SCIENTIFIC REALISM DEBATE IN THE PHILOSOPHY OF SCIENCE

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ABSTRACT

SCIENTIFIC REALISM DEBATE IN THE PHILOSOPHY OF SCIENCE

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The primary concern of this piece of work is to reconsider scientific realism debate in the philosophy of science. Accordingly, the overall aim is to come up with the clues of a viable scientific realist attitude in the face of anti-realist interpretations of scientific theories. To accomplish this aim, I make use of two modified versions of scientific realism, that is, 'epistemic structural realism' and 'entity realism'. Epistemic structural realism is a realist position of which proponents claim that the only knowable part of the reality is the structure of it which is expressed by the mathematical equations of our best scientific theories. On the other hand, according to entity realism, the only assured knowledge obtained from scientific theories is the existence of theoretical entities posited by these theories. I argue that a combination of the properly construed versions of these two positions might fulfill the afore-mentioned aim of this thesis.

Keywords: Scientific realism, anti-realism, epistemic structural realism, entity realism, substantial structural realism.

ÖZ

BİLİM FELSEFESİNDE BİLİMSEL GERÇEKÇİLİK TARTIŞMASI

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Bu çalışmanın asıl konusu bilim felsefesindeki bilimsel gerçekçilik tartışmasını yeniden ele almaktır. Bu doğrultuda genel amaç bilimsel teorilerin anti-realist yorumları karşısında geçerli bir bilimsel gerçekçi bakış açısının ipuçlarına ulaşmaktır. Bu amacın yerine getirilebilmesi için bilimsel gerçekçiliğin değiştirilmiş iki biçiminden faydalanılacaktır. Bunlar 'epistemik structural realism' ve 'entity realism'dir. Epistemik structural realism, savunucularının gerçekliğin tek bilinebilir kısmının en iyi teorilerimizin matematiksel denklemlerinde ifade bulan yapısı olduğunu iddia ettikleri bir gerçekçi pozisyondur. Öte yandan, entity realisme göre bilimsel teorilerden elde edilen tek kesin bilgi bu teorilerin varsaydığı teorik antitelerdir. Sonuç olarak, bu iki pozisyonun uygun şekilde yorumlanmış biçimlerinin bir bileşiminin tezin yukarıda sözü edilen amacını yerine getirebileceği iddia edilmektedir.

Anahtar Kelimeler: Bilimsel gerçekçilik, anti-realism, epistemik structural realism, entity realism, tözel structural realism.

To My Father,

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CHAPTER I

Introduction

The primary concern of this thesis is the reconsideration of the scientific realism debate, which is a general term commonly held among philosophers of science to denominate the controversy between realist and anti-realist positions in the philosophy of science. Accordingly, the overall aim is to search for the conditions of possibility of a viable realist attitude. Thus, I am in need of dealing with several alleged realist and anti-realist positions in the philosophy of science such as ‘entity realism’, ‘epistemic structural realism’, ‘ontic structural realism’, ‘logical positivism’, ‘instrumentalism’ etc. A special emphasis is made on the epistemic structural realism and entity realism for the sake of the hardcore concerns of this thesis. At the end, I hope I can come up with the clues of a viable realist attitude. The alleged position may well be called ‘substantial structural realism’. In order to attain the above-mentioned aim, this thesis is supposed to pursue the following order: Ch.2: Preliminaries to the Scientific Realism Debate, Ch.3: A Closer Look to the Recent Approaches to the Debate, Ch.4: Towards a Tenable Realist Position: Structural Realism Ch.5: An Attempt to Reinforce Scientific Realism, Ch.6: Conclusion. In the following lines, I try to give some information on the contents of these chapters.

Firstly, I start with a chapter which is going to be devoted to give a historical background of the above-mentioned debate. It is investigated that why this debate have arisen and how realist and anti-realist philosophers of science have dealt with the problems stem from it. This is a debate of which ultimate concern is the status of scientific theories and the theoretical entities (unobservables) constituting these theories. This debate may well be defined as the tension between two main scientific attitudes which are (scientific) realism and instrumentalism. I want mainly to focus on the elaboration of the debate after the so-called ‘Einsteinian Revolution’—i.e., after the glory of the ‘General Theory of Relativity’ (Einstein, 1916). Following the drastic improvements in mathematics and geometry, Einstein’s theory led philosophers and scientists into a very deep investigation about the status of scientific theories and theoretical entities employed by them. Thus, in the first sections of this chapter, I explore these crucial improvements in geometry and physics before getting deep into the philosophical arguments and discussions of the debate. This is done in order to show that how an apparent connection holds between these improvements and the initiation of the debate.

In the early part of the nineteenth century, it was at last shown that the parallel axiom of Euclid’s geometry was independent of the other axioms of Euclid. Then, the idea that a statement incompatible with the parallel axiom can be substituted for it without logically contradicting the other axioms attracted many mathematicians. Two famous examples of non-Euclidean geometries were introduced by the Russian mathematician Nikolai Lobachevski and by the German mathematician Georg Friedrich Riemann.

One of these geometries, the Riemannian, made this development in geometry more than an exercise in logic, when Einstein adopted it in order to construct his ‘General Theory of Relativity’. Rudolf Carnap, in his (1966), clearly states this revolutionary progress.

Shortly after these improvements in mathematics and physics, the scientific realism debate has emerged. In Section 2.3, I concentrate on the philosophical reactions to the relevant scientific improvements. An archaic form of the debate can be found in the views of Henri Poincaré, Pierre Duhem and Ernst Mach. Afterwards, a period begins where the ontological and the epistemological characteristics of theories were needed to be clarified. This period may well be said to begin with the Vienna Circle. But I do not discuss the issue just around the views of Vienna Circle. I want to generalize this part of debate to all kind of logicist conceptions of the problem. For almost all of the philosophers of this period, including a remarkable number of Vienna Circle members as well, the matter was methodological. Thus, I do not divide this period into more parts and make problem more complicated. I just pick views of some important philosophers of this period as representative.

In the third chapter, I focus on the more recent approaches to the debate. I, firstly, begin with a brief explanation of the scientific realism debate. Then, I further with the revival of scientific realism after the failure of logical positivist program. The challenging ideas of 1960s against scientific realism forced all allegedly realist thinkers of the philosophy of science to rethink their attitudes towards scientific theories. It did not take so long for them to formulate new positions for the sake of scientific realism. There emerged lots of original positions all of which were claimed to be the ultimate solution for the debate. To tell the truth, they all enriched the debate instead of coming up with a satisfactory solution. In sections 3.2.1, 3.2.2 and 3.2.3, I explicate three of those realist positions and arguments regarding the debate.

Afterwards, I turn my attention to some anti-realist challenges which are emerged at about the same period with the revival of scientific realism through 1960s. I start with a brief exposition and discussion of a well-known problem against scientific realism, that is, 'underdetermination problem'. Then, I elaborate the conception of the problem after the impact of Thomas Kuhn and Larry Laudan. Kuhn's ideas on scientific revolutions and Laudan's apparently promising thesis of 'pessimistic meta-induction' revealed the problem that whether our scientific theories are reliable on the basis of the idea that science is a continuous and cumulative enterprise. The problem turned out to be the reliability of our scientific theories, i.e. there emerged the problem of the tension between our scientific theories and the real world. Science as a whole became doubtful. The most basic claims of science, namely rationality and objectivity, faced the danger of extinction. Their unquestioned sovereignty started to totter. The alleged progress of science seriously wounded. And, as a result, scientific realists needed to rethink their positions towards scientific theories.

In the fourth chapter, I explore in detail one of the most recent positions, structural realism. It seems that structural realism will be the most viable realist position if the appropriate modifications are made. In the first section of this chapter, I concentrate on the first versions of this position. The very early versions of structural realism can be traced back to the works of Henri Poincaré. His distinction between nature and structure tacitly presupposes an archaic form of structural realism. Another position, which can be interpreted as a kind of structural realism, may well be attributed to Bertrand Russell. I try to clarify his attempt to reconcile the structure of our perceptions with the structure of the causes of these perceptions by means of logico-mathematical abstract structures. Lastly, I discuss the structuralist views of Grover Maxwell, who tried to refine and reinforce Russell's views on the issue by the help of Ramsey sentences.

After the elaboration of Maxwell's claims, I turn my attention to the more explicit forms of structural realism—i.e. 'ontic structural realism' and 'epistemic structural realism'. Ontic structural realism is the position advocated by Steven French and James Ladyman. Basically, their seemingly bold claim is that there is no reality other than structure which obliges someone to commit ontologically. I try to restate their work on the issue and, then, argue that ontic structural realism has undeniable shortcomings.

The most crucial section of this chapter will be the discussion of epistemic structural realism. The aim of this part will be an attempt to investigate the adequacy of Worrall's account of structural realism. Firstly, I restate Worrall's conception. His overall aim is to accomplish a position which does not conflict with the most powerful arguments of both realism and anti-realism. These arguments are the 'no miracle argument' and the 'pessimistic meta-induction' respectively. No miracle argument is the one that relies on the success of empirical science. On the other hand, pessimistic meta-induction, which originates from scientific revolutions, asserts that upcoming scientific theories are likely to be false rather than true since all allegedly best theories of science were proved to be radically false so far. Worrall tries to construct a viable and, still, realist position between these two hardly refutable arguments. The promising and insufficient parts of his account will be discussed on a par. For the chronological convenience, I explicate the views of Worrall in the first place. Then, I proceed with the views of Steven French and James Ladyman. I conclude this chapter with Psillos's criticisms on Worrall's structural realism.

The fifth chapter is devoted to the elaboration of my own arguments regarding the debate. These arguments, fundamentally, initiate from structural realism. Nevertheless, I presuppose a more extensive ontological and, as a consequence, epistemological domain while proclaiming them. What I basically want to do is to stand between epistemic structural realism and scientific realism (strictly understood). I try to get rid of the unnecessary epistemological restrictions of epistemic structural realism while refraining from the excessive claims of scientific realism on the knowability of reality.

Hopefully, the hardcore idea of another realist position, namely ‘entity realism’, can help me to reinforce my arguments on the issue. The basic idea of entity realism is simply as follows: What we really know about the theoretical entities is their existence. We know that they *are*. But we cannot be sure of what our allegedly best theories tell us about them. That is, we cannot know *what* they are. It is hard for us to access their knowledge through descriptions of them embedded in scientific theories. A proper combination of epistemic structural realism and entity realism, I argue, will provide us with the clues of a viable realist position.

My alleged aim, after all, is to come up with this allegedly viable realist position in its general terms.

CHAPTER II

Preliminaries to the Scientific Realism Debate

As preliminaries for the scientific realism debate, I expose here the philosophical and scientific advancements between the 18th and early 20th centuries in a historical manner. These advancements of the above-mentioned centuries have a crucial importance in order to comprehend the nature of the relevant debate. The advancements in the mathematics and physics not only ended up with revolutionary scientific theories but also were the starting point of an intellectual shift in the field of philosophical enterprise which in turn led to very fruitful philosophical movements (e.g. logical positivism) and debates (e.g. scientific realism debate). The very first thing to mention in this respect is the discovery of non-Euclidean geometries. The reason for this is that the discovery of one of these geometries (Riemannian indeed) led to one of the greatest achievements in the history of physics—i.e., Einstein's 'General Theory of Relativity' (Einstein, 1916). Hence, in the subsequent section, I expose the process of the discovery of non-Euclidean geometries. Then, I further with the emergence of Einstein's theory. Lastly, I elucidate the immediate philosophical responses to these improvements and the emergence of logical positivism as the received view of the philosophical environment of the time.

2.1 Discovery of Non-Euclidean Geometries

For more than two millenniums, Euclidean geometry was thought to be the unique geometry which accounts for the relations among the objects of physical universe. However, one of Euclid's postulates (or axioms in a more recent terminology) had been controversial even from the time of Euclid itself to the time of Gauss. This postulate called the fifth postulate or parallel postulate. Several mathematicians tried to get rid of the problem that stem from this postulate and related definitions. Euclid himself was aware of the weird character of parallel postulate and he did not use it in his proofs until the 29th proposition of his *Elements* (Euclid, 1956).

But what is the reason that makes parallel postulate so controversial? According to many mathematicians this postulate was not as self-evident as the other four postulates. The first four postulates were simple and did not need any proof and require any empirical test. On the other hand, the fifth postulate seems to require to be empirically tested. That is, one has to add up line segments indefinitely in order to see whether the fifth postulate is true but it is clear enough that no one can continue the process of extending line segments to an indefinite extent. Thus, the thought that parallel postulate was rather a theorem which can be deduced from the other four postulates by rules of inference became a common notion among mathematicians. Some of them used only the first four postulates and still some others, in addition to four postulates, assumed a purportedly more self-evident axiom instead of fifth postulate in order to prove that Euclid’s parallel postulate is indeed a theorem. Not quite surprisingly, all these attempts turned out to be drastic failures.

Proclus, Wallis, Legendre, W. Bolyai and many others used the above-mentioned methods. The failure point of the mathematicians who used the former method was their unknowingly adoption of some hidden assumptions which were quite similar to those in Euclid’s fifth postulate. Namely, they were assuming some properties of parallel lines which are not allowed by the definition of parallel:

> DEFINITION. Two lines l and m are *parallel* if they do not intersect, i.e., if no point lies on both of them. We denote this by $l \parallel m$. (Greenberg, 1980, p. 16)

For example, they thought that two parallel lines as being equidistant everywhere or that two parallel lines have one common perpendicular segment. But the only thing that the definition of parallel tells us is that they do not intersect. On the other hand, the mathematicians who used the latter method failed too due to the circular reasoning in their alleged proofs. That is, what they assumed as a more self-evident axiom for a replacement of the fifth postulate turned out to be a logically equivalent of it (Greenberg, 1980).

Saccheri, an Italian mathematician, traced a different way. He used the method of *reductio ad absurdum*. He tried to show that if a contradiction in the negation of the fifth postulate occurs, then one can assert that the relevant postulate is independent of the remaining postulates, i.e. cannot be deduced from them as a theorem. Accordingly, he classified three cases of quadrilaterals of which summit angles were right angles, obtuse angles and acute angles respectively. What Saccheri had in his mind was to prove the first case by showing that the other two cases lead to contradictions. He thought that he found a contradiction in the case of obtuse angles. But he could not find one in the acute angles case though he found several interesting conclusions which were theorems of non-Euclidean geometries indeed. Although he did not see it as such, in fact, he was the original discoverer of non-Euclidean geometry. At last, he declared by an inaccurate explanation that the acute angles hypothesis is false (Greenberg, 1980).

Although Saccheri seems to be the unrecognized finder of non-Euclidean geometry, it is Karl Friedrich Gauss who must be honoured as the actual finder. He provided with lots of discoveries about non-Euclidean geometries. By the influence of these discoveries, he cast some severe doubts on the necessity of Euclidean geometry. Nevertheless, he did not publish any of his works on non-Euclidean geometries. The reason for this was the frustrating character of the philosophical environment at those days. Gauss was discouraged by the domination of Kantian position that Euclidean geometry is a necessity of thought.

The first mathematician who found the sufficient courage to publish his work on non-Euclidean geometries was a Russian mathematician, Nikolai Ivanovich Lobachevsky. But his work did not gain its deserved attention.

Almost at the same time, a Hungarian mathematician, Janos Bolyai published a text on non-Euclidean geometries as an appendix to his fathers book the *Tentamen* (1831). Neither of these works and the texts of Gauss on non-Euclidean geometries were paid enough attention until the death of Gauss in 1855. After this year mathematicians like Beltrami, Klein, Poincare and Riemann improved these geometries.

Beltrami is an important figure in the history of the development of non-Euclidean geometries. He proved that a non-Euclidean (hyperbolic indeed) geometry and Euclidean geometry are mutually consistent by showing that a non-Euclidean geometry in three dimensions can be mapped onto Euclidean geometry. This means in a way that the fifth postulate of Euclidean geometry can neither be proved nor be disproved as a theorem. It is independent of the other four postulates (Greenberg, 1980).

After the acknowledgement of non-Euclidean geometries, philosophers started to interpret mathematics and geometry in a different manner. They thought that mathematics is no more a discipline that conveys the absolute truths and geometry is no more a discipline that studies the relations among the objects of physical universe. Kantian epistemology and Newtonian physics was in vain. Mathematics must have become a formal game without substantial content. Geometry turned out to be a logical exercise of axiom sets which can be chosen freely. The only properties that the entities can have are the ones defined in those freely chosen axioms.

There had been gone for a long time that Euclidean geometry was the unique geometry that can represent the physical universe and the relations among the objects in that universe. Immanuel Kant had systematized this view in his epistemology (Kant, 1781[2007]). For him, Euclidean geometry is both synthetic and a priori. By being synthetic, one should understand that Euclidean geometry has factual content—i.e., it is about the physical space. On the other hand, by being a priori, it should be understood that the statements of Euclidean geometry do not need any empirical justification though they have indeed empirical origin. That is, Euclidean geometry is a necessity of human mind.

This was the standard view until the discoveries of non-Euclidean geometries and acknowledgement of them as consistent as Euclidean geometry. Once there occurred the possibility of infinitely many geometries, it is thought that which geometry being the geometry of the physical space turned out to be a matter of empirical investigation.

For Carnap, the flaw in Kant's reasoning was the lack of a special kind of distinction[1] between two types of geometries—i.e., mathematical geometry and physical geometry (Carnap, 1966). Mathematical geometry is the geometry of pure space (where no physical interpretation of axioms is needed) which means that the axioms and theorems derived from these axioms are a priori in their very nature and, thus, do not need to be a matter

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[1] It is a distinction which was impossible to be made by Kant due to the inexistence of Non-Euclidean geometries at that time.

of empirical investigation. Carnap concludes that this kind of geometry cannot be synthetic. On the other hand, physical geometry is synthetic in the sense that it has a factual content—i.e., it acts upon a physical space and physical objects which are achieved by a physical interpretation of the axioms of the relevant geometry. Accordingly, Carnap claims that physical geometries are subject matters of empirical investigation, that is, they cannot be a priori. It is clear enough that by the aid of these remarks one can see that there is no synthetic a priori mathematical geometry of physical space as Kant thinks there is and which is actually Euclidean.

Now, the question whether the physical space is Euclidean or non-Euclidean becomes an empirical question. But what kind of empirical method is appropriate to determine the geometry of physical world is an undecided controversy. There are mainly two camps on the issue. One of them contends that there is an intrinsic metric of the universe while the other contends that the metric of the universe is decided by convention. Here, I mention about the views of Henri Poincaré and Hans Reichenbach.

Poincaré is seen by many of the philosophers on the conventionalist camp. For him, "[t]he geometrical axioms are … neither synthetic a priori intuitions nor experimental facts" (Poincaré, 1905[1952], p. 50). Poincaré believes that geometry is an exact science. Thus, the only thing to do while choosing geometry in order to describe the physical space is to avoid every kind of contradictions. We know that both Euclidean and non-Euclidean geometries are equally consistent and devoid of contradictions. Thus, the empirical facts will lead to the most convenient geometry choice. Poincaré believed that physicians would have continued to describe the universe in Euclidean terms and make adjustments in their best physical theories where needed. For him, the overall simplicity of the entire theory will be achieved in this way.

Reichenbach thinks quite the contrary. A non-Euclidean geometry has much more complicated geometrical statements. It defines a subtler metric than Euclidean geometry. But we do not need to distort or adjust our physical theories if we adopt a non-Euclidean geometry (Reichenbach, 1958).

Another advantageous aspect of Reichenbach's view is that adopting a non-Euclidean geometry will automatically eliminate the inefficient universal forces of any kind. For him, among the equivalent descriptions of physical universe, we choose the one that which eliminates the universal forces as more convenient (Reichenbach, 1958).

Poincaré believes that geometry is not an objective feature of physical space. Hence, any consistent geometry can be chosen with no bearing on the objective reality. Reichenbach, on the other hand, claims that the class of the equivalent descriptions is unique and determined by the objective reality. That is, there is not a multiplicity of classes of equivalent descriptions. One can choose a description as more convenient among the competing descriptions but cannot choose the class where these descriptions are embraced (Reichenbach, 1958).

Things became much more sophisticated about the discussions of the geometry of physical space when Einstein adopted one of these non-Euclidean geometries (Riemannian) in the construction of his ‘General Theory of Relativity’. Hence, this much is said for the discovery of non-Euclidean geometries. In the next section, I focus on Einstein’s discovery of General Theory of Relativity.

2.2 Discovery of General Theory of Relativity

Before getting deep into the development of revolutionary philosophical movements that gradually paved the way for the scientific realism debate, it is necessary to mention about a noteworthy advancement in the history of physics, which is the discovery of General Theory of Relativity. The acknowledgement of this theory together with new approaches in geometry and logic led to an important shift in the history of philosophy, especially on epistemological grounds. The need for a proper philosophical evaluation of the scientific success of nineteenth and early twentieth centuries gave birth to abundant philosophical movements under the leadership of Vienna Circle—i.e., logical positivism. In fact, a subdiscipline of philosophy called philosophy of science was established and started to be widely appreciated among philosophers and scientists. I elaborate the connection between the relevant philosophical movements and the scientific success of that time in the subsequent section. For the time being, I direct my attention to the aforementioned development in the history of physics.

In 1915, after a decade from the formulation of his ‘Special Theory of Relativity’ (Einstein, 1905), Albert Einstein presented his famous work on the ‘General Theory of Relativity’ to the Prussian Academy of Sciences. A year after that, it is published in *Annalen der Physik* under the title of “The Foundations of the General Theory of Relativity” (Einstein, 1916). It is an undisputable fact that this theory comprises certain technical complexities in its formulation. Thus, it is far beyond the purpose of this section to examine the issue into these complexities. Rather, I try to look over it in order to attain a philosophical insight for the subsequent discussions.[2]

What Einstein accomplished by his general theory of relativity was the generalization of his ‘Principle of Relativity’ (by the help of his ‘Equivalence Principle’) to the extent that it incorporates the gravity. To investigate what the above lines correspond let me start with two basic principles of special theory of relativity. The first one is that the speed of light in empty space is constant whatever the velocity of the source of light is. And, the second is the so-called ‘Principle of Relativity’. The formulation of this principle in Einstein’s own words is as follows:

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[2] For a relatively lucid recapitulation of the basic ideas embraced in the general theory of relativity see Einstein (1961).

> If, relative to *K* [where *K* is a Galileian co-ordinate system], *K′* is a uniformly moving [Galileian] co-ordinate system devoid of rotation [that is, translatory], then natural phenomena run their course with respect to *K′* according to exactly the same general laws as with respect to *K*. This statement is called the principle of relativity (in the restricted sense). (Einstein, 1961, p. 18)

He stresses that the principle, thus construed, is in the restricted sense. This means that it is available only for the frames that move uniformly relative to each other. For accelerated frames, gravity becomes a crucial factor and Einstein's equivalence principle has to be taken into account to generalize the principle of relativity so that it can hold for strong gravitational fields where massive bodies curve the space and bend the rays of light. Equivalence principle is simply as follows: "The *gravitational* mass of a body is equal to its *inertial* mass" (Einstein, 1961, p. 65). This means that "[*t*]*he same* quality of a body manifests itself according to circumstances as "inertia" or as "weight" (lit. "heaviness")" (Einstein, 1961, p. 65). This consequence follows from the interpretation of the equations $\mathbf{f=m_i.a}$ and $\mathbf{f=m_g.g}$ where $\mathbf{m_i}$ is the inertial mass of a body and $\mathbf{m_g}$ is the gravitational mass of that body. By the help of a well-known thought experiment, Einstein drew the conclusion that one cannot distinguish between the situations of being at rest in an elevator on the surface of earth and being accelerated (let's say, in a spaceship in outer space where no gravitational field acts on the spaceship) with an acceleration that is equal to the intensity of the gravitational field of the earth which is **g**. Hence, it would be the case that a light beam would bend while propagating across the spaceship due to accelerated motion of spaceship. Since one cannot distinguish between the situations of being in an accelerating spaceship and being at rest in an elevator on the surface of earth, the light beam bends while propagating across the elevator as well. This means that light traces a curved path under the influence of gravitational fields. But since light has no mass, it cannot be the case that a gravitational force acts on light beams to bend them. On the contrary, for Einstein, it is the curvature of space-time that does the job. Massive bodies curve the space-time in proportion to their masses and create gravitational fields around them of which intensity diminish as one moves farther away from those bodies. Put it another way, it is a feature of the geometry of space-time, and not of the fictitious forces like gravitational force, that determines the behavior of objects (e.g. beams of light rays) in strong gravitational fields which are produced by massive bodies.

After the successful observation of bending light beams, which are propagated through a fixed star, around the gravitational field of the sun of our solar system during a total eclipse at 1919, Einstein's general theory of relativity became popular and accepted as describing things more accurately on behalf of reality than Newton's theory.

The most important feature of Einstein's theory, I think, is its adoption of a non-Euclidean geometry in order to represent curved space-time co-ordinate systems of different gravitational fields produced by the matter. This feature has two crucial consequences. One is that "space-time is not necessarily something to which one can ascribe a separate existence, independently of the actual objects of physical reality" (Einstein, 1961, vi). That is, physical space is a derivative of physical objects and there is no absolute space. The other, and, I think, more important, consequence is that the geometry of space (or, to be consistent with Einstein's insight, space-time) is no more decided a priori and become an empirical issue. This poses a serious difficulty for Kantian idea that the Euclidean geometry is the synthetic a priori geometry of the physical space.

In addition to above-mentioned departure from Newtonian mechanics, general theory of relativity drastically changed the received conception of theoretical terms like space, time, motion, mass, gravity etc.

In the following section, I deal with the philosophical reflections given to these scientific improvements.

2.3 Epistemological Shift in Philosophy and Logical Positivism

Shortly after the drastic improvements in physics and mathematics gained appreciation by the scientific community of the time, a group of philosophers and philosophically curious scientists, called Vienna Circle, gathered under the leadership of Moritz Schlick in order to evaluate and refine the current status of philosophy in the light of these improvements. What, in the main, they tried to accomplish was a reconsideration and reconceptualization of traditional empiricism[3] with the aid of mathematical and logical constructs and by taking into account the new findings of physics and mathematics. Their philosophical attitude is labeled as logical positivism. Here, I investigate, in its general terms, what kind of changes this attitude brought about to the philosophical thought instead of dealing with the different views of Vienna Circle's members on particular subject matters. For the central concerns of this thesis, I constrain my scope to the treatment of some particular issues by logical positivism in general.

I find it appropriate to begin with logical positivists' treatment of statements of empirical sciences. For them, the only statements that deserve to be called analytic are the mathematical and logical ones. These statements are true by virtue of their meaning since they have no factual (empirical) content. On the other hand, for logical positivists, the status of synthetic statements was much more complicated than analytic statements. For them, the only source of empirical knowledge was the sensory experience and testability. They claimed that if a statement has factual content, i.e., synthetic, then it should be empirically testable in some way. As it has been stated in the previous section, Einstein's general theory of relativity has showed that the geometry of physical space is a matter of empirical investigation and, thus, not necessarily Euclidean. Relying on this fact, logical positivists rejected any kind of factual knowledge claims of which justification does not depend on

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[3] By traditional empiricism, I mean the empiricism associated with the philosophical works of John Locke, George Berkeley and David Hume.

empirical testability. Hence, Kantian notion of synthetic a priori is no more tenable.

Another direct consequence of this idea is the rejection of any kind of metaphysical knowledge claims. However, logical positivists do not reject metaphysical knowledge claims on the ground that they are false. For them, these claims are rather cognitively meaningless. Beyond any doubt, metaphysical statements have factual content which means they are synthetic in their very nature. Nevertheless they are immune to empirical inquiry. Thus, they are neither analytic a priori nor synthetic a posteriori statements.

In order to give their treatment of statements of empirical sciences a rule-based character, logical positivists introduced a criterion for (cognitively) meaningfulness. It is called ‘verifiability criterion’ or ‘verificationism’. According to this criterion a statement which is alleged to be a statement of a sort of empirical science, i.e., a synthetic statement that purports to comprise factual content, has to be verifiable which means to be, at least potentially, empirically testable in virtue of which alone this statement can be rendered as true or false. Any synthetic statement which fails in the face of verifiability criterion is rejected as cognitively meaningless and, thus, neither true nor false.

However, there is some uneasiness with verificationism. One is that the statements expressing universal laws which are employed by the empirical sciences by means of induction from accumulated particular facts of the same sort are unverifiable since these statements range over an infinite domain (Psillos, 1999, p. 6). Another problem with this criterion concerns the status of theoretical terms which are posited and utilized by almost every theory of empirical sciences of any sort to predict and explain the phenomena. It is obvious that verifiability criterion can only be applicable to statements which are composed of observational terms. So, a statement like ‘All matter is made up of atoms’ is unverifiable because it contains an unobservable, or theoretical, term, i.e., atom. Thus, verifiability criterion renders this statement as cognitively meaningless in the absence of any further consideration on the issue. However, we know that theoretical law-like statements like the above one play crucial roles in the development of scientific theories and the explanation and prediction of observable phenomena. Rendering such theoretical statements as cognitively meaningless, then, does not do justice to empirical success enjoyed by scientific theories. It is clear that this would have been contrary to the empiricist concerns of logical positivists as well.

The above-mentioned difficulties necessitated a formal characterization of the verifiability criterion which would accommodate theoretical statements into the domain of cognitively meaningful statements. Rudolf Carnap attempted to accomplish this relying on his empiricist confines (Carnap, 1928). He argued that theoretical terms can be replaced and, thus, eliminated by virtue of proper observational terms which are alleged to *explicitly define* the former (Psillos, 1999, p. 4). Carnap called these observational substitutes 'scientific indicators' which "express observable states of affairs which are used as the definiens in the introduction of a term" (Psillos, 1999, p. 4). But, the project of defining theoretical terms explicitly by virtue of their alleged counterparts in observational language could not have been sustained.[4] Hence, Carnap conceded to weaken his formulation.

What Carnap (1936) attempted in his later work was to specify theoretical terms with reductive sentences instead of trying to define them explicitly in terms of observational terms (Psillos, 1999, p. 8). However, as Psillos notices:

> [The reductive sentence] can give only partial empirical significance to a theoretical term. A theoretical term can be associated with a whole set of reductive sentences which specify, in part, empirical situations to which the term applies … But no amount of reductive sentences is enough to explicitly define, and hence render eliminable, a theoretical term. (Psillos, 1999, p. 8)

And he continues:

> The shift from explicit definitions to reductive sentences marks also a shift from verification to confirmation. *T*-discourse [i.e., theoretical discourse] is rendered meaningful because it is confirmable. And it is confirmable because reductive sentences specify the conditions for the application of *t*-terms [i.e., theoretical terms] in certain observable situations. *T*-assertions [i.e., theoretical assertions] entail several observational predictions. Insofar as the latter can be confirmed, so can the former. (Psillos, 1999, p. 9)

As it has been stated, logical positivists' empiricist programme was started as a project of eliminating synthetic statements of the form which cannot be subjected to empirical tests as meaningless. And we saw that verifiability

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[4] For a profound explication of why this project was failed see Psillos (1999).

criterion could not do the job. The end result of this process was the confirmability of theoretical terms, and thus their ineliminability from scientific discourse. This indicates that theoretical statements and theoretical terms employed by these statements are indispensable for the scientific theorizing and practice. Hence, the interpretation of these theoretical statements and the ontological status of theoretical entities denoted by the relevant terms appear to be an important issue for the philosophers of science. In fact, varying interpretations of the issue led to the so called scientific realism debate. In the next chapter, I turn attention to this debate.

CHAPTER III

A Closer Look to the Recent Approaches to Scientific Realism Debate

In the reigning time of logical positivism, scientific enterprise was tried to be interpreted by empirical means. Verificationist and confirmationist attempts to interpret scientific theories yielded many powerful yet inconclusive consequences in favor of scientific anti-realism. It was not until 1960s that a cogent revival of scientific realism took place through the critiques of those methodological tools of logical positivists. At about the same time, drastic counter arguments against scientific realism were on their way to come. The relevant debate continued under the guidance of those opposite arguments until the emergence of a highly promising though not so new position called structural realism. John Worrall's (1989) article was paid so much attention that the ground of debate is shifted to a new and more refined level.

In this chapter, I try to examine this period which I took to be the maturity years of the debate. The controversy became so intense and sophisticated in this period that, at last, a reconciliation of ideas seemed almost inevitable. In what follows, firstly, I sketch a rough picture of the debate. Secondly, I expose some of the crucial arguments and special positions of scientific realism in details. Accordingly, I concentrate on ideas of some central figures of realist side for convenience. Then, I elaborate the most compelling arguments of anti-realists.

3.1 Scientific Realism Debate

As it has been stated in 2.3, empiricist programme of logical positivism proved that theoretical discourse is indispensable for the scientific enterprise. This result led various interpretations of theoretical statements and theoretical terms. The main distinguishing feature of these interpretations concerns the ontological and epistemological status of theoretical terms and theoretical statements which employ these terms in their constitution respectively. Thus, either one admits that theoretical statements carry truth-value as observational statements do since both are confirmable and theoretical terms have ontological commitments or that theoretical statements do not carry truth-value and, thus, theoretical terms do not have ontological commitments. The former view can be associated with scientific realism and the latter with instrumentalism. These two opposing views roughly form the basis of scientific realism debate.

Scientific realism, as Psillos notes, "incorporate[s] three theses (or stances), which can be differentiated as *metaphysical*, *semantic* and *epistemic*" (Psillos, 1999, xix). The metaphysical stance asserts that insofar as the theoretical entities posited by scientific theories exist, they do so mind-independently. The semantic stance, on the other hand, asserts that theoretical statements of scientific theories must be read literally, i.e., they are truth-conditioned assertions about theoretical terms which have putative factual reference provided that these theoretical statements are true (Psillos, 1999). And, “the epistemic stance regards mature and predictively successful scientific theories as well-confirmed and approximately true of the world. So, the entities posited by them, or, at any rate, entities very similar to those posited, do inhabit the world” (Psillos, 1999, xix).

Instrumentalism, on the other hand, is the view that “theories are ‘black boxes’, which when fed with true observational premisses yield true observational conclusions ….” (Psillos, 1999, p. 73).

> … [T]heoretical statements are not assertions, strictly speaking. They should be considered as merely syntactic constructs for the organization of experience, for connecting empirical laws and observations that would otherwise be taken to be irrelevant to one another, and for guiding further experimental investigation. (Psillos, 1999, p. 17)

As a consequence of this interpretation, theoretical terms are not held to refer to existent theoretical entities which are beyond the observational realm. Rather they are treated as “symbolic means … to organise experience ….” (Psillos, 1999, p. 17).

In fact, instrumentalism has become the generally accepted position among the philosophers of science after the failure of empiricist programme of logical positivism. A reframing of Ernst Mach’s (1893) operationalist views (instead of early Pierre Duhem’s (1906[1954]) non-eliminative instrumentalist views[5]) provided the basis for the instrumentalism of post-verificationist epoch (Psillos, 1999). In this period, instrumentalist philosophers of science tried to dispense with all theoretical discourse which obliges someone to commit ontologically to a realm behind the observable phenomena. This situation remained so until the late 1950s when thoroughgoing criticisms of methods of logical positivism and instrumentalism emerged. In the next few sections, I focus on these criticisms and consequent revival of scientific realism.

3.2 Revival of Scientific Realism

No sooner than the second half of the 20th century, scientific realism made an intense comeback. In this period, lots of arguments and positions in favor of scientific realism enunciated and advocated by some of the eminent philosophers of the time like J. J. S. Smart, Hilary Putnam, Karl Raimund Popper, Nancy Cartwright and Ian Hacking. Among these arguments and positions I mention three of them in brief. First one is Karl Raimund Popper's falsificationist method[6] which plays an important role in the history of scientific realism debate as being a transitional stage from logical positivism to a metaphysically more liberated period of the philosophy of

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[5] By non-eliminative instrumentalism, one should understand a position which claims that dispensing with theoretical discourse is not necessary. According to Duhem's anti-explanationist stand, one can remain agnostic as to whether there is an unobservable reality behind the phenomena or not. However, he believes that science does not need to explain phenomena by an appeal to unobservable reality in order to proceed successfully. (Psillos, 1999) For a detailed discussion of Duhem's views see Psillos (1999).

[6] See Popper (1959[1972]) and (1963[1972a]).

science. Moreover, his method can be labeled as realist though it faces severe problems as logical positivists did. Secondly, I make mention of an argument called the ‘no miracle argument’. This argument is rightly associated with Hilary Putnam (1975) notwithstanding that J. J. C. Smart (1963) has alluded to a quite similar argument before the former. Simply, the argument asserts that the empirical success of our currently accepted best theories would be a miracle unless some parts of them have latched onto or correctly described the real world. Lastly, I give an outline of a rather modest realist position—i.e., ‘entity realism’—to which I appeal in the last chapter again in order to support my own views concerning the issue.

3.2.1. Karl Raimund Popper and Falsificationism

As it has been seen in the previous chapter, logical positivists tried to distinguish allegedly cognitively meaningful and, thus, scientific knowledge claims from those of metaphysics by an appeal to verifiability criterion of meaning. Their intention was to establish that metaphysical claims are cognitively meaningless rather than being true or false. At the end, they came up with a weakened criterion of confirmation which opened up a place for the constant adoption of theoretical terms in scientific theories.

Karl Raimund Popper (1959[1972]; 1963[1972a]), an eminent opponent of Vienna Circle, criticized the verificaionist and confirmationist methods of logical positivism as being proper criteria for distinguishing science from metaphysics. He denied the idea that apparently metaphysical claims of a theory should be counted as meaningless. For instance, metaphysical doctrines of Marxist theory might well be as meaningful as scientific claims of Einstein's general theory of relativity. However, for Popper, these theories are differentiated with respect to a certain aspect which is not been held by both theories, that is, falsifiability. Popper argues that Einstein's theory of general relativity takes a great 'risk' in the face of genuine empirical tests which can falsify it while Marxist theory is immune to such attempts of disconfirmation (Popper, 1963[1972a]). Moreover, every theory can be confirmable in some sense. Thus, Popper concludes that it is not the verifiability that distinguishes science from metaphysics and cognitively meaningful statements from cognitively meaningless ones. Rather, it is a theory's capacity to be falsified which distinguishes scientific theories from unscientific ones. In his words, then, "*the criterion of the scientific status of a theory is its falsifiabilty, or refutability, or testability*" (Popper, 1963[1972a], p. 37).

For Popper, any scientific theory must be formulated as a 'bold conjecture', which indicates a high possibility of falsifiability in the face genuine empirical tests. If a theory survives many of such tests, then it is corroborated which means that it is more 'verisimilar' than any other scientific theory which cannot pass the very same set of those tests. Popper's characterization of 'verisimilitude' of scientific theories points to another promising idea of his account. That is, the aim of science must be to build up theories which strive for truth. Nevertheless, Popper thinks that this aim can never be achieved since every theory, at least in principle, is born refuted insofar as they are falsifiable. Psillos nicely summarizes this idea of Popper:

> One may well accept the view that all existing scientific theories are (likely to be) false, and yet also hold that they are closer to the truth than their predecessors. If, science as it grows, moves on to theories with higher verisimilitude, then there is a clear sense in which this process takes science closer to the truth (although, at any given point in time, we may not know how close to the truth science is). (Psillos, 1999, pp. 261-262)

At any rate, his emphasis on truth-likeness (verisimilitude) of scientific theories allows us to call Popper, as he himself acknowledges, a 'conjectural realist' philosopher of science. But, things get worse on behalf of Popper when he tries to give a formal account of the notion of verisimilitude.[7] The explication of his alleged formalization of the verisimilitude is beyond the scope of this thesis. However, regardless of his failed attempt to formalize the comparative verisimilitude of scientific theories, Popper initiated a progress for realist interpretations of scientific theories—at least on epistemological grounds.

In the next section, I explore the details of a highly influential argument in favor of scientific realism which is called the 'no miracle argument'.

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[7] For the explication of Popper's formal account of comparative verisimilitude of theories and the disproof of it see Psillos (1999).

3.2.2. The No Miracle Argument

In this section, I make mention of a highly appreciated argument in favor of scientific realism which is the so-called ‘no miracle argument’ (NMA). The appellation of this argument is due to the following line of Hilary Putnam: “The positive argument for realism is that it is the only philosophy that does not make the success of science a miracle” (Putnam, 1975, p. 73). This well-known argument is simply as follows: If a theory made many correct (novel) empirical predictions, then we should conclude that the theory has somehow ‘latched onto’ the world, that is, what it says about the furniture of the world is, at least in some parts, ‘essentially’ correct. Otherwise, the situation would be miraculous.

It is true that scientific realism is the only attitude towards scientific theories that accounts for the empirical success of science by giving an explanation of it. It is this explanation that is missing in the instrumentalist interpretations of scientific theories. As it has been mentioned before, instrumentalist interpretations treat scientific theories and theoretical terms employed by them as mere instruments to classify the already known observable phenomena. That is, scientific theories and theoretical terms have no meaning, existential status and epistemological value beyond their being useful tools for the systematization of observable phenomena. But, an explanation of the empirical success of scientific theories—especially when it comes to the vast number of novel predictions enjoyed by some of these theories—is impossible in this way of interpretations. As Psillos rightly puts: “The instrumentalist claim that theories are ‘black boxes’, which when fed with true observational premisses yield true observational conclusions, would offer no explanation whatsoever of the fact that these ‘black boxes’ are so successful” (Psillos, 1999, p. 73).

Accordingly, Smart hints at an argument quite similar to NMA to account for the empirical success of scientific theories when he criticizes the above-mentioned instrumentalist interpretations of scientific theories in the following words:

> If the phenomenalist[8] about theoretical entities is correct we must believe in a *cosmic coincidence*. That is, if this is so, statements about electrons, etc., are of only instrumental value: they simply enable us to predict phenomena on the level of galvanometers and cloud chambers. They do nothing to remove the *surprising character* of these phenomena. (Smart, 1963, p. 39)

And, he offers explicitly his argument when he says:

> On the other hand, if we interpret a theory in a realist way, then we have no need for such a cosmic coincidence: it is not surprising that galvanometers and cloud chambers behave in the sort of way they do, for if there really are electrons, etc., this is just what we should expect. (Smart, 1963, p. 39)

So, how realism provides us with the alleged explanation of empirical success of scientific theories that is missing in instrumentalism can be inferred from above quotation. Believing in the existence of theoretical entities and, to a certain extent, truth of statements including the terms denoting these entities is seemingly a proper way of explaining the empirical success of science.

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[8] For phenomenalist one can read instrumentalist for convenience.

Psillos claims that Putnam's version of NMA is an instance of another well-known argument, that is, 'inference to the best explanation' (IBE). This more general form of argument is allegedly meant to be a logical argument in favor of scientific realism relying on abductive reasoning. Nonetheless, its thus construed logical status is disputable. By means of abductive reasoning, IBE concludes that the truth of scientific realism can be inferred from the success of science. However, some critics of this argument claim that accounting for the success of science does not necessarily entail the truth of scientific realism. For example, Van Fraassen (1980) criticizes the legitimacy of abductive reasoning and IBE as being an argument in favor of the truth of scientific realism and asserts that the notion of empirical adequacy can equally account for the apparent empirical success of science.[9] The problems which will be discussed in sections 3.3.1 and 3.3.3 are highly motivated by the idea that although some past theories of mature science are empirically successful, it is not the case that one can interpret them realistically. History of science reveals that there is a bunch of empirically successful past theories though their central terms denoting the underlying reality allegedly responsible for the producing of phenomena are indeed non-referring. Thus, one must be cautious about adopting the IBE-based versions of NMA in order to avoid these sorts of historically inspired challenges.

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[9] Details of Van Fraassen's views on the issue is beyond the scope of this thesis. For a comprehensive discussion and criticism of his views see Psillos (1999).

On the other hand, Smart's version of NMA is not an instance of IBE. Rather it is, as Psillos puts it, "a general philosophical argument, what is sometimes called a plausibility argument …." (Psillos, 1999, p. 73). The different character of Smart's version of NMA from those of IBE-based versions is clarified by Psillos with the following:

> … Smart's 'no cosmic coincidence' argument relies on primarily intuitive judgements as to what is plausible and what requires explanation. It claims that it is intuitively more plausible to accept realism over instrumentalism because realism leaves less things unexplained and coincidental than does instrumentalism. Its argumentative force, if any, is that anyone with an open mind and good sense could and would find the conclusion of the argument intuitively plausible, persuasive and rational to accept—though [contrary to IBE-based versions of NMA] not logically compelling: not because one would recognize the argument as an instance of a trusted inferential scheme, but because of intuitive considerations about what is more and what is less plausible." (Psillos, 1999, p. 73)

My commitment to the NMA is akin to Smart's indeed. The most striking feature of some theories of mature science is their ability to make novel predictions. Rather than predicting already known phenomena, it is these novel predictions that need to be explained in order not to leave anything seem miraculous on behalf of scientific theories. For instance, the discovery of Neptune by the equations of Newtonian mechanics and the prediction of Fresnel's theory of light that a white spot appears at the center of the shadow of an opaque disk when it is intercepted by a light beam are the unexpected and, thus, explanation-begging consequences of relevant theories. Hence, the need for an explanation, then, must be a common *sine qua non* for both realist and anti-realist philosophers in accounting for the empirical success of science. From my standpoint, a realist interpretation of scientific theories to a certain extent is essential to give an adequate explanation of the empirical success (in particular, the novel predictive success) of scientific theories. But, to what extent this realist interpretation is adequate and tenable is the concern of subsequent chapters.

In the next section, I focus on an innovative though modest form of scientific realism, that is, entity realism.

3.2.3. Entity Realism

Ian Hacking and Nancy Cartwright concurrently though independently introduced a special version of scientific realism called entity realism.[10] This is a position that considers all kind of belief in the existence of theoretical entities embedded in scientific theories as legitimate while dispensing with belief in the theories themselves which allegedly give descriptions of these entities. Put it another way, one can know that a theoretical entity *is* but not *what* it is.

In her (1983) Cartwright makes two kinds of distinctions one of which is between theoretical laws and phenomenological laws and the other is between theoretical explanations and causal explanations. For her, a causal explanation explains phenomenological laws by means of arguing for the existence of entities that play causal roles in the explanation. That is, without a belief in the existence of theoretical entities one cannot maintain that

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[10] See Hacking (1984) and Cartwright (1983).

causal explanations explain phenomenological laws governing the behavior of these entities. Theoretical explanations, on the other hand, explain the same phenomenological laws by the help of theoretical laws which have their source not in the experiment but in the very constitution of theory itself. But incompatible theoretical laws can explain the same phenomenon on a par. Hence, what all these mean, then, is that we have confidence in the causal explanations that explain the phenomenological laws derived from nothing but the experiment itself once we have accepted the existence of theoretical entities playing the causal roles in the explanation, but not in the theoretical explanations which can be provided with several incompatible theoretical laws. Thus, Cartwright concludes that we have no indisputable reason to believe in theories similar to the one which we have to believe in entities (Cartwright, 1983).

Hacking treats the same issue with a method contra to Cartwright's one.[11] What he does is to pay attention to the less appreciated part of the scientific enterprise by the philosophers of science—i.e., experiment. Opening sentence of his (1984) is that: "Experimental physics provides the strongest evidence for scientific realism" (Hacking, 1984, p. 154). And, in the subsequent lines, he continues:

> Discussions about scientific realism or antirealism usually talk about theories, explanation, and prediction. Debates at that level are necessarily inconclusive. Only at the level of experimental practice is scientific realism unavoidable—but this realism is not about theories

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[11] Hacking himself mentions about the difference between his and Cartwright's approaches (see Hacking, 1984, note 3) .

> and truth. The experimentalist need only be a realist about the entities used as tools. (Hacking, 1984, p. 154)

As it can be noted from above quotations, Hacking argues for scientific realism on the ground that 'the entities used as tools' in the experiments are real independently of the relevant theories describing them. But this does not mean that experimenting on an entity establishes the existence of that entity. Rather, it is the capacity of that entity to be manipulated that does the job (Hacking, 1984, p. 156). For Hacking, manipulating an entity produces hitherto unseen phenomena or effects. But manipulation, thus construed, naturally implies that the manipulated entity is a causal agent and has certain causal properties. I think, Hacking himself points to this fact when he says:

> *We are completely convinced of the reality of electrons when we regularly set to build—and often enough succeed in building—new kinds of device that use various well understood causal properties of electrons to interfere in other more hypothetical parts of nature.* (Hacking, 1984, p. 161)

Thus, for Hacking, it is the central tenet of entity realism that the existence of the theoretical entities that play causal role in the producing of new phenomena is established on the basis of certain causal properties in virtue of which these entities can be manipulated.

Entity realism, as its being based upon experimental practice, may well seem to be an indisputable and safe realist position to adopt. Nonetheless, if one has to apprehend entity realism as Hacking and Cartwright do, then she faces serious problems. For example, how can we know how to manipulate an entity without any recourse to the relevant theory which describes some of its alleged causal properties by means of which alone we can manipulate the entity in question? But, for the time being, I leave aside such questions. It is chapter 4 where I get back to this subject and discuss it in more detail.

In the next three sections, I concentrate on some antirealist arguments and try to present what kind of threats they pose on scientific realism.

3.3 Challenges to the Realistic Interpretations of Scientific Theories

In the subsequent three sections, I examine three anti-realist challenges to the realistic interpretations of scientific theories of which insights regarding the scientific realism debate will provide me with some provisional help to make the arguments of chapter 5 sustainable. The first one is the so-called underdetermination problem which challenges the choice between competing theories underdetermined by available evidence. Then, I further with Thomas Kuhn's (1962[1970]) views on the 'structure of scientific revolutions'. His historical analysis of scientific endeavor made the rationality of science contestable. The realist claim of substantial retention of content through theory changes needed to be reconsidered in the face of Kuhn's idea of scientific revolutions. Lastly, I explore Laudan's 'pessimistic meta-induction' which is highly motivated with the notion of scientific revolutions in the history of science.

3.3.1 Underdetermination Problem

One of the most compelling challenges against scientific realism is the underdetermination problem (UP).[12] Before giving an appropriate characterization of this problem, it is necessary to clarify two crucial phrases, which are ‘being empirically equivalent’ and ‘being theoretically different’. A theory **T** is empirically equivalent to a theory **T'** given that all of the available observational consequences of these two theories are the same. And, **T** is theoretically different from **T'** given that they employ different sets of theoretical entities (or unobservables) in their construction, say **t** and **t'** respectively. Given these descriptions, a more or less formal characterization of the UP is as follows:

For a theory **T**, it is always possible to find a theory **T'** which is empirically equivalent to **T** but theoretically different from it. So, for such two theories empirical evidence cannot favor one of them over the other. Therefore, it is said that these two theories are underdetermined by available evidence. That is to say, any empirically indistinguishable theories are also epistemically indistinguishable. This amounts to saying that there is no sustainable ground for maintaining the belief in the truth of a theory which has an empirically equivalent rival.

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[12] Some authors prefer to use ‘underdetermination of theories by evidence’ to refer this problem (see Psillos, 1999). I find ‘underdetermination problem’ more convenient to denominate the problem.

The problem is usually stated for two theories. But it is obvious that it can be extended to infinitely many theories. That is, it is logically possible that for a theory **T** there exist, in principle, infinitely many theories which are empirically equivalent to but theoretically different from it.

In fact, this problem mainly originates from the reigning empiricist conception of scientific theories. Empiricists claim that the only way of confirming and, thus, justifying our knowledge of a scientific theory is possible through testing its empirical predictions. Since empirically equivalent theories have all their observational consequences in common, which means they make the same empirical predictions about the world, empirical evidence by no means can help us to distinguish adequately between any two empirically equivalent but theoretically different theories.

Now, the question is this: What kind of a threat is posed upon scientific realism by UP? The answer for this question is closely related with the notion of truth with which a considerable number of scientific realists deal. For scientific realists who work with the notion of truth it is an almost indispensable argument that if a scientific theory is empirically successful, then at least some of its theoretical claims which involve theoretical entities are true. But, as it has been stated above, if we do not have any tool other than observation—i.e., devising empirical tests—to distinguish between theories, then we are stuck in UP in the case of two empirically equivalent but theoretically different theories. Hence, it becomes impossible to infer the truth of a theory through its empirical success. Moreover, the utterance of existence and knowledge claims about theoretical entities turns out to be devoid of meaning which means a devastating effect on the most crucial realist contentions.

However, it is a fact that the issue of devising and conducting empirical tests to distinguish between theories is not so trivial insofar as the actual theories of mature scientific enterprise are concerned. It is known that the most of the real theories of mature science are not constituted by a single theory but by a bundle of theories and some auxiliary assumptions. This fact creates a difficulty for devising a unique empirical test which will decisively constitute a criterion to distinguish between two empirically equivalent theories. Since it is always possible to make use of different auxiliary assumptions for the purpose of making empirical predictions, one theory can make such a differentiating prediction by means of an auxiliary assumption that a properly devised empirical test can favor the relevant theory over its rival.

Another shortcoming of UP is that its ordinary formulation is assumingly effective insofar as the available evidence is considered. If we leave aside the logically constructed fictitious cases which might yield the underdetermination of theory choice in the face of future evidence, then one can legitimately claim that in actual cases, UP cannot conclusively underdetermine the theory choice. Given that any future evidence can favor one of the competing theories, UP ceases to be a real threat for the theory choice.

Moreover, in actual scientific practice, there is hardly any genuine case of UP. So, it seems that UP must be defined in a more refined way in order to see whether it poses a serious threat to the actual theories of mature empirical sciences. However, as it shall be seen in 4.3, there seemingly occurs a case of UP in quantum mechanics. Later on, I will try to suggest some arguments which might clarify how scientific realism can deal with this case of UP in quantum mechanics.

For the time being, I go on with another anti-realist challenge to the realist interpretations of scientific theories which is associated with Thomas Kuhn.

3.3.2 Thomas Kuhn and *The Structure of Scientific Revolutions*

In his highly influential work called *The Structure of Scientific Revolutions* (1962[1970]), Thomas Kuhn proposes a descriptive account of the scientific endeavor which states that the actual history of science is in conflict with the assumedly continuous and cumulative character of scientific enterprise. He asserts that constant revolutions takes place in the history of science and scientific enterprise progresses through such revolutions (Kuhn, 1962[1970]).

Kuhn claims that in the course of normal science, scientists engage in solving research puzzles of current paradigm. But, sometimes unexpected results which cannot be resolved in terms of ongoing paradigms occur. These results are treated as anomalies of normal science. The accumulation of these anomalies leads scientific community and their ongoing paradigm into a crisis. And, at last, a revolutionary approach which is alleged to solve the anomalies in the old paradigm emerges and replaces it (Kuhn, 1962[1970]).

For Kuhn, successive paradigms are incommensurable in the sense that each of them refers to totally different and incompatible world views. For instance, what Newtonian mechanics held the concept of mass to refer is totally different from the concept of mass adopted by Einstein's theory of relativity (Kuhn, 1962[1970]).

As a challenge to scientific realism, Kuhn's account undermines the cumulative and continuous aspect of scientific enterprise through theory changes. Realist philosophers of science need to account for it in order not to dispense with their most crucial realist contentions which is that the best explanation of the empirical success of science is possible through a realist interpretation of scientific theories which presupposes a substantial continuity at theoretical level. That is, there must be some kind of theoretical retention through theory changes which accounts for the empirical success of science.

In the next section, I mention about Larry Laudan's 'pessimistic meta-induction' which closely related with Kuhn's historical interpretation of scientific endeavor.

3.3.3 Larry Laudan and the Pessimistic Meta-Induction

Larry Laudan appeals to an interpretation of history of science which is quite similar to Kuhn's when he asserts the argument of 'pessimistic meta-induction' in his *A Confutation of Convergent Realism* (1981). Psillos summarizes Laudan's argument as follows:

> The history of science is full of theories which at different times and for long periods had been empirically successful, and yet were shown to be false in the deep-structure claims they made about the world. It is similarly full of theoretical terms featuring in successful theories which do not refer. Therefore, by a simple (meta-)induction on scientific theories, our current successful theories are likely to be false, … and many or most of the theoretical terms featuring in them will turn out to be non-referential. (Psillos, 1999, p. 101)

And, as a consequence, "the empirical success of a theory provides no warrant for the claim that the theory is approximately true. There is no substantial retention at the theoretical, or deep-structural, level and no referential stability in theory-change" (Psillos, 1999, p. 101).

In the following chapter, I focus on a recent promising form of scientific realism called structural realism which is a modified form of scientific realism in the face of above-mentioned anti-realist challenges.

CHAPTER IV

Towards a Tenable Realist Position: Structural Realism

Scientific realism debate may well be entitled as being the most inextricable controversy of the twentieth century's philosophy of science. Both camps—i.e., realists and anti-realists—have equally influential arguments on their own side. For instance, realists claim that we have to confess that our currently accepted theories must have latched onto the real world if we want to interpret the empirical success of them in a non-miraculous way while anti-realists claim that scientific revolutions showed that realism stops short of explaining the ontological discontinuities through theory changes. The coercion due to such arguments led to several modified versions of scientific realism. Structural realism has become, seemingly, the most promising attitude among others. In fact, what makes structural realism such a remarkable position among several others is that it really does justice to the challenge stemming from scientific revolutions. It simply tells us that what our currently accepted theories can capture at their best about the real world is the knowledge of its structure—i.e., the *real* relations among unobservables expressed by the mathematical equations of our theories. As I shall try to mention in detail later on, proponents of this position differ from each other, in the main, depending on their choice of constraint exerted upon the scientific theories.

Hence, in the course of this chapter, I try to recapitulate the history of structural realism and elaborate and discuss the views of some major proponents of it. In the first section, I keep my attention directed on the early forms of structural realism. The subsequent two sections will be on more refined forms of structural realism. In the second section, I mention about the structural realism of John Worrall. Positions of Worrall and the philosophers that will be mentioned in the first section can be labeled as epistemic structural realism (ESR). In the third section, I explicate the views of Steven French and James Ladyman on the issue, whose position can be labeled as ontic structural realism (OSR).[13] Lastly, I present Psillos's criticisms on Worrall's structural realist account.

4.1 Early forms of structural realism: Poincaré, Russell and Maxwell

4.1.1 Poincaré

John Worrall's highly influential paper called "Structural Realism: The Best of Both Worlds?" (Worrall, 1989) may well be named as the starting point of a productive period on the issue of structural realism. However, as Worrall himself points out quite fairly, the origin of this position can traced back to the writings of Henri Poincaré. Although his general attitude towards the status of scientific theories is a certain kind of conventionalism (especially when it comes to the issue of choice between different geometrical systems)

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[13] The denomination is due to Chakravartty (2003) and the distinction is due to Ladyman (1998).

and, thus, quite antirealistic, the following long passage from *Science and Hypothesis* (Poincaré, 1905[1952]) concerning the quite similar character of Fresnel's and Maxwell's equations of optical phenomena despite their radically different ontological claims on the issue can be rated as the first exemplary notion of structural realism:

> This Fresnel's theory enables us to do today as well as it did before Maxwell's time. The differential equations are always true, they may be always integrated by the same methods, and the results of this integration still preserve their value. It cannot be said that this reducing theories to simple practical recipes; these equations express relations, and if the equations remain true, it is because the relations preserve their reality. They teach us now, as they did then, that there is such and such a relation between this thing and that; only, the something which we then called *motion*, we now call *electric current*. But these are merely names of the images we substituted for the real objects which Nature will hide for ever from our eyes. The true relations between these real objects are the only reality we can attain, and the sole condition is that the same relations shall exist between these objects as between the images we are forced to put in their place. (Poincaré, 1905[1952], pp. 160-161)

As we shall see later in Worrall's position as well, it is important to notice here that the constraint which Poincaré exerts upon scientific theories is an epistemic one. His structural thesis is motivated by purely epistemological concerns. This kind of an attitude stems from the need for an appropriate response to the problems posed by radical theory changes through scientific revolutions. Poincaré's allowance of any kind of metaphysics of objects concerning unobservables can be inferred from his lines quoted above which points to a distinction between images and real objects. This amounts to saying that Poincaré's epistemic constraint poses no restriction on the ontology of unobservables. It basically tells us that the only knowable thing about unobservables is their structure—i.e., relations expressed by mathematical equations. On account of the nature of them nothing can be known other than their existence and relations that they obtain. In fact, this nature and structure distinction will become the basic driving force of more recent discussions on the issue, which I mention in the subsequent sections in detail.

4.1.2 Russell

Another early attempt of a structuralist thesis is attributed to Bertrand Russell. His primary aim is to find out what can legitimately be inferred about the underlying causes of our percepts.[14] From the underlying causes of percepts, within a more recent terminology, it can be understood theoretical entities or unobservables. His conclusion is quite similar to Poincaré's. He claims that one can know nothing about the intrinsic properties (or nature) of unobservables but the structural properties. This structure of unobservables has a logico-mathematical character and can be inferred by the logico-mathematical structure of our currently accepted scientific theories. Here is a passage from *The Analysis of Matter* that conveys Russell's views on the subject: "Thus it would seem that wherever we infer from perceptions it is only structure that we can validly infer: and structure is what can be expressed by mathematical logic, which includes mathematics" (Russell, 1927, p. 254).

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[14] See Russell (1927), chap. xx.

What poses serious difficulties to the account of Russell is that the structure which is constructed in a purely logico-mathematical way cannot determine a particular relation whatsoever. This is the direct consequence of Russell's view that the only thing we know is the logico-mathematical structure of the world with regard to relations about which we know nothing but their existence. M. H. A. Newman criticizes this point as:

> These statements can only mean, I think, that our knowledge of the external world takes this form: The world consists of objects, forming an aggregate whose structure with regard to a certain relation R is known, say W; but of the relation R nothing is known (or nothing need be assumed to be known) but its existence; that is, all we can say is, "*There is* a relation R such that the structure of the external world with reference to R is W". Now I have already pointed out that such a statement expresses only a trivial property of the world. Any collection of things can be organized so as to have the structure W, provided there are the right number of them. Hence the doctrine that *only* structure is known involves the doctrine that *nothing* can be known that is not logically deducible from the mere fact of existence, except ("theoretically") the number of constituting objects. (Newman, 1928, p. 144)

So, it seems that the world can have the same structure with a class of arbitrarily collected things provided that the cardinalities of their classes of objects are equal, and this is absurd. In order to overcome this absurdity, something other than their mere existence must be known about certain relations which define structures. James Ladyman points to a same kind of worry when he writes:

> The *formal* structure of a relation can easily be obtained with any collection of objects provided there are enough of them, so having the formal structure cannot single out a unique referent for this relation—in order to do so we must stipulate that we are talking about the *intended* relation, which is to go beyond the structural description. (Ladyman, 1998, p. 412)

Thus, a proper interpretation of intended relations in Russell's account seems missing. I will turn this issue in the fifth chapter. For the time being, I further with the structuralist account of Grover Maxwell.

4.1.3 Maxwell

What Grover Maxwell tried to accomplish on account of structural realism was to refine and integrate Russellian structuralist thesis by making use of Ramsey sentences[15]. His overall aim is to defend a certain kind of realism while trying to make it cohere with his concerns about 'concept empiricism'.[16] He claims that our scientific theories obtain 'things that we are not *acquainted* with'—i.e., unobservables. Still, we can have access to their *indirect* reference by Russell's theory of descriptions. That is, it is possible to refer to unobservables not by theoretical terms but by logical terms such as quantifiers, variables and connectives and predicate terms that have *direct* referents by acquaintance (Maxwell, 1970, p. 16). According to Maxwell, Ramsification is the appropriate tool for conducting this process:

> It is only necessary to replace the conjunction of the assertions of the theory by its Ramsey sentence; that is, each theoretical predicate (each term referring to unobservables) is replaced by an existentially quantified predicate variable, the scope of each such quantifier extending over the entire conjunction. (Maxwell, 1970, pp. 16-17)

By Ramsification, the *existence* of the intrinsic (or first order) properties of unobservables are warranted since they are still referred to by Ramsey

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[15] For a detailed account of Ramsey sentences and Ramsification see Ramsey (1929[1978]).

[16] Concept empiricism, as Maxwell puts it, is simply that "concepts must originate in experience". (Maxwell, 1970, p. 14)

sentence though in an indirect way. But this does not mean that we also know what these intrinsic properties are. The only thing that we can know is their structural (or higher order) properties attributed to them by their Ramsey sentence (Maxwell, 1970, p. 17).

Maxwell's use of Ramsey sentences enables him to assert a certain kind of structural realism (indeed, one with an epistemic constraint) which transforms theoretical entities of a scientific theory into something that can be expressible in logical and observational terms. But this leads to, as Ladyman puts it, that "if we treat a theory just as its Ramsey sentence then the notion of theoretical equivalence collapses onto that of empirical equivalence" (Ladyman, 1998, p. 413).

And he continues:

> … [E]quating the structure of a theory to what is embodied in its Ramsey sentence cannot do justice to Worrall's intention in proposing structural realism, since he is quite clear that commitment to the structure of a theory goes beyond commitment to its strictly empirical level. (Ladyman, 1998, p. 413)

In the following section, I try to explicate 'Worrall's intention in proposing structural realism' and his suggestive arguments on the issue.

4.2 John Worrall's Structural Realism

In his promising paper called "Structural Realism: The Best of Both Worlds?" (Worrall, 1989), Worrall elaborates and defends structural realism as being a viable realist position. What motivates him to defend this position is the presence of two main arguments: one of them is for scientific realism called ‘no miracle’ argument and the other is against scientific realism called ‘pessimistic induction’. As he states: “The central question addressed in this paper is whether there is some reasonable way to have the best of both worlds” (Worrall, 1989, p. 99). Namely, he tries to adopt a realist attitude, though he accepts the drastic discontinuities due to the scientific revolutions which make hard to see science as a cumulative enterprise.

What makes Worrall stick to realism is the ‘no miracle argument’. Any consent to this argument persuades someone to interpret scientific theories realistically. And, this argument is so persuasive that any alleged rival attitude towards scientific theories other than realism necessitates the ignorance of it or at any rate the construction of an alternative argument which refutes it. But Worrall does not bite the bullet and takes the ‘no miracle’ argument into account. This well-known argument is simply as follows: If a theory made many correct (novel) empirical predictions, then we should conclude that the theory has somehow ‘latched onto’ the world, that is, what it says about the furniture of the world is, at least in some part, ‘essentially’ correct. Otherwise, the situation would be miraculous.

Although Worrall lends credence to the ‘no miracle argument’, he strongly rejects the full-blown version of it. Namely, he rejects the idea that the validity of inference to the best explanation in science can be established through the ‘no miracle argument’. In other words, scientific realism cannot be inferred from the success of science. His reliance on the ‘no miracle argument’ is restricted and goal-oriented—i.e. it is in favor of his structural realist position. He interprets the claim of ‘no miracle argument’ as: “[O]ther things being equal, a theory’s predictive success supplies a *prima facie* plausibility argument in favor of its somehow or other having latched onto the truth” (Worrall, 1989, p.102).

Worrall is quite aware of the problems that stem from the commitment to the ‘no miracle argument’ in order to justify a viable realist attitude with no further consideration. He believes that this common-sensical inclination to realism will be weakened under the charge of scientific revolutions. For him, the modified forms of realism, which try to show the argument from scientific revolutions as a defeasible threat, will face several problems and, therefore, are disputable. So his strategy is to take the most powerful argument of anti-realists—i.e., pessimistic meta-induction—into account seriously in an effort to establish his structural realist position rather than to modify scientific realism in order to overcome this argument.

One of the modifications adopted by most of the presentday scientific realists is the replacement of the concept of ‘truth’ with the concept of ‘approximate truth’. Worrall criticizes and tries to refute the view which treats scientific theories as ‘approximately true’. He states some logical difficulties that have to be overcome by the proponents of this view. Since a discussion of the concept of ‘approximate truth’ is beyond the scope of this thesis, I will not mention about these logical difficulties in detail here. But, what I would like to express in this context is that Worrall directs his criticisms towards the scientific realists who claim there to exist an approximation of radical theoretical changes caused by scientific revolutions.

Worrall admits that scientific process at the empirical level is essentially cumulative. But, he rejects the accumulation of theoretical entities at the top theoretical levels. He states this situation as follows:

> This picture of theory-change in the past would seem to supply good inductive grounds for holding that those theories presently accepted in science will, within a reasonably brief period, themselves be replaced by theories retain (and extent) the empirical success of present theories, but do so on the basis of underlying theoretical assumptions entirely at odds with those presently accepted. (Worrall, 1989, p. 109)

Nevertheless, Worrall believes that this picture of theory-change can be shown to be inaccurate and some kind of accumulation at the theoretical level can be assured. For him, this issue can be achieved through adopting the appropriate position towards scientific theories—i.e. structural realism.

As Worrall himself notices, this position was already adopted and defended by Poincaré: “This largely forgotten thesis of Poincaré’s seems to me to offer the only hopeful way of *both* underwriting the ‘no miracles’ argument *and* accepting an accurate account of the extent of theory change in science” (Worrall, 1989, p. 117). What is the main claim made by Worrall’s structural realism is simply as follows: There is a theoretical continuity or accumulation in the shift led by scientific revolutions, but this continuity is one of structure, not of content. That is, there is an important retention in theory change which is more than just the retention of empirical content. But, this retention is also less than the retention of full theoretical content. And, for Worrall, this element of continuity in addition to the empirical content is the structure of superseded theory—i.e., the mathematical equations of superseded theory. What is important in this claim in favor of realism is that these mathematical equations express *real* relations among unobservable entities.

Worrall uses the example of the switch from Fresnel's to Maxwell's theory of light in order to illustrate his claims more clearly. He stresses on the point that Fresnel's theory undergone a radical change at the top theoretical level. That is, the elastic solid ether, of which assumed function was to be a medium for the light to oscillate through, was entirely overthrown by the advent of Maxwell's theory and it was replaced by the 'disembodied' electromagnetic field. Nonetheless, Fresnel's theory was a good example of 'mature' science and made many correct 'novel' empirical predictions. His theory accurately captured many of the optical phenomena. But, what did enable this theory to make so many correct predictions, though it went wrong about the nature of light? For Worrall, it was the correct structure of Fresnel's mathematical equations: "It wasn't, then, just that Fresnel's theory *happened* to make certain correct predictions, it made them because it had accurately identified certain relations between optical phenomena" (Worrall, 1989, p. 119).

It can be seen that Worrall, as all the aforementioned philosophers did, wants to make a distinction between nature and structure of unobservables which will be severely disputed by some eminent scientific realists like Stathis Psillos and the proponents of OSR. He believes that our knowledge is restricted to its structural characteristics. We can know something about the reality of unobservables if this something is in accordance with the structure. And, he claims that, by acknowledging Poincaréan preludes, this *knowable* part of *reality* is the relations among unobservables. For him, the *nature* of unobservables remains as the *unknowable* part of the *whole reality.* From these concerns, I think, it can be said that Worrall still admits the existence of a mind-independent world consisting in unobservable and observable phenomena but casts severe doubts on and indeed rejects the knowledge claims of our currently accepted scientific theories about the *nature* of unobservables. That is, he asserts that we should abandon the commitment to the ontology of our scientific theories which will be replaced through theory change but maintain our belief in the truth of the knowledge claims of them about the *structure* of unobservables which also holds among the objects of real world and is immune to theory change. Thus far, Worrall seems to agree with all the aforementioned philosophers. The important bit in Worrall's views is that he treats the retention of structure through theory changes as a retention at the theoretical or non-empirical level.

In the next section, I put forward the views of another conception of structural realism, proponents of which consider the talk of intrinsic nature of unobservables as pointless, that is, ontic structural realism.

4.3 Steven French and James Ladyman on Ontic Structural Realism

Steven French and James Ladyman are two eminent proponents of a special version of structural realism—i.e., ontic form of structural realism (OSR). They deduce their position from a highly productive theory of modern physics and a special version of it which are quantum mechanics and quantum field theory respectively. The distinctive feature underlying their position is the abolishment of the commonly held thought which is that the unobservable entities function as the basic ontology of our scientific theories.

Before elaborating the seemingly counterintuitive claims of OSR, an essential remark has to be made on this position. That is, OSR is a well-motivated attempt to overcome the underdetermination problem (UP). ESR, as we have already seen, is an alleged realist response to the problems stemming from scientific revolutions and pessimistic meta-induction. But, as a result of its very constitution, ESR leaves UP intact. At best, it offers the existence of unobservable entities and the knowledge of their structural properties and relations. But if this is the utmost information that theories provide us with, then it is inevitable that the unobservable entities are (at least, in principle) underdetermined by the structure of these theories. French and Ladyman rightly point out that there exists a drastic instance of UP in quantum mechanics:

> If we consider this most successful of our (mature) current theories in metaphysical terms, then we discover a kind of metaphysical underdetermination in that the physics is compatible with a view of quantum objects as non-individual—in the sense, as typically expressed, that they have 'lost' their identity—and also with a view of such objects as individuals… (French and Ladyman, 2003, p. 36)

For them, "the locus of this metaphysical underdetermination is the notion of an object so one way of avoiding it would be to reconceptualise this notion entirely in structural terms" (French and Ladyman, 2003, p. 37). Converting the notion of an object into something that is wholly defined in structural terms abolishes the notion of an object-based ontology and reintroduces the notion of a structure as the only thing in theories that is ontologically subsistent. Both the individual and the non-individual aspects of quantum mechanics can allegedly be captured by "two different (metaphysical) representations of the same structure" (French and Ladyman, 2003, p. 37). Thus, neither of these representations can be interpreted as favoring one of the ontologies—i.e., particle ontology and field ontology—as the basic ontology of quantum mechanics.

In fact, what French and Ladyman did is to reduce a physical theory to its mathematical structure. Tian Yu Cao recapitulates this view of French and Ladyman as: "You can take a particle ontology, or a field ontology if you like, this difference will make no cognitive difference to the physics physicists are doing, although a different degree of convenience may be involved" (Cao, 2003, p. 17). But he continues: "In the same spirit, we may claim that both quantized gauge field theory and the general theory of relativity are just different representations of the same mathematical structure, the fiber bundle" (Cao, 2003, p. 17). In the light of this example, Cao concludes that "the ontological difference, which underlies a conceptual revolution separating classical from quantum field theory, becomes invisible in this kind of reasoning" (Cao, 2003, p. 17). Thus, it can be said that an interpretation of physical theories in terms of unobservable entities seems reasonable. But what French and Ladyman suggest instead is a conceptual revision. They present their alleged conceptual revision in the following lines:

> … [H]ow can you have structure without (non-structural) objects? Here the structuralist finds herself hamstrung by the descriptive inadequacies of modern logic and set theory which retains the classical framework of individual objects represented by variables and which are the subject of predication or membership respectively … In lieu of a more appropriate framework for structuralist metaphysics, one has to resort to a kind of 'spatchcock' approach, treating the logical variables and constants as mere placeholders which allow us to define and describe the relevant relations which bear all the ontological weight. (French and Ladyman, 2003, p. 41)

However, it does not seem possible to me that we can dispense with an object-based ontology no matter whether or not our conceptual framework depends on the inadequacies of modern logic and set theory. Chakravartty emphasize the same point of view when he writes:

> It is part of the very concept of a concrete relation that it relate *something*. According to our concepts of these things, the former cannot exist without the latter, and in this sense, objects play an important, constitutive, explanatory role in our notion of structure. (Chakravartty, 2003, p. 871)

Thus, we need to clarify the ‘basic ontology’ of a scientific theory in terms of physical entities in order to do justice to the undeniable process of ‘conceptual revolutions’ occurring in science. Cao suggests that although there seems no way of having direct access to unobservable reality, we can attain the objective knowledge of this reality through our structural knowledge of it. He asserts that there are representational and conventional elements in mathematical structures of our scientific theories. The representational part includes the ontologically primary entities (basic ontology) and derivative entities. It is by the help of this representational part that we can infer or construct the knowledge of unobservable reality (Cao, 2003). Turning back to quantum field theory, Cao claims that field is the basic ontology of this theory and particles as being the observable manifestations of the field are derivative entities of the representational part of mathematical structure. This approach seems to solve the problem of underdetermination for quantum field theory without dismissing the concept of object-based ontology by distinguishing between ontologically primary entities and derivative entities. But there remains a problem. How can we be admissibly justified in the transition from the reality of a structure to the reality of an entity? Cao argues that it is a matter of empirical investigation. Historical process, sooner or later, will provide us with empirical evidence which supports the reality of our constructions about unobservable entities. He applies this idea to the relevant case as: “If the equations and various structural statements about the particles (which are observable manifestations of the field) are confirmed by empirical investigations, then the reality of the fields is established" (Cao, 2003, p. 21).

4.4 Stathis Psillos versus John Worrall

Before getting into the details of an allegedly viable account of scientific realism, it is necessary to mention one of the criticisms on Worrall's structural realist account which is exposed by Stathis Psillos.

Psillos, a prominent exponent of scientific realism, criticizes the structural realist position of Worrall in his book named *Scientific Realism—How Science Tracks Truth* (Psillos, 1999). Chapter 7 of this book is fully devoted to this task. What plays a central role in Psillos' criticisms is his rejection of the structure versus content—or nature—distinction made by Worrall.

Psillos accepts the retention of some mathematical equations in the transition from old to new theories. He also accepts that not all the theoretical content of old theory is retained in the new theory. Thus far, Psillos seems to be with Worrall, though he manifests some differences in the interpretation of these claims. The fundamental disagreement between Psillos and Worrall stems from the fact that they attribute different roles to mathematical equations. On the one hand, Worrall claims that mathematical equations expressing true relations among unobservable entities enable theories to make correct 'novel' empirical predictions. On the other hand, Psillos rejects the view that mathematical equations alone can make these predictions: “What is not true, however, is that mathematical equations alone—devoid of their theoretical content—can give rise to any predictions whatsoever” (Psillos, 1999, p.153). What Psillos wants to emphasis by this claim is that mathematical equations together with their theoretical interpretations can give rise to correct ‘novel’ predictions. And, thus, these theoretical interpretations reveal some substantive properties, causal mechanisms and law-like behaviours of unobservable entities, which must also be retained in transition from old to new theory. He states this view as follows:

> If the empirical success of a theory offers any grounds for thinking that some parts of a theory have ‘latched on to’ the world, those parts cannot be just some (uninterpreted) mathematical equations of the theory, but must include some theoretical assertions concerning some substantive properties as well as the law-like behaviour of the entities and mechanisms posited by the theory. These theoretical parts include, but are not exhausted by, mathematical equations. (Psillos, 1999, p.154)

Psillos, then, unlike Worrall, claims that these theoretical parts, which are more than the relations expressed by mathematical equations, fall under the scope of our knowledge as well. But, Psillos does not stop at this point and makes a crucial claim to break off structural realism. He rejects the distinction between structure and nature and claims “that the ‘nature’ of an entity forms a continuum with its ‘structure’, and that knowing the one involves and entails knowing the other” (Psillos, 1999, pp.156-157). What motivates Psillos to assert such a claim is his conception of nature of an entity as the totality of properties and relations of that entity. He puts it as follows:

> … [T]o 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. An exhaustive specification of this set of properties and relations leaves nothing left out. Any talk of something else remaining uncaptured when this specification is made is, I think, obscure. (Psillos, 1999, p.156)

I think, this 'talk of something remaining uncaptured' is not something what a structural realist intend to do. Instead, she states a dichotomy between nature and structure. The full 'specification' of structure might reveal the full knowledge of this structure. But, the nature is always independent of this specification. Nature is something which cannot be the object of our knowledge—we cannot state the nature of a thing by giving a description which describes it as the totality of its predicates, relations, etc. Put it another way, we cannot have any direct access to the real objects described by our theories. What our representations of them (i.e., mathematical equations of our theories) tell us is the only possible objective knowledge of these objects. It can be suggested that there is more to our knowledge than the relations among unobservables. However, Worrall does not discuss the issue to this extent. What I want to do is to show that it is possible to remain structural realist while claiming there are more things to be known other than purely formal or mathematical relations among unobservables. Namely, I try to state that things retained in the transition from old to new theory which are different than the mathematical relations among unobservables can be integrated into the structure as the parts and parcels of it. In the fifth chapter, I try to clarify this view.

CHAPTER V

An Attempt to Reinforce Scientific Realism

Thus far, I have elaborated and clarified the origin of scientific realism debate and its current status. Improvements in mathematics and physics at the turn of the century led philosophers, of whom Poincare, Duhem and Mach were the spearheading, to generate up and discuss the idea of scientific realism. After the recognized triumph of General Theory of Relativity, Logical Positivism became the received view in the philosophy of science. Accordingly, the methodological tools of logical positivists—i.e., verificationism and, later on, confirmationism— accepted as the basic way of evaluating the scientific theories. General treatment of these philosophers concerning the ontological and epistemological status of theoretical entities was either the total elimination or reduction (for sure, to observable entities which are allegedly the only meaningful and truth conditioned parts of theories) of them. Subsequent problems regarding the misapprehension of confirmationism and, hence, inadequacy of eliminative and reductionist approaches towards theoretical entities paved the way to alternative methods of theory evaluation. In the meantime, some brand new arguments and positions, by means of which scientific realism gained its reputation back, began to develop. But, the coercion of the idea of scientific revolutions generated primarily by the works of Thomas S. Kuhn forced scientific realism to a certain modification. It was not until the publication of highly appreciated 1989 paper of John Worrall that an appropriate strategy for this modification initiated.

As it has been stated in the previous chapter, Worrall's invoking of structuralism presented a good enough reason for scientific realists to assert confidently that some substantial parts of theories (i.e., relations expressed in mathematical equations which allegedly represent the structure of the unobservables), which are different from empirical content, retained through the theory changes dictated by scientific revolutions in the history of science. In a recent paper, however, Worrall claims that the appropriate way to represent the structure of unobservables is through the Ramsey sentence of the statement that involves the relevant unobservable entities.[17] But, as it has been discussed, the method of Ramsification leads to severe problems as well. Moreover, Worrall's account cannot say anything about the underdetermination of theoretical entities by available empirical evidence. It was the proponents of OSR who saw the issue of underdetermination as manageable by an appropriate adoption of the structure. But, the price that we have to pay is to dispense with the idea of an object-based, or at least a basic, ontology which will be too much for a realist to afford, who believes in the reality of physical entity which is quite different in kind from the mere mathematical representation of it.

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[17] For an explication of this matter see Worrall (2007).

In this chapter, I try to overcome the deficient parts of both structuralist accounts and come up with a tenable realist position. In essence, this position allegedly has its roots in epistemic form of structural realism; however, I make use of another realist position in order to supplement and establish it, that is, entity realism. Modestly, I call this position substantial structural realism.

5.1 Structural Realism Scrutinized

In the course of this section, I clarify the intended strategy to attain the above-mentioned allegedly (structural) realist position. My overall claim about the issue, after all, is that the only viable scientific realist position may be an epistemic form of structural realism combined with entity realism, which I prefer to call substantial structural realism. This position, I urge, does justice to coercive arguments of anti-realists stemming from radical theory changes while reckoning ultimate confines of the cognitive content of our presently accepted theories.

However, attaining to such a position does not seem possible through the standard evaluations of ESR and entity realism. Both positions need to be reconsidered and modified in a way to make it possible to combine them. For that reason, firstly, I discuss the problems of entity realism stemming from its standard construal. This discussion presumably reveals the misconceived nature of the dichotomy between realism about entities and realism about theories. Then, I pay my attention to the issue of combining entity realism with ESR. Assumedly, the entrenched form of entity realism might offer a solution to the confines of ESR and partially overcome its ontological neutrality in the face of UP. On the other hand, ESR might well be considered as providing a basis for entity realism by hypothesizing causal properties of entities prior to their testing.

5.1.2 Entity Realism: A Supplementary Position?

As it has already been stated in section 3.2.3, entity realism is a modest realist position which only commits itself to the belief in the existence of theoretical entities while refraining from commitment to the truth of descriptions of these entities made by the relevant theories. However, as I have mentioned briefly in the concluding lines of section 3.2.3, entity realism, thus understood, has serious problems. I urge that a reinterpretation of entity realism may solve these problems and let us treat it as a position which can supplement a broader version of scientific realism when combined with a modified form of epistemic structural realism.

The strategy in order to attain a proper construal of entity realism is, I argue, through careful investigation of its epistemological and ontological confines. What entity realism permits us to commit ontologically is the existence of theoretical entities of our best scientific theories. It claims that experimental practice will provide us with good enough reason to assert their existence.

Likewise, the only attainable knowledge is the existence of these entities and some of their causal properties functioning in the experiments to produce new phenomena by means of manipulation. But to recapitulate the question asked in the section 3.2.3, how can we come to know about how to manipulate an entity without any recourse to the relevant theory which describes some of its alleged causal properties by means of which alone we can manipulate the entity in question?

In his (1984, *passim*), Hacking argues that some 'well-understood' causal properties of theoretical entities are responsible for the producing of new phenomena but as far as I am concerned, he never mentions about how we gain the knowledge of causal properties that make them possible to be manipulated in order to yield relevant phenomena. For instance, he claims that "[b]y now we design apparatus relying on a modest number of home truths about [that is, some knowledge of the causal properties of] electrons, in order to produce some other phenomenon that we wish to investigate" (Hacking, 1984, p. 161). However, the derivation procedure of this 'modest number of home truths about electrons' is obscure. Here is another passage from the same article:

> Understanding some causal properties of electrons, you guess how to build a very ingenious, complex device that enables you to line up the electrons the way you want, in order to see what will happen to something else. Once you have the right experimental idea, you know in advance roughly how to try to build the device, because you know that this is the way to get the electrons to behave in such and such a way. (Hacking, 1984, p. 156)

It can be legitimately inferred from these lines that Hacking is quite sure about that ‘the way to get the electrons to behave in such and such a way’ is possible only by means of ‘understanding some causal properties of electrons’. But, again, ‘the understanding’ of these causal properties is indecisive.

The only proper way to address the relevant source of the knowledge, or preferably the understanding, of the causal properties of theoretical entities is, I suggest, the recourse to the theoretical descriptions of these entities. It is these theoretical descriptions that guide us to manipulate entities properly in order to produce certain phenomena. Psillos argues for this requirement when he writes:

> Experimenters do not know what exactly it is that they manipulate, although they can know that they are manipulating something, unless they adopt some theoretical descriptions of the entities they manipulate. It is by means of such theoretical descriptions that they make the relevant identifications and discriminations. What makes electrons different from, say, neutrinos is that they have different properties, and obey different laws. One should rely on these theoretical descriptions in order to manipulate these entities effectively and exploit their causal powers. (Psillos, 1999, p. 256)

Thus, it is by means of these descriptions that we are able to identify entities on the basis of their properties. But how can we infer the knowledge of these properties from theoretical descriptions? To answer this question, an elucidation of theoretical descriptions is necessary. As it has been already put, we identify entities through our theoretical descriptions. But to pay regard to the historical fact, incompatible theories can describe the very same entity in different manners. For instance, both Fresnel and Maxwell were unequivocally referring to the same entity, i.e., light, although they adopted different descriptions to identify it. For Fresnel, light is a wave constituted by the vibrations of the molecules of the ether, which is an elastic, solid medium. On the other hand, Maxwell claims that light is a wave produced by the oscillations of electric and magnetic field strengths in a *sui generis* electromagnetic field. However, despite this crucial difference between these theories, they both enjoyed a very high degree of predictive success. Moreover, these theories shared a remarkable amount of similarities at the theoretical level (e.g. mathematical equations). Hence, we must interpret both theories as being correct about some of their descriptions of theoretical entities to explain the relevant continuities between them and their predictive success. So, a proper kind of discrimination between theoretical descriptions must be held in order not to abandon the claim that some of theoretical descriptions enable us to manipulate entities properly to produce certain phenomena.

I claim that the relevant distinction can be made between descriptions for which mathematical formalism of a theory is responsible and descriptions that cannot be expressed in terms of this formalism. The latter describe, to say roughly, some properties that are alleged to be the complementary parts of the metaphysical schemes of scientific theories. For instance, in Fresnel's theory, employment of the molecules of ether served as a means to give a metaphysical explanation for his theory. He believed that the optical phenomena regarding light were a mechanical process after all. Thus, he attributed some properties to the carrier of light (i.e., its being an elastic, solid, all-pervading medium) to accommodate this *supposedly* mechanical character of the phenomena into his theory. However, as Psillos rightly notices: "[I]n his proof, Fresnel did not appeal to any specific mechanical model of the ether in order to derive his laws"[18] (Psillos, 1999, p. 158). That is, these properties assigned to the carrier of light by Fresnel did not appear in the constitution of the mathematical equations of his theory. On the other hand, the former kind of descriptions is the ones that are exhausted by the mathematical equations of theories. As I noticed earlier, although Fresnel's theory of light was eventually superseded by Maxwell's, mathematical equations of the former retained in the latter. So, the acknowledged truth of these equations must have captured correctly something about light.

To further the analysis, we should concentrate on the formation of a mathematical equation. For instance, let's take Newton's second law, which is expressed by the equation **F=m.a**, where **m** stands for the mass of a body, **F** for the force exerted upon this body and **a** for the acceleration of this body.[19] This equation tells us that whenever a certain amount of force is exerted upon a body, this body accelerates proportional to its mass. In other words, whenever we exert, let's say, 10 units of force upon a body whose mass is 5 units, we see that this body *moves* with an acceleration of 2 units.

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[18] For the proof, see Fresnel (1822).

[19] The example that I gave may be objected on the ground that it does not account for the unobservable objects like quantum particles. However, mathematical equations relate unobservable objects in no principled way different from that they relate observable objects. Thus, the illustrative aspect of this example is unproblematic.

Accordingly, if we double the force exerted, then we observe that the body moves with a doubled acceleration. Hence, there is a causal relation among these terms since their interaction leads to a certain phenomena, i.e., the determined movement of the body under certain circumstances, and the relevant mathematical equation is meant to represent this relation. The term mass, for instance, then, can be said to be causally efficacious in the producing of the relevant phenomena. Thus, it can be concluded that the mathematical equation **F=m.a** describes the term mass as a causal property of a body in terms of force and acceleration which are causally related to mass by the same equation.

Therefore, as a matter of fact, what the mathematical equations of successful theories describe, in the last analysis, are the causal relations between some causal properties of theoretical entities. By means of relating their causal properties, these equations determine some of the behaviors of theoretical entities under certain circumstances. So, we come to know about how to manipulate theoretical entities in virtue of their causal properties. The required knowledge of these properties in order to manipulate a theoretical entity is derived from the mathematical equations of particular theories. This means that some of the credit has to go to some theoretical descriptions, i.e. the ones acquired by mathematical equations, so as to rescue the central tenet of entity realism which is that the belief in the existence of theoretical entities is construed by the knowledge of their causal properties in virtue of which these entities can be manipulated.

It is, I argue, this construal of entity realism that can play a supplementary role in establishing a more promising version of scientific realism. In the next section, I try to make use of this entrenched form of entity realism to achieve my aim of establishing the relevant position. But, before concluding this section, one last remark has to be put forward concerning the advantageous character of the entrenched form of entity realism. As far as I understood, Hacking hints at the role of theories when he makes a distinction between a real entity and a hypothetical entity. Witness the following quotation:

> Note the complete contrast between electrons and neutral bosons. Nobody can yet manipulate a bunch of neutral bosons, if there are any. Even weak neutral currents are only just emerging from the mists of hypothesis….When might they lose their hypothetical status and become commonplace reality like electrons?—when we use them to investigate something else. (Hacking, 1984, p. 168)

What is hinted in the above sentences is that hypothesizing precedes experiment. To fulfill the explanatory picture of their theories, scientists constantly posit hypothetical entities. For instance, relying on an experiment regarding the manipulation of electrons scientists hypothesize on the produced phenomena in terms of some hypothetical entities like neutral bosons. These putative entities are posited as causal agents to explain the explanation-begging parts in the causal processes of the produced phenomena which cannot be explained by electrons. Scientists try to describe some assumed behaviors of bosons under certain circumstances by means of mathematical equations. Then, relying on these descriptions, they try to build necessary devices to conduct new experiments in order to manipulate these newly attained hypothetical entities. If, in the end of this process, they come up with some new phenomena (even some of these might well be predicted by the hypothesis itself), then it can be concluded that the hypothetical entities of the new hypothesis and some of their causal properties described by the mathematical equations of the hypothesis are real. This is *how* neutral bosons might 'lose their hypothetical status and become commonplace reality like electrons'.

In the next section, I explore the potential contribution of this entrenched form of entity realism to epistemic structural realism.

5.1.2 Epistemic Structural Realism Revisited

As it has been stated previously, ESR claims that there occurs a retention structures through theory change which is the retention of true relations expressed in the mathematical equations of the superseded theory. Accordingly the only knowable part of unobservable reality is the existence of unobservable entities and their true relations expressed in the mathematical equations of the relevant theory. And we know nothing about the intrinsic nature of these unobservable entities. But we have seen that all of the epistemic structuralist realist conceptions assert that the retained structure is the formal structure of a relation "which can easily be obtained with any collection of objects provided there are enough of them, so having the formal structure cannot single out a unique referent for this relation …." (Ladyman, 1998, p. 412). Thus, the purely formal or mathematical structure of relations does not provide us with genuine substantial knowledge of unobservable reality.

In the course of this section, I investigate the availability of substantial knowledge by a reinterpretation of ESR. But, how can this reinterpretation of ESR possibly be made? I argue that it is possible by the help of properly construed entity realism.

Psillos is right when he says that "an appeal to intensions [i.e. intensional understanding of relations] may be enough to answer the Newman challenge only at the price of abandoning pure structuralism …." (Psillos, 2001, p. 18, note 1). I propose that such an intensional understanding is necessary to attain the genuine substantial knowledge of unobservable entities.

Here, I want to make use of a distinction made by Cao (2003) which is the distinction between a mathematical structure and a physical structure. For him, a physical entity cannot be identified with the sum of its structural properties expressed in mathematical equations (Cao, 2003, p. 9). He claims that a physical entity "has its own intrinsic and measurable properties" and "[a]lthough these properties can be defined in mathematical terms, the mathematical structure, as a structure of relational statements, is neutral to the nature of relata and thus cannot exhaust the content of the relata" (Cao, 2003, p. 9). However, Cao declares that the only possible way to attain the intrinsic knowledge of a physical entity, i.e., the knowledge of physical structure of this entity, is through the interpretation of mathematical structure, i.e., relations expressed in mathematical equations, of that entity. He manifests his contention about issue as follows:

> … [E]ither we can dissolve a physical entity into a net of mathematical relations, as Howard Stein (1989) and other mathematics-oriented structuralists or Platonists would try to do; or we can take a net of more and more refined mathematical relations as a means to know the physical entity, as realists would surely try to do. I am in favor of the latter option. (Cao, 2003, p. 9)

But how one can possibly make use of mathematical structure as a means to know the physical entity? I claim that it can be achieved by a proper interpretation of mathematically expressed relations. Psillos makes a distinction between 'relation descriptions' and 'property descriptions' (Psillos, 2001, p. 20). He states the interaction between these two descriptions as follows:

> Although "relation descriptions" don't entail unique "property descriptions," they do offer some information about an object because, generally, they entail *some* of its properties. For instance, from the relation description '*a* is the father of *b*' we can conclude that *a* is male, that *a* is a parent, etc. More interestingly, from relational descriptions about electrons, for example, we can legitimately infer the existence of some first-order properties, namely, negative charge or mass." (Psillos, 2001, p. 20)

The close relation between the relational descriptions and the property descriptions is, then, the most promising way which can lead us to the intrinsic knowledge of unobservable entities. As I presented in 5.1.1, mathematical equations of scientific theories provides us with some of the descriptions of the properties of unobservable entities if these equations are interpreted properly. And I argued that these descriptions are the ones which describe, as Hacking calls them, the causal properties of unobservable entities.

But how can we be sure that these causal properties are the intrinsic properties of unobservable entities. This time, we should have recourse to entity realism. If we are justified in the existence of an unobservable entity by successfully manipulating it, then we are also justified in that the properties which are causally efficacious in the producing of new phenomena are real and, thus, intrinsic properties of the relevant unobservable entity.

Therefore, it is shown that the intrinsic knowledge of unobservable entities can be attained by an appeal to properly construed versions of ESR and entity realism.

5.2 A Viable Realist Position: Substantial Structural Realism

I modestly claim that the position attained above by a proper combination of ESR and entity realism is a viable scientific realist position. I call it ‘substantial structural realism’. I think this position refrains from commitment to the unnecessary theoretical posits of scientific theories which does not survive after rigorous scientific revolutions. It only commits someone to causally efficacious and thus really existent theoretical posits of scientific theories. This aspect of substantial structural realism distances it from full-blown versions of scientific realism which suffers in the face of arguments stemming from scientific revolutions. Moreover, substantial structural realism asserts that more aspects of scientific theories than their purely structural aspects are attainable and knowable which boosts the constrained epistemological and ontological domain of ESR.

However, there remains a problem concerning the UP. Substantial structural realism does not conclusively determine that whether an unobservable entity can be shown to be the totality of its intrinsic causal properties or it is just a substance like entity by which these properties are inhered. Chakravartty claims that we encounter a similar kind of underdetermination 'at the level of everyday objects'. For instance, an ordinary table can be conceived of both as a substance and a totality of its properties. Hence, he concludes that the realist can remain indifferent to such cases of underdetermination (French and Ladyman, 2003, pp. 50-51, note 14).

CHAPTER VI

Conclusion

In this chapter, I summarize what I hopefully have accomplished in the previous chapters of this thesis.

The ultimate aim of this thesis is to defend scientific realism in the face of several challenges against it. However, to attain such an aim, proper modifications should have been made in order to resist relevant challenges.

After giving preliminary developments prior to the initiation of scientific realism debate in chapter 2, I have elaborated the period where the debate becomes sophisticated in chapter 3. A vast amount of arguments both for and against the scientific realism have accrued in this period.

However, arguments against scientific realism especially those stemming from scientific revolutions were hard to resist without a reconsideration of basic contentions of scientific realism. Philosophers of science like John Worrall take this charge of arguments into account very seriously in order to defend a viable version of scientific realism. But, it has been discussed that such attempts turned out to be too restrictive considering the epistemological and ontological aspects of scientific theories.

Hence, I have tried to investigate the conditions of possibility of a entrenched version of scientific realism which can confidently accommodate the epistemological and ontological confines of the full-blown version of scientific realism as far as possible.

After all, I have come up with a position called substantial structural realism which is, in a certain manner, a combination of ESR and entity realism. I argue that this position is the only available version of scientific realism which resists the charge of scientific revolutions while reckoning the most crucial contentions of scientific realism.

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