt it. Laudan's criteria seem too loose. For him an empirically successful theory is one that 'has functioned in a variety of explanatory contexts, has led to several confirmed predictions, and has been of broad explanatory scope.' (Laudan 1984, ) Realists

would demand more rigorous criteria. Psillos, for example, sees that Laudan's criteria amounts to simply getting the facts right, or telling a story that fits the facts. 'The notion of empirical success that realists are happy with is such that it includes the generation of novel predictions which are in principle testable.' (Psillos 1999, 105) The notion of novel prediction has been developed by many realists, a prominent example of whom is Jarett Leplin, whose *A Novel Defence of Scientific Realism* (1997) concentrates on precisely this issue. The point of making a more rigorous notion of success concerns the scope of the no-miracle argument. When realists use NMA, the success that is to be linked with (approximate) truth concerns only theories successful in the more rigorous sense. Continuity in theory change also in part concerns those theories.

Realists would add to this another requirement that what should be taken into account are only those *mature* theories that have reached a certain point which, in Psillos, 'can be characterised by the presence of a body of well-entrenched background beliefs about the domain of inquiry which, in effect, delineate the boundaries of that domain, inform theoretical research and constrain the proposal of theories and hypotheses. This corpus of beliefs gives a broad identity to the discipline by being, normally, the common ground that rival theories of the phenomena under investigation share.' (Psillos 1999, 107-108)

Another thing that should be looked at more closely in Laudan's examples is the role of the entities postulated. Laudan's argument works only in the cases where



the entities function essentially in the theory, and not simply as an intuitive postulation to fill in the loopholes in the description of the world as suggested by the theory. With this restriction, the crystalline sphere of ancient astronomy, the universal Deluge of catastrophic geology, theories of spontaneous generation, for example, would quickly be disqualified, even if the scientists who propose them firmly believe in them. '[R]ealism is not a blanket approval for all entities postulated by long-supported theories of the past.' (McMullin 1981, 17)

With all those specifications, Laudan's list would be significantly reduced in size, thus severely weakening his pessimistic induction. However, even after this, there will still be a few cases left in Laudan's list, where entities or processes postulated in the mature and successful theories turn out to be mistaken. It is in those few cases that realists have to show that somehow some important parts, not only the empirical contents, of the theories are carried over to the succeeding theories. Among them is the optics of the nineteenth century. Worrall's treatment of Fresnel and Maxwell's theories is important in this respect, as an attempt to show some continuity deeper than the empirical level, between the theories. In the response to Worrall's SR and in the case of the caloric theory of heat which he specifically discusses (Chapter 6 of his 1999), Psillos tries to show that the continuity goes deep enough; substantial theoretical constituents were preserved in the change. (Psillos 1999, 144-145)

Another way to find the continuity, which is discussed by McMullin concerns the continuity in terms of metaphors.<sup>29</sup>

It is not an *ad hoc* move to say that such metaphors, which, strictly speaking, are false, are needed in the earlier period as the ladder to a better description and explanation of the phenomena studied by scientists. Only through gradual refinements is it possible for scientists to come up with a good description of the phenomena. What is important for realists is to establish that there has been explanatory progress during the long history of science, and for this it is sufficient to show that earlier theories, which were successful at their time, contribute something significant theoretically to the later theories. To this we still need to add a caveat: that at any time, any ontological claim made by scientists is tentative.

In general, the way to rebut Laudan's argument is by showing that in the few theories that are genuinely successful and mature, the abandonment of them is not complete; some important parts of the theories are still retained. After all, while, upon reading Laudan's pessimistic conclusion by induction, one can be left with an impression that the product of science in its history is an accumulation of ontological fictions (i.e. falsehoods), many different examples can be given that show many entities and mechanisms survive scientific revolutions. Beside, 'scientists are not prone to acquire only false beliefs.' (Psillos 1999, 104) But they learned from past

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<sup>29</sup> In his many writings McMullin emphasizes that metaphors hold a quite important role in the formation of theories, and he proposes criteria to evaluate their merit. See for example his 1994.

mistakes as well, to construct better theories. Though they seem trivial, these points are in danger of getting buried in Laudan's focus on discarded theories.

At this point we have qualified realism in many ways, such that realists are entitled to believe—to a certain extent—in the description of the world as suggested by a number of theories. In the next chapter, I will further qualify realist beliefs in the course of investigating the reach of evidence that supports theories. That is, how to justify the inference from empirical data to ontological claims; how far beyond the observable world empirical evidence can reach. The instrument for this is abduction-retroduction or inference to the best explanation (IBE). It is one of the most important positive arguments on the realist part. I will show when and under what conditions IBE works, and when it does not work. It will turn out that realists have to make further “sacrifices” to preserve their beliefs; nevertheless, I would argue that the resulting very minimal realism is still worth-calling realism, and still makes important claims that differentiate realists from the antirealists, though it might not be sufficient to support a full-fledged “experimental metaphysics”. \*\*\*

## **Chapter 3:**

### **Inference to the Best Explanation and Its Limitations**

#### **1. The reach of evidence and retrodution**

Commemorating the 20 years of the publication of Bas van Fraassen's *Scientific Image*, Arthur Fine (2001) notes that, for better or worse, the work has contributed to a change of direction in the realism debate. In van Fraassen's hand, realism is taken almost exclusively as an epistemological problem, a problem about truth and belief in truth, leaving aside its metaphysical component. The debate about realism, then, is a debate over the reach of evidence. (Fine 2001) Van Fraassen's main question is, 'Does the evidence support belief in the truth of scientific theories, or does it only reach as far as belief in their empirical adequacy?' His answer is an affirmation of the second choice. Realism depends, at least partly, on its proponents' ability to justify their attempt at stretching the reach of evidence beyond empirical adequacy. Regardless of whether Fine's assesment is correct, he has correctly identified one important issue in the recent realism debate.

Many realists would dispute such formulation of the main issue in realism debate. McMullin, for example, takes it that the dispute in the realism debate is

ultimately about existence.<sup>30</sup> Realists usually want to affirm that theories enlarge our known world by discovering things of whose existence we were previously unaware. (McMullin 1994, 80) As a matter of fact even van Fraassen, as an anti-realist, explicitly expresses his belief in terms of existence. He believes that appearances alone are real, and all the rest is “a unifying myth to light our path.” (van Fraassen 1994, 133)

Regardless of how to formulate the problem in the realism debate, we can see that ultimately the epistemological component of the issue would lead to the issue concerning existence. “Reach of evidence” may be understood as bearing on the truth of theories, but also about what may be tentatively called “ontological depth”—does theory afford us knowledge only of phenomena or does it extend deeper. In a sense, the debate about phenomena, “structure”, and noumena, as discussed in the previous chapter, can also be couched in terms of reach of evidence. Does the evidence for a scientific theory support belief only in phenomena, or “structure”, or even further than that? Belief that scientific theories reveal the “underlying structure of phenomena” is based on the belief that evidence reaches further than a mere description of phenomena. In general, the belief about the reach of evidence would determine one’s belief about whether science enlarges our known world.

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<sup>30</sup> In light of the division of realism into epistemological and metaphysical components, the word “ultimately” above is important. As I argued in the previous chapter, the central issues in realism are epistemological, but the realists’ ultimate aim is “metaphysical”, i.e. to be able to formulate claims about existence (not necessarily limited to the level of phenomena) that is scientifically credible.

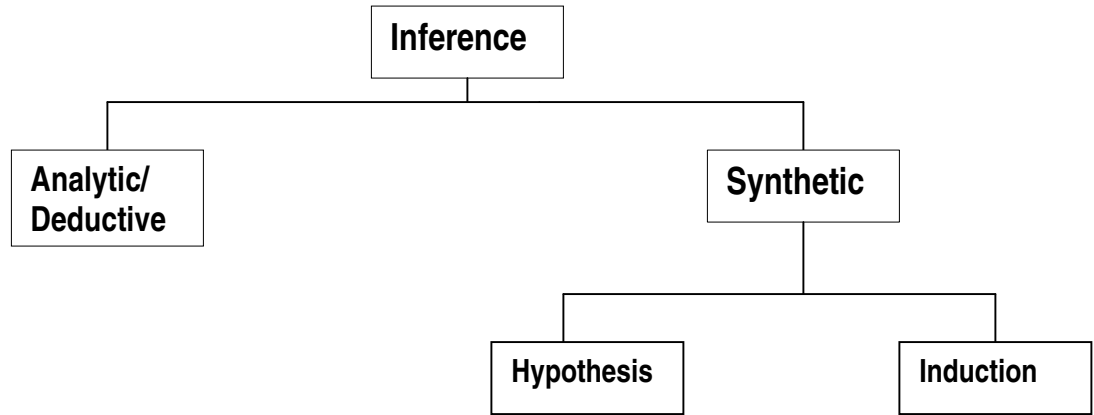
Now the main task of “scientific method”—whatever that is—is supposedly to stretch the reach of empirical evidence far enough (or, as far as possible) so that from the evidence, we can speak about more than similar phenomena in general, or create a universal formula that describes, and probably explain, the behavior of certain phenomena in general. Any aspiration to construct a “scientific metaphysics” could be seen as a way to stretch the evidence that supports a theory to a far broader, non-empirical (“metaphysical”) realm, such that the evidence may be considered to support metaphysics. Realism, as an attempt to force scientific theories to speak about things as they really are, would be very much dependent on whether or not attempts to stretch the reach of evidence can be justified. There is no question that whatever bits of evidence a scientific theory can muster, they are intended to support propositions whose applicability go far beyond those observed.

One instrument for enlarging the world is what is accordingly called “ampliative inference”, the reasoning that aims to infer statements whose scope are significantly larger than the scope of the premises. As defined by many today, ampliative inference *is* induction. And by its very definition, this reasoning never yields certainty, which is expressed in the currently popular definition of induction. While deduction is the kind of reasoning in which the truth of the premises *guarantees* the conclusion, in induction the truth of the premises only *gives some evidence* to believe in the truth of the conclusion, since the domain of its conclusion is usually much larger than the evidence as stated in the premises.

However, many realists insist on distinguishing two kinds of ampliative inference: induction and retrodution (and/or abduction); in recent philosophy of science literature, the latter is sometimes identified with Inference to the Best Explanation (IBE). I shall clarify these terms shortly. But one thing we may immediately see is that if induction is defined broadly as above, then abduction/retodution *is* induction. Yet realists insist that the two have importantly different characteristics. Induction is only weakly ampliative, since it usually involves only generalization: what is observed is taken to represent a larger, unobserved class. Abduction/retrodution, on the other hand, starts with some regularity and tries to *explain* it by postulating some underlying processes or introduced entities. Here explanation is an essential part of the inference: “explanatory considerations are a guide to inference.” (Lipton 2000, 184)

Before proceeding further, I shall clarify several terms which I have hitherto used loosely, i.e. “abduction”, “retrodution”, and “inference to the best explanation (IBE)”. The first two terms (and another term, “hypothesis”) were initially used by C. S. Peirce. He uses them variously as the third kind of inference in addition to deduction and induction. But he sometimes used them inconsistently and the way he uses them had apparently changed in his later writings, so that we find different accounts about the meanings and scope of those terms in recent literature. In at least one place he mentions three kinds of inferences, i.e. abduction, deduction, and induction, but in some places he divides inference into analytic/deductive and

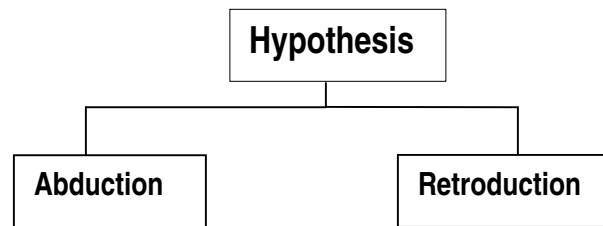
synthetic, which is further divided into induction and hypothesis (see the following diagram). (Peirce 1957, 129)



Among recent philosophers who relate Peirce with the notion of IBE, Peter Lipton, for example, mentions “abduction” as one of the many kinds of inductive inferences. IBE, on the other hand, is designed to give a partial account of inductive inferences, which involves the notion of “explanation”; another model is enumerative induction, which is inadequate as an account of inference in science. (Lipton 2000, 184, 185)

In his reconstruction of Peirce’s account of the methodology of science, Nicholas Rescher divides inductive methodology into quantitative induction and qualitative induction, which is further divided into *abduction* (understood as hypothesis formulation and selection) and *retroduction* (hypothesis testing and elimination). In this way, we can complete the above diagram by expanding the “Hypothesis” box as follows:





As noted by McMullin, Peirce himself changed his views regarding this issue during his long career. (McMullin 1992, 85-89) McMullin seems to understand Peirce along the same lines as Rescher does, but in his *The Inference that Makes Science* (1992) restricts the use of the terms as follows. Abduction is the process whereby initially plausible and testable causal hypotheses are formulated. Retroduction is understood in a broader way which includes abduction. McMullin acknowledges that this departs from Peirce's understanding. But this does seem to be more consistent, and as such I shall take retroduction in this way.

Most of recent discussion about "ampliative inference" (as a means of enlarging our known world) is carried out under the rubric of IBE. On the other hand, the meaning of IBE is itself still widely debated. Doing away with IBE does not seem to be a good way, since it will make the discussion here unconnected with what is going on with the discussion in recent philosophy of science literature. In what follows I shall discuss the antirealist (mostly van Fraassen's) charges against IBE and defend this notion while at the same time clarify what it should and should not be. In my defense, I shall move in the direction suggested mostly by McMullin and conceive IBE as very close to what he means with retroduction, and as such later in

this chapter I shall use the term interchangeably. However, in the next section, when I discuss Peirce's initial idea about abduction/retroduction, I shall stick with his own understanding of them.

Before proceeding further, one question can be raised here: why is it important for realists to justify retroduction/IBE? Anti-realists tend to blur the distinction. For them, there is only one ampliative inference, which is induction, and there is no inference which can yield conclusion about unobserved entities or structures. Retroduction is simply a more complex form of induction and is not a causal explanation. 'It is explanatory only in the weak Humean sense which permitted the deductive-nomological (DN) model of explanation to imply that explanation and prediction are two sides of the same coin.' (McMullin 1994, 82) For McMullin, the disagreement between realism and anti-realism is primarily about retroduction, because it is the primary means whereby the natural sciences enlarge our world. There are other partly metaphysical issues like whether there are "necessities" in nature or whether there are "laws of nature". But the main issue is retroduction: 'Can it afford a tentative warrant for affirming the existence of the theoretical entities it introduces to explain observed regularities?' (McMullin 1994, 83)

## 2. The structure of retroduction and IBE

IBE is used in two ways to argue for scientific realism. First, it is used by realists to argue for the truth of theories in general, given the pragmatic success of science as a

whole. Hilary Putnam expresses what is known as the ‘no-miracles argument’: ‘the positive argument for realism is that it is the philosophy that doesn’t make the success of science a miracle.’ The explanandum here is the success of science. Since “miracle” is not an explanation, in effect this argument says that realism is the only position that can explain science well. What are the alternatives to this explanation? As an example, take the empiricist van Fraassen who invokes Darwin to “explain” the alleged miracle: ‘Any scientific theory is born into a life of fierce competition, a jungle red in tooth and claw. Only the successful theories survive—the ones which *in fact* latched onto actual regularities in nature’ (van Fraassen 1980, 40) But, this, as van Fraassen himself rightly indicates, is in fact a rejection of the demand for explanation. “Success” is in general understood as predictive success. Why is it successful? Because its theories give accurate predictions, otherwise it wouldn’t be successful and would be discarded. And that’s all we need to know. Disagreements about this issue have formed a very large and substantial part of realism debate. This is not my concern in this chapter. What I will discuss here is about the use of IBE in the scientific activities itself.

Acceptance of IBE as a legitimate form of inference used by scientists would ground realists’ belief that entities and relations postulated by science are in general real. That they do refer, and the reference is relatively stable; that the reach of evidence is long enough to give us a relatively firm ground to speak about (hidden) mechanisms that might explain phenomena and enable one to speak about reality as such. One popular example on the use of IBE concerns the inference about the

existence of an airplane from observation of jet tracks in the sky; or the existence of electrons upon observation of its tracks in a cloud chamber; the existence of dinosaurs upon certain fossil findings; the existence of species' common ancestry upon anatomical or genetical similarity. Science, it seems, has always worked this way, using intensively this kind of inference. But is it a legitimate kind of inference?

Charles Peirce was the first philosopher who explicitly expressed that science proceeds using a kind of inference which he calls hypothesis in addition to the two that had hitherto been acknowledged as playing the role, i.e. induction and deduction. (As mentioned earlier, he variably calls this inference hypothesis, abduction and retroduction.) (Peirce 1957, 129) Deduction is an explicative inference, the conclusion of which does not contain more information than the premisses. The other two are ampliative inference, whose conclusions are "amplified" relative to the premisses. The three kinds of inference may be represented as syllogisms in the following way:

Deduction:

*Rule:* All the beans from this bag are white.

*Case:* These beans are from this bag.

*∴ Result:* These beans are white.

Induction:

*Rule:* These beans are white.

*Case*: These beans are from this bag.

$\therefore$  *Result*: All the beans from this bag are white.

Hypothesis:

*Rule*: All the beans from this bag are white.

*Result*: These beans are white.

$\therefore$  *Case*: These beans are from this bag.

Peirce insists on distinguishing induction from hypothesis (Peirce 1957, 129):<sup>31</sup>

*Induction* is where we generalize from a number of cases of which something is true, and infer that the same thing is true of a whole class. Or, where we find a certain thing to be true of a certain proportion of cases and infer that it is true of the same proportion of the whole class. *Hypothesis* is where we find some very curious circumstance, which would be *explained* by the supposition that it was a case of a certain general rule, and thereupon adopt that supposition. Or, where we find that in certain respects two objects have a strong resemblance, and infer that they resemble one another strongly in other respects. (Italics added)

Though there might be moments when it is difficult to classify an inference as to whether it is of an inductive form or hypothesis, that distinction is “broad and decided”:

By induction we conclude that facts, similar to observed facts, are true in cases not examined. By hypothesis, we conclude the existence of a fact quite different from anything observed, from which, according to known laws, something observed

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<sup>31</sup> Quotations from Charles Peirce are taken from *Charles Sanders Peirce, Essays in the Philosophy of Science*, ed. Vincent Thomas, The Liberal Arts Press, New York, 1957. Two articles on abduction that I use here are dated 1893 and 1901.

would necessarily result. The former is reasoning from particulars to the general law; the latter, from effect to cause. The former classifies, the latter explains.' (Peirce 1957, 136)

So, the main difference between induction and hypothesis is that the latter is an inference to an explanation. For example, to explain the finding of fossils of fishes, in the interior of a country, we suppose that the area was once a sea. As another example of hypothesis, Peirce mentions how we infer that a man whose name was Napoleon Bonaparte once really existed, though we never saw him, because that is what can explain the fact that many documents and monuments refer to a conqueror called by that name. No matter how much data we collect, inductive generalization will never take us to that inference. Induction does not introduce new items, but in these examples new items are introduced to explain the known data. As such the step taken in adopting a hypothesis is more risky than inductive generalization. (Peirce 1957, 134) It is also important to note that in the above examples, adopting a contrary hypothesis would probably lead to results contrary to those observed. (Peirce 1957, 132) This certainly adds to the plausibility of the hypothesis.

Abduction, as part of hypothesis, consists in 'studying facts and devising a theory to explain them'. The more general form of abduction is as follows (Peirce 1957, 189)<sup>32</sup>:

The surprising fact C is observed;

But if A were true, C would be a matter of course,

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<sup>32</sup> Cf. Niiniluoto 1999, S439.

Hence there is reason to suspect that A is true.

In this form, the conclusion is not A itself, but the assertion that ‘there is reason to suspect that A is true.’<sup>33</sup>

At this point it is important to distinguish, following Hanson (1961), between suggesting a hypothesis and accepting it. Abduction can be seen as giving reasons for suggesting a hypothesis, but reasons for accepting it have to be sought somewhere else (by deriving and testing observable predictions from it). Peirce—and, in a different way, also contemporary proponent of IBE, as I shall discuss shortly—seems to want to go further by attributing likeliness to the result of abductive inference. While deductively there is no doubt that abduction is invalid, he sees that in many cases inference from effect to cause is psychologically irresistible. The example about Napoleon, in which we believe in something that is not susceptible of direct observation because its effects (the histories, monuments, etc.) are observed, shows how compelling such an inference is. Similarly, perceptual judgment involved in our knowledge about the observational properties of ordinary physical objects also involves abduction. These examples are intended to show Peirce’s taking abduction not only as belonging to the realm of discovery but also in justifying explanation.

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<sup>33</sup> N.R. Hanson suggested that the schema could be understood as providing a “logic of discovery”. For discussions about objections to this and a possible way of amending the schema, see Niiniluoto 1999, S440 ff.

As noted by McMullin (1978, 245), retrodution blends Aristotelian *oti* and *dioiti* (proof and explanation) that makes up his demonstrative reasoning. ‘The hypothesis is confirmed or justified precisely to the extent that it is shown to have explanatory power. Retrodution is the mode of inference which allows the scientist to conclude that his theory is warranted.’ (McMullin 1978, 245) At present the discussion about abduction/retrodution, under the rubric of Inference to the Best Explanation (IBE), goes along this route. IBE is inference to the *best* (and sometimes the only) explanation, not merely to potential explanation. The contemporary debate about IBE has its source in this position; it is problematic precisely because it is regarded as justifying the inferred explanation.

An encyclopedic definition of IBE goes as follows: ‘[IBE] is the procedure of choosing an hypothesis or theory that best explains the available data. The factors that make one explanation better than another may include depth, comprehensiveness, simplicity and unifying power.’ (Vogel 1998) The fact that IBE is invalid only shows that IBE, and the most simple of induction too, is not expected to guarantee truth, to retain the truth of the premises. Justification for this practice has come from many sources. Some try to use a Bayesian approach to IBE, others invoke lessons from the history of science which supposedly show that this mode of inference has worked quite well. I shall leave a further elaboration of IBE for later, after a discussion of objections to the legitimacy of IBE as a mode of inference. This discussion can bring to light many important points as regards the idea of IBE.



### 3. Objections and replies

In this section I shall start with van Fraassen. His critique of IBE is perhaps the most sustained in the recent discussion of the topic. Furthermore, his polemics with other philosophers of science have shed significantly light on the issue.<sup>34</sup> Van Fraassen seems a natural choice that could help us explore the many dimensions as well as problems of IBE. Later I will show that some of van Fraassen's important insights do indeed pose challenges to the legitimacy of IBE and scientific realism, while at the same time van Fraassen's constructive empiricism, as a matter of fact, has important common denominators with scientific realism. Besides the polemics mentioned in the earlier footnote, my main sources are his *The Scientific Image* (1980), and *Laws and Symmetry* (1989).

#### *a. IBE as a rule of inference*

In his *Scientific Image*, Van Fraassen's objection is directed mainly to IBE as a *rule* of inference. That is a rule that allows us to be committed to the existence of entities postulated by a theory when we accept the theory. Van Fraassen here quotes Wilfrid Sellars: 'As I see it, to have good reason for holding a theory is *ipso facto* to have good reason for holding that the entities postulated by the theory exist.' (Quoted in van Fraassen 1980, 19)

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<sup>34</sup> Van Fraassen has been involved in many discussions about his position, including IBE. The latest ones on which I shall focus in this section are: Psillos (1996), Ladyman, *et.al.* (1997) [reply to Psillos, written by Ladyman, Douven, Horsten, and van Fraassen]; and Psillos (1997)

Simplified, this rule says that of two hypotheses H and H' we should infer H if it is a better explanation of the evidence E than H' is. We seem to follow this rule in ordinary cases. A famous example is this: I infer that there is a mouse in my house when I hear scratching in the wall, the patter of little feet at midnight, and my cheese disappears. My inference is not only that all the observable phenomena will be as if there is a mouse, but that there really is a mouse. The question is, does this pattern of inference also apply to unobservable entities? 'Is the scientific realist simply someone who consistently follows the rules of inference that we all follow in more mundane contexts?' (1980, 20) Fraassen's own answer is in the negative: 'Merely following the ordinary patterns of inference in science does not obviously and automatically make realists of us all.' (1980, 23) His arguments are as follows.

We may take IBE as the realist hypothesis that the theory which best explains the evidence is true. To this, a constructive empiricist, as Van Fraassen is, has a rival hypothesis: that such a theory is empirically adequate (that all observable phenomena are as the theory says they are). The realist has a set of criteria used to evaluate the explanatory power on the basis of which he chooses a theory that best explains the evidence. A constructive empiricist, on the other hand, may well have an agreement with regard to all those criteria, but accepts the theory only as empirically adequate. How are we to decide between the hypothesis of scientific realism and constructive empiricism? In the case of the mouse, which is an observable thing, the two hypotheses are equivalent, since in this "all observable phenomena are as if there is a mouse in the wainscoting" is, for the constructive empiricist, equivalent with the

realist's "there is a mouse in the wainscoting". The issue is totally different when the theory involves unobservable entities. In that situation, the two hypotheses make a difference: one is committed to its existence, the other defers judgment about it. Van Fraassen's argument here hinges on his observable-unobservable distinction, which has long been a subject of debate. The main point van Fraassen wants to emphasize here is that for every realist's acceptance of a theory as true, a constructive empiricist always has a rival hypothesis that the theory is empirically adequate. In other words, pragmatically there is, at the very least, no difference in being a realist or constructive empiricist. As such rationally the realist cannot compel his opponents to be a realist.<sup>35</sup>

Van Fraassen here could be easily misunderstood<sup>36</sup> as applying a double standard: IBE is fine in the realm of the observables, but cannot be applied to posit theoretical, unobservable entities. If that is the case, there is a question as to how the observable-unobservable distinction applies with regard to the use of IBE. Why IBE in the case of observables is accepted, while in the realm of unobservables it is considered unreliable. However, in his later writing he and his co-authors (Ladyman, et.al. 1997) insist that as a matter of fact he does not accept IBE *simpliciter*, in any realms. In their defense of van Fraassen's critique of IBE, the example about the use

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<sup>35</sup> To this argument van Fraassen adds another one: when a realist regards that science is unfinished as long as some pervasive regularity is left unexplained, an empiricist simply begs to differ. He simply refuses this demand for explanation. That is just what distinguishes the two camps. (1980, 21). I shall discuss this in the next chapter. Van Fraassen wants to assert that, at least, empiricists can make equal sense of the success of science as realists do, without supposing that the success owes to the theory being true.

<sup>36</sup> Psillos, for example, understands van Fraassen's argument this way (1999 and 2001), which is rejected by Ladyman, et.al. (1997), as shall be discussed shortly.

of IBE in the realm of observables was, as a matter of fact, put forward as part of a critique of IBE, not as part of van Fraassen's new empiricist epistemology. The example serves as a foil. As emphasized further in his later works, for van Fraassen 'the rule of IBE is unacceptable in general' (Ladyman, et.al. 1997., 312) What then does he do when he infers the existence of the mouse upon observing all the evidence that seems to point to the mouse? The reasoning he used might be what realists describe as IBE; that, however, only shows that under certain conditions perhaps it is *pragmatically* indispensable, 'but that would not make it a rule of reasoning that issues in rationally compelled belief.' (Ladyman, et.al. 1997., 312)

In his exchange with van Fraassen and his co-authors, Psillos responds to this line of defense by saying that if the kind of inference a constructive empiricist uses is, as his opponents admitted, equivalent with IBE in reference to observables, then IBE is certainly equally reliable in the case of observables, thus the constructive empiricist do not have any reason to reject it. (Psillos 1997, 371 and 1999, 214) Doubting IBE in the case of observables would also be equivalent to doubting the empiricist's inferences to empirical adequacy. If IBE about observables have to be accepted, then what would be the reason not to accept IBE which involves unobservables? An answer to this question hinges very much on the legitimacy of the observable-unobservable distinction. In relation to this, Van Fraassen mentions "immediacy of experience" as what characterizes observables: '[W]e can see the truth about many things: ourselves, trees and animals, clouds and rivers in the immediacy of experinece.' (van Fraassen 1989, 178) However, "immediacy of experience" does not

do the work of distinguishing the observable and unobservables well. Instead, practically what it distinguishes is between the observed and unobserved. Extinct animals, for example, could hardly be said to be available in our immediate experience; the fossils are. Our experience of the animals comes through inference—an IBE, realists would insist.

Another way to defend IBE about only observables is by saying that in that case, we do not introduce new ontological commitments, since ‘*we already believe that mice exist*, that is we use IBE to conclude new facts about tokens of types that are already included within our ontological commitments.’ (Ladyman 2002, 221) But, taking the example of (observable but unobserved) extinct animals again, would we say that in this case we only conclude new facts about tokens of a familiar type? We certainly want to say that mice which ate the cheese in the house is more believable than dinosaurs which we only know from their fossils, and both more believable than electrons. And that probably is all we can say meaningfully about observable-unobservable distinction. As pointed out by many authors, the distinction of observable and unobservable is difficult to draw strictly. In many cases, this distinction clearly comes in degrees. Therefore accepting IBE in reference to observables but not to unobservables is difficult to sustain.

What about the charge that IBE is not a rule of inference? In the exchange mentioned earlier, Psillos has not answered satisfactorily one of van Fraassen’s most important points when he insists that he rejects IBE as a *rule* of inference. As Ladyman et. al. explicitly say, their arguments are directed against ‘IBE understood

as a *rule* of inference, not as an inferential practice.’ (Ladyman 2002, 221) A constructive empiricist does not need to dispute that scientists might indeed routinely use some IBE-like inference, but may say something like the following: ‘Where scientists adopt theory *T* on the grounds of its explanatory power, the realist construes this to mean that *T* is true, but the non-realist can assert that *T* is merely empirically adequate.’ (Ladyman, et.al. 1997, 314) The criticisms of IBE as a rule has indeed been repeatedly emphasized by van Fraassen. In his *Laws and Symmetry*, under the section ‘Why I do not believe in inference to the best explanation’, he says:

Someone who comes to hold a belief because he found it explanatory is not *thereby* irrational. He becomes irrational, however, if he adopts it as a rule to do so, and even more if he regards us as rationally compelled by it. (van Fraassen 1989, 142)

Indeed, this does not seem controversial. Deduction is certainly governed by rule, and we are rationally compelled to believe the conclusion of a valid deduction. Enumerative induction, while it does not have a rigorous rule as deduction does, can be assessed rigorously enough by using statistics. An explanation, on the other hand, is evaluated by a set of criteria that are difficult to define precisely, and our assent to it comes in degree—it is not always “rationally compelling”. As such, as pointed out by McMullin, the very term “inference *to* the best explanation” may be misleading, since it is more an “inference *that*”, which is not so rule-governed. (McMullin 2003, 467). It is not a matter of rule, but of value judgment. We shall discuss this issue later. For now, it should be noted that van Fraassen’s objection to IBE is based on a particular conception of IBE which, as a rule realists need not hold.

***b. The argument from the best of a bad lot***

In van Fraassen's understanding, IBE is supposed to justify the way scientists form 'warranted new beliefs on the basis of the evidence... in a purely objective manner.' (van Fraassen 1989, 142) But this is simply impossible, since it only selects the best among the *historically* given hypotheses. At any point in time, the best explanation may have not yet been born. 'So our selection may well be the best of a bad lot.' (van Fraassen 1989, 143) Our best explanation may very probably be the best of a set of false explanations. If IBE is as van Fraassen presents it, the belief that the best of a set *X* will be more likely to be true it indeed 'requires a prior belief that the truth is already more likely to be found in *X*, than not.' (van Fraassen 1989, 143) In principle, realists still need the warranty that somehow the best explanation is already included in the set of their rival hypotheses. The warranty may be given by some principle of privilege, by an acceptance that somehow 'we are by nature predisposed to hit on the right range of hypotheses.' (van Fraassen 1989, 143) But this would be very difficult for empiricists to accept.

How would a realist respond to this charge? One way is that he might accept the charge that he needs to appeal to just the kind of epistemic privilege mentioned by van Fraassen, but try to defend it not in the way van Fraassen envisages it.<sup>37</sup> Psillos

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<sup>37</sup> Van Fraassen considers two ways realists may find the justification for that privilege. The *Naturalistic* response, which argues that our evolutionary success in the past has given us that privilege; and the *rationalistic* response by Alvin Plantinga.

appeals to a *background knowledge privilege*. He sees that theory selection does not operate in a knowledge vacuum. The case is *not* that the scientists' only information is a set of probable hypotheses which entail the evidence. 'Rather, theory choice operates within and is guided by a network of background knowledge.' (Psillos 1999, 217; cf. Psillos 1997) As the first step, background knowledge can rule out a very significant number of competing hypotheses as candidates for a potential explanation. Secondly, when the background knowledge still leaves more than one hypothesis, explanatory considerations (simplicity, fertility, etc.) then come into play to select the best hypothesis.

There might be objections here that the set of accepted theories that count as background knowledge might themselves issue from a bad lot of hypotheses. But entertaining this doubt too far might amount to an assumption that evidence can never guide scientists to form approximately true beliefs; a point of view that can easily lead to skepticism that van Fraassen himself may not be ready to accept, since it may undermine even the acceptance of theories based on their empirical adequacy. For empirical adequacy demands that the hypothesis saves *all* phenomena—not only past and present ones, but also future phenomena. As such, claims with regard to empirical adequacy are already ampliative claims that go beyond known evidence. An unbridled skepticism would undermine all ampliative inference, including inference to the empirically adequate hypothesis. Van Fraassen's constructive empiricism needs the privilege of background knowledge as well.



It can not be denied, however, that the epistemic risk taken by constructive empiricists in this regard, which issues from that epistemic privilege, is significantly less than the one that realists have to take. The realists demand more from theories (i.e. approximate truth) than the empiricists do (i.e. empirical adequacy). It is only natural that the former has to take more risks. Obviously, there is always a trade off between the risk taken and one's epistemic stand with regard to background knowledge. As a realist, Psillos acknowledges this trade-off, and is ready to pay the price. 'In taking this extra risk, realists want to know more about scientific theories than do constructive empiricists. So the latter are unjustified in suggesting that this risk is not worth taking on safety grounds... if risk is the price to pay for pushing back the frontiers of ignorance, it is well worth the expense.' (Psillos 1999, 222) If this statement is intended to persuade an empiricist to enter the realist game, then most probably it won't work, since it begs the very issue at the center of the debate between realist and empiricist. The latter has chosen not to aspire to knowledge about unobservables precisely because it is regarded as unreliable—in other words, too risky. For him, science is already very worthy without talking about truth.

Realists, however, do not need to bite the bullet shot by van Fraassen in his argument from the best of a bad lot. They can propose a very different response to the argument by questioning its presuppositions. As argued by McMullin (2003), anti-realists tend to put too much emphasis on the search for the "best explanation" among its rivals. If that is the case, the charge that the best explanation may come from a bad

lot is indeed quite reasonable.<sup>38</sup> ‘What matters to the realist case, on the contrary, are the intrinsic merits of the theory under consideration, quite apart from the altogether contingent availability of alternatives.’ (McMullin 2003, 468) There are two different questions with regard to the explanation of a phenomenon. First, how plausible is the hypothesis to *causally* explain the data? Second, how does it fare in competition with its rivals? The answer to the second question is dependent on the answer to the first, but *not vice versa*. Beside the question of whether a hypothesis is true, there is an independent question about whether it does causally explain the data. Anti-realists emphasizes the question about truth and tend to disregard the other question, which could be assessed by considering the hypothesis’ intrinsic merits as a causal explanation.

As a matter of fact, it seems that what van Fraassen ultimately objects to is not so much the existence of alternatives as the denial of the epistemic value of explanation. Explanation is, first of all, a function of human interests in theory assessment. It is devoid of epistemic values. Here van Fraassen seems to take explanation in a very limited sense, that is, only as one of the factors involved in theory-assessment. Proponents of IBE, on the other hand, see it in a more global sense, in which a multiplicity of epistemic factors (like data fit, logical consistency) contributes to explanation. (McMullin 2003, 470)

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<sup>38</sup> This is another reason why “inference to the best explanation” is not the appropriate term to call the retroductive inference as elaborated by Peirce, the first reason being, as discussed earlier in this chapter, that such an inference is actually an “inference *that*”, instead of an “inference *to*”.

Assessment of the intrinsic merits has to do with the criteria of good explanation, such as fit with the data at hand, logical consistency, coherence with background knowledge, absence of ad hoc features, fertility, and unifying power. (McMullin 2003, 469) Furthermore, McMullin points out an important point that these criteria do not carry the realist weight equally. The criticisms that fulfillment of these criteria do not necessarily legitimate belief in the real structure of the phenomena (which usually involves the unobservables) are mainly directed to virtues such as data-fit, consistency, and coherence. These virtues may be fulfilled by a clever tinkering by the theorist. But this charge does not apply to what McMullin calls diachronic virtues, like fertility, that manifest themselves over time by giving rise to novel predictions that do not falsify the hypothesis. '[T]he fertility of the theory in directing research over time could not be attributed to [the ingenuity of the mathematician]: only some purchase on real structure could explain it.' (McMullin 2003, 469)<sup>39</sup>

So, it should be noted that non-existence of rival hypotheses carries much less weight than the critics of IBE assume it, so since comparison between a hypothesis with its rivals does not figure as importantly in the decision to accept the (approximate) truth of that hypothesis, as anti-realists tend to construe it. When Psillos in his defense of IBE against the argument from "a bad lot" invokes the capability of evidence in general to guide scientists to form approximately true

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<sup>39</sup> Cf. (McMullin 1978) already elaborates the structural explanation. See the discussion below.

beliefs, his argument can be sharpened by pointing to evidence which strengthens the diachronic virtues. Such evidence compels scientists much more strongly to believe that they have reached the causal structure of the phenomena. In the final analysis, if impressive diachronic virtues of a theory can not move an anti-realist at all, it becomes really difficult for him to say that his understanding of the world could ever increase—even with regard to non-controversial cases like the reality of the earth's motion. (Cf. McMullin 2003, 470; 1994, 100)

If that's the case, it is, then, a matter of (personal) choice, not strength of anti-realist argument against IBE or other realist weapons. In this regard van Fraassen explicitly says that he rejects the demand for explanation for what he sees as legitimate reasons. The reasons include cases where demand for explanation is (1) rejected, because it is not available (why an individual with syphilis contracted paresis but not others with the same conditions); (2) impossible (quantum mechanics, time of a single radioactive atom's decay); or (3) not needed (principle of inertia) (van Fraassen 1980, 111) Yet van Fraassen clearly too easily dismisses the fact that explanations *can* be given in the case of many more specific scientific theories, and ones whose plausibility and fertility are very compelling. In these cases, van Fraassen's rejection really loses ground. Besides, in those cases, realists do indeed seem to offer the best explanation. It might be true that, as Psillos complains, the empiricist wants to play it safe, does not want to take reasonable risk. But that is not all. It turns out that he also has so much tolerance for the unexplained. This is, then, not only a matter of playing safe, but in this case such as attitude verges on

irrationality—despite van Fraassen’s conviction that the realist cannot compel the anti-realist to acknowledge the irrationality of antirealism (van Fraassen 1989, 142). The task of the realist at this point is to persuade his opponent by giving an account of the epistemic virtues of explanation.

#### **4. McMullin on structural explanation**

Another positive argument on the part of the realist concerns the idea of structural/causal explanation. Ernan McMullin is one of the very few who has consistently developed this idea for almost three decades. In one of his early papers (“Structural Explanation”, 1978) he traces this idea from Aristotle to Hume, to Mill, to down and Charles Peirce. He explains what he means by structural explanation at the beginning of the article:

When the properties or behavior of a complex entity are explained by alluding to the structure of that entity, the resultant explanation may be called a *structural* one. The term “structure” here refers to a set of constituent entities or processes and the relationships between them. Such explanations are causal, since the structure invoked to explain can also be called the *cause* of the feature being explained. (McMullin 1978, 139)

His examples for structural/causal explanation is hydrogen fusion as the cause of sun’s emission of radiant energy; or the solid-liquid interface between the mantle and the core of the earth as the cause of certain seismographic tremors recorded on the earth’s surface; or the covalent bonding between individual atoms of the diamond

crystal lattice as the cause of the hardness of the diamond; or the interaction of incident light with the outer electron orbitals on the surface atoms of the reflecting body as the cause of characteristic light frequencies reflected by solid objects and perceived by us as color; etc. The more simple mechanical explanation that portrays the observable operation of a machine, such as clockwork, is structural too. In this case the explanation is inferred directly from cause to effect. But what is more common in science is when the structure is postulated to account for the observed properties or behavior investigated, in which the inference proceeds from effect to cause. This explanation is hypothetical, since the postulated structure might not be unique. The theory that stems from the hypothesis ordinarily includes a specification of the structure, which is often called a physical model. This explanation could be called “hypothetico-structural”, in contrast with a hypothetico-deductive explanation. The warrant for the theory that postulates the structure as the cause is its success and the success of the broader theory of which it forms a part.<sup>40</sup>

McMullin sees that structural explanation has become the dominant form in science only since the 19<sup>th</sup> century. In the periods preceding it, there were no obvious examples of successful micro-structural theories (ones whose [theoretical] entities are below the level of observation). (142) One lesson that McMullin draws from this episode is that the lack of good account of structural explanation in this period is due not to its principal impossibility, but more to the state of science of the day.

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<sup>40</sup> In the previous chapter, we discussed a criticism of Worrall’s notion of “structure” in his structural realism. McMullin’s discussion about structural explanation here may indicate how “structure”, which in Worrall is in between phenomena and noumena, can be understood.

Explanation in mechanics was not structural, since the force postulated to explain motions was very unclear. This led Hume to suppose that microstructural explanation is forever beyond our reach. The best causal explanation scientist can give is the simple clockwork kind of explanation, where the paradigm of the notion of “cause-effect” is a billiard ball’s motion causing another ball to move: one perceived motion is invariably followed by another. “Gravity”, “cohesion”, “elasticity” became the ultimate explanation, of which no deeper inquiry is possible. (McMullin 1978, 139) As a matter of fact, as McMullin argues in many of his other writings, mechanics is indeed a special case posing special problems, even until today. But in other areas of science, such as geology and biology, structural explanation postulating unobservable entities or processes was possible. This observation has an important implication on the realism debate, as I shall discuss later.

Before delving into that issue, there is a question as to why structural explanation is preferred. McMullin mentions two reasons. *First*, it reaches beyond empirical co-occurrence (as in the simple billiard kind of mechanical explanation), to postulating a structure of the unobserved entity that could account for the co-occurrence. It enables what Newton calls “penetration into the invisible realm”, but which he sees as very difficult. Structural explanation for the most part is indeed metaphorical and always tentative, but nevertheless it allows an ontological claim to be made. Some unobserved causes are held responsible for the observed phenomena. This kind of explanation provides scientists with more knowledge of processes and structure (“enlargement of the world”), which is beyond the aim of prediction and

control only. *Second*, the ontological claim is reached through retroductive steps. While induction (in its limited sense of reasoning from particular to general) involves a leap in extension only, retroduction makes a much greater leap. What grounds the ontological claim made through retroduction is its explanatory power.

At this point there is another sort of argument an anti-realist can use, i.e. an argument from the history of science. Many cases in history of science can be cited to show that structural explanation may be totally mistaken. one example, is the oft-quoted caloric explanation of heat phenomena. The explanation was good, but it turned out to be incorrect. The first thing that should be mentioned in this regard is that ontological assertions about the structure of a phenomenon is provisional; there is no claim to certainty. Secondly, indeed, we can always find examples like this, but there are also impressive cases of successful explanation. It is, therefore, instructive to look in general at cases where retroduction is successful and those where it is not.

### **5. Differentiation of areas of retroductive success: which science should be the paradigm for realism debate?**

As a start, it can be said that in issues where certain cases are invoked to show failure or success of an explanation, the history of science can not immediately decide in favor of one of the opposing parties. The problem is that anti-realists tend to focus on some special cases to argue for their anti-realism. I have mentioned earlier that McMullin's analysis of the failure before the 19<sup>th</sup> century of structural explanation, as the kind of explanation that aims at the realist knowledge of the structure underlying



the phenomena, was due more to the state of science of the day than to any principal difficulty in constructing a structural explanation. If this is accepted, one of its important implications for the realism debate is that we better not speak of “realism” in science in a global sense, since the case for realism depends very much on the science involved. Therefore, when we speak about realism, it would be helpful to differentiate between theories or branches of science. This way of arguing for realism has been developed by McMullin consistently since his early writings almost three decades ago (e.g. McMullin 1984, 27-30; cf. McMullin 1978, 1994, and 2003).

As a start, we can take examples from geology, which in general has displayed an impressive success of structural explanation. For instance, structural explanation of surface phenomena such as sonar pulses is a reason to believe in the existence of sub-surface structures like pockets of water or oil. In this case, whether or not the water or oil is there can be directly determined. But in principle, the structures here play a role very similar to that played by molecular structures in the explanation of chemical phenomena. Concerning the history of earth, from the visible strata and their fossil contents geologists were able to reconstruct the formation of mountains, etc. The order of the geological periods have been established with increasing accuracy, and our knowledge in this area has been cumulative, despite the fact that a geological period, say the Devonian, is an unobservable, theoretical entity. In a similar way we have increasingly known more about the (unobservable) living species of the Devonian period.

In cell biology, our knowledge of the constituents of the cell has also been steadily increasing. Despite controversies about what a gene actually is, and the changing conceptions of the gene, it is quite difficult to deny the fact that we have a cumulative and reliable knowledge of it. In this respect, Philip Kitcher shows that the changing conceptions of the gene held by biologists are *not* incommensurable. Based partly on linguistic analysis, he shows that indeed scientific concepts such as “gene” do change, but what took place in the history of the concept was a continuous refining of earlier concepts. (Kitcher 1982) In this sense, our knowledge can be said to be cumulative. As in the earlier case, the cumulativeness of our knowledge indicates that we got the picture (“the ontology”) approximately right; progress here means refinements. This suggests that the realist belief that our theories are capable of probing deeper into the nature/structure of things is well founded. Further, Michael Simon has also noted that the story of the progressive unraveling of the structure of chromosome, and the way scientists define the structure of a gene can be best understood in terms of scientific realism. (Michael Simon, *The Matter of Life*; quoted from McMullin 1984, 28).

In those areas, many objections to realism can be met satisfactorily enough. If we take a broader view of the history of science, we can still find abundant cases of successful structural explanation which aims to unravel the causes of things, such that, for McMullin, ‘It can be argued ... that there is a presumption in favor of [the ontological reliability of the retrodution] and that the burden of proof rests on the instrumentalist in consequence.’ (McMullin, 1978, 147)

On the other hand, as McMullin has pointed out repeatedly (cf. McMullin 1978, 1984, 1994, 2001, 2003) some of the most important sources of anti-realism lie in a selective reading of the history of science. More specifically, he points to three areas in science that have become the source of anti-realism, i.e. classical mechanics, quantum mechanics, and elementary-particle physics. (McMullin 1984, 9-16). In his very recent article (2003), commenting on van Fraassen's anti-realist stance, he repeats this point: 'It is not accidental ... that van Fraassen's early concern in the philosophy of science lay in significant part with quantum mechanics.... And as everyone knows, the ontological status of the quantum world is indeed problematic' (McMullin 2003, 475)—that is, compared to the world investigated in geology or biology.

In mechanics in general scientists themselves were often faced with enormous difficulty as they attempted to characterize their theoretical constructs. This does not necessarily mean that they endorse antirealism, but simply that they encountered difficulties in depicting the constructs. A famous example is Newton's "force" or "attraction". Newton was not able to pinpoint his "force"; what is it actually? Where does it reside? Does the inverse-square law say something about the force other than that the sun and planet tend to move in certain ways? The ideas of active principles or ether could not satisfy him. All this despite the predictive success of his mechanics. Does this difficulty go against realism? To be sure, this difficulty (to find a good ontology) is only faced by realists; but this does not mean that realism is thereby refuted. On the realist part, this instead means an admission that 'no decision can be

made in this case as to what the theory, on a realist reading, commits us to.’ (McMullin 1984, 11).

A very similar problem arises in the case of quantum mechanics. For instance, the behaviour of light as both wave and particle indeed has puzzled scientists. While many see this as implying antirealism, this actually only means that the view of the world suggested by quantum mechanics is nothing like the classical worldview at all, and as such scientists and philosophers find enormous difficulty in depicting this world. This is surely different from saying that we cannot infer anything about the entities that make up the world. Indeed,—an assertion that this world is “strange” already implies a realist stance, though a poor one. Similarly, the fact that dynamical variables and its constituents are measurement-dependent (or, in some interpretations, even observer-dependent) may suggest an idealist view of the world, which is opposed to a realist *metaphysics*, but not to scientific realism.

Similarly, in elementary particle physics there is a difficulty in thinking about microentities such as electrons—that is, when one is forced to think about the electron in a conventional way as a discriminable entity (particle) with a certain spatial magnitude and obeying standard mechanical properties. The difficulty is even harder when one thinks of quarks as “constituent” of entities like protons, since they do not exist in a free state and cannot be pinpointed. When we compare the existence-claims in these cases with one with one regarding to the plate tectonic model in geology, for example, we will quickly note the significant differences. Even though both models are unobservable, there is no question about the meaning of the

existence-claim of large plates of material underlying continents and oceans which move very slowly.

In all these cases in physics, problems in imagining the constructs of science in familiar categories never refutes realism as such, but a certain metaphysics associated with it. As such, the main lesson to be drawn from them is not about the plausibility of scientific realism. Instead, the main lesson is that constructs created by physicists have oftentimes posed grave interpretative difficulties. The more general lesson is, as we have mentioned earlier, that one should not expect to find an ontology as a direct “implication” of scientific theories, nor as something readily understandable or interpretable in familiar categories. On the other hand, physics tends to explore a world in terms which are deeper than categories already familiar to physicists at the time, and as such it is always difficult to find satisfactory metaphors. McMullin suggests that probably the difficulty in understanding the world as depicted in physics derives from the status of physics as the “ultimate” natural science. The explanation attempted tends to be more and more ultimate, and as such no evidence is then available that could decide the matter empirically.

Another indication of the peculiarity of mechanics and physics is the fact that (ontological) *interpretation* is felt more urgently here compared to other fields of science. Interpretation here is understood *not* as interpretation of pure formalism which gives physical meaning to the mathematical formalism, but as one that connects the physical formalism to ontology. In this second sense, interpretation is commonly understood as an answer to the question of what the world would be like if

the theory is true (while formalism is an algorithm for predicting the behaviors of physical system). (Cf. Albert 1992, 17) Interpretation in that sense refers to the underlying structure of phenomena; it is a move further than merely saving the phenomena. As such (ontological) interpretation is not a genuine issue for anti-realists who would argue that such a move is illegitimate and do not care about what the theory says about the world.

It should be noted that the issue of interpretation comes up most visibly in the case of quantum mechanics. On one hand it is a theory which is of incredible accuracy in predicting the behaviors of the “quantum world”, but, on the other hand, what it says about the world is really not clear. It begets several competing and contradicting interpretations. Since it is a move further than the realm of the phenomena, there is no way to empirically decide which is true.

Sklar sees that as a matter of fact the essential motivation for interpretation comes from within science itself.

*‘Demands for the interpretation or reinterpretation of a theory come from a sense, within theoretical scientific community itself, that ‘something has gone wrong’ with the theory itself...[although] there is no reasonable question that the theory is ‘the very best available’ alternative to account for the data that was in need of explanation.’ (Sklar 2000, 730)*

That “something has gone wrong” is felt most urgently in the case of quantum mechanics. Sklar points out further that in this sense, quantum mechanics is quite similar to many other foundational theories, which include Newtonian mechanics, statistical mechanics, and general and special relativity. That is, they are afflicted

with certain “puzzles” or “dilemmas” or “anomalies”. Those theories might come into “conflict with evidence” in a different way than other theories conflict with evidence.

There are then at least two reasons to make a differentiation between branches of science when we discuss scientific realism; more specifically, two reasons not to use mechanics as the paradigm of the realism debate, due to its peculiarity. McMullin has shown that in its history mechanics and, in general, physics, always posed difficulties for realists. He suggested that the difficulties are due to the status of physics as the “ultimate” natural science; it always seeks more ultimate explanation. Similarly, Sklar argues that “foundational theories” are quite unlike other theories in some senses; and they require interpretation more than other theories—and, as mentioned earlier, urgency of interpretation indicates that the world as depicted by the theories lacks clarity.

At this point it is interesting to look at Peirce’s differentiation of sciences based on their use of either induction and retrodution. After urging the distinction of these two modes of inference, he shows its merits. One of them is ‘that it leads to a very natural classification of the sciences and of the minds which prosecute them.’ (Peirce, 143) The three groups of science are:

1. Classificatory science (purely inductive): botany, zoology, chemistry
2. Science of theory: astronomy, pure physics, etc.
3. Sciences of hypothesis: geology, biology, etc.

We see that in the examples discussed earlier, retrodution is more successful and trustworthy in sciences such as geology and biology (sciences of hypothesis or

retroduction), while it poses difficulty in “sciences of theory”. In other words, this division in a sense, and to a certain extent, parallels the division of sciences where realism is more plausible, and those where the realist position seems to draw many difficulties. This gives another ground to suggest that when we concentrate on a certain group of sciences realism becomes much less problematic. A consequence of this position is admission that indeed retrodution is less reliable in mechanics, in particularls, and physics, generally.

One conclusion we can draw is that if we set aside mechanics (quantum mechanics as well as 18<sup>th</sup> century mechanics) and, more generally, physics, and concentrate on other areas of science, realism becomes much more plausible and easy to accept. In other words, by not making mechanics, when recent anti-realism gains its most plausibility, as the paradigm of realism debate, the prospect for scientific realism looks brighter. Sklar’s reference to “foundational theories” and McMullin’s to “ultimate explanation” give us a good sense of how mechanics must be treated with special care when we discuss realism. The main issue, of course, is not which science should be the paradigm of realism debate. Rather, it is that generalization of difficulties found in particular branches of science would not give us a one-sided picture of the problem.

I shall trace more comprehensively the consequences of the points I made here in the next chapter. For now, I shall very briefly wrap up my main points. In this chapter I have (1) tried to delineate what IBE or retrodution, as one of the main terms of realism debate, is and (2) shown its logical structure, (3) discussed some



objections to it and replied to them. One of the most important arguments for retrodution is historical: it has worked very well in many cases, though logically it is impossible to defend its validity, as is the case with induction. As such the argument for retrodution can never be conclusive. The fact that it worked well in some sciences but is dubious in others should not be taken as refuting retrodution. Rather than abandoning the possibility of retrodution as a way to enlarge our understanding of the world, I would suggest that it is more constructive to say that retrodution does have certain important limitations. In turn, it is these limitations that would impose restrictions to any attempt to draw a “scientific picture”, a broad picture of what the world really is like based on our scientific knowledge. I will explicitly state these limitations in the next chapter, after trying to provide a better shape to the kind of realism I espouse, so that we get a more complete picture. \*\*\*

## Chapter 4

### **Realism, Interpretation, and Metaphysics**

*“It is only a myth that modern science had arrived at a clear and well-integrated world-picture, or that contemporary science has already effectively given us a new one. At best, we are in process of replacing what never has existed by something that never will.”*  
(van Fraassen 1991, vii)

In the second chapter, following many authors, I distinguished two components of realism, i.e. metaphysical and epistemological. The first asserts the existence of certain entities or structures postulated by scientists, independent of the awareness of the scientists individually or as a social community. Anti-realists, in this sense of realism, include the idealists, who deny the existence of mind-independent world; and social constructivists, who regard that those entities and the so-called natural laws that describe them do not have an objective existence, instead they are the products of human creation agreed upon by the community of scientists. The epistemological component, on the other hand, asserts that we can know entities or structures mentioned above that inhabit the world and the laws that govern them. In one version of realism, it is claimed by the realists that the aim of science is to find out true theories about the world. Anti-realists might dispute our capability of probing into the

reality beyond mere appearance; she might believe that the aim of science is limited to making predictions, that theories (along with the entities they postulate) are mere fictions created to be useful as tools of prediction (instrumentalism), and that successful theories only warrant belief in the empirical adequacy of the theories, and not an indication of their truth (constructive empiricism).

In the previous chapters I restrict the discussion about realism to its epistemological dimension. While what many realists ultimately want to have is a description about the world that is based on scientific theories, it is also important to first disentangle the two different issues in realism, since scientific realism has too often been unnecessarily discredited by opponents of realism due to its alleged association with certain metaphysical theses.

In this chapter I will first put an emphasis on how scientific realism needs to be disengaged from certain metaphysical theses or particular views of reality in both its components (epistemological and metaphysical). Later in the chapter, I will defend John Searle's "external realism" which actually is a sort of metaphysical realism, but one which manages to avoid commitment to a particular metaphysics except in a very general way. Ultimately, however, a realist position should be able to give a picture of the world that is scientifically credible. In the last part of this chapter I shall argue that the activity that bridges the gap between scientific theories and a world picture supposedly based on them is interpretation. Since not much has been discussed about interpretation as a (scientific or extra-scientific) activity, I will only discuss this in a general way. An important point here is that any attempt to draw a big picture of the

world (“worldview”, or “metaphysics”) should be viewed as an interpretative attempt and evaluated accordingly.

## **1. Realism and commitment to metaphysics.**

Scientific realism has unfortunately oftentimes been associated with metaphysical commitments of a global scope or with a particular view of reality. The example for the first can be found in what Hilary Putnam calls “metaphysical realism”, which he characterizes as consisting of the theses that ‘the world consists of some fixed totality of mind-independent objects. There is exactly one true and complete description of “the way the world is”.<sup>41</sup> Truth involves some sort of correspondence relation between words or thought-signs and external things and sets of things.’ (Putnam 1981, 49) Further, ‘its favorite point of view is a God’s Eye point of view.’ These theses certainly pertain to issues of a much broader scope than scientific realism. This is probably what Fine (1984) has in mind when he vehemently criticizes the realism which is saddled with excessive metaphysical claims and offers his metaphysically modest kind of realism, the Natural Ontological Attitude.<sup>42</sup> The first thesis (‘the world consists of some fixed totality of mind-independent objects’), especially, while

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<sup>41</sup> Popper in his *Conjectures and Refutations* mentions three views of knowledge, one of them is “essentialism”, which is very similar to Putnam’s metaphysical realism. The other two views are instrumentalism and realism. It is interesting that realism is here made into a distinct category, indicating that “essentialism” (or metaphysical realism) is more than just realism.

<sup>42</sup> Fine unequivocally characterizes his position as neither realism nor antirealism. But his acceptance of unobservable entities postulated by theories and other important realist theses would put him in the same camp with the majority of scientific realists—though surely not Putnam’s metaphysical realism. Further, his refusal to give justification for this acceptance makes him not a “non-realist”, as he would call himself, but a poor realist.

vague, seems a really unnecessary claim that a realist should make. A realist can accept the existence of entities postulated by mature scientific theories without believing that the world consists of some “fixed totality of objects”—whatever that means. Similarly, there is no need for a realist to claim his as “God’s Eye point of view”, which indicates absoluteness, to claim that scientific theories give us objective (or inter-subjective) knowledge of some phenomena.

Second, the idea of realism as espoused here so far is not committed to uphold some kind of comprehensive and coherent “scientific worldview” as a particular view of reality, but to assert that science can afford us with some epistemically reliable knowledge or understanding of the world, which might be piecemeal. Thus the opponent of such realism is not so much idealism (as was the case with the old realism debate), but instrumentalism or constructive empiricism, whose proponents avoid attributing to science any capability of giving us a relatively deep understanding of the world, and for whom the function of science is limited to giving us pragmatically reliable knowledge of the world. Understood in this way, the appearance of new scientific theories that suggests a new ontology does not necessarily refute realism. It *may* refute realism only if an understanding of the phenomena dealt with by the new theory is made impossible. Such a new theory which readily comes to mind is quantum mechanics. If, as formulated by van Fraassen (1989), realism includes the following thesis: *‘Reasonable expectation of future events is possible only on the basis of some understanding of (or reasonable*

*certainty about) causal mechanisms that produce those events'*, then realism is refuted by the empirical violation of Bell's inequality. But as discussed in the previous chapter, quantum mechanics (and mechanics in general) deserves special treatment. The important point is that a realist *can* change her view of nature as suggested by new theories, without having her realism, i.e. her belief that scientific theories furnish her with some understanding of the natural world, being simultaneously refuted.

Thus the following famous quote from Bohr seems a bit odd, if not a contradiction in itself: 'There is no quantum world ... only an abstract quantum description. It is wrong to think that the task of physics is to find out how nature is. Physics concerns what we can say about nature.' The first sentence is a metaphysical assertion, supposedly based on quantum mechanics, but whose very possibility is ruled out by the second sentence. If physics can't tell us how nature is, how can it tell us that there is no quantum world? Bohr's position on the realism/instrumentalism debate is certainly more complex, and the quotation above certainly does not do justice to him. But my point is to show that there are two different issues here, one concerns views of reality, the other the capability of science to inform us about it. If the latter is denied, one is not justified to say anything about the former. Following many realists, my suggestion is that the theses of scientific realism should *not* be regarded as a conjunction of an answer to the first issue and a positive answer to the second one. Refutation of a particular view of reality, then, does not imply refutation

of scientific realism. Realists should be open-minded as to what the world is like, and they might disagree among themselves in answering that question.

According to Fine, the death of realism was ‘hastened by the debates over the interpretation of quantum theory, where Bohr’s non-realist philosophy was seen to win out over Einstein’s passionate realism.’ (Fine 1984, 21) Fine, indeed, sees Bohr as a through and through antirealist. As a matter of fact, he sees the whole progress of 20<sup>th</sup> century physics as led by antirealist attitude; this includes the theory of relativity. The latter was developed single-handedly by Einstein in his positivist, antirealist period. As a realist, Einstein was, in Fine’s phrase, at war with the non-realist Bohr, who believed that ‘Einstein’s realism, if taken seriously, would block the consolidation and articulation of the new physics and, thereby, stop the progress of science.’ (Fine 1984, 32) However, Fine seems over-confident in telling this story, which is not uncontroversial at all. An alternative story, such as told by Folse (1989), would present Bohr as a realist, though holding a kind of realism totally different from Einstein’s. He was not opposed to Einstein as a realist, but, based on his belief about the completeness of quantum theory, Bohr called for ‘a radical revision of our attitude towards the problem of physical reality.’ (Folse 1989, 262) The Bohrian world picture would be one that is based on complementarity, while Einstein insisted on understanding the theory on “classical” terms. In this understanding, “the war” between Einstein and Bohr was not one between realism and non-realism, as Fine

presents it, but between what may be termed “classical realism” and “non-classical realism”.

In the foregoing analysis, Einstein is more than a realist; the additional component to his realism being his specific, classical view of about the world, in which dynamic variables have unique real values at all times and that there is deterministic relationship between successive sets of these values. What is disputed by Bohr is those additional, classical components. So, the controversy here is precisely about the kind of world we live in as presented by quantum theory—a controversy that can take place only among realists. It is not that Bohr believes nothing can be inferred from quantum mechanics about the entities that constitute the world. Instead, based on his complementarity principle, ‘what can be inferred is entirely at odds with what the classical world view would have led one to expect.’ (McMullin 1984, 12) This is of course not to say that a satisfactory realist, yet non-classical, world view is already in place; and probably at present this demand can’t be met by the realist. But that is different from saying that realism is refuted. The strikingly unfamiliar categories by which quantum mechanics may be understood only means that the world may be fundamentally different from the world understood in more familiar categories.

As a matter of fact, if being a realist means commitment to interpretation in familiar categories, realism can be said to have already been refuted very early in the development of Newtonian mechanics. As shown by McMullin (1989, 1992), despite



the success of his mechanics, the realist Newton was never comfortable with the notions of attraction and force. Throughout his career he tried to understand these notions to no avail. The Cartesians, Leibniz, and Berkeley, thought that his mechanics did not explain motion, since force could not be given an acceptable interpretation. What is the moral to be drawn from these cases? For McMullin, this is not about refutation of realism. ‘This would be so, however, only if one were to suppose the realist to be committed to theories that permit interpretation in familiar categories or, at the very least, in categories that are immediately interpretable.’ (McMullin 1984, 10) Instead, it simply means ‘an admission that no decision can be made in this case as to what the theory, on a realist reading, commits us to.’ (McMullin 1984, 11) A similar case, where attempts to understand phenomena in familiar categories (in this case, that of the microworld) did not work, happens also in the case of elementary-particle physics. McMullin sees that actually the complex problems that philosophers face in the case of quantum mechanics (such as how to explain correlation in nonlocal situation) have many parallels in the development of Newtonian mechanics.<sup>43</sup>

The most important point of this section is that the question of the feasibility of scientific realism is independent of the question as to what kind of world picture is suggested by certain scientific theories. Failure to find a satisfactory solution or to reach an agreement in answering the second question does not refute realism.

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<sup>43</sup> This is the basis of the argument in the previous chapter that mechanics is unique and as such could not serve properly as the paradigm for the discussion of realism.

## 2. Searle's "External realism"

So far I have defended a version of epistemological realism, and avoided discussion about the other, metaphysical component of realism. However, as mentioned by McMullin (and quoted in the earlier chapter), the dispute in the realism debate is *ultimately* about existence claims. Thus the realist aspiration to have some scientifically credible picture of the world should provide at least an outline of a viable metaphysical realism. For this I will defend John R. Searle's "external realism", a sort of metaphysical realism which, in parallel with what has been suggested so far, manages to avoid commitment to a particular metaphysics except in a very general way. In the following my discussion of Searle's idea will focus on his *The Construction of Social Reality*, which is the only place where Searle comprehensively lays out his view about realism.<sup>44</sup>

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<sup>44</sup> More specifically, the discussion about realism is concentrated in the last three chapters of the work. It was originally intended as just one introductory chapter, an effort at philosophical housekeeping. Thus this part, which makes up one third of the book, was not meant to be controversial. Yet, judging from the reviews of the book, they turn out to be among the issues that attract most attention. Some reviews focus exclusively on realism; some even see the book as an attempt to rebut social constructionism, which, in this book, is characterized as a variant of antirealism. The latter is the view of Colin McGinn (1995). McGinn sees that Searle would not have written the book were it not for 'the polemical purpose of refuting certain fashionable doctrines to the effect that reality itself is a construction from human institutions.' (McGinn 1995, 40) Lyn Spillman, seeing the title of Searle's book as a variant of Berger and Luckman's classic sociological treatise *The Social Construction of Reality*, takes Searle as wishing to challenge 'any unreflectively large claims for the various forms of social constructionism that are influential in many sociological fields, as well as in postmodern scholarship and philosophical antirealism.' (Spillman 1996, 548) Curiously, a social constructionist's (Harry Collins) review (in the scientific journal *Nature*!) sees the book as giving a clarification of 'what can and cannot be socially constructed,' thus gives an impression that the book approves social constructionism to some extent by acknowledging the existence of socially constructed facts. Yet, though the word "construction" appears in many places in the *Construction*, "social construction" is mentioned only disapprovingly. Cf. Hacking (1997, 84)

Searle calls his realism “external realism” (ER). But as will be shown later, it should be understood from the beginning that the way Searle understands his “external realism” is different from the way authors like Putnam use it. Early on he emphasizes that ER is not a belief or a thesis, but a presupposition, i.e. a presupposition that the world exists independently of our representations of it. By representation he means beliefs, desires, language, etc. He clarifies it by saying that ‘If we never existed, most of the world would remain the same.’ Not only a presupposition, it is a *necessary* one, that is, of the meaningfulness of human communication. ‘External realism is thus not a thesis nor an hypothesis but the condition of having certain sorts of theses or hypotheses.’ (Searle 1995, 178) It relates quite closely with his conception of Background.

Background is a set of capacities (like abilities, dispositions, tendencies) that enable intentional states (such as beliefs, desires, rules). The capacities themselves do not consist in the intentional phenomena, but are the necessary conditions of the occurrence of the intentional phenomena. One of Searle’s arguments for it is that the literal meaning of a sentence alone cannot determine its conditions of satisfaction; it does so only against a Background of capacities, dispositions, know-how, etc. The Background is not part of the semantic content of the sentence, since, otherwise, it would need another kind of Background to get its meaning, and thus it would lead to infinite regress. But without the Background, the semantic content may lead to a host of possible meanings. Having the Background, we can most of the time accurately select a true or intended meaning of the sentence. The point is that there is ‘a radical

underdetermination of what is said by the literal meaning of the sentence.’ (Searle 1995, 131) The Background fills in this gap. Searle’s example is the word “cut”. “Sally cuts the cake” and “Billy cuts the grass” use the same verb, yet we know exactly that they have different conditions of satisfaction. Upon hearing the first sentence, we wouldn’t imagine that Sally uses lawn mower to cut the cake. And it is not only about semantic content, but intentional content generally.

So, for example, it is also the Background that enables perceptual interpretation to take place, analogous with the linguistic interpretation above. Background also facilitates certain kinds of readiness (I’m ready to see other skiers on the ski slope; but when giving a lecture I am not ready for a skier to come skiing through the lecture hall.) We are not really conscious of what enables us to accurately understand “Sally cuts the cake”, or to be shocked in seeing a skier come skiing through the lecture hall, yet there must be something in myself that enables me to do so. In these cases, for Searle, it is not the case that we follow some rules, since most of the time we are not even aware of the rules. But the rules somehow make us disposed to behave in certain ways. The result is some kind of structure within ourselves, which is called the Background. And the Background disposes us to behave in certain ways (in response to certain events).

Now ER functions as a part—perhaps the most important one—of the Background. (Searle 1995, 181-182) It is thus a necessary condition for what Searle calls “normal understanding”, which I will discuss shortly. Before that, how would Searle justify his external realism?

Since ER is a presupposition, for Searle, to demand proof for it is odd: it is like demanding proof for rationality—the moment we want to prove it (positively or negatively), we already assume it, since any argument or proof of it would presuppose some standard of rationality. It is like asking “is validity valid?” or “does representation represent”? Especially in the case of ER, it is *purely formal, without any content*: It states that *there is a way* that things are that is independent of all representations of how things are, without determining *a particular way* that things are. So what Searle needs to do is show that it is already there, presupposed by us human beings. This he does by, following Kant, giving a “transcendental argument” for it. Such an argument starts with an assumption that a certain condition holds, and then shows the presuppositions of that condition. Specifically, what Searle tries to argue against are two versions of antirealism: what he labels phenomenalist idealism (all reality consists in conscious states), and social constructionism (all reality is socially constructed). (Searle 1995, 183)

In his move against phenomenalist idealism, Searle asserts that the *condition* that holds is that we do attempt to communicate with each other by making certain sorts of utterances in a public language, while the *presupposition* that Searle wants to show is external realism. That is to say, in any attempt at communication, and for a large class of utterance, the speakers and the hearers share what is called “normal understanding” of those utterances.

Normal understanding is what is presupposed in the above examples of “Sally cuts the cake” and the skier in the lecture hall. Now to understand a large class of

utterances, which includes, for example, “Mt. Everest has snow and ice near the summit”, “Hydrogen atoms each contain one electron”, and “My dog has fleas”, we presuppose that there is a way that things are that is independent of human representations. In other words, ER is a part of the Background presuppositions on the normal understanding of such a large class of utterances.<sup>45</sup>

‘Normal understanding requires sameness of understanding by both speaker and hearer; and sameness of understanding in these cases requires that utterances of the referring expressions purport to make reference to a publicly accessible reality....’ (Searle 1995, 186) Publicly accessible reality means the way things are, in those cases, does not depend on any one’s representation. So, sentences of a public language presuppose a public world equally accessible to the speakers and hearers; and a public world means a world of reality the existence of which does not depend on any one’s representation. That means: there is a way that things are independent of all representations—and this is none other than ER.

ER thus is indifferent to exactly *the* way that things are. Even if it turns out that Mt. Everest and hydrogen atoms had never existed, we still understand the utterances as depending for their normal intelligibility on the existence of an external reality, that is, a world of reality in which there is no Mt. Everest and no hydrogen atoms. But here it is important to emphasize that ER does *not* assert either the

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<sup>45</sup> Later Searle shows that some utterances do not yet have normal understanding, like the case of quantum mechanics. ‘The struggles over the interpretation of quantum mechanics are, at least in part, an attempt to provide a normal understanding of these claims.’ (Searle 1995, 195) But the fact that a large class of utterances presupposes some normal understanding is sufficient for the argument.

existence or non-existence of things. The existence of Mt. Everest is a *truth condition* for the statement “Mt. Everest has snow and ice near the summit.” But even if the statement turns out to be false, that there is actually no Mt. Everest, all the same the statement presupposes that there is a way that the world is independent of our representations as a *condition of intelligibility* of such statements. ER is not a presupposition of existence of specific items in the world, or objects of reference. So, the point here is not epistemic; it is not a condition of truth or knowledge, but comes into play prior to ascribing truth or falsity to statements; as a condition of the very intelligibility of the statement.

At this stage, if all his arguments are valid, Searle has refuted phenomenalist idealism: reality does not consist in conscious states, which are not publicly accessible; instead, a large class of utterances require a publicly accessible reality as a condition of their intelligibility. However, this argument is still not sufficient to refute a social constructionist ontology, which states that reality is socially constructed. We may suppose that all realities in the public world are socially constructed, and still say that they are publicly accessible, just like money, property, marriage—Searle’s examples—are publicly accessible.

With regard to social constructionism, given Searle’s analysis of the logical structure of social (or socially constructed) facts, it is not too difficult to establish the impossibility of the view that all of reality is socially constructed. Very early in his book Searle has stated that institutional facts exist only within systems of constitutive rules; and the rules have the form “X counts as Y in C”. As examples, we may take X

as certain kind of paper which counts as money in certain conditions; or a wall which counts as border between two countries in certain conditions. The socially constructed reality consists in iterations of that structure. In this case, X may be a brute fact or another institutional or social fact. If X is an institutional fact, then we can state X, using a “more basic” fact, say,  $X_1$ , in the similar form “ $X_1$  counts as X in  $C_1$ ”. Repeating this procedure often enough, we will reach a rock bottom of something that is non-institutional, which cannot be stated in the above formula. What would happen if we suppose that all facts are socially constructed? In that case, the iteration would never stop. Since supposing that all facts are socially constructed would produce an infinite regress or circularity in the account of institutional facts (Searle 1995, 56), we can conclude then that the very existence of institutional facts logically necessitates the existence of brute facts. Thus, it is not the case that all of reality is socially constructed.

At this point there are two different charges of circularity against Searle. The first comes from Ian Hacking (1997). At the end of the argument Searle concludes, ‘It is a logical consequence of the main argument of the book that you cannot have institutional facts without brute facts.’ (Searle 1995, 191) Indeed, the existence of brute facts is a logical consequence of the main argument. But Hacking sees that the main argument of the book starts exactly with an assumption that there are two kinds of facts, brute facts and institutional facts. Thus, Hacking argues, it is obvious that the existence of brute facts would follow from the main argument; but the argument then becomes circular.



For Searle, however, his aim in this transcendental argument is not to *prove* the existence of the world that is independent of human representation. As mentioned above, for Searle it is simply odd to demand a proof for the existence of the real world. It is taken for granted. His main argument in the book proceeds from this point, while the thrust of the last argument is ‘to show that you cannot have institutional facts without brute facts.’ (Searle 1997, 106) Actually Hacking (1997) himself, when criticizing H. M. Collins’s construal of *Construction* as *aiming* to show the difference between what can and what cannot be socially constructed, emphasizes Searle’s *starting point*: the distinction between brute and institutional facts. Searle’s transcendental argument shows that institutional facts have to bottom out on brute facts; that the existence of brute facts is *presupposed* by the existence of the institutional facts; or, that brute facts are *logically* prior to institutional facts. In his transcendental argument, the institutional facts are the conditions that hold, and the brute facts are their presupposition.

A different criticism related to this specific argument of Searle’s is made by Lyn Spillman (1996, 548) in her review of *Construction*. One of the intuitive features of social reality Searle notes early in his book is that many social concepts are self-referential. For example, money: ‘in order that the concept “money” applies to the stuff in my pocket, it has to be the sort of thing that people think is money. If everybody stops believing it is money, it ceases to function as money, and eventually ceases to be money.’ (Searle 1995, 32) Although self-referential, Searle contends that the partial definition of “money” as “being thought of, or believed to be money” does

not lead to circularity or infinite regress. This is because “money” here functions only as a placeholder for the linguistic articulation of practices like owning, buying, selling, earning, paying, etc. “Being thought of as money” is merely a description of these practices. Had we wanted to define money, then, we do not actually need the word “money”. “We can cash out the description in terms of the set of practices in which the phenomenon is embedded.” (Searle 1995, 53)

Now, Spillman thinks that if Searle can explain away the apparent circularity or infinite regress in the definition of money, he shouldn’t be worried that supposing all reality as socially constructed would lead to circularity or infinite regress such that he has to posit the existence of brute facts in the second part of the transcendental argument in order to avoid it. Here Spillman seems to confuse the definition of an institutional fact with its structure.

We can solve this by looking at the example of money. In the first case, the case of definition of money, the source of the apparent circularity or infinite regress is that “believing that a certain sort of thing is money” is *constitutive* of the definition of “money”;<sup>46</sup> that is, that an institutional concept seems to refer to itself in its definition. In general, to define Y in C we may start with the structure “X counts as Y in C”, then we will always need to add “and X is regarded as Y”.<sup>47</sup> In defining money

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<sup>46</sup> ‘Something can be molecule/mountain, even if no one believes it as a mountain. But for social facts the attitude that we take to the phenomena is partly constitutive of the phenomenon; if no one believes that it is money, then it is not money.’ (Searle 1995, 33)

<sup>47</sup> Instead of “X is regarded as Y” we may as well say, “X is being used as Y” or “X functions as Y”. Cf. Searle 1995, 32: ‘Logically speaking, the statement “A certain type of substance, x, is money” implies an indefinite inclusive disjunction of the form “x is used as money or x is regarded as money or x is believed to be money, etc.”’

in the U.S., for example, we can start by describing it using the structure “X counts as Y in C” as “Bills issued by the Bureau of Engraving and Printing (X) counts as money (Y) in the United States (C).” There are many ways to describe money; we can also use as our X term “bits of paper with a particular material ingredient and particular printing on it”. In any case, normally our description would consist in several “X counts as Y in C” structures, in which the X term moves closer and closer to brute facts, until finally we reach the “bedrock of brute phenomena”. Having this description, we can define money, or any institutional fact as follows:

Y in C  $\equiv$  Xs (or X<sub>1</sub> and X<sub>2</sub> and X<sub>3</sub> and ...) *and* X is regarded as Y.

The conjunct in the right hand side, “X is regarded as Y”, has to be added since the mere conjunction of Xs does not amount to Y. This is a consequence of a function assignment by agents which is present in institutional facts. To see this more clearly, we can take a simpler example which is actually not an institutional fact, but involves function assignment, such as paperweight. Part of this particular rock’s being a paperweight, for example, is “being thought as a paperweight”. The definition would be “the paperweight (Y) = the rock (X) which is being used (or functions) as a paperweight”. Without the part that mentions “being used as paperweight”, the rock is not a paperweight. Now Searle contends that the concept Y in “is regarded as Y” functions merely as a “placeholder for the linguistic articulation of practices” which uses Y. In the case of a paperweight, we can substitute “being used as a paperweight” with “being used to keep the papers in their place”. In the case of money, such practices are owning, buying, selling, earning, paying, etc. That the very word

“money” has come to function in so many kinds of practices, such that it is difficult to understand it merely as a placeholder or impractical to substitute it with the mention of those practices, is beside the fact. In principle, “money” as well as “paperweight” are mere placeholders. When the word “money” or “paperweight”, which occurs in the “being thought as” part of their definition, is substituted with other facts, the apparent circularity disappears.

However, in the second case, the case of deriving the existence of brute fact from the existence of institutional fact, the source of circularity or infinite regress is the X term itself; that is, the supposition that all Xs are institutional facts. Our form of definition of an institutional fact Y is:

Y in C  $\equiv$  Xs (or X<sub>1</sub> and X<sub>2</sub> and X<sub>3</sub> and ...) **and** X is regarded as Y.

Now in this case we are not dealing with the second part of the definiens (“X is regarded as Y”), but with its first part, which consists in X terms themselves. Definitely the X terms are essential to Y. It is true that “X is regarded as Y” is constitutive of the definition too, but the occurrence of Y here, which is the source of circularity or infinite regress, is not essential, since it is only a placeholder for certain practices or functions, thus can be substituted. But X cannot be substituted, so it must always stay there. Supposing that all reality is socially constructed would mean that the Y and the Xs are all also socially constructed. The circularity or infinite regress results from defining institutional facts by other institutional facts. Since any institutional fact has the form “X counts as Y in C”, the iteration would never stop. The circularity or infinite regress is actual here.

To conclude, it should be noted that Searle's external realism (again, to be distinguished from the external realism that Hilary Putnam attacks in many of his works), has (almost) no epistemic content at all. He does not deny that there is always some perspective in our view of the world, but his realism is a realism of a "pre-perspectival" world. Although he endorses a correspondence theory of truth, he insists that ER does not necessarily imply it, though the reverse is true. ER is not a theory of knowledge, thus it can be consistent with different kinds of theories of knowledge. It doesn't speak about what makes our knowledge true or false. It is also not a theory of language, and it doesn't make a claim that there is a privileged system of representation. To a certain extent, it is not even an ontological theory, since it does not specify exactly *the* way that things are. Its claim is limited to this, that there is a way that things are independent of all representation. So, even if it should turn out that the only actual reality consists in mental states, that there is no material object, ER is still true. Whether or not the world turns out to consist of material phenomena in space and time is not relevant to ER. In any case, whatever one's belief about the constitution of the world is, it is consistent with and presupposes ER. (Searle 1995, 157) Seale's and Putnam's external realism are identical only in name.

A realism which is very minimal in terms of its content opens a very wide range of possibilities for different varieties of "constituents" of the world. As indicated by Searle, even an idealist metaphysics is not in principle ruled out by this realism. Realists can as well assert the kind of "metaphysics of disunity" (Cf. Dupre

and Rosenberg) which in terms of our earlier understanding probably cannot be called a metaphysics proper. Another interesting possibility is expressed in David Mermin's commentary on the world as presented by quantum mechanics: 'what is real is correlation; those which correlate are not'. All this reminds us of McMullin's assertion that realism does not have to mean interpretation in familiar categories.

This kind of metaphysics will lose the (dogmatic) certainty that traditional metaphysicians aspire to. It is subject to changes in scientific theory. But probably this is a fair price for some scientific credibility.

### **3. Interpretation and "Metaphysics"**

The realist view that the way science speaks about the world leads to another question: what does the world look like, given the accepted scientific theories? Even if it is accepted that the aim of science is to find true theories about the world, the theories usually do not immediately tell us about the world. *Interpretation* may be understood as the activity that bridges scientific theories and the views about the world to the world picture that are supposedly based on them. Not much has been discussed about interpretation of scientific theories, so I will only discuss this in a general way with a view to find its main characteristics and limitations. An important point here is that any attempt to draw a big picture of the world ("worldview", or "metaphysics") should be viewed as an interpretative attempt and evaluated accordingly.

In the previous chapter I briefly discussed interpretation. “Interpretation” is used to refer to two levels of activity. At the first level, there is the interpretation of a formalism, which is an algorithm for predicting the behaviors of physical systems. Interpretation at this level aims at giving physical meaning to the mathematical formalism. The result of this interpretation may be called “interpreted formalism”. For example, the mathematical formalism  $s = \frac{1}{2} at^2$  does not tell us anything about the world. This formalism may be manipulated mathematically in many ways; for example, from that formalism we can derive  $ds/dt = at$ . But only after interpreting this, by ascribing its terms with some measurable physical quantities, does it become the law of falling bodies. Interpretation here links the terms to the world. The interpreted formalism now makes truth claim about the observed world.

At the next level, the interpreted formalism may further be interpreted to find the ontology “implied” or suggested by the theory. For example, deterministic (Copenhagen) or indeterministic (Bohm’s) interpretations of the formalism of quantum mechanics belong to this category. This interpretation tries to ‘fix on an appropriate ontology for a world that theory describes’ (Sklar 2000, 730) or give answers to the question of what the world would be like if the theory is true. (Albert 1992, 17; McMullin 2002; Cf. Cushing 2001, 99; van Fraassen 1991, 5-12) Parallel to this, van Fraassen uses the term “interpretation “ in two senses: Scientific theories are not merely *representations* of the world, but interpretations of it, and, secondly, the metaphysics arising from them are also the result of interpretative work. In this

section I focus on the second level, i.e. (ontological) interpretation of (physical, not pure) formalism.

Unfortunately, however, while the urgency of the problem of interpretation has been felt by many philosophers of science, discussions of this issue, as noted by Sklar, has not been conducted systematically enough. “An intensive, extended and systematic methodological study of its aims and methods is long overdue.” (Sklar 2000, 742) In his essay Sklar lists some global issues in the problems of interpretation—issues independent of any attempt at providing interpretation of *specific* theories, such as: What are the general structures of the interpretive problems? What are the resources available in this enterprise? How does interpretation as a scientific program fit together with the more familiar programs in science of hypothesizing theories, testing them, accepting and rejecting them? Sklar, as I shall discuss later, differentiates two kind of attempts at interpretation, which are stimulated by metaphysical and scientific motivations. He dismisses the first attempt and concentrates on the second. Some of those questions are relevant only for the second kind of attempt. But in general his contention about inadequate attention to the activity of interpretation applies well to the whole spectrum of interpretative attempts.

Putting that aside, Sklar’s concern is part of the recent trend among scientists and philosophers of science who have become aware of the problem of interpretation. In its earlier history, scientific theories were regarded as coming in one whole package. As remarked by van Fraassen, the belief that scientific theories—their



formalism and interpretation, mathematical equations and “metaphysics”—come in one whole package was not abandoned until recently in this century. It was still held until relatively late in the development of quantum mechanics, when the Copenhagen hegemony in the interpretation of quantum mechanics was questioned. Heisenberg believes in the uniqueness of the theory in two senses: he sees a successful formalism as unique and of a piece, and that the formalism *uniquely determines* its interpretation. (Cushing 1994, 114) In the face of the availability of two formalisms, the Copenhagen matrix mechanics and wave mechanics, Heisenberg (and other early members of the Copenhagen school) initially sought to provide *the* correct interpretation of matrix mechanics to show its superiority over its rival. Yet, later the two formalisms were shown to be equivalent. Thus, in a sense, the situation got worse, because the two formalisms which are equivalent have two interpretations. Instead of unique determination, what he finds is underdetermination.

Even in classical physics, the two senses of interpretation come into play. Until quite late, in the 20<sup>th</sup> century, Newton’s theory was regarded as the hallmark of determinism. Acceptance of this theory seemed to imply acceptance of the deterministic world picture it suggests. Newtonian science is regarded as, in the words of van Fraassen, a closed text which admits no interpretation. (van Fraassen 1991, 8-11) Newton, in his *Scholia*, dictates comprehensively how his work should be interpreted. The classical world picture thus was thought of as demanded by classical mechanics. But, as shown in history, it was just not clear whether subsequent scientists subscribed to Newton’s own interpretation when they

completed the so-called ‘Newtonian world picture’. This concerns technical issues such as what counts as a force function (first-level interpretation), but also metaphysical issues such as determinism (second-level interpretation). In his study of determinism, for example, John Earman has shown that Newtonian mechanics did not have the allegedly intrinsic deterministic character. ‘[I]t is clear that the long-standing confident pronouncements about classical determinism have been premature.’ (Earman 1986, 53) Not only that determinism is not implied by classical physics, there is a possibility that the two might not even be compatible! Classical physics becomes deterministic only when additional assumptions are made.

The case is similar with relativity theory. Schlick, for example, supposes that General Relativity has conclusively shown that substantivalism is wrong; that spacetime has no definite structure in itself, independently of the things “in” it. But later discussions found this to be a too quick dismissal; debates about relationalism-substantivalism, and indeterminism as well, in the (second-level) interpretation of the theory are alive today. (Cf. Belot and Earman 1999)

Next, in quantum mechanics the kind of problems which have plagued earlier theories come up more intensively. As a matter of fact, it is the development of quantum mechanics which has brought up the problem of interpretation in science in general more vividly. The division between formalism and interpretation is quite common here, especially in philosophical literature on quantum mechanics.

For example, Hughes’s book (1989, titled *The Structure and Interpretation of Quantum Mechanics*) is divided into two parts: the structure (the mathematical

formalism) and the interpretation. Hughes states the merits of presenting the theory firstly in an abstract mathematical form when he responds to a potential criticism such as the one by Cartwright (“One may know all this [formalism] and not know any quantum mechanics”). (Hughes 1989, 296) For him, first of all this presentation is indispensable for pragmatic reasons when physical applications of the theory are at issue, since they “will involve a common set of mathematical models, and these abstract structures repay investigation.” (Hughes 1989, 297) Besides, when the interpretation of the theory is sought, it is better to have a more abstract presentation of the theory to find a “categorical framework” within which to interpret the theory. By categorical framework, what Hughes means is “a set of fundamental metaphysical assumptions about what sorts of entities and what sorts of processes lie within the theory’s domain.” (Hughes 1989, 175) In the case of classical mechanics, Kant’s *Metaphysical Foundations of Natural Science*, which tries to “deduce” Newton’s laws of motion from a set of principles, is an example of such a categorical framework. What Kant did was, given the truth of Newtonian mechanics, ask what the constitution of the world is from which Newtonian mechanics can be deduced. In other words, to interpret a theory is to see what kind of worlds are representable within the class of models the theory employs. “We obtain an interpretation by the dialectical process of bringing to a theory a conceptual scheme, and then seeing how this conceptual scheme needs to be adjusted to fit it.” (Hughes 1989, 176) In the case of quantum mechanics, however, it is clear that there is no unique solution to this problem. Hence the underdetermination of the interpretations by the formalism.

Cushing (1994) uses the distinction between formalism and interpretation mainly to account for just this clear case of underdetermination in quantum mechanics. In general, for any theory in physics, formalism and interpretation are two distinct parts of a theory. The first refers to the equations and calculational rules which “save the phenomena”, while the latter is the story about what the formalism really tells us about the world, or ‘what the theory tells us about the underlying structure of these phenomena.’ (Cushing 1998, 336; 1994, 9)<sup>48</sup>

Similarly, starting with the fact that there are underdetermined theories (e.g. Copenhagen and Bohmian versions of quantum mechanics), Michael Dickson (1999) proposes a distinction between formalism and interpretation as an alternative to one between observation and theory, which he sees as problematic in view of the idea that observation is theory-laden. This distinction can be justified by referring to the fact that we can think of (mathematical) formalisms without thinking about their (physical) interpretations. In the case of quantum mechanics, that means we can think of the theory of Hilbert space without thinking about its use in quantum theory. (Dickson 1999, 56) Quantum mechanics gives a stronger justification because here, as we will elaborate further later, there is an *actual* underdetermination: Copenhagen theory and Bohm’s theory both can be expressed by similar mathematical equations, and make identical empirical predictions, yet they portray contradicting world

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<sup>48</sup> This is probably the right place to clarify what is meant by theory in this paper. Some writers use “theory” to refer to the formalism. Others regard theory as comprising both the formalism and the interpretation. In this paper I follow Cushing’s usage; i.e. that formalism and interpretation are two distinct *components* of theory. ‘Hence *one* formalism with *two* different interpretations counts as *two* different theories.’ (Cushing 1994, 9)

pictures, as I shall briefly discuss shortly. Saying that both theories share a common formalism means that both predict certain empirical tests equally, or both are observationally equivalent. Dickson's case here is the quantum tunneling, which are predicted by both theories with the same probabilities. The reason why such is the case is simply because of the common formalism they use. Yet, to the question "What was really happening in quantum tunneling?" the theories give different answers. In the Copenhagen story, the trajectory for a particle does not have a well-defined meaning, while in Bohm's theory particles have a definite position all the time. So what exactly happens when the particle "gets through the barrier" is related differently by the two theories. (There is for sure a change of position from one position before the tunneling to another position after the particle gets past the barrier, but since Copenhagen does not allow a definite trajectory, we actually, strictly speaking, can't even say that the particle, in Copenhagen theory, "gets through" the barrier.)

As we have seen, due to many perplexing questions it has raised, quantum mechanics has brought to the fore many interpretative questions, which actually could as well be asked with regard to earlier theories. As pointed by van Frassen, interpretation of a theory comes into play when there are questions whose answers are left open by that theory. A theory is incomplete in various ways, "and so itself calls for completion." (van Frassen 1991, 9) To return to the example of Newton, the open questions are not left as they are to be answered by further experimentation and

observation. Instead, Newton tried to complete his theory by *interpreting* it, which actually was not demanded by theoretical necessity nor empirical phenomena. (van Frassen 1991, 11). Similarly in quantum mechanics, faced with difficult questions about the meanings of terms like probability, (in)determinism, measurement, etc., physics itself does not dictate *entirely* how we should understand them.

What van Frassen calls “incomplete” is quite similar with Sklar’s concern about the intuition frequently felt by scientists that there is “something wrong” in theory, especially with regard to foundational ones. At this point, Sklar differentiates several interpretative attempts, that is in the context of the second level interpretation (of interpreted/physical formalism).

He sees that there is interpretation which is motivated by metaphysical concerns and another one by scientific concerns. In the first, one might, for example, presuppose some metaphysical stance about what may be construed as real. Interpreting a theory here means saddling it with a presupposed metaphysical perspective. (Sklar 2000, 730) This kind of interpretation is surely a metaphysical work. A scientist does not have an interest in this kind of interpretation. Alternatively, one may construe interpretation, as Sklar does, as an activity whose essential motivation comes from within science itself.

In this second kind, “[d]emands for the interpretation or reinterpretation of a theory come from a sense, within theoretical scientific community itself, that ‘something has gone wrong’ with the theory itself....[although] there is no reasonable question whether the theory is ‘the very best available’ alternative to account for the

data that was in need of explanation.” (Sklar 2000, 730) But even here there is a spectrum of interpretative efforts that range from philosophical commentary on theories to replacement of theories.

Sklar sees that “interpretive reconstruction” of theory is an alternative to theoretical replacement. These two efforts are both responses to the theory when it “goes wrong”. This “going wrong” may take many forms. For example, the theory might be based on an “over-rich representational” structure that lends itself to alternative models for the world that are empirically indiscriminable from one another. It might rest on some fundamental concepts that are inappropriate or difficult to define in an exact manner. It may also seem to fail to relate to other theories in the wider scientific context. (Sklar 2000, 731-732) These kinds of “going wrong” do not straightforwardly call for dismissal of the theory, but for its (re-)interpretation. So there are a cluster of issues that motivate the interpretative project. From here, we may identify what interpreters do. The following is Sklar’s attempt to characterize interpretater’s work. (Sklar 2000, 732-735)

(1) Interpreters may leave the theory completely alone, and work on the theory. What she does is give a kind of philosophical commentary on the existing body of science. She may suggest that we ought to rethink several terms (e.g. spacetime) in different ways. In the example of spacetime, construal of spacetime in a relational or a substatal ontology does not change the empirical content of the theory at all.

(2) Alternatively, interpreters may attempt a systematic reformulation of a theory by, say, axiomatizing it, with the hope that this will throw light on what is really fundamental in the theory and, ultimately, what the theory is telling us about the world.

(3) Another kind of interpretation is shown in the invocation of curved spacetime in place of flat spacetime and a gravitational force field, which is a combination of two things. Here one delineates the consequences of the theory that can be legitimately held to represent facts, and then searches for an alternative formulation of the theory which entails the original theory's empirical consequences but which may be an improvement of the theory in some way. The improvement may take the form of getting rid of metaphysically objectionable posits in the original theory, or eliminating troublesome mathematical artifacts (e.g. in the initial proposal of Copernican system as an alternative to Ptolemaic system).

(4) Finally, interpretation may suggest adding new theories to science and positing new physical features of the world beyond those introduced in the interpreted theory, sometimes without any change in its empirical predictions. An example is the introduction of a hidden variable in Bohmian interpretation of quantum mechanics.

Thus, there is a continuum in terms of the distance an interpreter takes from the theory interpreted. On one end, the interpreter leaves the theory alone. He may progressively work further and further away on the theory until arriving at the other end of the spectrum, where his interpretation may be debated as to whether it has already constituted a new theory. There is indeed an ambiguity in the term



“interpretation”. Referring to Bohr’s and Bohm’s as the two main interpretations of quantum formalism, Cushing suggests that here we are actually dealing with two *theories*. “The (physical) interpretation refers to what the theory tells us about the underlying structure of these phenomena (i.e. the corresponding story about the furniture of the world—an ontology). Hence, *one* formalism with *two* different interpretations counts as *two* different theories.” (Cushing 2001, 100)

It seems that this is ultimately a mere semantic debate. Nevertheless, it points to two important issues. *First*, Cushing points to an important difference between the realist and anti-realist. For a realist, who expects the theory to say something about (“the underlying structure of”) the world, Bohr’s and Bohm’s interpretations are indeed two totally different theories, since their descriptions of the world are quite different, even contradictory in a sense. *Second*, interpretation is a story about the world which *corresponds to* the theory. It is constructed after the formalism is worked out, and it always goes beyond what empirical evidence provides the scientists with. Interpretation is, in van Fraassen’s term, “empirically superfluous”. (van Fraassen 1991, 9; 336) By its nature it does not add empirical content to the theory. (van Fraassen 1991, 9) As such, the choice between alternative interpretations cannot be decided empirically. This criterion might be used to demarcate theory from interpretation. As indicated by Sklar, interpretation is a spectrum that spans from an activity done at some distant from the actual scientific work, in which the interpreter gives philosophical commentary to an existing body of scientific knowledge, up to reconstruction of theory and adding new posits that sometimes is close to a

replacement of the theory. We might say that as long as no empirical content is added or no new empirical consequences are predicted, we should not call the result of the interpretative work a new “theory”. In this case, one formalism with two different interpretations still counts as one theory. Van Fraassen (1991, 243) gives an example of the introduction of hidden variables. If the introduction of a hidden variable yields different predictions from the original theory, then we have to speak of an alternative theory, and scientists would be very much interested in it. On the other hand, if it does not have any empirical consequences, then it is an interpretation.

If such is the case, should a scientist or, for that matter, a non-realist philosopher of science be concerned with interpretation, since it does not (empirically) enrich the theory? How could we understand Sklar’s contention that interpretative work is scientifically motivated? For one, there is a pragmatic interest in interpretation. As noted by McMullin, in natural science in general, “the specification of an ontology eventually tends to drive further development of the formalism (think of plate tectonics or molecular biology).” (McMullin 2001, 58) Yet, if we focus on quantum mechanics, which is the source of most discussions of interpretation, this has not been the case. Are there other scientific motivations to do interpretation? At this point we see an important difference between Sklar and van Fraassen. For Sklar, scientists should be interested in interpretative works, since theories can still be improved through the process of interpretation, even when there are no empirical consequences of the interpretations.

With regard to the empiricist van Fraassen, we might expect that he would not take the interpretative efforts as significant. Yet, his brand of anti-realism is indeed atypical. He approvingly says that “the interpretational demands of *What is really going on (according to this theory)?* or even the more modest *How could the world possibly be how this theory says it is?* will not disappear if science is to help us construct and revise our world pictures.” (van Fraassen 1991, 9) Moreover, in the footnote to that passage, he envisages “rational life as ... vitally concerned with what world-pictures might be tenable for us, and what forms of life each might open to us.” At the same time, though, rational life is not “necessarily committed to a world picture.” (van Fraassen 1991, 483) More explicitly, he himself, in the specific context of quantum mechanics, asks the above question and answers it:

“Why then be interested in interpretation at all? If we are not interested in the metaphysical question of what the world is really like, what need is there to look into these issues? Well, we should still be interested in the question of how the world *could be* the way quantum mechanics—in its metaphysical vagueness but empirical audacity—says it is. That is the real question of understanding. *To understand a scientific theory, we need to see how the world could be the way that the theory says it is.* An interpretation tells us that. The answer is not unique, because the question ‘how the world could be the way that the theory says it is?’ is not the sort of question to call for a unique answer. Faith in the actual truth of a good answer, so interpreted, is not required by understanding, nor does it help. (van Fraassen 1991, 337)

The key here is his pluralist attitude: he strongly rejects the assumption of the uniqueness of a correct interpretation. Interpretation is fine, but one has to be reminded that it is not necessarily *the* truth. (van Fraassen 1991, 483) What we have is

a number of tenable interpretations, not a uniquely correct interpretation. But realism as defended in this and earlier chapters does not necessitate the assumption of uniqueness. Both the modest scientific realism as well as Searlean external realism open the door for alternative worldpictures. Scientific realism, especially, does not determine at all which world picture one should subscribe to, but simply affirms the capability of science to give us such possible worldpictures. In this sense, then, van Fraassen *is* a realist.<sup>49</sup> What van Fraassen rightly rejects is what he calls scientism (or what Popper calls essentialism—see fn. 1), though the way he defines it might still be questioned: “a view in which science, which allows us so admirably to find our way around in the world, is elevated (?) to the status of metaphysics.” (van Fraassen 1991, 17) A realist, he sees, is committed to defending some kind of “scientific metaphysics”; a metaphysics directly and squarely based on scientific theories; one which, probably, is allegedly supported by the empirical evidence that supports the theory on which it is based. This attitude might as well be called “literalism”: taking scientific theories as literally speaking about the world as it is, or “things as they are”.<sup>50</sup> Again, this is not realism *per se*, but a particular, very restrictive brand of it.

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<sup>49</sup> Indeed, van Fraassen’s position with regard to realism-anti-realism debate is very delicate, not to say puzzling sometimes. Unsurprisingly McMullin speaks of van Fraassen’s “unappreciated realism”. (McMullin 2003)

<sup>50</sup> Note how van Fraassen describes realism: for realists, ‘the aim of science is to give us (literally) true theories about what the world is like.’ (van Fraassen 1991, 3) The problem here concerns the phrase “(literally) true”, because in the next page he mentions many fundamental things about which realists and constructive empiricist might agree, which include ‘a crucial interest in interpretation, i.e. finding out what this theory says the world is like.’ (van Fraassen 1991, 4) This is another indication of van Fraassen’s unappreciated realism.

At this point we may note a different kind of effort undertaken not necessarily by scientists but more typically by philosophers. That is, an effort to somehow base the metaphysics they construct on scientific theories. It tries to answer traditional metaphysical questions by looking into science—however imperfect that may be—as one of its most important resources. The motive to make use of scientific theories here may be as simple as that. Science is the best kind of knowledge about the world, so it is only natural to use it to help one’s effort to answer his or her ultimate questions about the world.<sup>51</sup> This effort is not necessarily the kind of scientism rebuked by van Fraassen. I will illustrate some such non-scientific efforts in the next chapter. In any case, whether the legitimacy of such efforts might be acquiesced to would depend on whether its claims do not breach the constraints of interpretation of theories as discussed earlier. At this point, we can already discern several important constraints on such efforts.

The most important one concerns the issue of underdetermination. From the above discussion and several illustrations of scientific theories which all lead to the problem of underdetermination, we can expect that what theories say about the world is, at best, ambiguous. For in addition to the empirical evidence that supports a theory, interpretation is needed as a bridge to reach what we can say about the world based on the theory. And interpretation is, again, hardly unique. Admission of the

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<sup>51</sup> The relevance of natural science to metaphysics can be expressed as simple as indicated in Schlick’s criticism of Hegel, quoted in the first chapter: ‘We can put no trust in a philosophy which deduces from concepts that the number of planets in the solar system must be seven (Hegel), at the very moment when the eighth was being discovered.’ (Schlick 1915, 153)

role of interpretation, thus, points to a familiar problem in the philosophy of science: underdetermination. If even scientific theories are underdetermined by available empirical evidence, the same predicament could be expected to apply, *a fortiori*, to the case of metaphysical views supposedly grounded on them. Hence Abner Shimony's warning<sup>52</sup>: 'One should not anticipate a straightforward and decisive resolution of metaphysical disputes by the outcomes of experiments.' (Shimony 1989, 64)

This undoubtedly poses a serious question to any attempt at grounding metaphysics on science. Most of the rejections of metaphysics by philosophers, at least since Hume, are motivated by the impossibility of resolving metaphysical issues, as shown in its history. Now, in light of the potentially acute problem of underdetermination, would a metaphysics that is in some way "grounded" in science fare better than the traditional metaphysics in terms of resolving its disputes? How should we view the role of interpretation in our attempt to know the world?

It might be difficult to find satisfactory answers to those questions. By its nature, meta-physics goes beyond the empirical science. Similarly, interpretation goes beyond the empirical evidence that supports the theory being interpreted, such that there is no empirical criteria to resolve the issues. It seems, then, that this predicament simply has to be accepted. When I said in the beginning of this section that attempts to construct a metaphysics should be viewed as an interpretive attempt and be

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<sup>52</sup> In the previous footnote there is a quotation which shows Schlick's aspiration to make metaphysics scientifically credible. Furthermore, as discussed in the first chapter, Schlick also hopes that empirical science can resolve metaphysical issues. Shimony's warning prudently tempers this optimism.

evaluated accordingly, what I meant is precisely this: we can never expect empirical justification to rule out all alternative interpretations and leave us with only one “scientific metaphysics”.

Nevertheless, this does not mean that scientific theories are not effective at all. Just like in the case of underdetermination of theory by empirical evidence, in which the evidence serves to rule out certain possible theories and leave some of them, similarly, the theories put some constraints on interpretations. The fact that we cannot have a single, true, “scientific metaphysics” does not mean that anything goes. Just as theory should save the phenomena and empirical evidence rules out certain theories, so too interpretation should “save the theories”, and the theories may rule out certain interpretations. Just as new empirical evidence may at some point force revision or change of theories, so too the change of theories may accordingly further rule out certain interpretations. As mentioned by Redhead, ‘there is *some* control; the world kicks back.’ (Redhead 1995, 15) For example, while it is not exactly clear what the world looks like as presented by quantum mechanics, it is clear that whatever world picture emerged from the theory it would be very different from the world presented by the Newtonian mechanics. The fact that quantum mechanics plays some role in revision of our world picture (again, despite our inability to come into a definite conclusion about the view of reality it presents) shows that the theory is to some important extent metaphysically effective.

To the question “what does the world as portrayed by science look like?”, we may then, at every point in time, only answer it cautiously, in the form of the

available alternative interpretations. It is important to note here that this metaphysical work is not essentially demanded by science. As a matter of fact, the very division of theory/formalism and interpretation emphasizes the fact that what is warranted by empirical evidence is only its formal part. That is partly the reason why scientists might not be interested in this work. Metaphysical interpretation is indeed more in the interest of philosophers who want to make use of what science tells us about the world.

From the point of view of such philosophers, certainly the degree of certainty of this kind of metaphysics is significantly lower than that aimed at by traditional metaphysics. Yet, the fact that empirical evidence may indirectly force change of that kind of metaphysics shows that it is connected closely to “reality as it is”, whatever that means.

#### **4. Conclusions**

In Chapters 2, 3 and this chapter I have tried to defend a version of scientific realism. To put it briefly, at this point I can conclude that

- (1) A modest version of scientific realism is possible.
- (2) Even though this realism does not immediately present to us a certain view of reality, through some interpretive works it can nevertheless be the basis of making metaphysical claim about what the world looks like based on acceptable scientific theories.



(3) Another limitation concerns areas where metaphysical interpretation is more fertile. Some theories are more readily interpretable compared to others (the most prominent example for the latter is quantum mechanics). With regard to the latter, realists should be much more cautious in drawing conclusions about the picture of the world.

(4) Finally, as a consequence of the involvement of interpretation, the resulting world picture cannot be said to be directly “implied” by scientific theories, and is not unique. There is the problem of underdetermination, though this does not mean any metaphysics is acceptable. The function of the theories is more in the direction of putting constraints on possible metaphysical interpretation of them.

In short, I have tried to show what I would like to call “the metaphysical ambiguity of science”. With this expression I intend to emphasize the two most important points: *first*, that a restricted version of realism is justified, which asserts that science speaks about a mind-independent world, and that true theories are theories that adequately represent the world. But, *second*, the theories are ambiguous with regard to the world as it really is. \*\*\*

## **Chapter 5:**

### **“Grounding Metaphysics on Science”:**

#### **An Illustration**

In this chapter I shall outline some of the consequences of the views argued for in the previous chapters by giving illustrations of how those views entail important consequences beyond the scope of philosophy of science proper (as conceived by the majority of today’s philosophers of science). Realism licenses belief in the *content* of scientific theories—to different degrees, depending on its versions. In this sense, science, as a conceptual undertaking trying to explain the world, should be relevant with metaphysics in general.

Of course, one may choose to simply dismiss those perennial questions as unimportant. But, by way of introduction, we can note the pronouncement of Abner Shimony, who tries to construct an experimental metaphysics. He indicates that this project is part of the “analytic continuation” of the historical Enlightenment; realism itself is part of the core commitments of the Enlightenment. Further, in relation to theology, as part of the same analytic continuation, ‘natural theology, essentially employing the methods of natural sciences, is the primary mode of theological

inquiry.’ (Shimony 1997, S2) This suggests that for a realist, a logical next step *in* theology is constructing some sort of natural theology—just as the logical next step in metaphysics is some sort of an “experimental or naturalistic metaphysics”.<sup>53</sup> It is an attempt to ground theology, in one way or another, on science. So, in a sense they can be seen as nothing else than a step further from, or a consequence of, an uncontroversial attitude of taking science seriously.

In metaphysics and (philosophical) theology<sup>54</sup>, the answer to my main question (“does science give us a picture of the world?”), if it is answered positively, could become the basis of the ensuing exploration. Paying attention to these two areas, therefore, will give us a better idea of the significance of the realist question and the consequences of the answers given to it.

Taking the step further from science to “experimental metaphysics” or “natural theology” is, to be sure, not demanded by science itself. The motivation to ground the metaphysical quest firmly on what science tells us may most conveniently be understood as an instance of the Aristotelian dictum: “All men by nature desire to know”, which is the first sentence of the philosopher’s *Metaphysics*. The meaning “to know” has now been dominated by science; to know something is almost synonymous with knowing it scientifically. In this sense, the motivation seems to

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<sup>53</sup> “Natural theology” may be used as a generic form to refer to any attempt at grounding theology on science. But some scholars distinguish natural theology from similar attempts to ground theology on science, such as Ian Barbour’s “theology of nature” and the 19<sup>th</sup> century “physico-theology”, which I shall touch upon later in this chapter.

<sup>54</sup> Though this will be clarified later in this chapter, I need to mention at the outset that by theology I mean here *not* dogmatic theology, but philosophical or constructive theology.

appear more strongly among certain groups of religious believers. On one hand, “experimental metaphysics” might look too esoteric for many people, including scientists; especially, done by scientists or philosophers of science, one may question its urgency. (Would it contribute anything to science? Why metaphysics, when there are other seemingly more important agendas in philosophy of science?) On the other hand, for theologians, the need for a theology which is scientifically credible, which coheres with the world as we know it from science, seems in general more urgent.<sup>55</sup> In its history, theology always learns from science and philosophy. For theologians, then, it is important to know what scientific theories say about the world. Therefore, a look into attempts to appropriate the finding of modern science into a theology might give us a better clue about how we should understand “what science says about the world” and its consequences.

Another reason why I choose to focus on discussions about science and theology rather than, for example, “experimental metaphysics” to illustrate the issue of grounding metaphysics on science is that the literature on the former is significantly richer, and has been developing rapidly in the last few decades. As such many of the points I made about modest realism are more vividly illustrated there. In this chapter I shall evaluate mainly scholars (scientists, philosophers, and theologians) who identify themselves as “critical realists”. This is a version of realism

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<sup>55</sup> At this point, it is important to also note that one’s understanding of “science and religion” also depends very much on one’s understanding about what religion is. For some religious believers who don’t see the importance of philosophical theology, who understand religion more in terms of experience, not knowledge or belief, the content of scientific theories may have much less relevance.

quite close to what I argued for in the previous chapters. Several important criticisms to efforts done by those scholars will be discussed here but not very elaborately, since my main purpose here is limited to seeing how a realist understanding of science affect the reformulation of theology.

I shall address first the question of how a realist answer to the main question of this dissertation has important implications on our thinking about the relationship between science and religion (to be more precise, beginning the second section, I shall focus on theology, broadly construed as religious thoughts). I shall later discuss to what extent this attempt to ground theology on scientific theories may be limited by the limitations of the modest realism as discussed in the previous three chapters. Besides answering internal questions in science and religion discourse, I hope, at the same time, to throw more light on that modest version of realism itself by looking at its consequences.

In the previous chapter I conclude four important limitations of the kind of realism I espouse. In this chapter I shall look at their consequences in attempts to ground theology on science.

1. First and foremost, the kind of realism proposed in the earlier chapters brings up a prospect for a theology which in some way is grounded on science. In other words, the realism gives a basis for engagement between science and theology. This is actually true of any kind of realism worthy of the label. Despite their differences we may agree with J. Leplin that “what realists do share in common are the convictions that scientific change is, on

balance, progressive and that science makes possible knowledge of the world beyond its accessible, empirical manifestations.” (Leplin 1984, 2)<sup>56</sup>

2. Method: From an empirical science to metaphysics/theology, interpretation plays a central role. As mentioned in the previous chapter, interpretation is the way to bring up the metaphysical/theological “implications” of scientific theories.
3. Plurality of interpretations: A consequence of constructing theology as interpretation of scientific theories is that the result would be not unique. Such a theology, therefore, is always hypothetical.
4. The distinction between scientific disciplines suggested in previous chapter is carried over to attempts to construct a theology based on science. The fertility of theological reflection is different from one scientific field to another. Areas where realism presents stronger case are also areas where possible theological discussions are more fertile.

My focus here will be mostly with methodological questions in relating science to theology. As such I shall not dwell too much with the contents of certain theories as well theologies, except when examples may help me in making a point.

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<sup>56</sup> In the “Introduction” to *Scientific Realism* (1984), Leplin lists ten realist theses, which as a matter of fact are not all shared by realists of different versions. But the general characterization above is more acceptable.

## **1. The relevance of science to (philosophical) theology**

It is a truism to say that one's conception of what science is (and, of course, of what religion is) determines how one sees the relation between the two. Different conceptions of science brought different implications in terms of the relation between theology and science. In a sense, it may be said that realism opens ways for deeper engagement between science and theology, since only realism goes far enough to say something about what the world is like, which overlaps with theology. This derives from what Dennis Dieks concludes about the realism as a presupposition of metaphysics based on it, since '[t]he existence of a fundamental physical world-picture is something very natural only in the realist view of science; there its possibility is built in from the very beginning.' (Dieks 1994, 7) For an instrumentalist, for whom science does not have much to say about the world, there is no urgency to involve science in any broader intellectual enterprise, be that metaphysics or theology. An instrument is simply irrelevant to any intellectual attempt at describing the world. However, the matter is not always that straightforward.

### a. Constructive empiricism

For an anti-realist, *constructive empiricist* like van Fraassen, the situation is a bit more complicated. On one hand, he admits that empiricists, who are staunchly against any metaphysical "reification of the *content* of science", have often been

tempted by some form of instrumentalism, and as such there is no talk of science transforming our view of the world. On the other hand, he categorically says that '[s]cience has transformed our world-view.' (van Fraassen 1994a, 124-125) Science, for him is something like myth, which is not a synonym for falsehood, but serves the functions of explaining natural phenomena and of providing foundations of social life. Like myth, science is cosmological (providing a global picture of the whole world and world history), narrative (presenting a drama unfolding in time), explanatory (explaining the natural and social order), and providing categorial framework within which every subject is placed and understood. (van Fraassen 1994a, 129)

However, unlike myths, science puts much greater emphasis on the *method* of inquiry rather than the *content* as the outcome of that inquiry. In science the primary commitment is to that critical method which applies rigorously to everything. Acceptance of science does not require belief that it is true; scientific activity is not about producing truth about the world but, instead, about continually giving up beliefs and changing them.

If this conclusion about the primacy of method *vis à vis* content in science is correct, then realism has throughout mis-focused the debate. For if realist metaphysicians reify content, then they do for science what the superstitious do for religion: *they avert attention from its significance to the vehicle of that significance.* (van Fraassen 1994a, 132)



So science has some impact on, or transforms, our worldview not by providing a (rival) worldview but, it seems, by relativizing it. The values it suggests are not in the form of production of beliefs about the world, but in the critical attitude that should never allow acceptance of any belief as final.

The core of van Fraassen's empiricism is rebellion against metaphysics. This is what he sees as uniting empiricisms throughout the ages. The rebellion is not only against certain metaphysical beliefs, but the very way philosophy, understood as the producer of theories about the world, is practiced. This leads him to conceive his empiricism as a stance in his *Empirical Stance* (2002): '[A] philosophical position can consist in something other than a belief in what the world is like... [It] can consist in a stance (attitude, commitment, approach, a cluster of such—possibly including some propositional attitudes such as beliefs as well). Such a stance can of course be expressed, and may involve or presuppose some beliefs as well, but cannot be simply equated with having beliefs or making assertions about what there is" (van Fraassen 2002, 47-48)

The question he proposes with regard to the relation between science and religion is: 'Does the empirical stance allow for anything other than a secular orientation?' (van Fraassen 2002, 153) Secularism is about a stance towards science. The secular is content with the scientific as sufficient in itself, while the religious displays discontent, though it does not preclude the possibility of peaceful coexistence with science. (van Fraassen 2002, 155) The secular standpoint is merely one possible orientation for participants in science. (van Fraassen 2002, 182) One

way for such a peaceful coexistence lies in reinterpreting religion. This is done, for example, by the existentialist theologian Rudolf Bultmann who chooses to carry out a demythologizing project by reinterpreting the scriptural mythology in existentialist terms. (van Fraassen 2002, 188)

As such, in this case science indeed has some relevance to or impact on religion but only in a very limited way. Even though this attempt may still be liberally called as one of “grounding theology on science”, i.e. by applying the empirical stance (understood as the position which is most loyal to scientific attitude) to understand religion, but it does not provide the stability of content which seems to be demanded by theology more than it is by science.

One thing which van Fraassen is here guilty of is conflating a wide spectrum of realist positions into a single characterization as “reification of content”. Though content is indeed important for any realism, but across the spectrum of realist positions, from one extreme of naïve realism to the more modest versions the degree of “reification of content” decreases. He is right to criticize realism which degenerates into *scientism* (‘a sort of science on a metaphysical pedestal, with the current content of science erected into a final measure of all truth and value.’ [van Fraassen 1994a, 133]) It is true that a realist’s aspiration to get to things as they really are—from where one tries to build a “scientific cosmology” or “analytic ontology”—could easily lead to a dominating scientism. But obviously realism does not have to be as naïve as what van Fraassen represents it to be.

### b. Naïve realism

Van Fraassen's accusation seems to apply mostly to naïve realism. For naïve realists, science is quite capable of telling us what the world is really like. Science of course is still distinguishable from metaphysics, but the latter can presumably be derived directly from the former. This seems to be the root of both the views that science and theology is in conflict as well as that the two are in perfect harmony—the views which John Haught calls Conflict and Conflation accordingly. (Haught 1995)<sup>57</sup> Conflict is the conviction that science and religion are fundamentally irreconcilable, while Conflation is the almost identification of what science says and what is believed through religious sources.

It seems that both Conflict and Conflation stems from the same naïve realist optimism of the epistemic capability of science. Both take empirical science as having the capability to say, with a significant degree of certainty, about things that are outside the empirical world. This is an area where there is significant overlap with what religion speaks of. The difference is that one position sees what science says is in conflict with what religion says while the other sees the two to be in harmony.<sup>58</sup> To take an example from cosmology, proponents of Conflict position may say that it was a matter of pure chance that the balance of forces in the early universe produced

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<sup>57</sup> In the typology of views regarding the relation between science and theology, Haught (1995) speaks about positions which he labels Conflict, Conflation, Contrast, Contact, and Confirmation. I identify the first two as positions which naïve realists may hold; the third is identifiable with instrumentalism, and the fourth (Contact) with a modest realism I argue for here.

<sup>58</sup> Both Conflict and Conflation actually may take place with regard to both what van Fraassen calls the form as well as the content of science. But here I focus mostly to the latter.

conditions favorable to the appearance of life and intelligence; thus, as mentioned by Steven Weinberg, ‘the more the universe seems comprehensible, the more it also seems pointless.’ (Barbour 2000, 42) But the proponents Conflation would identify the  $t=0$  which is seen as implied by the Big Bang cosmology, with the time of creation of the universe.<sup>59</sup> In physics, we see that physicists like Paul Davies have tried to show that the notion of God as creator can be read off directly from theories in physics. (Davies seems to be reluctant to speak explicitly of God, but his readers can’t help concluding that the creating and designing entity that is supposedly derivable directly by way of logic from physics is quite suggestive of God, at least as conceived in the three Abrahamic religions.<sup>60</sup>)

In history, the attempt at building what Kant calls “physico-theology”, that is, a form of theology based squarely on physics, is based on the belief that physical theories have direct theological implications. But the main objection to this kind of attempt is that it is too close to current theories, which always change, so it is too vulnerable. Besides, the fact that later modern scientists show that science does not need any other than natural principles to explain natural phenomena has been seen to be an important factor in the popularity of intellectual atheism of 19<sup>th</sup> century. So

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<sup>59</sup> It is typical of the two positions that they oversimplify both the scientific as well as theological concepts. In the example above, the meaning of  $t=0$  is still subject to many interpretations; similarly, identifying creation with the beginning of time, as done by the astrophysicist Robert Jastrow (Barbour 2000, 41), is very problematic. The view of creation as something historical is indeed commonsense. But serious theological discussions about creation do not put a specific importance on “the beginning”. I discuss this further in the next section.

<sup>60</sup> See Davies’ *God and the New Physics* (1983) and *The Mind of God: The Scientific Basis for A Rational World* (1992).

associating theology too close to science has proven to be theologically disastrous, instead of giving it the intellectual prestige of modern science. (Haught 1995, 113)

A slightly milder form is natural theology.<sup>61</sup> It purports to show that the existence of God may be derived directly from what scientific theories say about the natural world. The form it takes usually is argument from design. But there have been refutations of this kind of argument, most prominently the one by David Hume, and continued by later philosophers.

In all those forms, in which there is a very close relation between theology and (naïve realist) science, the theology is taken to be directly derivable from scientific theories, to the point of suggesting its scientific necessity. Yet the naïve realist science failed to be a sure ground for theology, mostly due to the weakness of naïve realism. When certain theological doctrines are derived directly from science, it is as if the evidence that supports science is strong enough to support its metaphysical expansion. In a more extreme version, science is sometimes taken as giving “proof” for the doctrines. The purportedly “scientific theology” fails to the extent that naïve realism fails due to its failure to acknowledge qualifications that needs to be taken into account in terms of the extent to which the content of science is believable.

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<sup>61</sup> That is, in a more specific form than what Shimony means when he uses this term. See footnote #1. My use of the term is closer to Barbour, who distinguishes between “natural theology” and “theology of nature”. (Barbour 2001) Both are based on some form of realism. But whereas, it seems to me, the first could be associated more closely to naïve realism, the second is more modest. The first tries to prove by using argument from design the convergence of science and theology, the second tries to construct a theology by taking into account many sources, one of which is science.

### c. “Critical realism”

As we have discussed in earlier chapters, what characterizes the modest version of realism is precisely its acknowledgement that the content of scientific theories may be extended metaphysically in a very limited way. Modest realists offer more ample room for an engagement between science and religion than what is allowed by instrumentalists or constructive empiricists, but more limited than what is attempted by naïve realists. Even though this position can still be regarded as guilty of “reification of the content of science”, but it is so only to a limited extent. A number of authors in recent literature on science and religion have shown this methodological awareness, and propose what they call “critical realism” as part of their presuppositions before embarking on what may be called integration of science and theology.<sup>62</sup>

I shall discuss more fully the theological implications of the kind of realism I argued for in the next sections. Before that, it is helpful to understand the motivation of scientists, philosophers and theologians who attempt to ground theology on science that is based on “critical realism”. First of all, as I mention in the beginning of this chapter, this actually can be understood as something natural. For

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<sup>62</sup> Those scientists, philosophers and theologians include Ian Barbour, John Polkinghorne, and Arthur Peacocke. Needless to say, their “critical realisms” are not identical, and not all of them give sufficient arguments for their positions. The most elaborate discussion is by Arthur Peacocke, who wrote a work specifically on this issue, *Intimations of Reality: Critical Realism in Science and Religion* (University of Notre Dame Press, 1984). They try to navigate between naïve realism and anti-realism of many sorts.

instance, for theologian John Haught our knowledge about the history of the universe, and more specifically the history of the earth, has incredibly improved that to ignore this significant body of knowledge is intellectually irresponsible. But why should our knowledge about the world be relevant at all to theology? The reason is that theology makes claims about the world, about the actions of an agency in the world, and as such to a certain extent is a rival of science—that is, realist science. For a scientific realist who happens to be a theist, it would seem almost natural to seek for a coherent worldview which would incorporate knowledge he gets from science. On the other hand, from theological side, ‘if one is a theist—and if one believes that her theism should be responsive to results of the natural sciences—then she has a strong motivation, and perhaps obligation, to travel further along the continuum toward metaphysics and theology.’ (Clayton 2001, 224)

Telling stories has always been part of human lives and the source of meanings. Therefore the more believable a story, or the more grounded on experience, the more effective it is. The progress of science from its classical to modern period may be seen as a continuous refinement of these stories, with modern science as its peak. If one believes that science can tell us about the world, then engaging it is an intellectually responsible effort in any attempt to have a coherent view of the world. But this needs to be qualified: science has been so powerful in its explanation and accurate in its prediction precisely because it restrains from making grand claims. When, as in the case of science and theology discourse, it is liberated

from those limitations and stretched to a great extent, its degree of certainty is also decreasing. This seems to be an inevitable trade-off.

As a methodological principle, science of course is not expected to say anything at all about God's action as part of its explanation of the world. And just as we need to preserve the methodological autonomy of science, the autonomy of theology needs to be preserved as well. Having said that, in *its* explanation of the world theology may, or even should, include the scientific. (Russell 1998, 196; Cf. Polkinghorne 2001, 188)

While most of the critical realist scholars of science and religion I mentioned above are aware of the limitation—not to say dangers—of involving scientific theories in theological discussions, they see that there seems to be no choice to engage in it; otherwise, as mentioned by philosopher Philip Clayton, theological discussions would amount to “tennis without a net”. For him, where possible such discussions have to be tied to empirical studies. ‘[I]f there *is* to be any contemporary theology, it cannot be carried out in ignorance of natural scientific results, even the most difficult ones. The one thing worse than a theology that attempts to draw connections between physics and God is a theology that believes it has no need of any such connections, a theology that believes it can concoct the divine out of metaphysical whole cloth.’ (Clayton 2001, 211) The question, then, is how does one get from empirical science to a coherent theology or worldview?

Before answering this question, a conclusion we can reach at this point is that there is a strong motivation to ground theology on science—again, with awareness of



its many important limitations, which we shall discuss later. Further, a realist view of science opens the possibility for this enterprise. Moreover, the kind of modest realism I argue for here will make a difference in the characters of the relation between theology and science, as I show in the next two sections.

## **2. The Interpretive Method:**

In the more modest version of realism, it is acknowledged that while empirical science has important things to say about the world, it does not readily give us metaphysics. The latter, so to speak, is not directly derived from the former. There is a gap between the two which is bridged by the activity of interpretation. What is called the metaphysical or theological “implications” of scientific theories are the result of this activity. In this way the resultant theology can be said to be “grounded on science”, though in a weaker sense than the supposedly direct derivation attempted by naïve realists.

The theological import of a scientific theory (or a collection of them) unfolds when theological questions are imposed to interpretive models of the theory. An example of such a question is “given what the theory says about the phenomena it tries to explain, how could we conceive God’s action in the world?”, or more specific question about issues like determinism versus indeterminism, chance and necessity, etc.

Conceiving interpretation broadly as any comment on a particular physical theory, Michael Heller (1999, 96) usefully classifies (theological) interpretation into three logical possibilities. With regard to the formalism/interpreted formalism, a comment can be (a) inconsistent; (b) neutral; and (c) strict agreement.<sup>63</sup> The first one would definitely be ruled out (as a matter of fact it can hardly be called interpretation); while the last one is the position I characterize as naïve realists' attitude in the previous section, who suppose that scientific theory can directly derive metaphysical claims about the world. It is clear that theories do not say anything about theological matters.

Yet many examples can be cited that hold this position. In the previous section I have mentioned the example of Jastrow who sees strict agreement between  $t=0$  implied by the Big Bang theory and a conception of divine creation in which God created the universe at a certain historical point (the metaphor he gives is the scientists reaching the same peak which had been reached by theologians; this means scientists speak about precisely the same thing theologians speak about, only with more precision). In this kind of approach, usually the problem lies in the oversimplification of either one or both concepts that are being equated. In this example, "creation" as a theological concept is seldom taken as meaning the origination of the universe at a specific point in time. In general, we may suspect any

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<sup>63</sup> Heller as a matter of fact speaks as if theological interpretation is of the second-order. This probably does not mean that theological interpretations come into contact directly with the mathematical formalism; the contact between the two is mediated by interpreted formalism. Yet, this difference does not seem to matter much.

idea of “strict agreement” between a scientific theory and a theological concept for the same reason: agreement comes only after the ideas in either one or both fields have been oversimplified or misrepresented.<sup>64</sup>

Since positions (a) and (c) are not tenable, we are left with (b): the only possible theological interpretation would be neutral with regard to what the theory says about the world. That mostly means that theories do not readily yield metaphysical implications as proponents of (c) maintains. But if that’s the case, what role then can theories play in theological interpretation? Heller still sees an important role here: ‘Interpretations of this type should be taken “seriously but not literally”. They can show more than the non-contradiction between a given scientific theory and a given philosophical (or theological) doctrine; if they are successful, they can be regarded as demonstrating a certain consistency or consonance between them.’ (Heller 1999, 98)

To be more precise, instead of regarding theories as determining exclusively a certain theological doctrine, what they do is provide *constraints* for any possible interpretation. As Haught says, in the context of Einstein’s relativity theory, ‘Theology should not be based directly on physics, of course, but physical cosmology does place constraints on what may plausibly be said about God and God’s relation to

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<sup>64</sup> A prominent example in this regard is found in the New Age literature represented by Fritjof Capra’s *Tao of Physics*, which finds striking parallels between the most modern of physical theories with ancient “Eastern mysticism”. Upon careful scrutiny, it turns out that the parallel is between a particular, simplified interpretation of quantum mechanics (whose domain is stretched beyond its original domain, and the plurality of interpretation of quantum mechanics is virtually ignored) and simplified (or distorted?) concepts in what he calls “Eastern mysticism” (which, again, ignores the specificity of different tradition which he lumps into this single broad construct).

the world.’ (Haught 1995, 41) Similarly, Polkinghorne stresses that ‘Physics does not determine metaphysics, but it can certainly constrain it.’ (Polkinghorne 2001, 188) “Physics determines metaphysics” is the position of naïve realists; on the other extreme, the relativist, anti-realist end of the spectrum, physics is not able to even provide constraints—thus “anything goes”. Modest realism acknowledges the capability of physics to have some say about metaphysics, in a much less direct way (i.e. the latter is not “derived” from the former), but mediated by the extra-scientific activity of (third-order) interpretation. In the field of quantum mechanics, rejecting positions that we may identify as Conflict and Conflation, Clayton asserts:

‘The conceptual space defined by the quantum mathematical formalism and the associated empirical observation neither proves nor disproves the existence of any divine being. ... But it *may* tell us something about how a being (human or divine) must act if it acts in the physical world and in conformity with physical law. That is, the conceptual space of quantum physics may *constrain* [italic added] the ways such a being could be manifested and the sorts of actions a human observer could in principle detect.’ (Clayton 2001, 213)

Clayton’s proposal would be to look at scientific theories in order to help him understand better the divine being and its action in the world, where the theories function as constraints on possible theological models. This assumes that the divine action (if there is such a being and it acts at all) would have to leave, in some way, traces of its actions in the natural world, which is studied by science. Otherwise, ‘If God exists and has not “acted” at all, or if these actions fail to indicate anything about the divine nature—and especially if the actions lead us to infer things about the divine

nature that are false—then we are completely sunk, epistemically (and perhaps in other ways as well!).’ (Clayton 2001, 213) To those who are skeptical about the fruitfulness or even the very possibility of this enterprise (especially in the case of quantum physics), Clayton would ask them to at least recognize that his proposal is not excluded by sound empirical science and that they are ‘different from many of the dogmatic and unquestioning theologies of the past’ (Clayton 2001, 215) To the optimists who move much further than he does, to the extent of detecting almost perfect convergence between scientific conclusions and religious/mystical doctrines, he advises them to be aware of the hypothetical and contingent nature of all such theological reflections. (Clayton 2001, 215)

The cautiousness displayed by the critical realists reflect the fact that from the empirical data, which is the backbone of any scientific theory, to a full-fledged theological view supposedly grounded on them, it has to go a long way characterized by possible underdetermination in its steps. Though different scientific fields pose different interpretive problems, there is a problem of interpretation in any scientific theory. As I shall discuss in the fourth section below, the case is more complex with regard to quantum mechanics; nevertheless, even the theory of evolution which is relatively much clearer in terms of what it says about the world poses similar interpretive problem when (i.e. when it comes to philosophical and theological interpretation).

As I discuss in the previous chapter, the first of several levels of interpretations concerns interpretation of the empirical data. The problem of underdetermination discussed in philosophy of science concerns mostly this level. The next level is interpretation of the theory in question to find out its relation with reality. Again, there is underdetermination here and talk of constraints. To get a more adequate view of reality theories from other fields must be involved, since each theory speaks only of a fragment of reality. When one wants to ground theology on science, the material to be interpreted is the result of the last level of interpretation. But due to the underdetermination that takes place in every step of the way, sometimes theological discussion needs to go deeper and consider other possibilities of interpretation at the level below the level that comes immediately after.

In a slightly different exposition, Clayton calls the theology that comes as the result of interpretation of scientific theories as a third-order discipline. That is, it is 'reflections on the "data set" of theories from multiple disciplines, which are in turn responses to the data within those disciplines.' In terms of what is discussed in Chapter 3, at this level of the discourse the theological interpretation is of the interpreted theory, or the second-order interpretation, the first order being interpretation of pure formalism. It starts with looking into (second-order) interpretation in a specific scientific field, and taking notice of its relation with certain theological models. The scientific interpretation and the theological views inform each other and may force a revision of both of them. This is repeated across different scientific fields. In this process certain theological options shall become more (or

less) credible in light of the constraints imposed by the scientific interpretations. (Clayton 2001, 212)

To each of those levels the above statement (that in interpretation it's more appropriate to talk about constraining rather than determining) applies. The work here is one of "saving the phenomena" but at higher and higher levels of abstraction (i.e. further and further from the world or the initial empirical data). To bring the similarity further, it is interesting to note a proposal put forward by Arthur Peacocke. He proposes that theological interpretations be done by adapting from philosophy of science the procedure of inference to the best explanation (IBE), which we discuss at length in Chapter 3. Peacocke urges that 'IBE is the procedure that best leads to public truth about the relation of nature, humanity and God which is both communicable and convincing by its reasonableness through reflection on our most reliable and generally available knowledge of nature and humanity.' (Peacocke 2001, 30) Of course it needs emphasis again that the IBE in this case is done outside the scope of science as such. So when we talk about the criteria such as fruitfulness, consistency, parsimony, etc., they are considered in theological, not scientific, context.

Finally, as pointed out in the earlier chapter, one very important consequence of any work of interpretation (of our inability to talk about a theory directly "implying" theological views) is non-uniqueness of its results. This, as I have shown, results in the metaphysical ambiguity of scientific theory, which in our present

context means theological ambiguity. This surely has many important consequences which I shall turn to now.

### **3. Plurality of interpretations:**

Speaking in the context of quantum mechanics, for James T. Cushing, given the fact that many contesting interpretations of quantum mechanics are not empirically decidable, the only reason for any theological position is purely subjective. (Cushing 2001) That is, one starts with her preference of certain theological views, and then tries to find justification for them. If that is the case, there seems to be no strong case for this attempt. Cushing's view seems to be based on his assumption that theology can be nothing other than a wholly *a priori* enterprise. His example is about the "free choice" between determinism and indeterminism, which he is right in seeing as a fundamental question in the issue of God's action in the world. He shows that both interpretations can be argued for based on quantum mechanics, without any prospect of empirically and logically ruling out either one. Consistent stories based on either interpretation can be told, 'but truth is another matter.' (Cushing 2001, 110) So any choice of this must be arbitrary or subjective—in the case of theology, the choice would presumably be determined by a preconception one already made up independently of what quantum mechanics might say on the subject.

However, in light of the foregoing discussion, Clayton (and many other critical realist theologians as well) would remind him that the proposal for a



constructive theology is ‘different from many of the dogmatic and unquestioning theologies of the past’ (Clayton 2001, 215) This kind of theology is not wholly *a priori*, it’s open for re-evaluation upon new knowledge that comes from the sciences (be it natural or social) and truth is not the only thing that matters. Clayton fully realizes that the theology he attempts to construct is ‘much more hypothetical, fallible, open to revision and provisional than was traditionally allowed.’ (Clayton 2001, 211) It also admits the underdetermination of theological theory by data. Though one may not know which of the competing theological theories will turn out to be true, ‘[s]ometimes a stronger case can be made for one theological option than another.’ (Clayton 2001, 212)

The contrast is drawn by Peacocke who, as discussed in the previous section, suggests that theology employs IBE. The use of IBE in doing theology with the sciences as one of its sources will result in a “revised theology”, which will be very different from the natural theology or physico-theology. The latter tries to logically deduce the existence and attributes of God from natural phenomena. Whereas in Peacocke’s “revised theology”, ‘we can only infer to the best explanation and no claim can be made for logical proof in this process (as claimed in the classical Five Ways to prove the existence of God).’ (Peacocke 2001, 33)<sup>65</sup> Peacocke rightly sees that hard proof is available only in logic and mathematics, which deduce from

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<sup>65</sup> See also a source quoted by Peacocke on the use of IBE in theology: P. Clayton and S. Knapp, “Rationality and Christian Self-Conceptions”, in *Religion and Science: History, Method, and Dialogue*, ed. W. M. Richardson and W. J. Wildman (Routledge, 1996), pp. 134, 138.

axioms, and, just like the sciences, theology has to dispel the illusion of providing hard proof.

A similar case is made by another critical realist Robert J. Russell. '[M]y approach is best seen as a form of constructive theology with a focus on nature (what Barbour calls a "theology of nature"), *not* a form of natural theology, let alone physico-theology. Hence a change in science or its philosophical interpretation would challenge the constructive proposal at hand, but not the overall viability of a theology of divine action in nature, whose primary warrant and sources lies elsewhere in scripture, tradition, reason and experience.' (Russell 2001, 304) He explains this last statement in another writing of his: 'The positive grounds for an alleged divine action are theological, not scientific. This hypothesis is not drawn from science even though it aims to be consistent with science.... Science would not be expected to include anything explicitly about God's action in nature as part of its scientific explanation of the world. Theology, however, in *its* explanation of the world, can and should include both.' (Russell 1998, 196)

As a matter of fact, the history of theology already shows that it has always been provisional. It developed in accordance with the state of knowledge of the time; it presupposed some cosmology or another provided by natural philosophy or science. Therefore, theological language was always tentative and metaphorical. With the advance of new knowledge, it is only natural that theology changes accordingly; new developments in our knowledge may provide us with new language to express God's

relation to the world—a new language which is more fitting with contemporary language used to express our knowledge of the world.<sup>66</sup>

To conclude, the need to engage science responsibly in doing theology (i.e. being aware of important limitations posed by the theological ambiguity of science) means at the same time the willingness to be content with results that are not final. As such theology no more provides certainty and stability of content as aspired by traditional theologians.<sup>67</sup> It is also no longer worthy of the high status as “queen of the sciences”. But that seems to be the price for any theology that wants to be, at least, not inconsistent with science. At the same time, this seems to be the source of objections of many philosophers and theologians who see that in its attempt to be contemporary theology has surrendered too much to science.

Among philosophers/theologians who mount such objections, we can mention the perennial philosophers Seyyed Hossein Nasr and Huston Smith. Smith accuses theologians we discussed above as allowing theology to be colonized by science. But as is clear from his own description of that attempt (Smith 2001, 75-76) he seems to

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<sup>66</sup> The theologians I cited so far almost exclusively come from Western Christian tradition, though when they speak about the relation between science and religion they try to speak in a general sense. But we can see very similar sentiment in a prominent Muslim philosopher, M. Iqbal of India. Writing in 1920s, he attempted to do what he calls “reconstruction of religious thoughts in Islam” (which is the title of his major work). By “reconstruction” he meant expressing Islamic theology in a new language which is colored mostly by the state of natural science of his time. One of the main sources for such reconstruction is philosophical systems prominent at his time, mostly A. N. Whitehead’s process philosophy. The other important source is science, more specifically Einstein’s relativity theory and, less importantly, Darwinian theory of evolution. Unfortunately, it is difficult to find comparison with more recent Muslim writings.

<sup>67</sup> A more radical statement is given by Peacocke following his suggestion that theology employs IBE. For him, theology would be ‘an open exploration in which nothing is unrevisable.’ (Peacocke 2001, 30)

have misunderstood what they do, and their cautiousness in dealing with possible theological implications of scientific theories. In any case, the very idea that theology's certainty is replaced by its hypotheticality, for the sake of making it more scientifically credible, seems too difficult for Smith (and many others) to swallow. It is difficult to answer this kind of objection, since its source lies in the different preconceptions they have as to the characters of theology *and* science. The proponents (or we may as well say, the practitioners) of perennial philosophy pride themselves as being the vanguard of the Tradition, which is a special type of philosophy (or theology in the broader sense of the term as religious thought). It is a philosophy whose contents have been verified by scholars who come from different metaphysical/religious background and has been verified for thousands of years. Its contents are stable and represent the Truth, so there is no question of making it more "up to date". On the other hand, science is only about the details; it is principally incapable of entering the lofty realms of metaphysics *par excellence*. So it is almost impossible to suggest that science may force revision of one's worldview/theology. As a matter of fact, what those philosophers attempt to do is precisely the reverse: making the perennial philosophy the yardstick against which any scientific theory must be selected on the basis of the metaphysical sense it makes.<sup>68</sup>

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<sup>68</sup> Among the contemporary proponents of perennial philosophy Nasr discusses this more elaborately. See for example, his *Knowledge and the Sacred*; *The Need for Sacred Science*; and *The Philosophy of Seyyed Hossein Nasr*, which is the volume of Library of Living Philosopher dedicated to him.

#### **4. Distinction between scientific disciplines**

Finally, another important aspect of the modest realism I argued for concerns differentiation between scientific disciplines. Different theories have different problems with regard to a possible realism, and contain different degrees of the prospect for realism. In other words, some theories may be more readily expressible in more familiar terms than others. Though being expressible in familiar terms is not a criterion of realism, it makes difference in conveying to us the possibility of having a more or less well-rounded view of reality. The clear example here is quantum mechanics, which poses most problems for realism.

As such, quantum mechanics will be *the* scientific theory which any constructive theologians have to be most cautious about. Despite the fact that it is the most precise theory in terms of its formalism, if it is simply not clear what quantum mechanics is telling us about the world, how can we muse on its theological imports? That is precisely the reason why even scholars like John Polkinghorne who engages in science and religion discourse seem very reluctant when it comes to quantum mechanics. The radical ambiguity of quantum mechanics makes it very difficult to generate theological conjectures. On a more positive note, it is also quite clear that, nevertheless, we know whatever picture of the world quantum mechanics presents to us, it would be quite unfamiliar and different from the more closed and static universe that classical mechanics presents. This shows that despite the radical metaphysical

ambiguity here, we still can say something positive about the world.<sup>69</sup> In particular, a more specific lesson shown by McMullin from his historical study of mechanics in general down to quantum mechanics, is that ‘what constitutes agency at the most basic level in the explanation of motion is immensely more complicated and more mysterious than earlier generations could ever have imagined.’ A general theological moral that can be drawn, then, is ‘if this is the case, how much more complex, how much more mysterious, ought one to expect the agency that brings into being a galactic universe and with it, beings who can pose such questions such as these?’ (McMullin 2001, 78) But in general, any theological musing on quantum mechanics has to be accompanied with strong cautiousness and awareness that it is highly speculative. (Clayton 2001, 214) As such too optimistic conclusions (like the one drawn by Capra and a few other New Agers) on striking parallels between results of quantum mechanics and “Eastern mysticism” are always suspect.

When it comes to the theory of evolution, the matter is quite different. It is not that the possible theological implications of quantum mechanics are less relevant, since issues discussed in interpretation of quantum mechanics such as determinism

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<sup>69</sup> Some theologians have gone further by saying that from the little we can say about the world based on quantum mechanics, as compared to the one based on classical mechanics, the one thing which is clear is that it is more hospitable for divine action in the world. The old mechanistic world was too static, closed and deterministic to allow ample room for divine action. Quantum mechanics, even as interpreted deterministically by David Bohm, still allows more space for divine action; the case, for those theologians, gets stronger when we also incorporate modern cosmology and the theory of evolution. If that’s the case, then this shows that the theological ambiguity of science is not totally hopeless; i.e. despite the ambiguity we still can make evaluations and wager rationally between different theories and their relation to theology. However, I would restrain from making these further claims, since they need more expansive arguments. Besides, they are also not essential for the purpose of this chapter.

vs. indeterminism is not less fundamental—though it is also true that the theological implications of the theory of evolution bears more directly on thorny issues such as whether the world was designed or came into being as the result of a random process, just by chance; or whether it rules out belief in a personal God; or whether human being is bestowed a special place in the order of existence. Rather, the controversy surrounding the theory indicates, more than anything else, that what this theory says about the world is much less ambiguous, at least compared with quantum mechanics. The very fact that evolution does take place already poses many theological questions; discussions about the mechanism of evolution bring up more fundamental questions. As such theological reflections are most fertile in this context.<sup>70</sup> The challenges directly posed by evolution have historically prompted full-fledged proposals labeled “evolutionary theology” (or: theology of evolution). Teilhard de Chardin is obviously the most prominent example; more recently Arthur Peacocke and John Haught try to found a complete theology based almost exclusively on the theory of evolution.

Cosmology, above all, poses the questions in the preceding paragraph with more urgency. More like the theory of evolution rather than quantum mechanics, theories in cosmology are relatively clear in what they say about the way the world is and its history. However, much of what is going on in cosmology at present is

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<sup>70</sup> Another, sociological, explanation that can be given to the popularity of theological discussions of evolution is the fact that atheistic pronouncements in popular literature are more common among biologists than physicists. Thus the theological need to defend theistic belief against the onslaught by the atheist scientists, many of whom are not careful enough in their arguments, is felt more urgently here.

relatively still at its more speculative phase compared to the theory of evolution. The theological interpretation of cosmology varies wildly such that from a single theory different philosophers have proposed interpretations which are contradicting each other. More modest assessment would be content with more general theological moral and avoid involvement at the technical levels. 'It is the sheer existence of the universe which becomes the datum of theology and that the details of scientific cosmology are irrelevant here.' (Barbour 1999, 302) What matters more here, then, is not exactly the different proposals that explain the coming into being of the Universe; it is our quite recent knowledge about the age and the size of the universe, which would not fail to move a believer to reflect on it and wonder about its theological bearings. Another issue which is more established at this time and has theological significance concerns the character of our continually expanding universe.

## **5. Conclusions:**

In conclusion, as the existing literature on science and religion (i.e. especially among those in the camp of "critical realism") has shown, the scientific fields or theories fare differently with regard to their possibility as the object of theological reflection. Fertile theological reflections can be done mostly with regard to theories which have relative clarity of what it says about the world; that is, those expressible in relatively more familiar terms. Differentiation of areas with regard to how strong realism can be made, therefore, is quite plausible here. Areas where realism presents



stronger case are also areas where possible theological discussions are more fertile. Where, as in the case of quantum mechanics, interpretation is quite difficult and highly contested, probably it is wiser to be content with drawing general theological morals. Even outside the technical details of a field, general understanding would already poses intriguing theological lessons (such as the age of the universe, its size, the relatively very young age of human species compared to other living beings on the earth, the fact that all living beings descent from the same common ancestor).

All this requires some sort of realism to be meaningful. The kind of modest realism I argued for would be sufficient for this. Even then, all theological reflections on scientific theories will remain always tentative. The tentativeness owes to, first of all, the theological ambiguity of theories; but also, obviously, the tentativeness of any scientific theories themselves.

Finally, it needs to be said that theology may as well provide a more abstract interpretation in both two senses of the term. First, it may not deal with particular theories, but with a set of them at the same time. This is due to the different theological issues that are touched by different theories. Speaking about chance and necessity based on our understanding of evolution, for example, may not settle the whole theological question of determinism vs. indeterminism. A foray into quantum mechanics may provide one with different possible understanding of the issue (remembering, again, in the latter field the level of tentativeness may be much higher). Second, theology may as well reflect on the metaphysical presuppositions of scientific enterprise more generally. This includes issues such as the rationality of the

world order; the very comprehensibility or intelligibility of the world despite its utter complexity; the notion of laws of nature; etc. \*\*\*

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