PRAGMATIC FOUNDATIONS OF ONTIC STRUCTURAL REALISM

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ABSTRACT

PRAGMATIC FOUNDATIONS OF ONTIC
STRUCTURAL REALISM

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This thesis defends Epistemic Structural Realism (ESR) against both Ontic Structural Realism (OSR) and Traditional Scientific Realism (TSR). It is argued that TSR cannot properly explain what actually happens throughout radical theory changes in science; in the sense that a plausible version of Scientific Realism should, somehow, satisfy Scientific Anti-Realists' concerns about the link between "truth" and "success" of our scientific theories. On the other hand, it is claimed that OSR is not a form of Scientific Realism but rather basically a modified form of Pragmatism. To that effect, it is further argued that Modern Physics does not provide convincing reasons to accept the conclusions that advocates of OSR derive from it. It is finally asserted that a Structural Realist understanding of Scientific Explanation is not possible. In that regard, it is argued that a defense of Structural Realism by No Miracle Argument (NMA) against Pessimistic Meta Induction Argument (PMIA) will be effective if and only if the NMA is formulated by the predictive success of scientific theories, rather than constructing it on the explanatory power of them.

Keywords: Scientific Realism, Structural Realism, Partial Structures

Philosophy of Modern Physics, Scientific Explanation

ÖZ

VARLIK BİLİMSEL YAPISAL GERÇEKÇİLİK'İN
FAYDACI TEMELLERİ

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Bu tez Bilgi Bilimsel Yapısal Gerçekçilik'i hem Varlık Bilimsel Yapısal Gerçekçilik'e hem de Geneleksel Bilimsel Gerçekçilik'e karşı savunmaktadır. Bilimsel Gerçekçilik'in kabul edilebilir bir versiyonunun bilimsel kuramlarımızın "doğruluğu" ve "başarısı" arasındaki ilişki ile ilgili Bilimsel Anti-Gerçekçi kaygıları bir şekilde karşılaması gerektiği bağlamında Geleneksel Bilimsel Gerçekçilik'in bilimdeki radikal kuram değişiklikleri boyunca tam olarak ne gerçekleştiğini açıklayamadığı iddia edilmiştir. Diğer yandan, Varlık Bilimsel Yapısal Gerçekçilik'in bir Bilimsel Gerçekçilik türü değil, temelde Faydacılık'ın yeniden düzenlenmiş bir çeşidi olduğu ileri sürülmüştür. Bu bağlamda, Varlık Bilimsel Yapısal Gerçekçilik savunucularının çağdaş fizikten çıkardıkları sonuçları kabul etmek için çağdaş fiziğin ikna edici sebepler ortaya koymadığı iddia edilmiştir. Son olarak, Yapısal Gerçekçi bir Bilimsel Açıklama anlayışının mümkün olmadığı ileri sürülmüştür. Bu bağlamda, Yapısal Gerçekçilik'in Mucizeye Geçit Yok Argumanı ile Karamsar Meta İndüksiyonu Argumanı'na karşı etkili bir biçimde savunulabilmesinin, Mucizeye Geçit Yok Argumanı'nın bilimsel kuramlarımızın açıklayıcı güçleri üzerine kurulmasıyla değil, ancak bu argumanın bilimsel kuramlarımızın öngörü başarılarıyla formüle edilmesiyle mümkün olacağı ileri sürülmüştür.

Anahtar Kelimeler: Bilimsel Gerçekçilik, Yapısal Gerçekçilik, Kısmi Yapılar,
Çağdaş Fizik Felsefesi, Bilimsel Açıklama

To the memory of my much-missed Father

“Armut dibine düştü...”

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INTRODUCTION

The debate between Realism and Empiricism has been at the focus of the philosophy of science for centuries. Within the recent realm of the state of art, realist argument asserts roughly that our best, and most mature high level scientific theories, which refer to unobservable entities, give us, at least approximate, knowledge about the external world. Notice that the argument is an *epistemological* one. There is the mind-independent external world (this is the ontological part of the claim) and we somehow know it. On the other hand, empiricism asserts that we do not have this mentioned epistemic access to the external world. As for the ontology, some empiricists accept the existence of mind independent world; but they deny that our high level scientific theories are able to refer to this external realm. Van Fraassen, for instance, accepts the existence of the electrons, but he denies that we have the true knowledge of them, even not that of approximate truth. According to van Fraassen's constructive empiricism

> …theories which putatively refer to unobservable entities are to be taken literally as assertoric and truth-apt claims about the world (in particular, this includes existence claims about unobservable entities)". The part of realism that van Fraassen rejects is the idea that "acceptance of … theories (or at least best of them) commit one to belief in their truth or approximate truth in the correspondence sense (in particular, to belief that tokens of the types postulated by the theories in fact exist)…. Instead he argues that acceptance of the best theories in modern science does not require belief in the entities postulated by them, and that the nature and success of modern science relative to its aims can be understood without invoking the existence of such entities. Hence, his constructive empiricism is fundamentally a view about the aims of science and the nature of "acceptance" of scientific theories, rather than a view about whether electrons and the like exist. (in Ladyman, 2000, pp 838-839)

Therefore, the disagreement between realism and empiricism in the contemporary philosophy of science occurs as an epistemological controversy; whereas the ontological commitments of both positions might be similar. As regard to the more recent focus within the dichotomy between realism and empiricism, the long run contention between “No Miracle (NMA)” and “Pessimistic Meta Induction (PMIA)” arguments has recently been reformulated under the shed of the “Structural Realism” debate. In the present thesis, I defend Worrall’s epistemic form of Structural Realism (ESR) against both Psillos’ Traditional Scientific Realism and Ladyman’s ontic form of Structural Realism (OSR). I argue that Psillos’ account cannot capture what actually happens throughout theory changes in the history of science; claiming that a plausible form of realism should, somehow, satisfy Scientific Anti-Realists’ concerns about the link between “truth” and “success” of our scientific theories. On the other hand, as the main claim of the thesis, I assert that Ladyman’s OSR is not a form of Scientific Realism but rather it is basically a modified form of Pragmatism. Along this line of argument, I investigate some important notions in OSR such as partial isomorphism, partial structures, and pragmatic truth; and criticize the position arguing that the introduction of such notions does not provide any advantage for scientific realism over anti-realist arguments. I particularly search for the answer for this specific question: can semantic approach to scientific theories (when it is based on the notion of pragmatic truth, especially as it is formulated by French and his co-workers) be used to establish a solid background for scientific realism? I give a negative answer for this question under the shed of related arguments of Otavio Bueno, supporting his argument that shifting from syntactic to the semantic approach might be useful for an empiricist setting of theory change in the history of science, but does not provide any advantage to scientific realism over empiricism. Then, I will explore advocate of OSR’s argument that modern physics provides very strong reasons to support the elimination of the objects from the ontological realm; in the sense that the structure has more fundamental ontological status and objects do not supervene on it. I will be dealing especially with their arguments coming from identity and individuality problems in quantum field theory (QFT). I will accept that individual objects might have very severe identity problems in QFT; however, this does not give us enough reason to deny the existence of sub atomic particles, or fields, etc. Such an argument might be formulated as a pragmatic claim; arguing that formulating the ultimate underlying stuff of the universe in structural terms, rather than formulating them in the traditional object language, might be *more useful*, or in that way *our scientific theories works better*; however, such claims should remain only pragmatic concerns and cannot be formulated as realist claims. At the end of the chapter, I will mention about some recent developments in quantum information theory (QIT), arguing that the idea that there is just structures all the way down is deeply problematic given the concept of "entanglement" in most recent QIT; since entanglement might be taken to be the most basic of physical ontology and the elimination of it does not seem to as easy as the elimination of objects in QFT, and hence the identity and individuality problems of subatomic particles in QFT does not emerge in QIT. In the last chapter, I will touch upon to the "scientific explanation" issue from a structural realist point of view. In this last chapter, I will be dealing with the question of whether a structural realist defense of scientific explanation is possible. In a certain respect, most accounts of explanation are tailored to realist views about science. In some cases, they invoke laws of nature, which are taken to be true (as in the case in the Deductive-Nomological account of explanation), or the accounts of explanation invoke relevant causal relations among the objects under consideration (as in the case in causal accounts). I will try to challenge this historical dogma and argue for the idea that a traditional realist defense of scientific explanation is not possible on the face of PMIA. Then the question to be replied is whether we can give a structural, as opposed to traditional, realist account of scientific explanation despite the PMIA. The answer for this question will be a negative one. But I will not stop there and will further claim that if we do not attribute above mentioned historical importance to the explanation within the scientific realism debate, we might still claim that structural realism is a plausible position, constructing the argument on the novel predictive success of our scientific theories.

CHAPTER 1

THE PLACE OF STRUCTURAL REALISM WITHIN THE TRADITIONAL DEBATE

1.1 Pessimistic Meta Induction Argument vs. No Miracle Argument

Put roughly, NMA asserts that the success, especially predictive ones, of our scientific theories cannot be explained without accepting the fact that they do refer to the external world and have definite truth values. As a response, PMIA claims that, although our past theories were able to make some true predictions, they have been falsified[1] anyway. Therefore, predictive or explanatory success of our scientific theories does not have anything to do with truth of them. So, in order to defend realism against instrumentalism, or empiricism, we have to find some further arguments. The burden of proof, by the way, is at realist's side; meaning that realists should prove that our scientific theories have some further purposes than simply saving the phenomena", such as "truth" of the theory. Realists, therefore, should provide some extra reasons beside the success of scientific theories in order to strengthen their position. NMA has been formulated as such as extra reason in the historical realm by realists, and PMIA has been formulated to response this realist move from an instrumentalist point of view. Let me give an example of predictive success of a theory, at this point, to illustrate the controversy between NMA and PMIA more suitably.

[1] This claim, certainly, is not an anti-realist one since scientific theories cannot have truth values, hence they cannot be false according to scientific anti-realism. However, it demonstrates a contradiction we would face if we accept scientific realism: if we accept that truth can be attributed to scientific theories, then we are left with a contradiction since, from a realist point of view, all successfull scientific theories in the history of science has been falsified anyway.

In 1919, Sir Arthur Eddington and his research group made an observation about a prediction of Einstein's General Theory of Relativity (GTR). The observation was of a total solar eclipse on 29 May 1919, and was carried out by two expeditions. The aim for each of them was to measure the gravitational deflection of the light coming from a star behind the Sun. The value of this deflection had been predicted by Einstein in one of his earlier papers in 1911. Theory predicted that starlight passing near the Sun would be deflected due to the gravitational effect of the sun; for the theory says that massive objects such as Sun or Earth curves the spacetime and shapes geodesics accordingly. The starlight was expected to follow these geodesic and the amount of the deflection was calculated before the exposition. The observed results were exactly same with Einstein's expectation[2].

The Advocate of NMA uses this predictive success of Einstein's theory as a weapon against PMIA within the realism – empiricism war. If Einstein's theory, say, were substantially successful when predicting the deflection of the light coming from the star behind the sun, then we would, it seems, be forced to believe that the theory cannot just happens to be correct in predicting effects like that of the deflection; otherwise, we would be forced to accept that the theory's success in this prediction was a mere coincidence or 'miracle'. However, within a truly scientific inquiry, we should not accept that miracles have happened, at any rate, if we have an alternative non-miraculous explanation on our hand. And in this case the assumption that Einstein's theory itself is correct or approximately correct is exactly such a non-miraculous alternative explanation of its predictive success. Therefore, the acceptable assumption is that Einstein's theory is indeed (at least approximately) correct.

Advocate of PMIA, on the other hand, might still argue that she has good reasons to deny the apparent plausibility of NMA in Einstein's case. According

[2] As a historical fact, results were, within error margins, compatible with Newtonian physics at the beginning. Later on, further and more accurate experiments have proven that the results were in accordance with GTR's prediction.

to GTR, the spacetime is a kind of concrete manifold curved by massive objects such as sun. This curvature called geodesics and when explaining the deflection of the light coming from the star behind the sun, GTR claims that the deflection occurs due to the fact that the light follows so called geodesics bent by massive objects. In quantum theories, on the other hand, spacetime is not such a manifold that can be curved by such massive objects; hence, the deflection of the light cannot be due to the bending of geodesics. It was a success for Einstein's theory to predict the behavior of the starlight passing near the sun, but since the most fundamental concepts such as the structure of spacetime has been replaced by later theories to some extent, and the deflection cannot be due to the nature of spacetime itself in quantum mechanics, we cannot assert that the predictive success of earlier theory guaranties the truth of it. In other words, this success was due to some other factors then the truth of the theory[3].

1.2 Laudan's Critique of Convergent Epistemological Realism

On the other hand, if realist can show that some part of the falsified theory has been preserved within the new theory after the scientific revolution, or the theory change occurred, we can still satisfy the basic reference or correspondence claim of the realism argument, appealing to this retained part. In that way, realist can answer to the advocate of PMIA by stating that falsification of the older theory is about a certain part of it; however some other "true" part of the theory was inherited within the new theory and this inherited part was responsible for the predictive success of the earlier theory. Therefore, we can meet the concerns of PMIA and explain the success of the science in a non-

[3] I am not saying here that the GTR has been falsified by Quantum Mechanics. However, the nature of spacetime in GTR and QM is fundamentally different, and after QM we cannot aseert that, as a realist claim, the deflection of light is due to the bent geodesics; for massive objects do not bend spacetime and shape geodesics accordingly in QM. Therefore, the earlier predictive success of GTR does not seem to be used to support NMA.

miraculous manner. In the following remarks by Laudan, it is obvious that an anti-realist might be ready to accept realist epistemology, if realist can provide some further reasons about why we should go beyond the empirical adequacy and look for whether our scientific theories refer.

> *Nothing* I have said here refutes the possibility in principle of a realistic epistemology of science. To conclude as much would be to fall prey to the same inferential prematurity with which many realists have rejected in principle the possibility of explaining science in a non-realist way. My task here is, rather, that of reminding ourselves that there *is* a difference between wanting to believe something and having good reasons for believing it. All of us would like realism to be true; we would like to think that science works because it has got a grip on how things really are. But such claims have yet to be made out. Given the *present* state of the art, it can only be wish fulfillment that gives rise to the claim that realism, and realism alone, explains why science works. (Laudan, 1981, p. 48)

The problem, therefore, is about being able to give some plausible arguments that we have good reasons in favor of the idea that theoretical entities in our high level theories in deed refer to the reality. As he explicitly claims at the end of the above quote, Laudan does not think that we have enough reasons to accept such realist claims. He understands scientific realist concerns that lead to the NMA as follows: "The empirical success of science (in the sense of giving detailed explanations and accurate predictions) accordingly provides striking empirical confirmation for realism." (Laudan, 1981, 21), and calls this position as "Convergent Epistemological Realism (CER)". At a first glance, Laudan is curies about what can the term success mean in that regard. He mentions two possibilities concerning what a scientific realist could mean when he uses the term. On the one hand, when he uses the term "success", scientific realist can be claiming that the scientific theory *works* well, which is, of course a *pragmatic* claim, for the success of the theory is explained by its being useful. On the other hand, if the realist is referring something more than mere pragmatic concerns such as the theory's being "true", or if he derives some further conclusion "(such as those advocated by inductive logicians or Popperians) then it would probably turn out that science has been largely ‘unsuccessful’ (because it does not have high confirmation)” (Laudan, 1981, 23). What Laudan means here when he asserts that science has been unsuccessful, as I understand it, is the claim that regardless of the predictive or explanatory power that theories have enjoyed in the past, we gave up to regard all those theories as “true” ones. Therefore, this pessimistic induction from the fates of past theories in the historical process implies that we have quite enough reason to accept the idea that we will, soon or late, give up accepting our current theories as true.

Thus, according to Laudan, scientific realists either base their argument on pragmatic concerns, or, if not, if they intended to offer something more than mere pragmatic claims, their realist argument cannot provide a refutation of PMIA. Notice, by the way, that if scientific realists favor pragmatic approach, then Laudan is happy with this. In so far as scientific realist does not claim that our scientific theories are “true”, at least approximately, and admit that we accept them due to some pragmatic concerns, then Laudan agrees with her that our best scientific theories are highly *useful* and they *work* quite well.

1.3 Worrall’s Structuralist Suggestion

Worrall accepts Laudan’s criticisms against NMA and asserts that this argument cannot be defended as a realist position against PMIA in a traditional fashion. Success of science cannot be used as an argument for scientific realism, according to Worrall. It merely has some pragmatic plausibility. It might be argued that science’s success cannot be a miracle, but if all successful scientific theories in the past have been falsified, then we cannot construct the relation between theories’ success and their being truth, for it is an obvious fact that successful theories have always turned out to be false. Worrall says,

> …in the case of realism's explanation of the success of our current theories there can of course be no question of any independent tests. Scientific realism can surely not be inferred in any interesting sense from science's success. The "no miracles" argument cannot establish scientific realism; the claim is only that, other 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, p102)

Worrall also does not accept the idea that scientific realism can be defended against anti-realist concerns by appealing to "approximate truth". In other words, we cannot argue that successor theories in scientific change are just an extended form of predecessor ones. Worrall is quite obvious at that point and his example is the shift from Newton's to Einstein's physics: "it is not the case that Einstein's theory is simply an extension of Newton's. The two theories are logically inconsistent: if Einstein's theory is true, then Newton's has to be false." (Worrall, 1989, p.104)

Having explained all his concerns about defending scientific realism against anti-realism from a traditional point of view, Worrall goes on and offers positive part of his realist argument. According to Worrall, it is true that there is an ontological discontinuity throughout theory changes in science, such as the ontological discontinuity about the nature of light. For the light has been conceived as corpuscular before Fresnel, it has been conceived as something consisting of vibratory motion in ether in Fresnel's theory, and as an electromagnetic wave in Maxwell's theory. However, this discontinuity is not sufficient to infer the conclusions that anti-realists derive from this situation. There is the discontinuity in the ontological, or the empirical level, but if we look at the formal or structural part of theories, we will see that there is continuity there. And this continuity prevents us from making anti-realist conclusions on the base of PMIA. Worrall asserts that

> [t]here was an important element of continuity in the shift from Fresnel to Maxwell and this was much more than a simple question of carrying over the successful *empirical* content into new theory… There was continuity or

> accumulation in the shift, but the continuity is one of *form* or *structure*, not of content." (Worrall, 1989, p.117)

To sum up, Worrall appreciates the power of PMIA, unlike most of the standard, or traditional, realists, and asserts that realism cannot be defended against this argument unless it is reformulated in a non-traditional way. His suggestion is about to commit ourselves to the structure of the theory and be silent about the ontological realm. He thought that, only in that way, we could give a realist account for the historical fact of the ontological discontinuity that establishes the main ground for PMIA. However, some realists were not satisfied with such a realist defense, claiming that ontology cannot be a forbidden area for a truly realist understanding of the scientific inquiry. Opponents have been split in to two camps: one is defending traditional scientific realism against Worrall, claiming that the structure – nature distinction cannot be made since they demonstrate a continuum (Psillos, 1995, p.31). Other camp, taking Psillos' criticisms seriously, argued that if it is formulated along the way Worrall does, structural realism cannot be defended both against instrumentalism and standard realism. Instead, if we could give an account of scientific change in a structural ontologist manner, destroying the nature – structure dichotomy by effectively eliminating the nature part, on the base of some quantum mechanical concerns (especially those coming from QFT), then we can talk about a kind of structural realism that can face criticisms coming from both instrumentalism and standard realism (Ladyman, 1998). Ladyman made a distinction between his own and Worrall's way of formulating structural realism and called Worrall's position as Epistemological Structural Realism (ESR) and defended his own version of Ontic Structural Realism (OSR). Later on, Ladyman and French suggested establishing this structuralist ontology on to the model theoretical or semantic approach arguing that semantic approach itself gives an importance to the structure of scientific theories (Ladyman, 1998 and, French and Ladyman, 1999). They formulated the notion of "partial truth" in order to be able to give an account of the radical theory changes in science, within this semantic –

structuralist ontology. I will investigate the details of the semantic – structuralist ontology suggested by French and Ladyman in the third chapter; however, let's see in the next chapter what are the fundamental characteristics of traditional and ontic structural realists' criticisms to Worrall's position.

CHAPTER 2

PSILLOS AND LADYMAN AGAINST WORRALL

2.1 Psillos' Traditional Realist Critique of Structural Realism

Making a distinction between empirical and formal part of a theory is problematic (empirical part is the ontological part of the theory and formal part is the structural part, in Worrall's way of formulating the issue), according to Psillos, since these two realms demonstrate a continuum (Psillos, 1995, p.31). Even if the distinction is made, a structural realist should show two things; first, the formal part, not the empirical content, of the theory should be responsible about the continuity within the theory change; second, the structural realist should explain what kind of relation exactly is the relation between formal and empirical part of the theory and how these two parts connect to each other. The answer for the first question why the formal part but not the empirical part is retained in theory change has already been provided, I believe, in Worrall's above mentioned argument. There should be continuity throughout radical theory changes in the history of science; otherwise success of our theories would be miracle. This continuity cannot be due to empirical content given the ontological discontinuity; for it is clear in the example of the ontological status of the light where the answer for the question "what is light?" has three different answers in all three different theories. Therefore, as the only left option, the continuity should be due to the formal or structural part of the theory.

As for the second criticism of Psillos that Worrall should make it clear exactly how empirical part is connected to formal part in a scientific theory, I will question this criticism by investigating whether asking this question to Worrall would be legitimate.

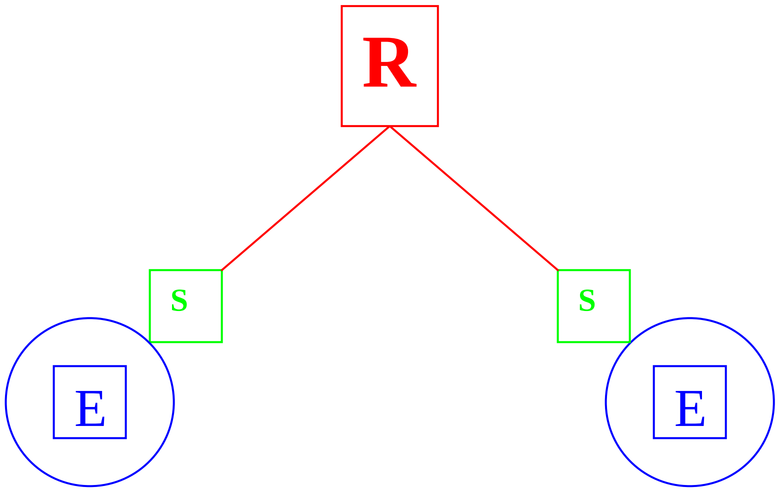

Figure 1 – Two electrons and the R relation between, and supervening on, them.

In figure-1 above, there is a physical system where E represents one particular electron, R is the relation between the two electrons, and S is the supervening relation which makes possible R relation to supervene on E. Worrall, as we have seen, does not accept the idea that we can know the ontological commitments of our theories, which are the blueprint of reality. In our figure, this means we cannot know E part of our theory. We have a cognitive closure with respect to E part of the theory and this cognitive closure has let to concerns that anti-realists have. As for the R part, we have a cognitive access to this part of our theories and this part enables us to know the structure of the external world. This cognitive access enables us to assert that there is continuity across theory changes and on the base of this continuity we can explain both why predictive power of our theories cannot be miracle and why anti-realists conclusions are exaggerated.

When Psillos asks the question exactly how formal and empirical part of the theory is connected to each other, he is asking in our figure how R supervenes on E. In other words, Psillos is asking about the nature of S relation. This question, I claim, cannot be asked to Worrall, given his admission that we have a cognitive closure to E part. Notice that S relation has two parts, namely E and relation R. Therefore, in order to be able to explain S relation properly, we should have clear cognitive access to both E and R part. However, we do not have this cognitive access, according to Worrall, to the E part. It seems to me that Psillos is asking about something that is already admitted by Worrall; namely that we have a cognitive closure to E and hence we cannot state S relation properly.

2.2 Ladyman's Distinction between Ontic and Epistemic Forms of Structural Realism

As we have seen in the previous chapter, we should give up talking about the blueprint of the universe, according to ESR of Worrall. Ladyman criticizes Worrall at this point. According to Ladyman, ontology cannot be a restricted area for a genuine form of scientific realism. He thinks that Psillos's criticisms against Worrall's are plausible and asserts that structural realism cannot be defended against the criticisms of traditional scientific realism in so far as it is provided in an epistemological form. According to Ladyman, it is even not clear whether Worrall's position is an epistemic one or not. He says

> [t]here is fundamental question about the nature of structural realism that should be answered: is it metaphysics or epistemology? Worrall's paper is ambiguous in this respect…
>
> On the other hand, Worrall's position is not explicitly an epistemic one. (Ladyman, 1998, p.410)

Unless a scientific realist account form a continuum between formal and empirical part of a theory, asserts Ladyman following Psillos, it cannot have any advantage over scientific anti-realist arguments. Because the problem of ontological discontinuity, which is used as an important refutation of scientific realism, is left untouched since Worrall's position cannot establish a continuum between formal and ontological part of the theory. Therefore, Worrall's ESR cannot be defended against both anti-realist ontological discontinuity argument and traditional realist concerns that it cannot make a link between empirical and ontological part of our scientific theories. As an attempt to make Structural Realism much more defensible position against such criticism, Ladyman asserts:

> [t]o be an alternative to both traditional realism and instrumentalism, structural realism must incorporate epistemic commitment to more than the empirical content of a scientific theory, namely to the 'structure' of the theory, while stopping short of realists' commitment to the full ontology postulated by the theory. (Ladyman, 1998, p. 415)

Before I go on to explain what Ladyman offers in order to make Structural Realism much stronger, I want to ask whether his critique of Worrall is a legitimate one. In his argument, as I understand it, Worrall is not intended to give an account of ontological discontinuity. He basically accepts that such a discontinuity exists and a realist account of science cannot just ignore this fact; concerns emerged from the close investigation of the ontological discontinuity between Fresnel's and Maxwell's theories of light. Actually, this is exactly the reason why he asserts that scientific realism cannot be defended against scientific anti-realism by using the arguments of traditional scientific realism. Having admitted this, is it fair to ask to Worrall how to establish first, the continuity between empirical and ontological parts of a theory and second, the ontological continuity throughout theory changes? From Worrall's point of view, second continuity cannot be shown since he already admitted that there is a discontinuity at that level. As for the first continuity, namely the continuity between empirical and ontological part of the theory, revealing such continuity would be rejecting scientific realism from Worrall's point of view given his admission that there is the ontological discontinuity across theory changes. Because if there is a continuum between these two parts of the theory and there is a discontinuity across theory changes in one of these parts, namely in the ontological part, then there should be a radical discontinuity in empirical part, too; due to the continuum between empirical and ontological part of the theory. Therefore, this would be accepting that there is a radical change both at empirical and ontological level, and this would obviously lead us to anti-realism since we are not able to show any continuity across theory change both at empirical and ontological level. I believe, under the shed of mentioned concerns, that Worrall's account cannot establish a continuum between the empirical and ontological level of the theory not only because it is not able to do so, but also since doing so would lead us to reject the very foundational claim of realism, namely that there is progress in science.

However, there is one option left, in order to be able to establish both a continuity between the ontological and empirical part of a theory and at the same time give an account of ontological discontinuity across theory changes in the history of science. This can be achieved only by absolute elimination of the difference between structural and ontological part of a theory. And this difference can be eliminated by asserting that structures are all there is to be. This is what Ladyman does as an attempt to defend Structural Realism against the critiques coming from both traditional realists and anti-realists. Worrall is right, Ladyman asserts, when he claims that there is a structural continuity across theory change, however, unlike Worrall, he asserts that this is the end of the story: since there is no distinction between ontological and empirical level of a theory, revealing continuity at the structural level guarantees that there is continuity as regard to whole theory.

Two points here. First, remember from the first chapter where we have seen Laudan's pragmatic critique of scientific realism that any scientific realist claim should be necessarily an anti-pragmatic one, since realism's claim is that the theory refers to the nature; not only because it is useful, it works well, etc. When Ladyman asserts that structure is the only thing for there to *be*, if his position is a realist one, this claim should mean there is *really* nothing except structure in the nature all the way down. If this claim is a pragmatic one, on the other hand, asserting, for instance, his metaphysics works better than other possible metaphysics merely because it gives answers to criticisms coming from both traditional realists and anti-realists without claiming anything about whether theories in his account *actually refers* to the nature, then his position is not a realist one obviously. Therefore, when he asserts that there is nothing except structures in the nature, this claim should be taken literally. However, Ladyman is not clear whether his argument is a realist one. He says his argument has a realist perspective; but at the same time he makes some contradictory, or at least vague, claims that give his argument a pragmatic fashion. For instance, here is Ladyman's response to the people who criticize his idea that there is only structure all the way down, since it seems to be implying that structure does not supervene on any object in the traditional sense of the word "object"; which means, as I understand it, we can talk about relations without relata.

> This objection has no force against… [OSR], [since]…the claim that relata are constructed as abstractions from relations does not imply that there are no relata; rather the opposite. A core aspect of the claim that relations are logically prior to relata is that the relata of a given relation always turn out to be relational structures themselves on further analysis. (Ladyman and Ross, 2007, p.155)

It might be the case that every time we try to understand relata, say for instance sub atomic particles in QFT, this investigation might be ended up relational structures; however, this situation does not give enough reason to a realist to deny the existence of the relata. The situation might be well explained by reference to the limit of our knowledge as human beings: we can say that when we investigate the ultimate stuff of the universe, the highest stage that we could reach is the structural relations of the investigated systems; the inner ontological characteristic of nature might be unknowable for us, due to our limitations. This attitude is very similar to Worrall's position and it is still a realist, though silent about the blueprint, since we have the structural knowledge of the investigated system. If Ladyman claims that his position provides a pragmatist account for the situation, then we might accept it; because it might be *useful* to accept structures, rather than objects, as the ontological objects for some practical purposes; for the sake of making more predictions or explaining more phenomena with the theory, for instance. However, Ladyman insistently argue that his position is a realist one. I cannot understand how Ladyman can be a realist after he expresses the ideas in the above quotation; for, in my opinion, these claims are contradictory with any realist claim. Ladyman and French says that in order to be able to understand why their claims in the above quote does not contradict with realism, one should look at some phenomenon in modern physics, especially in QFT. I will discuss whether QFT does provide such an eliminativist vision in the forth chapter.

As the second point, in order to make his position more tenable than Worrall's account, Ladyman should show that any possible future metaphysics will always come with an ontology where there is nothing except structure. He might infer something from the ontology of QFT; however, if there is the possibility that one of the possible future ontologies come with a different ontology than that of QFT, then we are again face to the anti-realist argument of ontological discontinuity; in which case Ladyman's account would not have any advantage over Worrall's Structural Realism. In the forth chapter, again, I will talk about such a possibility, appealing a comparison of QFT and some recent conceptual developments in QIT. But, for the time being, let's look at how Ladyman and French combine their structuralist ontology with semantic approach to the scientific theories.

CHAPTER 3

ONTIC STRUCTURAL REALISM AND SEMANTIC APPROACH

3.1 Semantic Approach vs. Ramsey Sentences

One apparent characteristic of French and Ladyman's ontic structural realist argument is the fact that they have based their peculiar structuralist ontology on to the model theoretical or semantic approach (French and Ladyman, 1999). Additionally, they formulated the notion of "partial truth" in order to be able to give an account of theory change in science, within this semantic – structuralist ontology. In this chapter, I will investigate how Ladyman and French introduced model theoretic approach into the structural realism argument and how Bueno criticized their partial truth approach by unveiling that in fact this approach is not a realist move.

According to Semantic, or Model Theoretic conception of theories, theories are not merely propositions of first order logic; rather, they are models of the phenomena investigated. Let me emphasize right at the beginning that the claim is that models in science represent the empirical phenomena. Whether our models can represent nature as it is, or they can solely represent the appearances of nature is a vital question within model theoretic approach and not everybody accepts that theory can model the nature as it is. Van Fraassen, one of the pioneers of the model theoretic approach, for instance, denies that our theories are capable of representing the external world as it is.

As we have seen in the last chapter, Ladyman advocates an eliminativist attitude towards the ontological commitments of our theories and asserts that there is nothing except structure in the nature, in the sense that everything else is derivative from it. While doing so, Ladyman emphasizes the importance of semantic conceptions of theories because “the semantic approach itself contains an emphasis on structures.” (Ladyman, 1998, p.416) Ladyman criticizes Ramsey Sentences approach[4] , which is used as a logical tool to represent our theories by the advocates of Syntactic approach, and makes this critique under the shed of Friedman- Demopoulos argument that given enough cardinality, there can be more than one structure satisfying a particular, unique Ramsey sentences, in which case the question becomes not about whether our theories can represent phenomenon, but becomes about the discovery of different cardinalities (in Ladyman, 1998, p.412). Notice, by the way, that the argument is neutral to the question whether our theories can capture the nature as it is, or they solely represent the phenomena. It only states that there can be more than one structure for a particular Ramsey Sentence given specific conditions about cardinality. Therefore, if we look at to the issue from Laudan’s point of view, for instance, Laudan would have no problem to shift from Syntactic approach to the Semantic approach, since the shift has no realist implication. We have shifted from one approach to the other merely because the letter one is *pragmatically* more plausible.

3.2 Bueno on Partial Structures: Constructive Empiricism vs. Structural Realism

This pragmatic character of French and Ladyman’s model theoretic partial truth approach has been made explicit by Bueno. In one of his papers (Bueno, 1997), Bueno suggests extending van Fraassen’s constructive empiricism, particularly the notion of empirical adequacy, using more recent concepts such us partial truth and partial isomorphism, which are introduced by

[4] Ainsworth (2009) provides a terrific historical summary of Ramsey Sentence approach and its critics, and argues that Newman’s objection is still a powerful argument against it.

French and da Costa. Let me make it clear right at the beginning that Bueno has empiricist tendencies and he clearly expresses this inclination in his paper. Therefore, his aim is not anyway to provide any argument in favor of realism; he only tries to overcome some problems of empiricism by using "partial isomorphism" formulated by French and da Costa. He does not give any argument against realism and about why partial truth cannot be used as a realist tool in order to rescue semantic – structuralist ontology in (Bueno, 1997), however, he does this in (Bueno, 2008), and I will come to this point through the end of this chapter. So, this section investigates what kind of problems of empiricism can the introduction of the partial truth solve, within the empiricist tradition, and whether it can achieve this aim or not.

The problem of empiricism that Bueno deals with in his paper is so called Cartwright challenge. According to Cartwright there are two kinds of laws: fundamental laws and phenomenological laws.

> On the one hand, *fundamental laws* of physics are applied only to some *theoretical models* (and not to the world); these models are but idealized constructions that represent some aspects of the phenomena to be explained. On the other hand, phenomenological laws (obtained basically by experimental means) make the connection between theoretical models and appearances. Thus, there are two basic levels of consideration: fundamental laws (which *explain*, but due to their high degree of abstraction, are strictly *false*) and phenomenological laws (which have a more *specific* character, being truer than the latter, but unfortunately *do not explain)*. Between these two levels, one finds the theoretical models responsible for supplying the interplay between them. (in Bueno, 1997, p. 586)

Cartwright challenge, then, might be formulated, roughly, as follow: in theory construction a more central role is played by phenomenological laws than theoretical laws. On the other hand, due to high the level of idealizations within the current interpretations of science, the importance of phenomenological laws is less emphasized. Thus, "because they [the extant interpretations of science] do not take account of the role of the phenomenological laws…the extant interpretations of science (and, in particular, the semantic conception of it) do not seem to accommodate a faithful description of scientific practice." (in Bueno, 1997, p.587) So, how can we close such a gap between theoretical and phenomenological laws?

I won't be formulating the technical framework here, but I will try to make clear the problem in an informal setting. In order to be able to characterize *empirical adequacy* of a theory, van Fraassen defines a tree-part relation between theoretical model T of that theory; empirical substructures of T, E; and appearances A. Now, according to van Fraassen, a theory is *empirically adequate* if for some of its theoretical models T the appearances A are *embeddable* in those models: meaning that there is a bijection of E on to A; in other words, if there is a particular *isomorphism* between E and A (in Bueno, 1997, p.587). According to Bueno, there might be some counterexamples to such a formulation coming from contemporary physics. Bueno asserts that Suarez provides such a counterexample where it seems the case that although the data (it means appearance A, in van Fraassen's above formulation) is not embeddable into one of its theoretical models, it might still be possible for a theory to be empirically adequate (in Bueno, 1997, p.591). According to Bueno, Suarez's criticism is a particular version of Cartwright challenge: it is a kind of a demonstration of incongruence between theoretical models and appearances, since it asserts that a theory might be empirically adequate although data (appearances, it means here) is not embeddable in to T.

After introducing notions such as partial structures and pragmatic truth, I will later on turn back to how Bueno responses to Cartwright challenge, using these notions, within an empirical setting.

The main motive of French and da Costa is the fact that our knowledge is incomplete in any given particular state of science. Rather than talking about truth of our scientific theories, French and da Costa suggest considering partial truth of them, in order to be able to give an account of scientific inquiry which is compatible with its incomplete nature. Recall from above given van Fraassen's formulation, empirical adequacy is given in a triple relation between models of theory T, empirical substructures of that model E, and appearances A. In that formulation, there are also relations holding between the elements of the domain of the theory. French and da Costa split these relations into three realms where the first set is being identified with n-tuples that satisfy these relations, second set with n-tuples that do not satisfy these relations, and third set with n-tuples that are not defined whether or not they satisfy these relations. In virtue of this latter set, these n-place relations holding between the elements of the specified domain are *partial* (in Bueno, 1997, p.591). And *partial structure* S is based on the domain D and above mentioned partial relations. Before introducing the notion of partial truth, on given ground, da Costa and French give the definition of S- normal structures. "Basically, this is a structure that extends the partial relations…in S to normal ones (its n-place relations are defined for all n-tuples of elements of its domain)" (in Bueno, 1997, p.592). Now we can define partial truth on the base of these S-normal structures (S-normal structures is denoted by "B" in the paper): "a sentence 'a' is pragmatically true in a partial structure S if there is some S-normal (total) structure B in which a is true" (in Bueno, 1997, p.592).

Recall that the problem that French and da Costa were dealing with was how to give an account of the incomplete nature of the knowledge in a particular stage of science. By introducing the notion of partial truth, they somehow relate partial relations (those are being incomplete relations, in a sense) to total relations (and those are being complete relations) within the specified domain. Therefore, roughly speaking, by making the bridge between incomplete and complete relations, da Costa and French claim to give an account of theory change in scientific inquiry.

Bueno is critical at that point. He asserts that there might be some cases where it is impossible to extend a given partial structure into a total one. Based on the work of Mikenberg (1986) and de Souza, Bueno explains what these philosophers suggests to close the gap between partial and total structures by adding "set P of accepted (observational and theoretical) propositions, representing some aspects of what is known within a particular scientific field up to certain moment." (Bueno, 1997, p.592) In that formulation, therefore, it is assumed that the partial structure S consists of domain, a set of partial relations defined on this domain, and the set of P.

Bueno sees some further problems here, even if we add the set of P. However, he asserts that da Costa and French's formulation of partial structures and pragmatic truth can be used to extend the notion of empirical adequacy in constructive empiricism such that, in that way constructive empiricism might be successfully defended against Cartwright challenge. He formulates partial embedding and partial empirical adequacy in the following way:

> …it is then straightforward to formulate a notion of *partial embedding* and then of *partial empirical adequacy*. We say that a partial structure A is partially embeddable in a structure B if there is a partial substructure C of B such that C is partially isomorphic to A (where the notion of partial substructure is of course just the usual notion of substructure; restricted to partial structures)….A theory T (thought of, in conformity to the semantic view, as a family of partial structures) is *partially empirically adequate* if for some of its models there is a partial isomorphism holding between all the models of phenomena (conceived as partial structures) and the partial empirical substructures of the model – that is, if the appearances are partially embeddable in the theory. (Bueno, 1997, p.596)

The link between appearances and theory, or the link between phenomenological and theoretical laws in the language that Cartwright is using, therefore, is established by introducing the notions of partial embedding and partial empirical adequacy within a framework which is quite in conformity with semantic approach. This is a very nice and useful achievement for an empiricist. But what all those intense technical terminology and arguments in above mentioned works mean for a realist? Straightforwardly, I claim that they make no sense; and I give my argument in favor of why this is so in the following claims.

3.3 Ontic Structural Realism is not a Realist Project

As we have seen in the previous chapters, the main motive of Scientific Realism is to give a plausible account for how theoretical entities in our scientific theories refer. In other words, a realist tries to make a bridge between our high level theories and the external world. However, what is investigated in (Bueno, 1997) is about how to give a response to the Cartwright challenge from an empiricist point of view: Cartwright challenge assumes the impossibility of an isomorphism between theoretical laws and phenomenological laws, where former are represented by theoretical models and latter by appearances or phenomenological models; Bueno tries to tie these two independent realms by applying French and da Costa's notion of partial isomorphism. So, the link between theoretical and empirical, and an account of theory change in scientific inquiry, has been established, but the link between theoretical or empirical model and what it correspondence to in the external world has not been mentioned anyway. Therefore, the argument has no relevance to the foundational problem of scientific realism.

However, French and Ladyman insistently assert that partial structure approach, formulated in a semantic framework, is a strong candidate to be a realist argument. As we have seen above, in (Ladyman, 1998) it is argued that Worrall's epistemic version of structural realism cannot be defended against both traditional scientific realism and instrumentalism. In order to be a plausible alternative, structural realism should be constructed as a metaphysical project. In other words, it should give an account of ontology. In the same paper, Ladyman suggests to use models of theories, instead of Ramsey Sentences, when establishing structural realism. Notice here that the shift from Ramsey sentences to model theoretic approach is due to *pragmatic* reasons: the semantic approach is *more plausible* when we are representing the structure of phenomenon, however, there is no argument here about why we should appeal to semantic approach when we are dealing with the link between models, or theories and empirical reality. Therefore, only the semantic approach itself cannot be used in order to provide a plausible alternative to Ramsey sentence approach; and this point is clearly expressed by French and Ladyman:

> [t]hus, we are not claiming that the gap between a theory or model and reality can be closed simply by a formal relation between model-theoretic structures. The gap is rather more fundamental than that and lies between our most basic, bottom level scientific representations and that which is represented. What is required is an understanding of the relationship between one category of things, "the world", "reality", whatever, and another category, namely that which represents the world, whether that be propositions (whatever they are), set-theoretical models or whatever; to address this issue would be to take us beyond the scope of the present work. (Ladyman and French, 1999, p. 119)

This was from (French and Ladyman, 1999). When it comes to 2003 (French and Ladyman, 2003), they preserve the same attitude that partial structures form of semantic approach seem to provide the best formal framework for Structural Realism; but, again, they do not mention about how the gap between the theory and the external realm should be closed:

> [t]he realist representation of the relationship between theories and the world must be sought elsewhere, perhaps in a notion of reference appropriate for a broadly structuralist metaphysics. Our overall claim at this point is that the partial structures form of the semantic approach offers a general account of theoretical structure that extends beyond the mathematical equations and thus represents an appropriate formal framework for SR. (French and Ladyman, 2003, p.34).

According to this quote, in order to satisfy our realist sentiments we should look for “elsewhere”. As we have seen in the previous quote above, in (French and Ladyman, 1999), they said that they are not dealing with the link between theory and the external world, and suggested that this should be investigated "elsewhere". Now, we see (French and Ladyman, 2003), and again they suggest us to find the answer for our question "elsewhere", and Ladyman holds the same attitude in his most recent writings such as (Ladyman and Ross, 2007).

One of Bueno's most recent works has unveiled the fact that Ladyman and French's ontic structural realism cannot properly show that our theories refer to the external reality. Notice that the main problem of realism is to give a plausible answer to PMIA, which is based on Khunian ontological discontinuity argument. Any kind of realism should provide a kind of continuity between predecessor and successor theories in science. This continuum might be formulated in conceptual or structural frameworks. Bueno asserts that conceptual continuum seems to provide very severe problems for traditional scientific realism, and structural continuum seems to be something unacceptable given structural change within the scientific inquiry (Bueno, 2008). The problem for conceptual change is a problem for standard scientific realists; however, a discussion about this problem exceeds the limits of the present thesis. But I will give some attention to Bueno's criticism of structural change since it concerns us here.

Although, advocates of semantic approach criticizes epistemic structural realists arguing that it cannot give a reasonable response to Newman's objection, Bueno asserts that ontic version of structural realism has same problems, and explains this by some concerns derived from the Löwenheim-Skolem theorem for first order logic (Bueno, 2008, p. 222). According to Bueno both kinds of structural realisms, epistemic and ontic versions, have the same problem "to choose between non-equivalent interpretations – as far as structure is concerned – which deliver the same results, with regard to the truth – values of the sentences under consideration" (Bueno, 2008, p. 222). Therefore, according to Bueno, both kind of structural realisms, faced with the problem of Newman's objection, and it is not the case, as Ladyman suggests in his 1998 paper, that appealing to semantic approach does not make structural realist free from Newman's Objection.

Recall that I accused French and Ladyman of giving pragmatist arguments rather than realist ones above. Bueno says, in order to escape from the Newman problem mentioned at the previous paragraph, structural realists might assert that there are some intended interpretations of those structures. But in that case, just like I mention earlier, the argument is not a realist one anymore but a pragmatist one, according to Bueno. Here he makes it explicit:

> …this [intended interpretation] is a *non-structural* feature, and as such it goes beyond the purely structural features allowed by structuralism. The notion of intended interpretation is a pragmatic notion, *not* a structural one. *And if the structural realist were to introduce a pragmatic notion at this point, the grounds to support realism would be lost* [my emphasis]. After all, what is at stake is the determination of the structure that describes the world. And if the structural realist's choice between alternative non-equivalent structures is only pragmatic – and not epistemic since, it is not structural – this choice would be compatible with the one made by the empiricist. (Bueno, 2008, p.223)

In accordance with this last quote, Bueno implicitly asserts that notions such as partial truth and partial isomorphism cannot be used for a realist argument in order to give an account of structural continuum within theory changes, since their usage is by definition pragmatic, not structural. However, there is no problem for an empiricist to use such notions and gives an account of scientific change in an empiricist setting, because "empiricist can choose the *intended* structure, on the grounds that this is the structure that *we* intend to talk about." (Bueno, 2008, p.231)

Thus, it is obvious that both kinds of structural realism, epistemic and ontic, have severe problems and these problems cannot be avoided solely by a shift from syntactic to the semantic or model theoretic approach to our scientific theories and this shift does not provide any advantage to scientific realism. So, French and Ladyman's claim that such a shift works for realism seems to be unacceptable. Semantic – structuralist ontologists should give some further argument beside the syntactic vs. model theoretical approach debate in order to make their position an acceptable scientific realist one. Next chapter investigates whether such further realist arguments can be derived from modern physics, as French and Ladyman assert it does.

CHAPTER 4

QUANTUM MECHANICS AND SEMANTIC STRUCTURALIST ONTOLOGY

4.1 Metaphysical Underdetermination and Eliminativist Attitude Towards Quantum Objects

We have seen in the previous chapters that Ladyman denies the existence of anything except structure in order to make structural realism a more defensible position against arguments coming from both traditional realist and anti realist arguments. According to him, OSR

> …is the view that the world has an objective modal structure that is ontologically fundamental, in the sense of not supervening on the intrinsic properties of a set of individuals. According to OSR, even the identity and individuality of objects depends on the relational structure of the world. Hence, a first approximation to our metaphysics is: 'There are no things. Structure is all there is.' (Ladyman and Ross, 2007, p.130)

Notice that Ladyman here should be arguing from a realist point of view: when he says that there is nothing except structure, that should mean nothing actually exist in the nature except structures, in the sense that structure is the ultimate stuff of the nature on which everything else supervenes; including objects, fields, events, etc. If his argument is a pragmatic one, then his position has no advantage over Worrall's ESR since the debate between Worrall, traditional realists, and anti-realists is about realism: whether our theories can represent the nature as it is or not. Then the question becomes whether the claim about the elimination of everything except structure is a pragmatic or a realist one.

Quantum Mechanics (QM) is a familiar fishing-pond for Ontic Structural Realists. They derive examples from quantum mechanics to justify their theory. They especially talk about problems like “under determination” and “indistinguishability of particles”. At this point, I am directly lifting two quotes from Votsis and Chakravartty which might give a shed on the issue.

> The quantum view of elementary particles, say French and Ladyman, under determines the metaphysics of elementary particles. That is, they can be viewed as either individuals or non-individuals. (Votsis, 2004, p.62)

> QM appears to underdetermine the nature of quantum particles as regards their identity, or individuality….Microscopically and in classical physics, we think of objects as having identities which distinguish them from other things… it is unclear, however, whether quantum mechanics respect individuality….Neither Bose-Einstein nor Fermi-Dirac statistics count particle permutations as constituting different arrangements. Interchanging the particles has no physical significance to quantum mechanics. (Chakravarrty, 2004, pp.869-870)

The last sentence is very important. It says if we substitute an elementary particle with another, we still have the same structure; this substitution implies no physical difference. After the substitution, no physical change occurs. Of course, this far is not peculiar to QM since also in Newtonian Mechanics if we substitute, say, an electron with another in a closed system, then the change might make no difference as regard to the whole system. What is at stake here as regard to the QM is the radical fact that we cannot *distinguish* between *“*particle before the substitution” and “particle after the substitution”; whereas we can do this in Newtonian Mechanics. It seems that they are non-individuals as there is no difference in structure when substitution occurs and as we cannot distinguish them. In classical physics, there is the possibility to distinguish between two particles even if they have all the same properties: namely the spatio-temporal location of these particles occupy. In QM, however, even the spatio-temporal location of two particles cannot be used to distinguish them:

> …classical physics assumed a principle of impenetrability, according to which no two particles could occupy the same spatio-temporal location. Hence, classical particles were thought to be distinguishable in virtue of each one having a trajectory in spacetime distinct from every other one. Thus for everyday objects and for classical particles, the principle of the Identity of Indiscernibles (PII) is true, and it is plausible to argue (with Leibniz) that individuality and distinguishability amount to the same thing… In the formalism of QM [however] particles are not always assigned well-defined trajectories in spacetime. (Ladyman and Ross, 2007, pp. 134- 135)

The following illustration by Ladyman makes the situation much clear.

> Imagine that there are two particles (1 and 2), and two boxes (A and B), where each of the particles must be in one box or the other. Classically there are four possible configurations for the system:
>
> Both 1 and 2 in A; both 1 and 2 in B; 1 in A and 2 in B; 1 in Band 2 in A.
>
> If these are regarded equipossible, each will be assigned a probability of ¼. The situation is quite different in quantum mechanics (QM), where there are only three possible states:
>
> Both 1 and 2 in A; both 1 and 2 in B; one of 1 and 2 in A and the other in B.
>
> Hence, if these are regarded as equipossible, each will be assigned a probability of 1/3. In quantum statistics, then, what would be regarded as two possible states of affairs classically is treated as one possible state of affairs. (Ladyman and Ross, 2007, p.133)

Notice that the third possibility in the case of quantum mechanical investigation of the system is “one of 1 and 2 in A and the other in B”. This means that the question “whether 1 or the 2 is in the box A” is a meaningless one in quantum mechanics because we cannot physically differentiate the situation between when 1 is in A and 2 is in B, and when 2 is in A and 1 is in B. Therefore, the situation seems to implying that the individuality of 1 and 2 disappears at this example.

4.2 Ontology of Quantum Field Theory and Cao on Ontic Structural Realism

Concerns about the individuality and indistinguishability of particles had been existed since the beginning of QM. However, the situation has become much worse with the introduction of quantum field theories (QFT) and the realist interpretation of particles and fields became much harder due to new compelling interpretative problems. QFT supplies the framework for many fundamental theories in modern physics, and it has reached its best success in the early 1970s in the standard model, which is able to describe the fundamental interactions of nature within a unified theoretical structure. One of the philosophically interesting questions in QFT is about the basic ontology of it: whether fields or particles should be taken primitive in the sense that which one of them supervenes on the other. While trying to give a plausible answer for this question, structural realist should have in her mind a different but parallel question at this point. Recall that Ladyman asserts that structure does not supervene on anything else, in the sense that it is ontologically prior to everything else. Parallel to this idea, in QFT, we should ask the question of how there can be fields without objects that constitute these fields. In other words, the structural realist question "how there can be relation without relata?" might be converted, in the case of QFT, to the question of "how can we have the effect (field) without the thing (particle) that caused this effect?"

The main argument of French and Ladyman as regard to the ontology of QFT is roughly the idea that metaphysical underdetermination prevent us to choose between particle and field ontology. This means that it would make no change as regard to the empirical results of the theory whether we apply particle or field ontology. Therefore, we can avoid such an ambiguity in QFT by applying a “structure based ontology” where we do not face with the particle – field tension since both of them taken to have derivative ontological status supervening on more ontologically basic element: namely the structure.

> [I]n the context of quantum field theory, they [French and Ladyman] claim that particles or fields are merely different representations of the same Lagrangian or Hamiltonian structure and the related equations. 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. 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. (in Cao, 2003, p.17)

The important point here is to understand when French and Ladyman assert that no interpretation in terms of physical entities would be possible, whether they make such claims under the shed of given scientific data, or they simply claim that such an interpretation is not “desirable” due to the metaphysical underdetermination it leads. The former case does not seem to be a plausible option since the ontological indifference between particles and fields seems to be easily challenged given the fact that although mathematically the field has to be taken as the primary ontological element since the concept of particles seems to be extracted from it, empirically, only particles are observed and quantum fields, unlike classical fields, are not directly observable[5] (Cao, 2003, p.18).

It is true that it is very hard to define a notion of particle in QFT. Also, interactions between quantum fields cause problems for particle interpretation. Not to mention that in QFT particle number is not conserved. If there are objects (in the usual sense) in quantum field theories, they must be built up of particles; however, particles are themselves viewed as field quanta, so fields seem to be truly the fundamental things. However, as I mentioned above, not fields but

[5] I said “mathematically the field has to be taken as the primary ontological elements” due to the fact that a quantum field consist of operators associated with spacetime points, and these operators “represent not the values of physical quantities but those quantities themselves” (Ladyman and Ross, 2007, p.139)

particles are directly observable in QFT. Thus, it would be quite bizarre to take the fields instead of particles as the ontological primitive since the latter is observable but the former is not. In that sense, it is easy to understand French and Ladyman's attempt to eliminate the notion of objects from the ontology of modern physics and replace it with the structure as the basic ultimate stuff. However, to favor an interpretation on the face of another one is one thing, but to be able support our claims with the results of related empirical data is a completely different thing; and I do not think French and Ladyman provide convincing empirical reasons to favor their position. It is true that concerns or problems that lead French and Ladyman to favor an eliminative – structuralist ontology are very severe ontological problems for the traditional realist interpretation of the QFT; but this problematic case does not necessarily lead us to the structuralist ontology of them. Recall Worrall's epistemic position at this point. Worrall is also quite aware of the interpretative problems as regard to the ontology of QM. However, he provides a less radical but more convincing philosophical solution to such ambiguities. Since the ontology of science is fully populated by such ambiguities, he suggests being silent about the ultimate stuff of the nature. However, this does not mean that we do not have the objective knowledge of the nature; since we have the structural knowledge of it; and this fact was confirmed by NMA on the basis of predictive success of our scientific theories. Therefore, I found Ladyman and French's eliminativist position too radical and believe that Worrall's position is strong enough to give a plausible realist account of our scientific theories.

4.3 CBH Theorem, Entanglement, and Foundations of Physics: A New Ontology?

At the rest of the present chapter, I want to mention about some current developments in the foundations of physics. I will especially talk about the concept of entanglement in quantum information theory (QIT). I will assume, for the sake of the argument, that French and Ladyman indeed provide good reasons to convince us that problems in the ontology of QFT lead us to accept their eliminativist – structuralist ontology. Even in that case, I will argue, their position is not a plausible one, due to some concerns about the new ontology that can be derived from QIT.

The actual story goes all the way back to the dispute between Einstein and Bohr. Einstein, as it's well known, was extremely dissatisfied with the Copenhagen Interpretation of QM and tried to show that QM is "incomplete"; believing that some empirical data will be unveil the "hidden variables" of QM one day in the future. Einstein's concerns about the completeness of QM, which was formulated in his famous EPR[6] paper, has been investigated by further experiments by John Bell in the 1960s and the story turned out to be that Einstein was not right about his doubts as regard to the completeness of QM. Most recently, it has been argued that the results of Bell experiments has been confirmed by much up to date experiments by Anton Zeilinger and this has increased the confirmation level of the Copenhagen Interpretation. I won't be dealing with the historical and technical details of these developments here; but rather try to show that these currents developments in QIT imply a totally different ontology from the ontology of QFT. The main claim will be about the concept of entanglement and I argue, on the base of Bub's interpretation of Clifton, Bub, and Halverson (CBH) theorem, that although this concept has been at the focus of the debate between advocates of Copenhagen Interpretation and its rivals, and should have been conceived as a problem for a realist interpretation of QM, with the recent developments in quantum information theory, entanglement can be interpreted as a new ontological entity on its own right, in the sense that it does not supervene on any other ontologically superior physical source.

[6] EPR is the "Einstein – Podolsky – Rosen" argument that has been formulated in order to show that the Copenhagen interpretation of quantum mechanics is incomplete.

Clifton, Bub, and Halverson have argued that "one can derive the basic kinematic features of a quantum description of physical systems from three fundamental information-theoretic constraints" (in Bub, 2007, p.632). I won't mention the details of these constraints here; but what concerns us here is the fact that

> …CBH formulate these information-theoretic constraints in the general framework of C* - algebras, which allows a mathematically abstract characterization of a physical theory that includes, as special cases, all classical mechanical theories of both wave and particle varieties, and all variations on quantum theory, including quantum field theories (plus any hybrids of these theories, such as theories with superselection rules). (Bub, 2007, p.633)

This means that the mathematical structure of all quantum mechanical theories we had since the early years of QM, including all QFTs, can be derived from the mathematics used in the CBH theorem; namely C* algebra. I am not in a position to judge whether CBH can provide enough mathematically acceptable reasons to prove that it can achieve what it promises with the introduction of C* algebra. But what I am concerned here is about the further ontological analyses that Jeffry Bub derived from CBH theorem. According to Bub, "a quantum theory is best understood as a theory about the possibilities and impossibilities of information transfer, as opposed to a theory about the mechanics of nonclassical waves or particles." (Bub, 2004, p.242) In that way, for instance, the debate in QFT about whether particles or fields are ontological primitives becomes an issue which has a secondary importance in quantum mechanics since the main focus of the theory has been converted to the concept of information, not particles or fields. However, to assert that all mathematical formulations of earlier quantum mechanical theories can be derived from the C* algebra of CBH, does not guarantee that the new basic ontological entity, namely the "entanglement", introduced by QIT does not supervene on anything else. For an instrumentalist, for instance, mathematical derivation between different theories would not imply anything on the ontological level. A realist then should provide a totally different philosophical argument to show that “entanglement” does not supervene on any more basic ontological entity. Along the same line with such a realist attitude, Jeffry Bub has mentioned Einstein’s distinction between “principle” and “constructive” theories and made an analogy between Einstein’s special theory of relativity and quantum information theory, stating the fact that both of them being a principle theory: then he asserted that information has to be understood as a new physical primitive, just as the field was conceived as ontological primitive with the introduction of special theory of relativity. (Bub, 2005)

The issue of whether CBH can provide a strong enough mathematical formulation from which every quantum mechanical theory can mathematically be deduced; or whether Bub’s establishing an analogy between Einstein’s special theory of relativity and quantum information theory as both of them being a principle theory, as opposed to a constructive theory, and his attempt to take the information as the primitive physical entity just like the field has become a primitive in special theory of relativity can be proven mathematically or philosophically is left untouched here. The limit of the present thesis and my knowledge about these issues makes such further claims impossible.

All I want to show here is the fact that the ontology of QFT may not be same with the ontology that has been provided by our most recent physics. If we accept the ontological suggestion of Bub about taking entanglement as the new primitive, then Ladyman and French should show us how they can eliminate this new ontological basic, namely the “entanglement” in favor of structure. And this is not the end of the story. It is quite possible that a new physical theory in the future will probably replace the ontology of QIT, and possibly we might have new ontological primitive other then entanglement, field, etc. When this happens French and Ladyman have to show that the new ontologies of these possible future theories can also be eliminated in favor of structure. My personal opinion is that philosophical theories such as French and Ladyman’s OSR, which derive eternal conclusions from the current ontology of a given science, probably would not be an acceptable one any longer in the future when new ontologies are replaced by formers.

Therefore, French and Ladyman's OSR is problematic not only due to it makes pragmatist claims as regard to current scientific theories (such as suggesting to avoid particle – field ontology, and instead use a structural ontology, where they cannot give an account of this move from a truly realist point of view), but also due to the fact that their structuralist ontology takes the implications of the QFT as permanent and does not take in to the account any possible ontological change in the future as regard to the basic constituents of our scientific theories.

At the last chapter of the thesis, I will turn to a completely different issue, namely the scientific explanation debate and will search whether we can establish a structural realist understanding of scientific explanation.

CHAPTER 5

IS STRUCTURAL REALIST UNDERSTANDING OF SCIENTIFIC EXPLANATION POSSIBLE?

5.1 Historical Background to the Scientific Explanation Debate

In order to understand the recent discussion on scientific explanation that begins with the development of the Deductive – Nomological (DN) model by Hempel, one should go all the way back to Hume and his successors in the continental Europe in the 20th century, especially those people called logical positivists. Why the historical debate centered on explanation, rather than the causation; despite the fact that causation seems to play a central role in scientific inquiry, especially in the physical sciences, and explanation at a first glance seems to be subjective and metaphysical? The very answer for this question takes us back to the empiricism of Hume. As it is well known, Hume rejected the idea that we can have any "impression" of the necessary link between cause and effect. Early logical positivists, as the followers of Hume, applied this Humean idea to their verification principle. It seemed impossible for logical positivist to construct scientific explanation on causal theories. However, they realized that, they could give an account of causation if they analyze it in terms of the concept of explanation; and this was the reason for the above questioned central role of the explanation. The concept of causation, by the DN model therefore, became scientifically respectable because it was analyzed in terms of explanation, which was constituted by natural laws and deductive logic.

Some problems related to the interaction between "law" and "regularity" worth to be mentioned here. Most Humeans, including logical positivists, adopted the regularity view of laws, which simply states that laws of nature are regularities. On the other hand, as it is explained in the previous paragraph, the concept of causation was analyzed on the base of the concept of explanation by DN model; explanation being a conclusion of a deductively valid argument which includes at least one law like statement in its premises. What emerges from these two historical facts is that, the concept of causation is intimately linked with the concept of law, and the concept of law is intimately connected with the concept of regular succession. This was the way logical positivists established the relation between “regularity” and “causation”. But, when we look at the theories of causation in the literature, it is easy to see a common acceptation that regularities do not imply causation. This is especially so, for those theories within the empiricist tradition. Then appears the question of how logical positivists established above mentioned correlation between regularity and causation, in spite of the fact that this is extremely problematic from an empiricist point of view since regular succession cannot imply or grant any kind of causation within the empiricist tradition? Psillos gives the following answer for this question:

> [t]he operationalization of the concept of causation they [Schlick and Carnap] were after was not merely an attempt to legitimize the concept of causation. Rather, it was part and parcel of their view that science aims at *prediction*. If prediction is what *really* matters, then the fact that there can be regularities, which are not causal in the ordinary sense of the word, appears to be irrelevant. A regularity can be used to predict a future occurrence of an event irrespective of whether it is deemed to be causal or not. The former can predict that dawn has broken on hearing the cock’s crow irrespective of whether or not the crow causes the sun rise. In physics, one can *predict* the length of the pendulum’s rod, given its period, irrespective of the causal connection between these two magnitudes. Correlations can serve prediction, even though they leave untouched some intuitive aspect of causation, according to which not all regularities are causal. (Psillos, 2002, p.217)

One of the central arguments of the present thesis is the claim that NMA can be defended against PMIA only when it is constructed on the notion of prediction, rather than constructing it on the notion of explanation. I follow here the logical positivist tradition when answering the question of whether explanatory or predictive differences between two empirically equivalent theories carry more weight when they are computing[7]. Their reason to choose the notion of prediction was the fact mentioned by Psillos in the above quote: in that way, they would be able to talk about regularities without any reference to the causation. Therefore, since explanation consists of deductive logic and laws of nature, and laws of nature are explained by regularities, and regularities are introduced without any reference to causation, the Humean scruples about causation are satisfied to some extent and elimination of metaphysical terms from science was achieved.

Carnap (1966) touches upon prediction vs. explanation issue when he argues against German biologist and philosopher Hans Driesch. Driesch developed a vitalistic account in biology, according to which an inner force or purpose in each living organism, Driesch called this "entelechy", is responsible for directing the development of it. When Carnap and Reichenbach did not accept Driesch's notion of "entelechy" by arguing that it does not provide scientifically respectable explanation since it is deeply metaphysical, Driesch responded that there are some cases in science, even in physics, that we appeal to the metaphysics. The concept of magnetism is an example for such a case. According to him, the introduction of the term entelechy to explain the behavior of organisms was no different from physicists' introducing the term magnetism to explain the behavior of magnets and iron, since after all, we can neither see nor touch the force of magnetism. Carnap's answer reflected the logical positivists' attitude within prediction vs. explanation debate. According to

[7] The reason why logical positivists favored prediction on the face of explanation is in some ways different from the reason why I assert that NMA cannot be constructed on explanation hence we should formulate NMA by predictive success of our scientific theories. Logical positivist concern was much more about how to eliminate metaphysics from the science. My main concern here, rather, is to find an answer for the question of how can we formulate NMA argument, most suitably, in order to make it a plausible position against PMIA. However, I give the historical account in this chapter to understand in what environment the DN model has been constructed.

Carnap, when physicists introduced the term magnetism, they did not simply posit the existence of an unobservable entity, these laws can also be used to make predictions that can be tested by experiment. Driesch's entelechy theory does not allow for such experimentation and thus completely lacking in predictive power; thus, it does not give genuine explanation. Along with similar lines with Carnap, in DN model, laws of nature, which are introduced as premises in a deductive argument allows for prediction. Therefore, the question of whether prediction or explanation carries more weight replied in favor of prediction, since explanations that does not include lawlike statements, which does not make predictions, as in the case of Driesch's entelechy theory, are classified as problematically metaphysical.

5.2 Deductive Nomological, Unificationist, and Causal Theories of Explanation

According to the DN model of explanation, a scientific explanation consists of two parts: the "explanandum", referring to the phenomenon to be explained, and the "explanans", "the class of those sentences which are adduced to account for the phenomenon" (Hempel, 1965, p.247). In that model, the act of explanation, as a condition, is in a form of deductive argument, where the explanandum follows as the conclusion from the explanans, which are used as the premises of this deductive argument. Also, as another condition for this mentioned deductive argument to be an explanation, at least one "law of nature" should be included in the premises. Given this two conditions we can understand why this model is called as "Deductive – Nomological". The first condition makes the explanation deductive as the explanandum is derived from explanans by a deductive argument. The second condition makes the explanation nomological since we include at least one law in the premises and due to the fact that the philosophical term "nomological" means lawful.

It is obvious that above mentioned "laws of nature" should be "true laws". Hempel at this point makes a distinction between "accidentally true" generalizations, and generalizations that are "laws"[8]. The fact that "Haktan was involved in empirical sciences section, rather than being a member of social sciences section, when he was a student at high school" is accidentally true; whereas the statement "all metals conduct electricity" is not just accidentally true but rather it is a law. In this example, to explain why Haktan was involved in empirical sciences section at high school, we can construct a DN argument whose explanans are the following two statements: "Haktan went to a Science High School between 2000 and 2003" and "All students attended to Science High Schools between 2000 and 2003 are involved in empirical sciences sections simply because there was no social sciences section in such schools at those times". Since the major premise of the argument is true, but not law like, this argument lacks explanatory power. Conversely, the law like statement "all metals conduct electricity" can explain why when electric current is applied to one end of the lead, we can measure the current on the other hand.

As it is emphasized earlier, the Hempelian claim that laws used in DN model must be "true" was one of the conditions for the adequacy of an explanation. He made a distinction between potential explanation and actual

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[8]There have been some arguments against this distinction about being accidentally true generalizations and generalizations that are laws, especially those arguments related to the special sciences such as biology, economy, psychology, etc. The criticism, roughly, is the fact that although some generalizations which play important explanatory role in special sciences do not satisfy the standard criteria for lawfulness, and hence classified as accidentally true generalization according to Hempel, it is an undeniable situation that they are only available explanations used within these sciences. If we accept those generalizations as accidentally true, rather than laws, then it seems impossible to talk about DN model explanations within special sciences; in which case, the DN model becomes a model used merely in physical sciences.
I do find this criticism as a very appealing one. However, the point that I want to emphasize in this chapter is the fact that laws used as explanans in DN model cannot be defended as "true", given the PMIA. Therefore, my main concern is about the possibility of whether we might have epistemic access to the true laws, not about the question of whether accidental generalizations can be accepted as laws in special sciences.

explanation and claimed that an explanation is an actual explanation only if it includes “true” laws in one of its premises. At this point lays one of the main arguments of the present chapter. The basic problem between realism and empiricism in philosophy of science is about whether we are legitimate to attribute “truth” to our scientific theories; claiming that unobservable entities postulated by our scientific theories *refer* to those entities within the external realm, which have their existence independently of our minds. Realists give a positive answer for this question and argue that we are legitimate to accept this reference relation whereas empiricists claim that we cannot establish such a relation simply because such an act would transcend what is given as the result of empirical investigation, and will be deeply metaphysical. Now, in DN model this realism problem makes itself explicit as we have seen that it is a must in Hempel’s model to attribute “truth” to the laws. I have argued in the previous chapters that concerns about the truth cannot be defended from a traditional realist point of view against the arguments stemmed from PMIA; however, at the same time, we have seen in Worrall’s ESR the possibility that we can still have a different notion of truth on the base of NMA if we focus merely to the structural part of scientific theories, neglecting the ontological posits of them. Now, the question becomes that of whether we can satisfy conditions of DN model by shifting from traditional realism to the structural one; in other words, the question is “is structural realist understanding of scientific explanation possible?” Answers for this question will be made explicit throughout the end of this chapter; however, let’s now turn to the traditional counterexamples formulated against DN model to question its legitimacy.

Common point that different criticisms against DN model share is the fact that “there is more to the concept of causation than what can be captured by DN explanations” (Psillos, 2002, p.224). One of the famous criticisms is about the relation between objects and their shadows. For instance, one can derive the length of the shadow of a flagpole from the height of the pole, plus the angle between sun and flagpole, plus laws of optics. This derivation meets the DN criteria and seems explanatory. On the other hand, likewise, we can also derive the height of flagpole by the length of the shadow, with an appeal to the same laws of optics and the angel between sun and the flagpole. There seems to be no problem for such an explanation at a first glance, since it satisfies all criteria of the DN model; but such an argument wouldn't be an explanation since it is an obvious fact that the shadow does not cause the flagpole to have the height it does. Another counter example for the DN model is about the explanatory irrelevance problem. An explanation may satisfy all requirements of DN model, but still might have some problems since it contains irrelevancies. The classical example is due to Salmon: "John Jones avoided becoming pregnant during the past year [C], for he has taken his wife's birth control pills regularly [B], and every man who regularly takes birth control pills avoids pregnancy [A]" (Salmon, 1971, p.34). Although A, B and C certainly constitute a sound deductive argument in which A occurs as the essential premise, most people would argue that A and B are not an explanation of C; simply because John Jones' being failed to get pregnant is not due to A or B, but due to the physiological fact that males cannot get pregnant[9].

Recall the Psillos quote above which states criticisms against DN model centered on the idea that DN model does not attribute enough importance to the causation. If we follow this analysis, to explain an outcome we must cite its causes; however, as we have seen, two counterexamples against DN model given above fail to do this. Along with the similar lines of reasoning, Salmon argued that the asymmetry problem in the flagpole example "lie[s] in the fact that a flagpole of a certain height causes shadow of a given length, and thereby

[9] Hempel made a distinction between *law of coexistence* and *law of succession* to reply the asymmetry problem, appealing to the idea that only law of succession could be classified as causal, whereas law of co-existence does not. I will not discuss details of Hempel's answers to the criticisms introduced against DN Model here. I also will not talk about Salmon's early (1971) work here where he highlighted the above mentioned pregnancy counterexample against DN model and formulated Statistical Relevance model to overcome apparent problems of it. I will rather mention about Salmon's later studies, where he abandoned to characterize explanation or causal relations in purely statistical terms, but rather tried to explicate the issue by giving more attention to the causation and explanatory relationships.

explains the length of the shadow, whereas the shadow does not cause the flagpole, and consequently cannot explain its height" (Salmon, 1989, p.47). According to Salmon's own formulation of the explanation, every scientifically respectable explanation should be formulated with respect to certain causal laws of physics. The major difference between Salmon's own causal mechanical theory and Hempel's DN model, therefore, is the fact that the explanation in Salmon's theory is being constructed upon the causal laws of physics, whereas in DN model causation is not explicitly related to the laws, appeared as an explanans in the model, due to some Humean scruples[10]. In Salmon's model, as regard to the one of the main thesis of the present thesis, every causal claim entails some generalizations that qualify as laws. This is to say that when I explain the fall of a stone towards to the earth, I construct this explanation by reference to the gravitation; where the relation between gravitational force and acceleration of the stone with respect to the time is formulated within the Galileo's law of free fall. However, we do not explain the fall the stone by gravitational attraction in Einstein's theory of Relativity; rather, massive objects such as sun or earth curves the spacetime and objects such as stones just follow geodesics; there is no force between the earth and the stone. In other words, the causal relation between earth and stone as it is given in Galileo's law of free fall is truly false, according to the Einstein's theory. Therefore, there appears the question of whether Salmon's causal mechanical theory can be defended in the face of radical changes in the history of the science. This is the question of whether causal mechanical account of explanation is a plausible position in the

[10] There have been long run discussions over whether Salmon's causal mechanical model is anti-Humean, or is it compatible with Humean understanding of causation. Moreover, there have been some discussions on whether Salmon's account can overcome standard criticisms that have been face to Hempel's DN model. Still, there have been some concerns about whether causal claims in Salmon's causal mechanical account entail some direct or indirect generalizations that qualifies as law. All these discussions are out of the contextual framework of the present thesis. The conclusion I want to reach about Salmon's account here is the fact that when he formulates his causal model, he constructs the notion of explanation upon causal laws, which are the laws of physics. Therefore fundamental laws of physics play a central role in his system; and I will question the very possibility of such laws given the PMIA later in this chapter.

face of PMIA; and the answer for this question will be clear throughout the end of this chapter.

Another model that has been formulated to overcome standard criticisms faced to the DN model is due to Friedman and Kitcher. The basic idea is that explanation is a matter of providing a unified account of a range of different phenomena. To explain means to derive descriptions of many different phenomena by using as less law as possible. The fewer the laws used and the greater the range of different conclusions derived, the more unified is the explanation. According to Kitcher,

> [s]cience advances our understanding of nature by showing us how to derive descriptions of many phenomena, using the same pattern of derivation again and again, and in demonstrating this, it teaches us how to reduce the number of facts we have to accept as ultimate. (Kitcher, 1989, p.423)

Put aside the details of his project, Kitcher's main purpose was to tie the concept of explanation to the concept of the deductive systematization[11]. Explanations are deductive arguments in this sense. Thus, Kitcher's account gets very close to Hempel's DN model. Kitcher admits this point when he says "the systematization approach retains the Hempelian idea that to explain a phenomenon is to produce an argument whose conclusion describes the phenomenon" (Kitcher, 1989, 431). As for to the difference between Hempel's and Kitcher's accounts, the premises of explanatory arguments does not have to

[11] Kitcher (1989, p. 485) has provided some illustrations about how Unificationist model actually works; for instance he give an account of explanatory asymmetries (flagpole example) by introducing the notion of "origin and development" (OD) pattern of explanation, and asserted that the asymmetry problem emerges as a result of our confusing the OD patterns and shadow patterns; claiming that OD belongs to the explanatory store, but the shadow pattern does not. On the other hand, there have been some objections stating that Kitcher's view cannot provide a complete account of asymmetry problem. There have been some additional criticisms about the heterogeneity of Kitcher's unificationist account as well as criticisms about the epistemology of unification in his system. All this concerns are vital and important in Kitcher's way of explicating the explanation issue; however, such details are, again, out of the contextual framework of the present thesis. In what follows, I will assert, as regard to the main thesis of this chapter, Kitcher's account does not requires the premises of explanatory arguments be *true* laws of nature.

be laws of nature in Kitcher's account, whereas they are laws of nature in Hempel's account due to the nomological character of the scientific explanation. Psillos shows this defeat in Kitcher's approach as follows:

> Kitcher's account… does *not* demand that the premises of the explanatory arguments be laws of nature. It does not even demand that they be universally quantified statements. They may be and yet they may not. So, as it stands, Kitcher's account need not be a way to explicate what the laws of nature are. Nor does it demand that all explanation be *nomological*. (Psillos, 2002, p.275)

Later on in this chapter, I will argue that Hempel's and Salmon's models of explanation cannot be defended against PMIA argument because of the laws of nature in the premises of Hempel's DN model and causal laws of physics in Salmon's causal mechanical account are, at least in principle, subject to falsification in the face of radical theory changes in the history of science; and this falsification definitely implies the victory of PMIA. If we follow Psillos' reading of Kitcher's unificationist account, appealing to the idea that "Kitcher's account does not demand that the premises of the explanatory arguments be laws of nature", then it seems we even cannot apply PMIA to Kitcher's account in the way we apply it to Hempel's and Salmon's accounts; simply because the very possibility of such an application within Hempel's and Salmon's accounts is due to the fact that their models include law like statements; whereas, in Kitcher's account there is no claim that premises of the explanatory arguments are law like. Then the question becomes that of where to put Kitcher's unificationist account in the scientific realism debate since it does not include law like statements. If we follow Kitcher's own formulation, realist interpretation of his account does not seem to be possible. For he defines the explanatory store E(K) as the set of argument patterns that maximally unifies K, the set of beliefs accepted at a particular time in science. In Kitcher's own words "a theory unifies our *beliefs* when it provides one (more generally, a few) pattern(s) of argument which can be used in the derivation of a large number of sentences we accept (italics my emphasize)" (Kitcher, 1981, p.514). Therefore, the realism question whether sentences of our scientific theories refer to the external world does not seem to be an issue in Kitcher's account since he talks about "commonly accepted *beliefs"* within the scientific community, rather than "true sentences" that correspond or refer to the external reality.

In what follows, I will try to construct a relation between above investigated models of explanation and the scientific realism issue with its relation to the NMA vs. PMIA debate, and will search for a possibility of structural realist understanding of scientific explanation.

5.3 Pessimistic Meta Induction Argument against Hempel, Kitcher, and Salmon

I mentioned in one of the previous chapters about the Eddington observation where Einstein's theory has successfully predicted the deflection of starlight on the vicinity of the Sun. Notice that, in this situation, the predicted phenomenon had not been known before Einstein's theory made the prediction. In other words, the predicted phenomenon was not a commonly unexplained problem within that time's scientific paradigm. The question of whether the starlight would be subject to the deflection around the Sun did not exist. This situation makes the prediction of the theory a novel one: the prediction was not introduced to give a fresh explanatory approach to an already existed phenomenon; therefore, the theory cannot be constructed upon *ad hoc* maneuvers just in order to *save the phenomena*.

On the other hand, van Fraassen explains the success of theories as follows:

> …the success of current scientific theories is no miracle. It is not even surprising to the scientific (Darwinist) mind. For any scientific theory is

> born into a life of fierce competition, a jungle red in tooth and claw. Only the successful theories survive – the ones which in fact latched on to actual regularities in science. (van Fraassen, 1980, p.40)

In this view, acceptance and success of scientific theories depend on how it survives within the computation in a so called Darwinian jungle. However, the above mentioned predictive success of Einstein's Relativity theory does not support such a competition; for it is not the case, when Einstein's theory makes the prediction about starlight deflection, that there are a lot of computing theories about this phenomenon. Instead, there is even not a problem of such a phenomenon because the phenomenon itself is what the theory predicts. Phenomenon was not realized before the prediction.

When Einstein's theory *predicts* the deflection of the starlight, it also *explains* this phenomenon. When the question of "why the starlight is subjected to the deflection?" is asked, the only available answer around the beginning of the 20^{th} century was due to Einstein's theory. So, it is an obvious fact that Einstein's theory also enjoyed the explanatory success, beside the predictive success, at that time. The question at that point is, as regard to the realism debate, whether this explanatory success provides any reason for us to favor scientific realism. It does not seem so since the explanatory success of the theory constructed upon some basic claims about the nature of some fundamental concepts such as spacetime and such concepts are understood in quite different ways in quantum theories. The deflection of the starlight around the Sun is due to the fact that massive objects bend spacetime manifold according to Relativity Theory. However, according to quantum mechanics, such a manifold bending and geodesic shaping by massive objects and the deflection of the starlight due to this bending seem to be extremely problematic. Therefore, the explanatory success that Einstein's theory has enjoyed at that time does not mean anything as regard to the scientific realism debate because some of the foundational assertion upon which the theory's explanatory success constructed, such as the nature of spacetime, has been changed, or at least questioned, to some extent, in the history of science. This seems to imply, the idea that the explanatory success of theories gives us reasons to be a realist cannot be defended properly against the power of PMIA.

Hempel's, Kitcher's and Salmon's theories about explanation are subject to the same criticisms on the face of PMIA. In Hempel's account, as we have seen, the explanation is a deductive argument whose premises include initial conditions and law of nature. The fact that massive objects create geodesics (spacetime curvatures) by bending the spacetime manifold is one of the premises which is supposed to include law of nature. And the explanandum is the observed fact that starlight has been subjected to the deflection when passing near the Sun. However, we cannot say this explanation is a *true* explanation since the developments in the quantum mechanics has proven that fundamental concepts such as spacetime, which are included in this explanatory argument as one of the premises, is extremely problematic. In Salmon's account, remember, explanation is provided through causal theories of fundamental physics. The explanation of the deflection of starlight would be explained, for example, by the causal physical laws about how massive objects shape or bend geodesics, and how light follows these geodesics according to Riemannian geometry. However, again, the causal relation between massive objects and bent geodesics does not seem to be possible in quantum physics. As for Kitcher's account, it is true that Einstein's theory unified earlier irrelevant theories such as Maxwell's theory about electromagnetism and Newtonian mechanics; and this could be seen as an explanatory success of Einstein's theory, from a unificationist point of view. However, when we make explicit the problems of GTR on the base of above mentioned criticism of geodesic bending phenomenon on the face of quantum conception of spacetime, then GTR's unificatory explanatory power cannot imply that the explanation is a *true* one. Therefore, in any of these main stream accounts of scientific explanation, we cannot say that explanatory success of scientific theories support scientific realism anyway, given the PMIA.

5.4 Whither a Structural Realist Understanding of Scientific Explanation?

Then the question of whether we can give a "structural" realist account of scientific explanation appears. There is one crucial point as regard to structural realist argument here. In the first chapter, I have quoted from Worrall the following: "it is not the case that Einstein's theory is simply an extension of Newton's. The two theories are logically inconsistent: if Einstein's theory is true, then Newton's has to be false" (Worrall, 1989, p.104). Most of the traditional realist accounts have appealed to the idea that Newton's theory is a limit case for Relativity Theory or the Quantum Mechanics. According to this view, we cannot say that Newton's theory is falsified after successor theories in physics. When the macro physics is our concern, such as cosmology, the laws of physics that we are to look for is described by Relativity Theory; on the other hand, when our concern is micro physics, for instance the behavior of subatomic particles, the place that we have to look for is the quantum mechanics; and the behavior of middle sized objects are characterized by the Newtonian Mechanics. According to this view, there is no logical contradiction between relativity theory, quantum mechanics and Newtonian physics. Relativity Theory and Quantum Physics is just an extension of Newtonian Theory. According to the structural realist understanding of scientific theories, on the other hand, such a tolerance is not possible. As Worrall clearly asserts, we cannot say that Newton's theory is *true*, after Quantum and Relativity theories. Actually, the reason why Newton's Theory has been replaced by the Relativity Theory is the fact that there are some phenomena that relativity theory can give an account for, but Newton's theory cannot. Relativity Theory gives an account for everything that Newton's Theory can give, and additionally, it gives an account for some phenomena that Newton's theory cannot explain; such as the bending of the starlight on the vicinity of the Sun. In this understanding, if the Relativity Theory is true, then the Newtonian Mechanics must be false.

Although we cannot say that Newton's theory is *true* after its replacement by successor theories, structural realist argument asserts that we can still claim that some of its structural part has latched on to the structure of external world. As we have seen in the first chapter, this argument was based on NMA: the "structural" part of the theory that latched on to the "structural" part of the external reality has been introduced, as the responsible part of the theory that enables the theory to enjoy novel predictive success.

When it comes to explanation, however, things do not seem to be so straightforward. In order to be able to assert that a structural realist account of explanation is possible, we should be able to show that the part of the theory that has survived throughout theory change is the same with the part of the theory that enables us to provide successful explanations. However, parts of the theories that enable us to provide explanations are not the mathematical structure of these theories. In Hempel's DN model, for instance, when we explain the fall of the stone towards to the earth by the gravitational force, we do not mention Galileo's mathematical formula which gives the relation between height, time, and the gravitational force. Instead, we tell that the stone falls due to the gravitational attraction between stone and the earth. Mathematical formulae itself does not explain anything. On the other hand, if we accept that the physical formulae provides the basis for the explanation of the fall of the stone, then we have to accept that this explanation is plainly *wrong* since the mathematical formulae is wrong because of the fact that in the successor Relativity Theory of Einstein there is absolutely no such an attraction between stone and earth: the stone just follows the spacetime geodesics. Again we are faced to the PMIA.

So, are we to accept the power of PMIA and to be converted to an anti – realist? This does not seem to be necessary if we can construct the NMA upon the predictive success of our theories, rather than constructing it upon the explanatory power of them. When we look at the mathematical formulation of the optical laws in Fresnel’s, Maxwell’s, Einstein’s, and Quantum theories, we can find a common mathematical structure presented in all those theories; and that much would be enough to be optimistic about the truth of those theories. In that way, we can argue that these theories’ predictive success has been made possible by the fact that the preserved common structure in all these theories latches on to the structure of the external reality; and this latching or the congruence makes the predictive success of our scientific theories non-miraculous. When it comes to the concept of explanation, however, it seems impossible to construct such a congruent relation between the propositions of the theory and the external reality that they describe, because of the PMIA.

CONCLUSION

This thesis has been constructed upon a critique of ontic version of structural realism. This critique is not due to the very foundational claims of structural realist argument itself but rather due to the pragmatic sides of the ontic version. On the other hand, I hope it is obvious enough from what I have written above that I have very strong affinities for Worrall's epistemic version, and accept it as the most plausible philosophical position for the time being as regard to the ontological status of theoretical entities in scientific theories. The last chapter on the investigation of the possibility of a structuralist explanation might seem irrelevant to the rest of the thesis at a first glance. However, this last chapter and the rest of the paper are connected to each other in a very important manner: if we are to allow some pragmatic moves within the limits of scientific realism, as Ladyman and French tries to do when they interpret some phenomenon in QFT (concerns about taking the motto "there is just structures all way down" as a realist claim, under the shed of some results derived from QFT, which is, in my opinion, obviously a pragmatic move), or when they introduced some concepts such as partial truth (which is, again, obviously pragmatic notion, especially as it is explicated in the face of Bueno's "intended structures" criticism), then we can make similar pragmatic moves in the explanation issue and say, for instance, we can give a "structural realist" account of explanation appealing to Kitcher's account. I have mentioned earlier about Kitcher's idea that a theory unifies "commonly accepted *beliefs*", where there is no reference to the link between the theory and what theory refers to in external world. Since there is no reference to the external realm, I interpret Kitcher's position as a pragmatic or instrumentalist one. However, if we take Kitcher's unification of commonly accepted beliefs (which is obviously based on some pragmatic concerns since beliefs are subjective) as somehow a realist position, just like French and

Ladyman try to show that their pragmatic moves are indeed realist maneuvers, then we can talk about structural realist account of explanation on the base of unification. But I do not believe that this would be a plausible argument.

A scientific realist argument should, at the end of the day, demonstrate a correlation between scientific theories and the theoretical entities postulated by those theories. In most of the cases in practical scientific inquiry, it might be the case that we appeal to pragmatic concerns; there is nothing wrong with this situation. However, appealing to pragmatic devices in scientific practice is one thing, but using pragmatic arguments when investigating the very foundational issues about the nature of science and our knowledge about it is totally different thing; and philosophers should be very careful when making the distinction between them.

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