Restructuring the Real:
A Study in Scientific Realism Debate

DOCTOR OF PHILOSOPHY
IN
PHILOSOPHY

by

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JUNE 2012

Restructuring the Real:
A Study in Scientific Realism Debate

A Thesis Submitted to the University of Hyderabad in Partial Fulfilment of the Requirements for the Award of the Degree of

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CERTIFICATE

This is to certify that the thesis titled **'Restructuring the Real: A Study in Scientific Realism Debate'** submitted to the University of Hyderabad in fulfillment of the requirements for the award of the Degree of Doctor of Philosophy in Philosophy is a bona fide record of original work done by **Mr. Sreekumar J** during the period of his study in the Department of Philosophy, University of Hyderabad, under my guidance and supervision and that the thesis has not been submitted to any other University or Institute of learning for the award of any degree.

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DECLARATION

I hereby declare that this thesis titled **'Restructuring the Real: A Study in Scientific Realism Debate'** submitted for the award of the degree of Doctor of Philosophy in Philosophy to the University of Hyderabad, embodies the result of bona fide research work carried out by me under the supervision and guidance of **Prof. S. G. Kulkarni.** It has not been submitted to any other University or Institute of learning for the award of any degree.

Signature of the Candidate

Date: **29.06.2012**

Place: **Hyderabad**

Sreekumar J

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Acknowledgments

First and foremost, I am grateful to Soumya for the love, care and support at all times. Without her patience, this thesis would have been impossible. I am indebted to the motivation and support of my parents and sister who stood by my side always.

I am thankful to Prof. S. G Kulkarni, my PhD supervisor for helping me develop this thesis. He has a rather uncanny skill of expressing thoughts in apt words. His imagination and sense of humor are unparalleled. I have really enjoyed listening to his thoughts in philosophy as well as in matters of everyday issues. I am indebted to him especially for the meticulous correction of my drafts and also for commenting on it. His stunning scholarship in several areas in philosophy, and particularly in the philosophy of Kuhn and Kant helped me shape my beliefs.

Prof. Prajit. K. Basu has always been a major influence in my days in Hyderabad starting from my MPhil in 2005. He was always on the ball; be it philosophy, teaching, discussion, advice or motivation. I thank him for commenting on my drafts and discussing with me on a variety of philosophical issues at various times. Prof. Basu is clinical in his philosophical pursuits, always looking for intricate details and flaws in arguments. I thank him especially for his professionalism and work ethic which have already left lasting influences on me.

I am grateful to Prof. Amitabha Dasgupta, Prof. R C Pradhan, Prof. Raghuramaraju, Dr K. S Prasad, Mr. Ananda Wazalvar, Dr. Ananda Sagar, Dr. Venusa Tinyi and Dr Kavita Chauhan for their help and support. I thank the department teaching fraternity for the motivation in terms of conducting activities in academic philosophy.

I am indebted to Prof. Nizar Ahmed for his constant encouragement. Without his influence and motivation, I would not have ended up here. I thank him especially for the wonderful discussions on various issues at his home in Kalady as well as in Calicut.

I thank Prof. Sayd A Sayeed for lending motivation and support at all times. I am indebted to Prof. Priyedarshi Jetli for the wonderful correspondence and for his efforts in going through my writings.

I thank Prof. Stathis Psillos for luring me into the scientific realism debate. I am grateful to Prof. Anjan Chakravartty for giving me the kick start in the debate back in 2007. Apart from these two, there are a bunch of thinkers who helped me shape this thesis. I thank them all. I am indebted above all to Prof. Uskali Maki and Jacob Busch for providing me valuable literature.

Philose and Prasanna, two superb friends without whom my days in Hyderabad would have been colorless. I have learned a lot from my discussions with Philose on various aspects. I thank both of them.

I am thankful to Robin for commenting on my thoughts and discussing philosophical issues with me often. I thank Jolly, especially for his help at the basket ball court. I am thankful to Ali Asghar for various helps. I thank Sreejith, Shinod, Alem, Roshan, Shinumol, Saneesh, Ajay, Sunkhanna, Ashok, Haribabu, Jayasree and all other friends in the department (apologies if I had missed any).

I am indebted to my friend, late Anandaroop Bhattacharya, whose incredible sense of humor made me forget all worries in those wonderful years of 2008 and 2009. I thank my friend Antony J Kuttencherry for being supportive at all times and in giving me constant motivation. I learned several things in life from my associations with Batta and Kuttencherry.

I thank Santosh Peter and Dhanesh Kumar for keeping in constant touch and supporting me always. Talking to them eases the difficulties in life and makes my mind free of concerns.

I thank all the office staff (past and current) of the philosophy department; Sattiah, Nagarani, Praveen, Swaroopa Rani, Prakash, Jyoti and Gayatri.

I thank the University of Hyderabad for giving me wonderful facilities to work on, especially the academic environment and infrastructure. I thank the University Grants Commission for funding my research for the past five years.

Sreekumar J

It is a pure impossibility to hit on the exact knowledge of some reality, even by chance, for a coincidence between thought and reality is a genuine epistemological monster!

Gaston Bachelard

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ABBREVIATIONS

EDR:	Explanationist Defence of Realism
ERS:	Empirical Reliability of Science
IBE:	Inference to the Best Explanation
NMA:	No Miracle Argument
PI:	Pessimistic Induction

CHAPTER I

THE PROBLEM

I.1 Preliminaries

Analysis of the scientific realism debate is a novel trend in the literature of philosophy of science. Hitherto there has been a large amount of literature in various positions of the debate, starting from early positivism to structural empiricism and from conventionalism to scientific realism. Papers and books were devoted to explicate the nature of each of these positions and the varieties of ways in which one showcases the upper hand. However, recently the trend shifted to a curiosity about the debate itself. Thinkers started to address the trajectory of the debate, its nature, the worthiness of the debate etc. This constitutes a new field within general philosophy of science, namely the scientific realism debate. This trend started with the work of Alison Wylie (1986). Works of Hoyningen Huene and Eric Oberheim (1997), Uskali Maki(2005) and Juha Saatsi(2011) are a few examples of meta level enquiries about the debate, and here, the effort is to get a bird's eye view of the debate, assess the discourses involved and more particularly analyze the ways in which various concepts in the debate play their roles.

To be more precise, in the recent years, efforts have been made to analyze concepts such as 'truth', 'reference', 'representation' etc. and their connotation in the debate itself rather than in individual positions. In the early years, thinkers were interested to develop individual positions which constituted the texture of the debate. Now more and more attempts have been made to firstly, evaluate concepts entrenched in particular positions of the debate and secondly, evaluate the constitutive features of the debate as a whole. For example, rather than attending to the notion of truth in Thomas Kuhn's position or in Stathis Psillos' position, thinkers are interested to address the differences in the way 'truth' is elaborated by the two, and in effect its undertone to the debate as a whole, such as why the debate is or is not on the right direction?

This dissertation falls in a category where both these trends in the history of philosophy of science are incorporated. I discuss various positions in the scientific

realism debate in response to the challenge from history of science in order to take a bird's eye view. In addition, I take a more general stand towards the end where comments on the debate itself are made. Having made this slight remark on the trend in the literature on scientific realism and the location of the thesis in it, let us get into a brief history of the debate.

The thesis of scientific realism is to be taken differently from other realisms in philosophy. One can be realist in mathematics (belief in the existence of numbers), ethics (belief in the existence of ethical norms) or more generally realist in metaphysics, for example, belief in the existence of universals. However, barring episodes like the Mach-Boltzman controversy, scientific realism is a relatively young position which largely found attention in the twentieth century. It has incorporated tenets from the rationalism – empiricism era as well. The metaphysical stand of scientific realism about the existence of a mind independent world is mostly an offshoot from the eighteenth and nineteenth century discussions in philosophy. On the other hand, scientific realism evolved with the advent of developments in science. The early twentieth century developments in science, particularly in physics triggered a serious assessment of the appearance-reality discussion. Science moved beyond the explanation of appearances and came up with a series of theoretical entities which were not to stay in confinement of observational world. The notion of electron puzzled thinkers more than any other theoretical entity in the entire history of science. Scientists slowly moved beyond the canvass of appearance and observation to the realm of unobservables. Here lies the pivotal shift in the history of philosophy of science. Many believed in the existence of unobservable entities whereas many took them to be tools to explain and predict meaningfully. The question is, are we justified in believing in the existence of unobservable entities stated by theories? Scientific realism is a position which answers this question on the affirmative whereas antirealists answer in the negative.

The issue of unobservables forms the metaphysical component of scientific realism. Further there is an epistemological and semantic component associated with it. The semantic aspect of realism (also known as semantic realism) interprets scientific theories literally and claims that theories have truth value. The epistemological

component of realism is couched in a claim that theories are well confirmed and are true. In other words, theories give us knowledge about the world (observable and unobservable). These three aspects will be discussed more in the course of this chapter. In the early and mid years of twentieth century, the debate mainly revolved around semantic realism. The logical empiricist tradition believed that theoretical entities do not exist and everything that is said about a theoretical entity can be ultimately translated into observational language. For example, instead of talking about electron, we can talk about the 'white streak in the cloud chamber'. This stand by the empiricist was finally doomed when the observational term-theoretical term distinction was questioned. Finally the indispensability of theoretical terms was agreed by most philosophers of science.

With this admittance of the indispensability of theoretical terms, history of philosophy of science witnessed a crucial shift in the locus of the debate. From semantic realism, the locus shifted to epistemological tenets of realism. The question is, can we justifiably believe in the theoretical assertions about unobservables? The locus of the debate over scientific realism and the antagonists changed. Earlier, Empiricism, positivism and instrumentalism contended scientific realism in the semantic aspects. i.e. how to interpret the meaning of theoretical terms? In the recent years, the greatest adversaries are Larry Laudan and Bas van Fraassen. Laudan's attack on realism forms the entry point to the debate over scientific realism. It will be discussed in detail in the second chapter. Bas van Fraassen accepts part of the metaphysical stand of scientific realism and completely concedes semantic realism. However he does not believe that scientific theories do give us knowledge of the unobservables. He challenges the epistemological stance of scientific realism. The position of constructive empiricism (structural empiricism) is not only a challenge to the scientific realists but also to the selective skeptics like structural realists and entity realists.

Having mapped this short history, let us now briefly look at the problem at hand. Even though the track record of science in terms of frequent theory changes is agonizingly dismal, science nevertheless is a successful activity. Success here means achievement of empirical ends such as explanation, prediction and retrodiction. In other words, science is immensely reliable empirically. I call this the empirical reliability of

science (ERS). We are able to do a lot of empirical deeds such as launching of rockets up the orbits, accurately make interactions in the cloud chamber etc. One needs to answer why science is empirically reliable. At least a legitimate explanation is called for such a successful enterprise. Scientific realists and anti realists respond in different ways. Scientific realists respond to ERS in a simple and intuitively compelling manner. They respond that science is successful because the theories which are responsible for success are true. Theories which are used in prediction and manipulation are maps of reality. Laudan points out that history of science is full of successful yet false theories. Thus, history of science itself is the greatest roadblock in believing the thesis of scientific realism. Bas van Fraassen quickly points out that scientific realists explanation for ERS is not the case and opines that theories are successful because they are born to be successful. It is an evolutionary explanation. His analogy is that an organism survives in the surrounding because the organism adapted the right abilities at the right time. Theories have the features necessary for empirical reliability on the basis of their entrenchment, otherwise they would not have found the limelight. Explanation of ERS, lies in the fact that roughly, theories are right about the world and it is also the defense of scientific realism. The 'no miracle argument'(NMA) which is otherwise known as the 'explanationist defense of scientific realism' (EDR) constitute the scientific realists' reponse to ERS as well as their single major defense. Laudan's and van Fraassen's challenges are discussed in Chapter II. In this chapter, Scientific realist's response to ERS is discussed in detail, and this forms the significance of the problem because scientific realists take their explanation of ERS to be their defense too.

In addition, in this chapter, I intend to elaborate scientific realism by outlining its tenets in a simple manner. The tenets are put forth in order to see how different positions distance themselves from scientific realism. I also outline types of realism and antirealism which figure out in the debate. These positions will be discussed in greater detail in other chapters. I follow the most recent thinkers in doing so. I omit discussions on semantic realism, which I believe is altogether a different issue arising out when we juxtapose scientific realism with logical empiricism. Therefore, I do not discuss issues related to theoretical term-observational term divide or issues related to reference in this thesis.

I.2 Significance of the Debate: Explanationist Defense of Scientific Realism (EDR)

EDR or the 'no miracle argument' (NMA) is considered to be the single major argument for scientific realism. Realists argue that the empirical success of science is no less than a miracle had theories been not hitting truth. Recent commentators have softened their stances on the account of truth. Many believe that anything and everything a theory assert need not be taken as true, but as truth-like. The predictive success of scientific theories is explained by the realists by linking it with truth. Psillos' views on NMA and his defense of EDR, are considered to be the standard position amongst the scientific realists. I believe that after Boyd and Maxwell, the best defense of scientific realism is carried out by Psillos. I start by giving brief accounts of several versions of the 'no-miracle arguments' elaborated by Psillos' and then move on to his own version.

Putnam is said to be the designer of the 'no miracle argument' (the term was coined by him). Almost all the literature on NMA starts by giving Putnam's version of NMA. Putnam gave the single most striking argument for the defense of realism and then left it unnurtured. But I feel that many philosophers before Putnam had felt the need for an explanation for the empirical reliability of science. This is evident in Psillos' work. The best example he gives is that of J. J. C Smart, who believed that the success of science is to be explained in terms of truth, otherwise it's nothing other than a *cosmic coincidence* where theories turn out to be successful.

> Is it odd that the phenomenon of the world should be such as to make a purely instrumental theory true? On the other hand, if we interpret a theory in the realist way, then we have no need for such a cosmic coincidence: it is not surprising that galvanometers and cloud chambers behave in the sort of way they do, for if there are really electron, etc., this is just what we should expect. (Smart, J. J. C, 1963: 70)

Smart's argument seems to be an aged version of the 'no miracle argument. According to Psillos, Maxwell also came up with similar concerns, and it was Maxwell who came up with the idea of defending the reliability of scientific method while explaining the empirical success.

> As our theoretical knowledge increases in scope and power, the competitors of realism become more and more convoluted and ad hoc and explain less than realism. For one thing, they do not explain why theories which they maintain are mere cognitively meaningless instruments are so successful, and how it is that they can make such powerful, successful predictions. Realism explains this very simply by pointing out that the predictions are consequences of the true propositions that comprise the theories. (Maxwell, G. 1970: 12)

Maxwell's words bring out a peculiar tendency of scientific realists, that of constantly seeking a connection between success of science and truth of theories. Also, seeking the justification of the reliability of scientific method, they maintain that the truth of theories also implies that the method used for developing these theories must be reliable. If these methods are not leading to truth, the whole argument of NMA degenerates. Thus the scientific realist somehow found NMA to be very appealing precisely because of the scope it provides. This link between empirical success of theories and their truth, and the reliability of scientific method, I believe, is the backbone of the position of scientific realism. Boyd, Putnam and Maxwell, all seem to emphasize this rather interesting point by saying that their version of NMA was arrived at by using the method of science itself. In other words, the defense of realism is carried out by using the method of reasoning used in scientific practice- abductive reasoning or ampliative inference. These thinkers take the explanation of the empirical success if science (theories hitting truth) as the best explanation for the success of science and say that their position is arrived as an instance of inference to the best explanation (IBE). Putnam's argument in his own words is the following,

> The positive argument for realism is that it is the only philosophy that does not make the success of science a miracle. That terms in mature scientific theories typically refer [this formulation is due to Richard Boyd], that the theories accepted in mature science are typically approximately true, that the same terms can refer to the same even when they occur in different theories – these statements are viewed not as

> necessary truths but as part of the only scientific explanation of the success of science, and hence as part of any adequate description of science and its relations to its objects. (Putnam, H. 1975: 73)

Let me bring out two assumptions the realists are making when they venture in to the explanationist defense, they are:

1) A necessary relation between success of science and the truth of theories.
2) This relation depends on the fact that theoretical terms do refer or theories map reality.

There are thinkers who explain the empirical success of science without bringing in the idea of truth. van Fraassen does this by explaining the success of science in terms of a Darwinian insight, of *survival of the fittest*. van Frassen shares the concern that NMA is very appealing but nevertheless not logical. He says,

> The success of science is not a miracle. It is not even surprising to the scientific 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 have latched on to actual regularities in nature. (1980: 40)

According to van Fraassen's explanation, it is not at all a surprise that theories are empirically successful. If we operate the Darwinian principle of the *survival of the fittest,* theories are empirically successful because they are (were) the fittest among their competitors. Here, it should be noted that van Fraassen has given up the notion of truth, he is interested only in universal regularities. Fittest theories latch on to these universal regularities. Psillos entertains what van Frassen repudiates as the strength of scientific realism. The strength of NMA lies according to Psillos in the following characteristics.

1) It is intuitively plausible,
2) Intellectually persuasive, and
3) Rational to accept.

According to van Fraassen, it seems that NMA is not logically compelling. One can easily figure out that the force of NMA lies in the above three characteristics and not in the logicality of the argument. The simple concern that the success of science needs to be explained by facilitating an explanation based on truth of theories seems to be very appealing. But when one defends NMA on the basis that it is intuitively plausible, intellectually persuasive and rational to accept, the realist has many contenders. Unless the realist can logically bring about the link between success and truth, van Frasssen says that there are equally better explanations which explain the empirical success of science without linking the idea of truth and empirical success of theories, and these explanations are also intuitively plausible, intellectually persuasive and rational to accept[1]. Psillos straight away disagrees with van Fraassen. He says that van Fraassen's account of the empirical success of science is *phenotypical*:

> [It] provides an implicit selection mechanism according to which entities with the same *phenotype*, i.e. empirical success, have been selected. But a *phenotypical* account does not exclude a *genotypic* account: an explanation in terms of some underlying feature which all successful theories share in common. (1999: 96)

As an example, Psillos asks us to consider a group of people having red hair. Also suppose that an explanation for this observation is that, it is a sort of club where only red haired people were given membership. This is a phenotypical explanation where only the similarities are emphasized. In the case of success of science, a phenotypical explanation of the sort which van Fraassen gives is that theories are successful because they belong to this 'ought to be successful' group or club. But a genotypical explanation for the group of red haired people must say why one or the other of that group has red hair. May be it amounts to come up with a genetic feature in their constitutive body types. This is what the realist does, when she claims that the empirical success of scientific theories is not because they are born to be successful, but because they hit truth. Truth is the genotypical

[1] I elaborate Van Fraassen's critique of NMA and IBE in a much detailed manner in the chapter II when I consider his position.

feature by which scientific realist explain success. Psillos says that a genotypical explanation is to be always preferred over a phenotypical explanation.

The voraciousness of realists can be seen when one examines NMA closely. After their initial project of bringing in the connection between success and truth, the realist straight away gets on to the job of justifying the reliability of scientific method. This happens precisely because the link between empirical success of theories and truth opened a new door to them- the insight about what makes a theory true, which is the method one uses in arriving at these theories. Moreover, when one tries to bring in the justification of the reliability of scientific method by following IBE, then it turns circular. Hardcore supporters of realism want NMA to be an argument constituted by the method of science (Boyd, Maxwell, Psillos *et al*). This way, one can be sure about its plausibility but if one questions the reliability of IBE and abductive reasoning, NMA runs into trouble. Psillos shows that Maxwell categorically says that he accepts NMA precisely because it follows the method of science,

> My reasons for accepting realism are of the same kind as those for accepting any scientific theory over others which also explain current evidence. (Maxwell, G. 1970: 71)

Psillos tries to build upon this feature of NMA that it also lends support to the credibility of scientific reasoning. In other words, an explanation for the success of science preserves, according to the realists, the reliability of the method of abduction. What else they need because NMA is an instance of shooting two birds with one stone. It explains the success of science as well as the reliability of scientific method.

In articulating their position of NMA, the scientific realists make many desirable moves. One of which is their elegant proposal about conceding the argument from theory-ladenness[2]. The realists are very clever in admitting the view that theory-ladenness is an undisputed matter in philosophy of science. The reason for this admittance is that even if it is allowed that there is theory-ladenness in science, especially in predictions and

[2] By theory-ladenness of scientific method, Psillos has in mind the anti-empiricist idea of scientific method according to which observation is prior and theory is posterior-a methodological position first acknowledged by Popper in the twentieth century philosophy of science.

in setting up of experiments, the realist is confident of defending her thesis of the empirical success of science. It is a bigger achievement for the realists when they explain the empirical success of scientific theories where the whole method is theory laden, rather than explaining success of theories in a non-theory-laden environment of scientific practice. This boosts the persuasiveness of their argument that they attained an explanation even with the unavoidable doom of theory-ladenness. This, in some way, strengthens their metaphysical position of there being a mind-independent unobservable world. The claim that theory-laden method aptly tracks the constituents of this mind-independent world implies that realist position of theories hitting truth is a correct thesis.

> All aspects of scientific methodology are deeply theory-informed and theory-laden. In essence, scientific methodology is almost linearly dependent on accepted background theories: it is these theories that make scientist adopt, advance or modify their methods of interaction with the world and the procedures thay use in order to make measurements and test theories.
>
> These theory-laden methods lead to correct predictions and experimental success.
>
> How are we to explain this?
>
> The best explanation of the instrumental reliability of scientific methodology is that: the theoretical statements which assert the specific causal connections or mechanisms by virtue of which scientific methods yield successful predictions are approximately true.(Psillos, S. 1999: 76)

Psillos here concedes to the theory-ladenness of scientific method and comes up with a risky position. He wants to say that if we admit theory-ladenness and still come up with successful predictions, nothing other than theories being true is a possible explanation. This position also cements the reliability of abductive reasoning. The theories are true because they relied on abductive or ampliative reasoning. According to Psillos, the structure of EDR is a narrative constituted by a two tier abductive reasoning. The first abduction can be viewed as the usual scientific practice of arriving at theoretical

knowledge (what scientists normally do). The second is a meta-abduction of crediting truth to these theories while we explain the fact of empirical success. In other words, EDR explains the success of science by ascribing truth to theories and, brings about the reliability of abductive reasoning. One can easily see the flaw in EDR, that it invites circularity. EDR uses the very rule which is used in scientific reasoning. Let us indulge a bit more deeply into the whole argument of EDR.

The premisses of EDR are theory ladenness of scientific method , empirical success of theories etc. From these information, we draw the conclusion that theories hit truth. This is an abductive reasoning process. 'Theories hitting truth' are considered to be the best explanation for the success of these theories. But while doing this, the conclusion speaks about abduction too, because the claim that 'theories hit truth' indirectly points to the method of arriving at these theories, namely abduction. Hence it is abduction that is responsible for the truth of theories. Also it is abduction that is responsible for the explanation for the truth of these theories. First one, a particular abduction whereas second one, a meta-abduction. Psillos engages in an analysis to assess whether EDR is really circular. According to him, there are two kinds of circularities that are common in philosophical discourses,

1) Premiss circularity
2) Rule based circularity

In premiss circularity, if the conclusion is stating about at least one of the premisses, then the explanation lacks credence. All viciously circular arguments are premiss circular. That is, the conclusion directly assumes the premiss for its formation. In a standard circular argument, the conclusion is either identical to the premiss or a mere paraphrase of one of its premisses. But the scientific realist says that just by saying that the premiss is identical with the conclusion does not grant one the right for accusing circularity, because it depends a lot on the explanation one seeks and the context. For example, the argument type 'p, therefore p' is not circular if it is used to explain the logical notion that every sentence is a logical consequence of itself. But this would be a viciously circular argument if it is used to show that 'p' is true. Because, then it would pretend to prove that 'p' is true where it just assuming that 'p' is true.

Psillos suggest that Braithwaite had given a detailed analysis of circular arguments. Psillos subscribes to Braithwaite's views in analyzing the circularity associated with NMA. According to Braithwaite, if one claims to offer an argument for the truth of 'A', but explicitly presupposes 'A' in one's premisses, then that argument is viciously circular. It is clear that NMA is not viciously circular because its premisses neither explicitly nor implicitly state the information of the conclusion. The empirical success of science is explained in terms of the truth of theories. The conclusion states that scientific theories are true by using an abductive reasoning process. Now, let us move to the second type of circularity- rule based circularity.

> [It] begins with the premisses P_1,P_2……Pn, and then , by employing an inference rule R, draws a certain conclusion Q. however, Q has certain logical property: it asserts or implies something about the rule of inference R used in the argument, in particular that R is reliable.(Psillos, S. 1990: 80)

It seems somewhat evident that NMA is rule-circular. It explains the success of theories by using a rule of inference (abduction) and then asserts about the reliability of the same inference rule. Braithwaite and others, nevertheless, did not consider rule based circular arguments as viciously circular. Therefore, Psillos has the opinion that the charge of circularity does not apply to NMA. What happens in NMA is as follows: The premisses of NMA assert the theory-ladenness of scientific method and its widely accepted empirical success. Then by means of a meta-IBE, the argument concludes that theories in science are true. These true theories are typically arrived by applying first-order IBE's. This idea of theories being arrived by IBE's together with the meta-IBE (theories being true) entail the thesis that IBE is reliable. It is to be subtly analyzed to see whether there is an instance of rule-circularity in this particular argument. The interesting question though, is that, do we need to justify the reliability of a rule before using it to draw a conclusion? Psillos categorically says that scientific realists have no option but to subscribe to an externalist account of justification in the issue of reliability of a rule of inference. He says,

> When an instance of a rule is offered as the link between a set of (true) premisses and a conclusion. What matters for the correctness of the conclusion is whether or not the rule is reliable, that is, whether or not the contingent assumptions which are required to be in place in order for the rule to be reliable are in fact in place. If the rule of inference is reliable (this being an objective property of the rule) then, given true premisses, the conclusion will also be true (or, better, likely to be true-if the rule is ampliative). Any assumptions that need to be made about the reliability of the rule of inference, be they implicit or explicit, do not matter for the correctness of the conclusion. (Ibid: 83)

In NMA, whether the reliability of IBE is to be assumed before coming up with the conclusion about the truth of theories is a contentious matter. The debate revolves around the externalist's and internalist's account of epistemic justification. I will not venture into this terrain at this moment and would like to leave the doors open. The realists in the end stick with an externalist position and say that NMA does not have to assume anything about the reliability of IBE. The debate has moved outside the issue of circularity and is to be fought on an epistemological terrain. Psillos says that NMA should not depend on IBE for its defense of realism. Even after all this intense debate, Psillos comes up with a sharp analogy used by Ramsey to establish that there is nothing wrong in using an inference rule to bring into notice its own reliability.

> It is only via memory that we can examine the reliability of memory. Even if we were to carry out experiments to examine this, we should still rely on memory: we should have to remember the outcomes of the experiments. But there is nothing vicious in using memory to determine and enhance the degree of accuracy of memory. For there is no reason to doubt its overall reliability. (Ibid: 86)

This analogy, Psillos says, can be applied in the case of inference rules also. Rule based circularity, in the end is not as contentious as it seems at the surface level. Psillos concludes his EDR with the following words,

> NMA does not make IBE reliable. Nor does it add anything to its reliability, if it happens to be reliable. It merely generates a new belief about reliability of IBE which is justified just in case IBE is reliable…..
>
> Suppose we granted that NMA aimed to defend the reliability of IBE. This is certainly not excluded by externalism. It is just optional. Would the mere fact that the defense relies on a rule- circular argument make the attempted defense vicious- and hence lacking in rational force? I do not think so. If the rule-circularity of a defense is taken to be an outright vice, then we should simply have to forgo any attempt to explain or defend any of our basic inferential practices. What this implies is that even internalist defenses, ultimately, will have to rely on rule-circular arguments. When it comes to the defense of our basic modes of reasoning, both ampliative and deductive, it seems that we either have no reasonable defense to offer or else the attempted defense will be rule-circular. (Ibid: 86)

NMA definitely is rule-circular, but not viciously circular. As far as the realist is not commenting anything about the reliability of ampliative inferences, they are in a better position. Trying to kill two birds with a single stone fetched more worries than joys. In emphasizing the link between empirical success and truth, the realist saw an opportunity to affirm the reliability of scientific method and grabbed that opportunity with both hands, this I believe was a move that was not quite required.

The above discussion shows how important is the linking of success and truth for the scientific realist and how important it is for the antirealist to delink notion of truth from success. We may now look at scientific realism in terms of its various dimensions. The discussion also shows what appears to be a self evidently correct argument in favor of scientific realism is not really so, and hence, the scientific realism debate cannot be settled that easily.

I.3 Scientific Realism and its Tenets

Scientific realism has a set of tenets which helps us to see how the other positions differ from it. While carrying out this, I would also like to briefly hint how some positions locate themselves in the overall debate.

I.3.1 The Metaphysical Stance: Existence of a Mind-independent world and Mind-Independence of the Known

> The reality which scientific theories describe is largely independent of our thoughts or theoretical commitments. (Boyd, R. 1983: 195)

Richard Boyd in his well received paper 'On the Current Status of Scientific Scientific Realism' comes up with the above statement which explicates the existence of a mind-independent world (scientific theories are about this world). He regards this as one of the several aspects of the thesis of scientific realism, but an important one nevertheless. He goes further and says that we can rely on our epistemic abilities in such a way that they provide more or less exact representations of this mind-independent world. Here is the metaphysical bite in the thesis of scientific realism, that our epistemic means do not in any way affect the exactness of the representations we arrive at. This in fact is an epistemological issue and will be discussed in more detail later. The claim about the existence of a mind-independent world is what I regard as the most important and essential component in the thesis of scientific realism. Crispin Wright puts it in a similar fashion to that of Boyd. Wright takes it to be a modest thought.

> The modest kind of thought concerns the independence of the external world- for example, that the external world exists independently of us. That it is as it is independently of the conceptual vocabulary in terms of which we think about it. Fully fledged modesty has it that human thought is, as it were, at best a map of the world. Maps can better or worse represent the terrain which they concern. But nothing about that terrain will owe its existence, or character to the institution of cartography or to the conventions and techniques therein employed. (1994:1)

Wright's statements best capture what it is meant by what I call 'the mind-independence of the known'. It not only asserts that there exists a mind-independent world but also that our representations capture facts about this mind-independent world without in anyway affecting the constitutive features of the world. This means that the features of this world owe nothing to its existence or character to the method of arriving at them. This aspect of scientific realism makes it clear that our epistemic means and conceptual schemes do not control the information about the world. The non-epistemic character of the things that are posited in the mind-independent world is highlighted in this stance of scientific realism. I believe, this metaphysical stance (about the existence of a mind-independent world) is the starting point to a meaningful discourse on the thesis of scientific realism. Psillos calls this the 'metaphysical stance'.

> The metaphysical stance asserts that the world has a definite and mind-independent natural-kind structure. (1999: xvii)

Here, Psillos is reiterating what others have said so far. He takes the metaphysical stance to be the basic philosophical position of scientific realism. He says that this thesis alone will necessarily differentiate scientific realism from idealism and phenomenalism. It can even help in legitimately distancing scientific realism from the most recent verificationist accounts of Michael Dummett and the position of later Hilary Putnam (which reduces the content of the world to a set of epistemic conditions). Psillos further argues that,

> [The] metaphysical stance implies that if the unobservable natural kinds posited by theories exist at all, they exist independently of humans' ability to know, verify, recognise, that they do. Instead of projecting a structure onto the world, scientific theories, and scientific theorising in general, discover and map out an already structured and mind-independent world. (Ibid)

These sentences reflect what Wright calls the modest thought of scientific realism. Psillos also formulates further on the modest thought. He says that the mind-independent world has a definite natural-kind structure. Scientific theories describe these natural kinds

(contents of the mind-independent world) independent of any conceptual schemes. In fact, the contents of the mind-independent world are already structured. Theories just pick them out. The means of arriving at or theorizing about these natural-kind entities has nothing to do with the existence or nature of them. Psillos also comes out with a different manner of articulating the metaphysical stance. According to him, the world is comprised of truth-makers of our propositions.[3] Propositions in scientific theories are true or false according to these truth-makers. Truth-bearers are linguistic in nature and truth-makers are non-linguistic. Let us also make it clear that 'truth' is attributed to propositions whereas 'reality' is attributed to the world. Thus, in this context, truth-makers are at the realm of world (reality) and truth-bearers are at the realm of propositions (representations). What Psillos aims to assert here is that truth of the propositions and thus, the reality of the entities posited are not in any way dependent on the epistemic conditions as it is in the case of positions upheld by Dummet and later Putnam.[4] Truth is understood as a non-epistemic concept, and this precisely emphasizes the metaphysical stance of scientific realists.

The very notion of a non-epistemic idea of truth will necessarily bring about the notion of a mind-independent world. Of course it is possible not to subscribe to this two-tier model of representation and world or truth and reality. Michael Devitt and Ian Hacking believe that scientific realism is independent of truth, and can thus, be taken to be independent of representations. But this single-tier model also hinges on the existence of a mind-independent world. A classic example of this single-tier model is that of entity realism initially proposed by Hacking.[5] Hacking's position is the first of its kind where the existence of a mind-independent world is asserted without relying on any notion of truth or representation. Unlike Hacking, thinkers like Ellis and Thomas Kuhn assert the existence of a mind-independent world by relying on an epistemic conception of truth.

[3] He allows at the same time that there could be truth-makers for which we may not have truth-bearers. This is altogether a different issue, that of 'excess content in the world'.

[4] An elaborate discussion on this topic is carried out in chapter III by also including positions of Thomas Kuhn and Brian Ellis.

[5] Hacking's position is warranted by the epistemic virtue of causal-contact. For Hacking, representations give way for interventions. Hacking's is a single-tier model of realism. See Sankey, H. (1994) , 'The Semantic Stance of Scientific Entity Realism', *Philosophia, Vol 24, Nos 1-2*: pp 405-415.

They are realists but of a different genre. Thus, it is clear that there are two different ways of taking up a realist position with a metaphysical stance. One is by endorsing the more traditional two-tier: the truth-reality / the representation-world model. The other is the single tier model, that of reality/world alone.[6] The single tier model is adopted by Devitt and Hacking. The two-tier model is subscribed by Boyd, Psillos, Kuhn, Ellis and Putnam. The two-tier model according to Psillos is a philosophical discourse in which we see realism as a view about theories. The former on the other hand is a philosophical discourse that sees realism as a view about the world. The two-tier model of realism therefore is a talk about theories or representations which are about a mind-independent world. The single-tier model of realism is a talk about the world alone. One of the difficulties of the single-tier model of realism is to come up with a philosophically credible notion of 'existence' of unobservables, and subsequently to justify that notion. The proponents of single-tier model avoid the language-world relation and thus truth.

Let us be very clear on the above differences. First of all, all the single-tier proponents, even though they avoid the notion of truth and representation, assert the existence of a mind-independent world. Similarly all the two-tier proponents also claim the existence of a mind-independent world in their respective positions. The following table will make it clear.

	Two-tier model		Single-tier model	
	Psillos /Boyd	Kuhn/Ellis Putnam	Devitt	Hacking
Existence of a mind-independent world.	Yes	Yes	Yes	Yes
Mind-independence of the Known	Yes	No	Yes	Yes

(table 1) Types of Realism and Metaphysical Attitude

[6] Psillos calls the former 'the factualist conception' and the latter the 'fundamentalist' conception. I am not going into the details of Psillos' terminologies. For a detailed discussion See Psillos, S. (2009), *Knowing the Structure of Nature*, New York: Palgrave Mac Millan, p 35.

Let us maintain this distinction. The claim about the existence of a mind-independent world and mind-independence of the known are two different issues altogether. For example, Kuhn maintains that there exists a mind-independent world, but the representations of this world is conceptual-scheme-dependent or epistemically conditioned. Similarly, for him, truth is also an epistemic concept. This distinction (of single tier/two tier models of realism) is of foremost importance and will be maintained from hereafter in my work.

Whether Devitt and Hacking justify their positions by bringing in alternatives to representations is another issue. They agree on 'the mind-independence of the known' precisely on the grounds of a metaphysical belief that unobservables exist. Devitt invokes a metaphysical position of a causal relation between our minds and the world as warranting this existence. Hacking takes rescue in what he calls 'intervention' and 'manipulation'. For the single-tier proponents, the question of mind-independence of the known has to be made sense in terms of existence of unobservables, and the ability to know that they exist. I am not going into the intricacies of this debate. What I wished to show was that, scientific realism, be it of any genre, cannot do away with the metaphysical stance about the existence of a mind-independent world.

I.3.2 The Epistemic Stance: Knowing the Mind-Independent World

Let us now look into the second aspect of scientific realism, namely the epistemic aspect. The 'epistemic stance' as Psillos calls it:

> The epistemic stance regards mature and predictively successful scientific theories as well-confirmed and approximately true of the world. So, the entities posited by them, or, at any rate, entities very similar to those posited, do inhabit the world. (Ibid)

Psillos here emphasizes the role of well confirmed theories. They allow us to know about the mind-independent world. In other words, the mind-independent world is

known through mature and well-confirmed scientific theories.[7] He also believes that this thought consists of an 'epistemic optimism'. This claim will distinguish scientific realism from skepticism and agnosticism. The main facet of this stance is that it lays emphasis on the truth of propositions in scientific theories, and thereby allows us to be committed to the existence of unobservable entities posited by these theories. Boyd also shares similar beliefs as evidenced by his words:

> Scientific theories, interpreted realistically, are confirmable, and *in fact often confirmed* as approximately true by ordinary scientific evidence interpreted in accordance with ordinary methodological standards. (1983: 195)

Boyd here argues that, even with ordinary methodological standards, interpretation of evidence will confirm the theory. This, in fact is a bolder claim as compared to that of Psillos. The epistemic optimism seems to be more in Boyd than in Psillos. Another way to understand the epistemic stance is to say that our current methods of scientific investigation are reliable in coming up with approximately true descriptions about the mind-independent world. This reliability of scientific method gives rise to the epistemic optimism of scientific realists. Wright takes this epistemic optimism to be associated with a presumptuous thought.

> The presumptuous thought, by contrast, is that, while such fit as there may be between our thought and the world is determined independently of human cognitive activity, we are nonetheless, in favorable circumstances, capable of conceiving the world aright, and often, of knowing the truth about it. Not merely is there a good measure of non-accidental fit between the external world and our thoughts about it, but we are capable of winning through the knowledge that this is so, or at least to a perspective from which we may quite justifiably take it to be so. (Wright, C. 1994: 2)

[7] I am not discussing issues related to what would count as 'well confirmed theories' or what one means by 'succesful prediction' or what are 'mature theories'. Realists diverge on their views on such issues. All I am interested here is to elaborate the epistemic stance.

Wright says that even though there exists a mind-independent world and our means of arriving at it in no way modifies the content of the world, we are nevertheless justified in believing the truth of the theoretical assertions in a theory. That means, realism defends such a non-accidental fit between our thoughts and the world. Here is the presumptuousness in realism, the overtly bold and arrogant claim. For many thinkers, this is rather puzzling. If the world is independent of our cognitive activities, how can we possibly know it? In addition, if the world is independent to any of the epistemic conditions, how is it that we can confidently say that our theories are true of the world? How can we be so certain that our best scientific methods are yielding true descriptions of the external mind-independent world?

Many contemporary scientific realists therefore tried to reduce the force of the epistemic optimism or the presumptuous claim. In other words, scientific realists have to bring in epistemic humility as opposed to epistemic optimism in their position. One way to carry out this is by saying that scientific theories miss out on something about reality, or to say that there never is a perfect mapping. This can be made sense by admitting that all that is described by scientific theories are not true in the absolute sense of the term, but near to truth, by invoking the idea that scientific theories are approximately true of the world. We already saw Psillos and Boyd employing such terms. The question here is, how can we make sense of the term 'approximate-truth'? Of course Karl Popper had a not so appealing way of understanding the notion of approximate truth- verisimilitude. Many thinkers regard Popper's notion of verisimilitude as philosophically problematic. Popper's was one of the first attempts to formalize the notion of approximate truth. Subsequently, several novel ideas emerged in connection with the formalization of the notion of approximate truth.[8] Psillos believes that all formal approaches to approximate truth end up in trouble and therefore, we have to form an intuitive approach to truth-likeness. He gives some suggestions and leaves it to be further developed. I am not going into these details as they are not relevant to the problem I am handling here. I mentioned

[8] Psillos discusses in detail most of these attempts. The formal approaches are, Popper's verisimilitude, Graham Oddie's and Ilka Niiniluoto's 'Possible Worlds Approach', and Aronson, Harre and Way's 'Type Hierarchy Approach. Further he discusses intuitive approaches by Ronald Giere and then his own version. See Psillos, S. (1999) *Scientific Realism: How Science Tracks Truth*, New York: Routledge. p 252.

these approaches in order to claim that the epistemic stance invited more trouble than gains for the scientific realists. Nevertheless, by introducing the term 'approximate-truth', scientific realists were able to bring in epistemic humility and avoid the charge of presumptuousness, even though the term in itself needs further elaboration.

The second strategy to accommodate epistemic humility with scientific realism is to subscribe to the semantic conception of theories rather than the syntactic conception. This allows the realist to abort the strict semantic notion of truth (as correspondence) and subscribe to a similarity-based account of the relation between theory and the world. They believe that the term 'similarity' in itself carries an amount of elusiveness enough to supplement the needed epistemic humility. Similarity, unlike correspondence, leaves a gap between reality and representations. The syntactic conception of scientific theories or the received view was initially developed by the logical positivists. It identifies theories with pieces of language. More precisely, theories are axiomatic systems closed under deduction, expressed in a formal language whose elements are characterized by a syntactic structure. The centrality given to the syntax of linguistic formulations led thinkers to call this the syntactic view. As opposed to this, the semantic view takes theories to be characterized by a class of models. Theories are abstract, set theoretic entities. They are models of their linguistic formulations. There are however, many differences of opinion on the exact characterization of the concept 'model', but this is not relevant for the current discussion. What I am trying to evoke is the idea that, if scientific theories are regarded as non-linguistic representations, how would we be able to draw the relation between the external world and these representations? The relation can never be identified with truth, for the very reason that truth is attributed to propositions or linguistic formulations that stand in relation (correspond) to the world. Therefore, many thinkers who deviated from the syntactic view and are also scientific realists tried to develop the notion of 'similarity'. Ronald Giere came up with what he calls 'constructive realism'[9], where representations are non-linguistic (models) and the link to the external world is drawn by bringing in the notion of 'mapping' or by using the predicate 'similar to'. The similarity between a real world-system and the model is understood by what

[9] For a detailed elaboration, see Giere, R. (1985) ' Constructive Realism', in Churchland, P. M, and Hooker, C. A, *Images of science*, Chicago: Chicago University Press, pp 75-98.

Giere calls ‘theoretical hypotheses’ in scientific theories. These theoretical hypotheses’ guide us in drawing the similarity between the non-linguistic abstract model and the world.

Of course, there is no clear-cut, philosophically unchallenged view of the notion of ‘similarity’. But the force of presumptuousness that grew out of the epistemic stance is reduced by subscribing to the semantic conception of theories. The shift from syntactic view of theories to semantic view brought in certain kind of elusiveness in the link between representation and the unobservable world. As a result of this shift, notion of truth gave way to the notion of similarity, and linguistic representations gave way to non-linguistic representations. It is difficult to conclude one over the other, namely the approximate-truth-approach or the similarity-approach in tackling the presumptuousness of the epistemic stance. What I would like to conclude here is that, even though both the approaches leave insufficient accounts of ‘similarity’ and ‘approximate-truth’, they add epistemic humility to the thesis of scientific realism, because both leave a gap (elusiveness) between theory and the world. Let us now see how the scientific realist metaphysics about entities handle the issue of epistemic humility.

There has been a considerable amount of literature developed at the level of metaphysics of relations and properties corresponding to the debates in scientific realism. These are attempts to come up with a plausible understanding of the fundamental features of the mind-independent world. In other words, these investigations are directed towards the characteristics of unobservable entities, their constitutive properties and relations. According to scientific realism, the outside world is populated by both observable and unobservable entities. An Entity is constituted by properties, intrinsic and relational into a bare substratum. Intrinsic properties are those which stand independently and are not related in any sense to properties of other entities, they stand on their own. Relational properties are understood as standing in relation to properties of other entities, or participating in a structure.

One serious question that philosophers of science engage with is whether scientific knowledge leads us to intrinsic properties or relational properties or both? But this talk is definitely debatable and I would not want to bring in serious metaphysical

debates at this point which will force us to deviate from our task at hand. The reason why scientific realists are stranded at their metaphysical stance with this talk of intrinsic and relational properties and substrata is because they presuppose a key notion that when we know the unobservable world, it is both the relational and intrinsic nature that we know, not just one. This position of realists can be better understood when we talk in an old fashioned framework of *structure* (form) and *content* (nature). Realists root for a *structure-nature continuum*. This position is elaborated by Psillos. This view emerged in the scientific realism debate as a response to other contemporary realist positions such as structural realism.

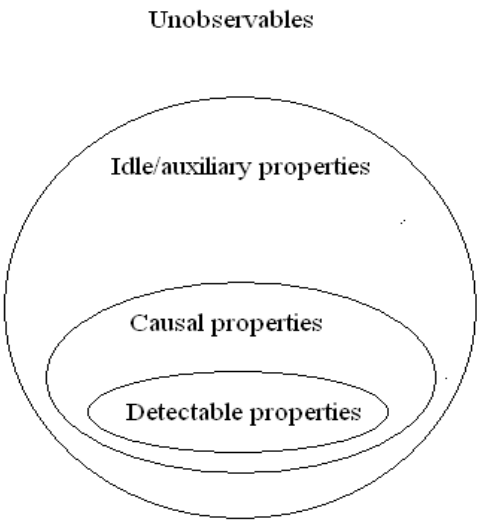
> [The] nature and structure of an entity form a continuum; and….that the nature of an entity, process, or physical mechanism is no less knowable than its structure. (Psillos, S. 1999: 149)

Scientific realism thus evokes the view that, what is known by scientific theories is a continuum of the structure of an entity as well as its nature. In other words structural features of an entity is known together with the features of its nature. It is impossible in any way to know one and not the other. They form a continuum and thus the scientific realist position on entities is an indiscriminate view in which a distinction between relation and relata is impossible in a process of knowing. Relation or relata alone cannot be known separately, they are known together. I am not in favor of proceeding the debate in this lead. Here, I intend to say that scientific realism has an non-discriminate view on the metaphysics of structural properties and natural (intrinsic) properties. That is, while we know an unobservable entity, it is not possible for us to just know the nature of an entity without at least knowing some structural features of it, and it is not possible for us to just know structural features of an entity without in some way knowing some of its nature too. This sums up the non-discriminate view of the realist position regarding structure and nature. The non-discriminate view is sometimes known as 'wholesale' realism as opposed to 'piecemeal realism'. 'Wholesale' in the sense that, scientific theories are approximately true about (or similar to) the mind-independent world by indiscriminating the properties of unobservables. Scientific realism doesn't subscribe to a view which says that we know only specific parts of reality, either structure or nature but

not both. Entity realism and structural realism are such 'piecemeal views'. Entity realism is the view that we know that entities exist, or that we know at least some of the natures of entities through our intervention and manipulation. Similarly, structural realism is the view that we can be sure about structures or relations in the scientific theories, not natures (these will be discussed in detail in chapter III).

Let us proceed further and analyze the subtle and finer metaphysics of entities employed in the debate. Scientific realism in the recent times dropped the vigor of the epistemic stance as discussed before. They no longer argue for an absolute conception of truth. In other words, they show epistemic humility. There are causal properties which can be detected if proper instruments are at place. Further there are idle/auxiliary properties which could be fictional or could be causal properties currently undetectable. Chakravartty (2007) believes that these idle properties are used for theoretical or explanatory reasons. He makes the ground clear by carefully categorizing properties of entities. I too subscribe to the image he proposes that brings epistemic humility in scientific realism.

The issue at stake is that theories miss out on certain aspects of the world. In order make this claim more nuanced, either one has to say that there are no such idle properties which play a constitutive role in forming their nature, or that they are reducible (can be inferred) from other detectable causal properties. The first strategy is a kind of blind realist optimism whereas the second one is a kind of extended empiricism.



(Figure 1) Chakravartty's Classification of Unobservables.

The above illustration shows the following expressions

i) All causal properties are not detectable properties.

ii) Idle/auxiliary properties are part of the nature of an entity. Because they can be causal but undetectable properties.

iii) Idle/auxiliary properties are non-detectable.

These expressions allow us to come to the view that scientific theories, even with ideal epistemic conditions and instruments leave way for something 'unsaid' about the unobservable world. This is the main reason why scientific realism adopted epistemic humility and reduced the presumptuous force in their claims. How are we to make sense of this excess content in the unobservable world?

Hitherto, I discussed two important stances of scientific realism and then the attempt of realists to fine-tune their view by responding to some of the challenges that emerged within the realist tradition. The difference between the two-tier model and the single tier model with regard to the metaphysical stance and the issues related to epistemic humility with respect to the epistemic stance have been discussed. Ways to implement epistemic humility by incorporating views on representation and a

metaphysics of entities are covered. In what follows, I lay out major tenets of scientific realism which can be made out from the discussion so far.

I.3. 3 Other Tenets of Scientific realism (Aim doctrine and Semantic Doctrine)

In this thesis, these two tenets do not play a major role. Therefore I would like to briefly mention them and get on with the debate. The aim doctrine of realism is the single tenet which distances realism from most of the other positions in the debate. The rough idea that science aims at a correct picture of the world is accounted for in the aim doctrine which is sometimes also known as aim realism. To put it in detail, this tenet claims that the aim of science is to arrive at explanatory theories that account for both observable and unobservable world.

The aim doctrine is advocated by realists in order to make their position rationally compelling. They state that it is unintuitive to think of a world where science is practiced just to arrange empirical data. This image of science is feeble and simply uninteresting. Scientific theories are attempts to map out reality and arrive at truth, at least approximately. The option of a world where scientific theories just organize data is highly uncalled for. Except structural empiricists, all other positions subscribe to the aim doctrine. However, the internal realists accept it with an added qualification. The notion of 'entities' and 'kinds' contained in the knowledge of science are not the knowledge of the world as *it is in itself* having a natural kind structure. They are knowledge of the world which is conceptual scheme dependent. Therefore, for the internal realist, science aims to explain the observable world as well as the unobservable world; but the world is not known as *it is in itself*.

Semantic tenet of realism was very significant in the scientific realism debate in the heydays of logical positivism. This tenet claims that scientific theories have to be interpreted literally and both the observational and theoretical terms are indispensible in a scientific language. It is also said that scientific statements have truth values, be it about theoretical terms or observational terms. In the case of semantic doctrine, Bas van Fraassen makes a decisive move. He accepts semantic realism. He allows the literal interpretation of scientific theories. It is clear that the semantic doctrine also has two

components. About the meaning of theoretical terms and observational terms and, a theory of truth with which the scientific realist tradition operates. The externalist positions of semirealism and structural realism accepts correspondence theory and the literal interpretation of theories. Bas van Fraassen and Internal realists reject correspondence theory but accept the interpretative component in semantic realism. Entity realism however rejects both the components of the semantic doctrine.

I.4 Types of Realism and Antirealism

Many types of realism and anti realism are discussed in detail in Chapter III. However, I use this section to see the vantage point where several positions depart from scientific realism. Let us start with anti realism. There are many types of anti realisms depending on the constitutive features within them. For example, Bas van Fraassen's constructive empiricism is anti realistic because of the epistemological position he endorses.

Constructive Empiricism: Bas van Fraassen says that even if theories are interpreted literally and even if there exists a mind independent world, the option of arriving at knowledge of the unobservable world is least rational. He believes that we can never have true picture of the unobservable world. However we can have an empirically adequate picture of the world where unobservable entities occur with explanatory roles. Truth is neither a constitutive feature of the image of science nor a complementary feature. Truth is simply not required for a proper interpretation of science about unobservable world. All that is required is empirical adequacy. van Fraassen argues for an alternative image of science where the measure of scientific enquiry in the unobservable world is empirical adequacy. However he concedes that, in observational world, we can accept the role of truth. He contends the scientific realist on a variety of issues. Firstly, as we have already seen, he proposes an evolutionary explanation to ERS. He admits that the empirical reliability of science needs to be explained, but endorses a phenotypical explanation. Secondly, he deviates from logical empiricists by admitting semantic realism. Constructive empiricists believe that theoretical terms are needed in the vocabulary of scientific knowledge, the meaning of these terms can be interpreted literally. However the knowledge of theoretical entities concerning unobservables is not

possible. We cannot be sure about the existence of theoretical entities. Thirdly, he argues that there is a need for an alternative image of science, where empirical adequacy is the measure of scientific knowledge.

Internal Realism: Internal realism deviates from scientific realism on several features. There are many types of internal realisms which are discussed in detail chapter III. Internal realists admit the existence of a mind independent world. But their worry is whether the world has a natural kind structure embedded in it or simply we humans carve out one upon it. The notion of 'projecting' a structure onto the world becomes important here. Internal realists agree on semantic realism too, however the representations they talk about are not representations of the world as *it is in itself*. These representations are conceptual-scheme dependent. They allow knowledge of the unobservables in the internalist sense, i.e. our language organizes the world into kinds rather than the scientific realist's notion of 'mapping' an already organized mind independent world. Further, all internal realists endorse an epistemic account of truth (Putnam, Kuhn and Ellis). The epistemic notion of truth compromises the externalist characteristic of 'what is known'. Internal realists like Putnam propose that knowledge of such a mind-independent world is not only impossible but also unintuitive. The model-theoretic argument, the brain in the vat thought experiment etc. are all attempts to show that a piece of language can never refer to the entity which exists in a mind independent world. Putnam opines that truth must be 'ideally justified'. The ideal conditions of human enquiry are what are termed as true. This forms a neo Kantian position.

Entity Realism: Entity realism was initially developed by Ian Hacking. This position is also known as experimental realism. In simple terms, it endorses the view that entities exist in the mind-independent world. The uniqueness of this position is not in this simple metaphysical claim, but in the assertion that the warrant for such a belief in the existence of entities comes not from confidence in the truth of our best representations of the world, but from the interventions we make with the world. Entity realism is the view that one has good reasons to believe that entities exist in a mind-independent world. It is this aspect of a scientific theory, the claims about the existence of specific entities, that one can reasonably believe to be real. This can be said as the positive thesis of entity

realism, but there is also a negative thesis. While endorsing certain existential claims, entity realism is skeptical about theories in which these entities are described. Entity realism here comes with a bolder claim. They are skeptical about theoretical descriptions of these entities (which they believe to exist). They doubt the credibility of theoretical descriptions and also the truth of them. On the one hand, they are realists about entities, but on the other hand they are skeptical about the truth of these theoretical assertions. This very well reinforces the view that entity realism is a selective skepticism. An entity realist thus is optimistic about the existence of entities in the unobservable world, but he/she is pessimistic about the truth of theoretical assertions about them

Structural Realism: This is a half way house between scientific realism and empiricism. Structural realists believe that there exists a mind independent world and the world has a definite natural kind structure. Further, they agree on semantic realism completely. However, the knowledge of the world is limited to structural features of the world. This can mean two things. Firstly, knowledge of the structure alone is possible (epistemic structuralism). Secondly, structure alone is all that is there in the world (ontic structuralism). The former position is the version where the entities in the world have both relational and intrinsic features, however we need to be agnostic about the intrinsic features. The latter version reduces all intrinsic features into relational features. In other words, epistemic structuralism takes priority to 'relation' over 'relata' in the case of knowledge. Whereas ontic version ontologically reduces 'relata' to 'relation' in their metaphysics.

Semirealism: Anjan Chakravartty recently came up with an idea that realism can be modified in such a way as to only accommodate our commitments to the detectable features of the unobservable world. Rather than classifying the whole debate on the traditional observable/unobservable distinction, Chakravartty draws a new line and evaluates the debate along the lines of a distinction between detectable/non-detectable aspects of the world. In doing this, he brings in a fine blend of entity realism and epistemic structural realism, but only after modifying both of them to an extent. Entity realism suggests that entities exist because we can manipulate them and intervene in causal processes involving them, and epistemic structural realism is the view that we can

know the structural features of the unobservable world. Chakravartty proposes that, what we know, is a blend of both entities and structure. We know them together, as 'concrete structures', we know both the structure as well as the nature through these concrete structures (the structure is not abstract as proponents of epistemic structuralism would sanction, but concrete). He says that these concrete structures are relations between first-order causal properties (these properties are responsible for causal interactions). These properties are to be understood as having a disposition to engage in causal processes. In other words, they have dispositional identities based on their causal properties. Science is able to detect these properties in most of the times. It is clear that semirealism is the most closer to scientific realism. It only finetunes the metaphysics of scientific realism.

All these above positions will be discussed in greater detail in chapter III. In this chapter, I discussed the position of scientific realism and its defense along with its metaphysical and epistemological stances. I also roughly sketched different positions which play significant roles in the debate over scientific realism. The thesis, as said in the beginning, is an attempt to evaluate the scientific realism debate itself rather than going into each position in greater detail. Therefore I have given more attention to the problems and challenges which determine the dynamics of the debate. I have omitted several details like the logical positivist view on theoretical/observational term divide, the aim and semantic doctrines (which I briefly explained) etc. In the next chapter, I elaborate on the challenges to scientific realism, especially, the challenge from the history of science.

CHAPTER II

THE CHALLENGE

II.1 Preliminaries

The Previous chapter contained an exposition of scientific realism and its major tenets. I also elaborated towards the end of last chapter on the ways in which contemporary scientific realists softened their stand and brought in epistemic humility as opposed to presumptuous epistemic optimism. This dissertation aims to discuss the scientific realism debate in relation to the challenge from history of science. In this chapter, I take up this task of articulating the challenge. I elaborate the trajectory and brief historical rudiments of this challenge. In the first section, I discuss traditional antirealism consisting of, namely, instrumentalism, descriptivism and conventionalism. These three forms of antirealism posed several challenges to scientific realism in the early part of twentieth century. However, the origin of the challenge from history of science can be traced back to the conventionalism, especially in the views of Henri Poincare. In the second and third sections, I discuss the views of Larry Laudan and van Fraassen concerning the historical challenge as well as the arguments questioning the overall tenability of scientific realism.

The challenge from the history of science contains a simple but philosophically interesting argument. It mainly runs against the 'no miracle argument'(NMA) which I discussed in the previous chapter. The challenge comes in many forms and it can be explained in different ways. To put it uncomplicated, the idea that the empirical reliability and success of science rationally compels the man on the street to believe in the truth of theories is at stake here. If the empirical reliability of current theories rationally compels us to believe in the truth of them, then what about the truth of past theories which were equally empirically realiable? This challenge is known as the argument from 'pessimistc induction' (PI). If we do an induction over the history of science of past successful theories, then we have the conclusion that our current theories can also go wrong in the future. ERS and truth are linked according to our pre-

philosophical intuition. But the challenge also seems to support our pre-philosophical intuition that if current successful theories are true, then the past successful theories cannot be. An argument for scientific realism which seemed the least problematic view runs into serious drawbacks.

Traditional antirealisms question the rationale behind scientific realism whereas the challenge from history of science questions both the rationale and the tenability of the defense of scientific realism. PI is not a challenge to any of the antirealisms, be it traditional or recent. They however offer interesting insights into the structure of PI and its import. The reason why history of science undercuts the argument for scientific realism can be understood by the importance given to successful prediction and truth of theories by scientific realists. This link is not seen in any of the antirealisms. Laudan pounces on this very aspect, the weakness immanent in the thesis of scientific realism.

Bas van Fraassen fits in a relatively young genre of several alternative positions to scientific realism. He admits most of the tenets of scientific realism but rejects their epistemological optimism about unobservable entities. Truth of observable phenomena is very much allowed in his constructive empiricism. The contribution of van Fraassen is to be seen as an effort to provide an alternative image of science where 'truth regarding statements about unobservable entities' can be suspended and is replaced by empirical adequacy. He also criticizes scientific realism in giving undue importance to abduction. These views will be discussed in the course of this chapter. Let us start the challenges to realism by discussing traditional antirealisms.

II. 2 Traditional Antirealism

Traditional anti-realism has three versions namely, instrumentalism, descriptivism and conventionalism. One positive point about the versions of traditional anti-realism is that they take a clear stand regarding what they consider to be the central question of the realism versus anti realism debate. They are namely,

i) What is the ontological status of theoretical entities, i.e. do they exist or not?
ii) What is the cognitive status of scientific theories, i.e. do scientific theories have truth-values? and

iii) What is the aim of science?

Obviously all the three versions of traditional anti-realism have the same answer namely that the theoretical entities do not exist or at least their existence or non existence is of no consequence to science. Also, as antirealists, they refuse to accept the realist position that scientific theories have full flooded and independent truth value. Further, all of them reject the realist view regarding the aim of science namely that it is to provide an account of observable world in terms of unobservable entities. However, they differ on their answer to the second and third questions. Instrumentalism, which is considered to be the least sophisticated of the three considers scientific theories to be devoid of any truth value, since they are only instruments of prediction and they function as rules of inference. Theories are black boxes with inputs as their data and predictions as their output. So long as predictions are borne out, the black boxes serve their purpose and we need not bother about their inner structure. Thus their answer to the third question is that the aim of science is accurate predictions.

The descriptivist version of traditional antirealism has different answers to the second and third questions. According to them, a scientific theory has a truth value, but the truth value is parasitic upon the truth value of the observation statements it summarizes. In other words, a theory is only a conjunction of some observation statements and it is true if all the conjuncts are true and it is false if at least one of the conjuncts is false. Theories as summaries of observation statements are needed to facilitate our empirical knowledge.

The main thrust of the descriptivist position is best expressed in the following words of Earnst Mach:

> The communication of scientific knowledge always involves description, that is, a mimetic reproduction of facts of thought, the object of which is to replace and save the trouble of new experience. Again, to save the labor of instruction and of acquisition, concise, abridged description is sought. This is really all that natural laws are. Knowing the value of the acceleration of gravity and Galilio's laws of descent, we possess simple and compendious

> directions for reproducing in thought all possible motions falling bodies. A formula of this kind is a complete substitute for a full table of motions of descends, because by means of the formula the data of such a table can be easily constructed at a moment's notice without the least burdening of the memory. (1986: 198, fn 12)

Thus the aim of science according to the descriptivist is the economical description of phenomena.

Before we proceed further, it may be noted that both the above mentioned versions of traditional antirealism are empiricist. Hence positivists who call themselves neo-empiricists accepted them. In fact it is difficult to clearly designate many positivists either as instrumentalists or descriptivists.

The third version of traditional antirealism namely conventionalism is considered to be the most sophisticated because conventionalist rejected the empiricist/positivist idea of theory-free observation. According to them, scientific theories have non-parasitic truth value but their truth value is a matter of convention, i.e. a matter of human decision. Hence according to them it is not that we accept a theory because it is true or reject it because it is false, but it is true because we accept it and false because we reject it. And our decision is based on whether a theory continues to serve it's purpose. According to the conventionalist, the aim of science is to systematically organize our (theory-dependent) observation or facts. So long as a theory facilitates a smooth organizing of facts, it is true. And once the organization becomes cumbersome and patchy, the theory is declared to be false, because it ceases to be simple. So according to the conventionalist, simplicity is the hallmark of a scientific theory. A new theory is chosen because it is simpler than the old one.

Before we go to the views of Henri Poincare, who is the most important champion of conventionalism, a couple of critical remarks on conventionalism may be made.

The most important criticisms concern the methodological significance the conventionalist attach to the notion of simplicity. These criticisms are both logical and historical. The logical criticism is that 'simplicity' has no one meaning. It may be

syntactical wherein it depends on the number and structure of the primitive concepts, the independent postulates and the rules of statement transformation. It may be semantic simplicity which concerns the economy of presuppositions as determined by the number of meaning specifiers of the basic predicates. It may be epistemological simplicity which is related to experimental proximity. It may be pragmatic simplicity that depends on factors like computational convenience, feasibility of experimental design. As Bunge points out "No dependable measure of any of the four kinds of simplicity is known at present" (1961: 120).

In other words, there is no objective criterion that can decide which sense of simplicity is more primary than the other. In short, the notion of simplicity itself is too complex. The historical objection seeks to show that simplicity has not paid a decisive role in theory-change. Conventionalists have always given the example from the change from Ptolemaic theory to Copernican theory and claim that the decisive reason for the change was relative simplicity of the Copernican theory. Historians of science have shown that Copernican theory was as complex as the Ptolemaic theory had become when the change occurred. As Kuhn points out, "Evaluated in terms of economy, the two sphere universe…remain what it has always been: An extremely successful theory." (1957: 75). Further, what is simple and what is not can be too subjective to decide the objectivity of our theory choice. "What to Copernicus was stretching and patching was to them [i.e. followers of Ptolemy] a natural process of adaptation and extension". (ibid: 37).

Further, simplicity might conflict with one of the guiding principles of scientific theorizing, namely, the more a theory covers wider phenomena, the more desirable it is. If we have a theory for electricity and a theory for magnetism, it is desirable to have an electromagnetic theory replacing both, but such a unified theory may not be more simple than the theories we unify.

We may now look at Poincare's chief argument against realism. One must have to say that Poincaré was aware of the negative side of science, the frequent changes and abandonment of several ill fated theories, which were once empirically reliable. This

attitude towards science can be traced back to his view on truth. Poincare in his address to the 1900 Congress of Physics says the following:

> The man of the world is struck to see how ephemeral scientific theories are. After some years of prosperity, he sees them successively abandoned; he sees ruins accumulated on ruins; he predicts that the theories in vogue today will in a short time succumb in their turn, and he concludes that they are absolutely in vain. This is what he calls the bankruptcy of science. (1900: 14–15)

It is clear that Poincare had a crude version of the Pessimistic Induction (PI) argument by invoking a conventionalist image of science. Psillos also points out Poincare's charge from history of science in his work.

Many recent thinkers like Worrall treat Poincare as a structural realist, but I believe that the structural realist in Poincare is a derivative from his dejection concerning frequent abandonment of scientific theories. He might have believed that theories should be hitting at least certain aspects of reality, at the level of relations in terms of mathematical equations. However, he was a deep rooted conventionalist when it came to the notion of truth of scientific theories. He believed that scientific theories have to be couched in a conventionalist image as that adequately answers the reason for frequent abandonment. In Poincare' views, we have one of the earliest challenges to realism with a historically sensitive argument. Perhaps he answered it too in a conventionalist way. Poincare may be a structural realist when it comes to the solution of PI but he is a conventionalist in giving a reason for PI. To make this clearer, let us turn to the following words of Poincare,

> If we look more closely [at the history of abandoned scientific theories], we see that what thus succumb are the theories properly so called, those which pretend to teach us what things are. But there is in them something which usually survives. If one of them taught us a true relation, this relation is definitely acquired, and it will be found

> again under a new disguise in the other theories which will successively come to reign in place of the old. (1905: 182)

The usage of the term 'new disguise' is very important in understanding Poincare's ideas. It has a certain structuralist as well as conventionalist flavor in it. The theoretical part which survives certain theory change reappears under a 'new disguise'. But we never are sure whether it is the same reference for both the new term and the old one. Poincare never tells that it is the same reference of the old terms to which the newly disguised terms also refer to in the successor theory. He always maintained the view that science can never tell us a story about the real world.

> Things themselves are not what [science] can reach as the naive dogmatists think, but only relations between things. Outside of these relations there is no knowable reality'. (Poincare, H.1902: 25).

It is clear that he believes that certain content of the abandoned theories are retained but at the same time he is skeptical about the possibility of it being referring to the same reference. The new disguise is one convention which fits well within one theory whereas the old terms are another convention which gets knitted well with the old theory. Both theories are true in their period due to convention. Poincare's importance in the history of philosophy of science rests with the fact that he paved the way for the historical challenge to scientific realism. Having mapped one of the earliest forms of the historical challenge and its conceptual allies like decriptivism and instrumentalism, I would like to discuss the contemporary challenge to scientific realism from history of science.

Before going into the details of Laudan's challenge, I would like to briefly mention the views of Mary Hesse who also hinted on a challenge from the History of Science. She calls her argument 'the Principle of No Privilege', according to which 'our own scientific theories are held to be as much subject to radical conceptual change as past theories are seen to be' (1976: 264). This statement precisely brings out the insight contained in the challenge from history of science. Psillos covers Hesse's intuition in his work when he ventures out to answer PI. According to Hesse, we are not in a privileged position where we can in some way peep and see whether our theoretical terms are

referring. The 'principle of no privilege', as the name itself rightly points out, is one of the concerns which the literature in philosophy of science paid only liitle attention. This view endorses the idea that the realist sense of a 'god's eye point of view' where we can warrantedly say that our theories are hitting truth is vulnerable to the challenge from history of science itself. This is because our own current theories or past theories are subjected to innumerable number of revisions and modifications. We can in no way assert that a particular theory has mapped reality because we are not in a privileged position as opined by Hesse.

I believe that the scientific realist's take on the notion of truth as a non-epistemic concept triggered Hesse's and Laudan's concerns. The view that truth of a sentence is independent of the way we arrive at it or assert it. If a statement corresponds to a state of affairs in the world, it is said to be true and the state of affairs are said to be real. However, this non-epistemic construal of truth earned the realist the best critics in the history of philosophy; thinkers like Putnam, Kuhn and Dummet unleashed a series of arguments against it. According to them, our knowledge of the world is ingrained in our language, conceptual schemes and theories. There is not a single way to get outside this rather unique epistemic trap consisting of the dependency of language, thought or conceptual-schemes. How are we to see that our theories are corresponding to the world from within the conceptual-scheme provided by the theories themselves? We don't have such an epistemic concession. Hesse, a positivist herself, perhaps with an influence of Kuhn and Putnam termed her worry in the right words "The Principle of no Privilege". However, she did not cite philosophical reasons for the 'no privilege'-the idea that our conceptual schemes or language restrict us from accessing reality in a conceptual-scheme-independent or thought-independent way. Like Poincare, she cites that history of science and points to the frequent theory-changes in science responsible for the idea of 'no privilege'. History of science has ample evidence that no theory is in a privileged position.

II. 3 Laudan's Challenge (Pessimistic Induction (PI))

In the nineteen eighties, Laudan came up with systematic papers which shook the foundations of scientific realism. Many thinkers regard Laudan's challenge uncalled for

because it is an outcome of an inductive process, and induction does not guarantee truth of the conclusion. However, the fact that history of science is a 'graveyard of scientific theories' as Poincare claimed is ample evidence that troubles the realist defense. Laudan's challenge is very peculiar because it is the only challenge where an argument with empirical evidences is coined against scientific realism. All other challenges to realism such as under-determination of theory by evidence and the problem of abduction are conceptual in nature. As discussed before, the scientific realist themselves are to blame for this rather peculiar empirical challenge. The scientific realist's epistemic stance contains a conceited claim that our current theories are true of the world. They also defend scientific realism by saying that the empirical reliability of science has to be accounted for by the fact that scientific theories are true. This straight away directs our common sense to the past theories which were once accepted as true and later on abandoned. Chakravartty very clearly spells out Laudan's worries:

> If one considers the history of scientific theories in any given discipline, what one typically finds is a regular turnover of older theories in favor of newer ones, as scientific knowledge develops. From the point of view of the present, most past theories must be considered false; indeed, this will be true from the point of view of most times. Therefore, by enumerative induction (that is, generalizing from these cases), surely theories at any given time will ultimately be replaced and regarded as false from some future perspective. Thus, current theories are also false. The general idea of the pessimistic induction has a rich pedigree. (2011a: 4)

One has to keep in mind that Laudan is carrying out a meta-induction, where history of science itself undercuts the idea that our current theories are true. To get into the details, let us turn to Laudan's own works. He came up with a series of works that directly challenged the link between empirical success of scientific theories and truth. He claimed that there is enough evidence in the history of science which shows that once successful theories were later on abandoned. In Psillos' opinion, the whole argument of Laudan can be summed up as follows:

> The history of science is full of theories which at different times and for long periods had been empirically successful, and yet were shown to be false in the deep-structure claims they made about the world. It is similarly full of theoretical terms featuring in successful theories which do not refer. Therefore, by a simple (meta-) induction on scientific theories, our current successful scientific theories are likely to be false (or, at any rate, are more likely to be false than true), and many or most of the theoretical terms featuring in them will turn out to be non-referential.
>
> Therefore the empirical success of a theory provides no warrant for the claim that the theory is approximately true. There is no substantial retention at the theoretical, or deep-structural level and no referential stability in theory change.(1999: 97)

The reliability of scientific method is at stake because one is never sure whether one's theory is hitting truth. Laudan says that history of science itself defeats the realist defense because it is full of cases of theories that were once empirically successful and yet later turned out to be false. He gives the following examples.

- the crystalline spheres of ancient and medieval astronomy
- the humoral theory of medicine
- the effluvial theory of static electricity
- catastrophist geology, with its commitment to a universal (Noachian) deluge
- the phlogiston theory of chemistry
- the caloric theory of heat
- the vibratory theory of heat
- the vital-force theory of physiology
- the theory of circular inertia
- theories of spontaneous generation
- the contact-action gravitational ether of Fatio and LeSage
- the optical ether

- the electromagnetic ether.

This list goes on and he highlights many more theories which were once empirically successful but later abandoned. All these examples bring out the doom of scientific theories which enjoy empirical success at a particular period and later credited as false and as misrepresentations.

Laudan designed the argument in its final shape. He very categorically argues that one cannot hold the idea that truth can be attained in science if we are keen students of the history of science.

> Because they (most past theories) have been based on what we now believe to be fundamentally mistaken theoretical models and structures, the realist cannot possibly hope to explain the empirical success such theories enjoyed in terms of the truth-likeness of their constituent theoretical terms. (Laudan, L. 1984b: 92)

Let us now have a close examination of Laudan's argument by taking cues from Psillos' discussion of PI. Psillos claims that, from a close range, it is a a *reductio* with the following pattern:

(A) Current successful theories are true.

(B) If current successful theories are accepted to be true, then past theories cannot be.

(C) These characteristically false theories were, nonetheless, empirically successful. So, empirical success isn't connected with truth and truth cannot explain success.

Now, the realist is very quick in responding to Laudan. The problem for the realist is that (A) cannot be refuted because it's their foremost argument. The realist will have to manipulate (B) and (C). One of the cleverest moves by the realist is to bring in the idea of truth-likeness or approximate truth into the whole argument of PI. Hence, the realist reformation of laudan's argument is the following.

(A*) Current successful theories are approximately true.

(B*) If current successful theories are accepted to be approximately true, then past theories cannot be.

(C*) These characteristically false theories were, nonetheless, empirically successful. So, empirical success isn't connected with truth-likeness and truth-likeness cannot explain success.

Now (B*) seems to be having less of a force compared to (B). The idea that current theories can be truth-like does not undermine the view that past theories can also be truth-like. This is a very interesting claim for the reason that the realists operate with truth-bearing constituents within scientific theories. A theory being abandoned depends on a lot of factors, not just the truth or falsity of it. There could have been a lot many truth-bearing constituents in a particular scientific theory which was once empirically successful, and also that these constituents might have been retained in the new theory. This means that the realist boldly defends the idea of stability of reference across conceptual-change or theory-change.

The second and most important response to PI is endorsed by many noted realists. It is to cut short as many possible theories from Laudan's list. Realists believe that not all theories in Laudan's list can be counted as mature and successful scientific theories. This will directly weaken (C*) and will reduce the overall appeal of PI. John Worrall reiterated that the realist view of success should necessarily bring in the idea of novel predictions. Many of the theories in Laudan's list fail to do so. But this does not weaken Laudan's challenge as much as the realists would have liked, because there are innumerable number of examples, still available which were once successful and were genuine theories in the current sense of the term. The caloric theory of heat and nineteenth century optical theory are both strong examples from Laudan's list. These were considered to be the best among their contenders, well entrenched (in the background beliefs of scientific practice of those periods) and matured too. Realists still accuse the proponents of PI that their list of abandoned theories does not contain 'mature' theories. By the term 'mature' the realists mean,

> [T]heories which have passed the "take-off point" (Boyd) of a specific discipline. This 'take-off point' is characterized by the presence of a body of well-entrenched background beliefs about the domain of inquiry which, in effect, delineate the boundaries of this domain, inform theoretical research and constrain the proposal of theories and hypotheses. This corpus of beliefs gives a broad identity to the discipline by being, normally, the common ground that rival theories of the phenomena under investigation share. (Psillos, S. 1999: 100)

But this kind of comprehension based on a "take-off point" runs into another trouble. How is it that the realists explain away this so called "take-off point", and what are its constitutive features? It is definitely an empirical issue where one can find a set of background beliefs that are entrenched in a particular scientific practice. It also depends immensely on the scientific community and the whole debate is showing signs of getting into a socio-historical terrain which I don't want to elaborate currently. The debate about which theories are qualified in describing as 'mature' and as 'genuine' is still open. I believe that the idea of importing truth-likeness is a very tenacious move but it is equally demanding in the sense that one has to explain truth-likeness in the context of scientific theories and identify the truth-bearing constituents.

Kitcher and Psillos do this rather challenging act of finding truth-bearing constituents from scientific theories which were retained across theory change. They also set up the method in a naturalistic atmosphere where scientists themselves who are experts in these theories decide among themselves about the culprits- the theoretical constituents which went wrong. It is a very challenging project, the attempt of finding theoretical constituents from theories of past research traditions from a current perspective. I would not undermine or take away the credit from Kitcher and Psillos, but this whole programme seems to be based on a fundamental mistake, as Chakravartty said, a mistake of *rationalization post hoc*. How is it that Psillos and Kitcher along with a handful of scientists who are all trained in contemporary research traditions decide on the truth-bearing theoretical constituents of past research traditions? These concerns in the debate are discussed in more detail later in the course of this thesis.

Another serious demand that the antirealists are making to the realists is about the idea of truth-likeness. I too am not completely convinced whether the realists are firm on their stance. Truth-likeness would mean that there would be a degree-based understanding for truth and falsity, now this would mean that there are truth-falsity operators which are scalable. How much of truth-likeness is necessary for a theoretical term to be carried over across theory change. Terms get retained across theory change for various reasons, much of them are done for pragmatic reasons, and not for epistemic reasons. Pragmatic virtues like predictive power, and scope etc. with other entrenched theoretical parts are all factors for retaining a part of a theory (or a particular constituent of theory) for the next period. Epistemic matters hardly arise when scientists decide on the ingredients of a particular theory. van Fraassen accuses the realists for precisely this reason that they must distinguish between epistemic virtues and pragmatic virtues, and it is pragmatic virtues which play a role while the scientific community makes decisions.

Let us now turn to some of the repercussions which arose in the history of philosophy of science from the debate so far. The scientific realists found themselves in trouble because theory-change poses a series of questions, about truth, reference and the empirical success of past theories. We saw that scientific realists endorse a necessary connection between truth and empirical success, and later had to amend it in favor of truth-likeness and empirical success. They found this notion extremely difficult to put in plain words. Nevertheless, one of the first responses that PI invited, and especially from thinkers of realist bend is to fundamentally alter the realist metaphysics of properties and relations. Worrall and Ladyman and their respective versions of structural realisms form this group. The question of what ultimately is latched on to truth and thus credited for success is answered metaphysically[10]. Several positions emerged as a result of Laudan's challenge.

Many realists also have the opinion that Laudan's understanding of 'empirical success' is deeply ingrained in the belief that success is a kind of entrenchment where a community believes it to be recognized and accepted. In addition, success is not to be understood with notions like novel predictions or coherence to other mature theories etc.

[10] I elaborate on this metaphysical discussion in chapter III with reference to selective skepticisms.

Laudan is clever in coming up with at least a couple of strong examples like the caloric theory and leaves the ball in the realist's court. Nevertheless, matters are not settled till now and a whole lot of literature in this field is needed to streamline the debate. The issue of what is retained across theory change is to be settled, according to contemporary thinkers, by building a firm metaphysics for the debate. This project is in its developing stage. Thinkers like Psillos, Worrall, Ladyman, French, Chakravarrtty are all in the process of coming up with a metaphysics for the realism-antirealism debate. I too would like to elaborate various stances regarding this in Chapter III and then take my own course in the subsequent chapters.

Psillos does agree that the realist should concede the existence of failures in scientific practice. But he doesn't want to thereby appreciate the antirealist claim about the non-reliability of scientific method.

> The realist argument should acknowledge the existence of failures. Their actuality does not impair scientific methodology. Nor does it sever the explanatory link between approximate truth and empirical success-especially novel empirical success. Clearly, the fact that I have occasionally failed to find my lost keys does not entail that a thorough search of the places where they could have been left is not a reliable method for finding lost keys……It is after all a salient feature of scientific methodology that it does lead to empirical success. (Psillos, S. 1999: 80)

Psillos' claim that its time for the realist to admit and account for failures in science is a very bold claim compared to past proponents of realism. Today's realists acknowledge that past theories have failed and failed miserably. But the realist is in no mood to break the link between empirical success of science and truth of scientific theories. NMA and PI definitely cannot go together. Realists nevertheless, have softened their stand on truth, they no more say that anything and everything the theory states about the unobservable world is true. They have come up with the idea of truth-likeness and truth-bearing constituents in the theory. What is hooked onto the unobservable world are these truth-bearing theoretical constituents.

The above discussion shows that Laudan's challenge from the history of science against scientific realism regarding the connection between success of theories and their truth has not undermined the realist position completely. But it has forced the realist to rethink about the core concepts of their position. Thus, Laudan has not been able to settle the debate in favor of antirealism but has succeeded in pushing the scientific realist to a corner. What follows in the next section is van Fraassen's challenge to realism along the lines of Laudan's argument. Van Fraassen also targets the realist defense, namely NMA or EDR.

II.4 Bas van Fraassen's Challenge

As we briefly outlined in the first chapter, Bas van Fraassen's position concedes to the metaphysical as well as semantic and aim doctrines of scientific realism. He suggests that it is not philosophically problematic to accept the existence of a mind-independent world and also one can take scientific theories literally, when it comes to their interpretation. However, he rejects the realist epistemological doctrine. Before venturing into the details, let us share some observations with the help of which he developed his unique position.

He believed that after the demise of logical positivism, there has never been a serious threat to scientific realism. He undertakes this task and compels the scientific realist to respond to his objections. At the same time, he comes up with an alternative position where the notion of truth is replaced with a similar but epistemically inferior concept- empirical adequacy. His alternative to realism, namely, constructive empiricism is an attempt to arrive at a philosophically feasible yet commonsensible position which can take over the place of scientific realism. According to him, scientific realism is a thesis where,

> Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true. This is the correct statement of scientific realism. (1980: 8)

He claims that history of science shows something else than what the above statements reflect. He elaborates that for the most part, history of science provides us

with ample evidence that science does not give us a literally true story of what the world is like, however science aims to do so according to scientific realists. Pragmatic considerations play a key role than truth in accepting a theory or rejecting one. He opines that science only aims to give us a literally true story of the world, but realism says something more, which we should not subscribe to, i.e. we should not say that science actually does provide a literally true story of what the world is like. This extra attitude creates all the trouble for scientific realism. Subsequently, he questions the very rationale of scientific realism, its epistemic tenet, the view that we can have knowledge of the unobservable world.

In order to execute this task of shaking the foundation of scientific realism, van Fraassen commits a crucial move, by coming up with the notion of empirical adequacy, which is roughly a weaker notion than truth (weaker in degree, not in kind). He endorses the notion of truth about observable world, but he does not hold truth in the case of unobservable world. An empirically adequate theory will have true sentences about the observable world. van Fraassen is selective in his approach to truth. Empirical adequacy is a notion which entails observational truth and therefore falls at a lesser degree than full fledged truth (both observational and theoretical). To exactly follow his proposal, a theory is 'empirically adequate' if it 'saves the phenomena'—if everything it says about observable phenomena is true.

> A little more precisely: such a theory [empirically adequate] has at least one model that all the actual phenomena fit inside. I must emphasize that this refers to all the phenomena; these are not exhausted by those actually observed, nor even by those observed at some time, whether past, present, or future….
>
> Anti-realism is a position according to which the aim of science can well be served without giving such a literally true story, and acceptance of a theory may properly involve something less (or other) than belief that it is true…..

> ….Science aims to give us theories which are empirically adequate; and acceptance of a theory involves a belief only that it is empirically adequate. This is the statement of the anti-realist position I advocate; I shall call it constructive empiricism. (Ibid: 12)

Theses sentences reflect the very texture of constructive empiricism, where the realist image of science is replaced with an antirealist one equally rationally compelling.

Let me start by the initial thesis of van Fraassen. He categorically says that there is a difference between accepting a theory and believing in the truth of that theory. van Fraassen suggests that accepting a theory in the history of science can be explained away by pragmatic means. In fact he believes that there is a considerable amount of difference between accepting a theory and accepting a theory to be true (belief in the theory). In order to accept a theory, it need not be true or we don't have to believe in the truth of it. This idea is the epitome of constructive empiricism.

Let us discuss with an example which van Fraassen himself employs on another occasion. Suppose I hear strange sounds of small taps in the night in my room and I see mouse droppings and my cheese are found missing in the morning, I can come up with a mice-theory where I believe that a mouse is responsible for the missing cheese and I can accept it. However, belief in the truth of the mice theory is an unnecessary epistemic effort and involves extra epistemic optimism. The mice theory is empirically adequate and saves the phenomena of strange sounds and missing cheese. However, my acceptance of mice theory and my belief in the truth of mice theory are entirely two cognitive procedures. The latter is not required in performing the former, i.e. belief in truth of a theory is not required in accepting that theory. Acceptance and rejection are dealt within the pragmatic realm and we can be skeptic about the epistemic realm. Mouse may exist in my house with all probabilities but that does not rationally compel us to believe in the theory that the mice are stealing my cheese[11]. The empirically adequate mice theory saves the phenomena and we can accept it to serve our purpose. van Fraassen suggests that the

[11] van Fraassen uses the mice example to show that we can believe in the theories which state facts about observable world, but this cannot be extended to science. I used it to explain his basic idea, the difference between accepting and believing a theory.

difference between acceptance and believing is not only limited to day today mundane affairs but also in science.

In the case of science, a similar example can be drawn. The trails in cloud chamber are evidence that requires the explanation by a theory of electrons. We can either accept or reject such a theory too because it saves or fails to save the evidence available. However believing in that theory is an unwanted epistemic endeavor. Here lies the rather presumptuous attempt of scientific realists. The existence of electrons is not warranted if we accept a theory of electrons. Electrons may or may not exist and the theory of electrons may or may not correspond to actual state of affairs in the world. But this has absolutely no import on the matter of accepting a theory or rejecting one.

He says that, in the case of science, "we do not routinely introduce new ontological commitments. In the case of the earlier example, we already believe that mice exist" (1997: 316). However, the inference of existence of electrons from trails in the cloud chamber is different since we do not know the existence of them prior to this situation. In science, according to van Fraassen, an explanation of acceptance of theory is what is called for and an explanation of belief in the truth of theories is uncalled for. To conclude, let me quote Psillos who beautifully coins van Fraassen's efforts:

> van Fraassen's main position is the following. Even if it were shown that theoretical truth was attainable in a non-accidental way, realism would not be rationally compelling. For there is an alternative empiricist image of science in which search for theoretical truth, and belief in the truth of theories, drop out of the picture without any loss for the practice of science. van Fraassen's Constructive Empiricism, is then, the view that in our philosophical reflection on science, we (philosophers) do not have to interpret science as an activity which involves search for, and belief in, theoretical truth in order to account for its salient features and for its empirical success.(1999: xix)

Whether van Fraassen succeeds in creating an alternative image of science is subject to discussions and evaluations. Having mapped some important aspects of his

thesis, let us move to his major criticism of scientific realism which directly targets its defense, namely the reliability of abduction and NMA.

Scientific realists believe that abduction is a reliable method of arriving at true theories. NMA, which claims that the success of science would be a miracle had theories been not hitting truth is also an instance of abduction. The reason for any scientific theory's empirical reliability such as successful prediction is explained in terms of truth. The view that truth explains the success of theories, therefore, is an outcome of an abductive inference. Since science also relies on abduction, according to the scientific realist, it is doubly warranted to believe in the no miracle argument, i.e. the success of science is explained with the same method of inference which practitioners of science use. We discussed criticisms like circularity in relation with NMA in the previous chapter. van Fraassen's criticism of NMA is slightly different. Let us evaluate his argument by paying attention to the following table.

Explanation of	Evidence	Method	Result
Physical Phenomena	Missing cheese, Mouse droppings	Abduction/IBE	Theory of a mouse in the house
Physical Phenomena	Trails in the cloud chamber	Abduction/IBE	Theory of electrons
Success of Science	Empirical reliability	Abduction/IBE	Truth of scientific theories

Table 2. Explanation and IBE

van Fraassen very categorically says that the hallmark of the best explanations of physical phenomena is 'causal' in nature. The link between theory and observation in the case of physical phenomena is explained causally, even though we can be skeptical about its truth. The reason for a missing cheese is the possibility of a mouse somewhere in the house, this explains a causal connection. Similarly, the reason for trails in the cloud chamber causally relates it to the theory of electrons (even though we don't have to believe in the truth of such a theory). Both the physical phenomena in the table are

explained by keeping in mind a causal role performed by ‘mouse’ in the first case and the ‘electron’ in the second case.

However, when it comes to the third case in the table, we are completely startled and seek a vague expression about how truth can trigger empirical success as in the case of the physical phenomena. What does it mean to say that truth ‘causes’ the fit between theories and evidence (i.e. empirical success of scientific theories)? Perhaps I should invoke an analogous example from the medical sciences. What does it mean to say that antibodies ‘cause’ immunity? The greater number of antibodies itself is another way of saying that the body is immune. Here, the relation seems to be one of entailment. In the other examples, the relation seems to be one of causal effect, i.e. one process or entity affecting to provide an effect upon the other. In the first case, mouse eating or stealing the cheese causes the disappearance of cheese. In the second case, electrons causally interact with other particles in the cloud chamber to leave the trails.

van Fraassen’s claims that, as per the scientific realist, rooting for abduction will put one on the wrong philosophical track. Abduction does not guarantee entailment. In arriving at the explanation for the success of science, the scientific realist is using abduction. However she is neatly mistaken to conflate the two types of explanation we discussed above. Truth explaining empirical success is a kind of deductive explanation where abduction cannot account for it. In the case of science, abduction is what pays and not deduction. How is it that abduction unlike deduction can guarantee entailment? He further adds that abduction is not a rule which preserves anything. It is just a *post hoc* activity where we say that the best explanation from a given lot is such and such. We can never see the causal picture with the use of abduction. It is in fact a common guesswork where the philosophers of science gave some name of selecting one best explanation from a set of possible alternatives. In the mice example, it could be the case that the cheese was taken by a kitten. The kitten theory would then be equally empirically adequate. May be the cheese is taken by the kitten and the mice at different times. The use of abduction is just a modest way of saying that we have guessed the right reason of the missing cheese to the mice or the kitten. However, deduction and entailment are not like this, they are stronger in the sense that if the scientific realist says that she

deductively concludes that truth is the reason for empirical success of theories, then all empirically successful theories must be true.

It should be noted that the pivotal change that was added to Putnam's initial no miracle argument was Alan Musgrave. Putnam's explanation for the success of science was initially taken to be deductive by the scientific realists. Musgrave (1988) said that we cannot deductively conclude that truth is the reason for success. Hence he suggested that we can employ an abductive inference that truth is the best explanation for the success of science. This move by Musgrave was well received in the scientific realist circles. He was very keen to change the stature of NMA and bring in the very method of science, an ampliative inference, to account for the explanation for the empirical reliability of science. This tone of keenness is visible all throughout his works in the nineteen eighties. But he overlooked the nature of explanation that science demands and the one that the success of science needs. Abduction works well within science, giving the right amount of uncertainity to scientific knowledge. The explanation for the success of science cannot bear such uncertainities. The realist cannot accept anything other than truth as an explanation for the success of science. Such adamancy cannot be entertained when we use abduction. This adamancy can however be considered in the case of deduction where certainty is the hallmark. Let us look at Maxwell's views on accepting scientific realism to make the discussion more appropriate.

In the first chapter, I briefly discussed the view of Maxwell and his reason for accepting the thesis of scientific realism.

> My reasons for accepting realism are of the same kind as those for accepting any scientific theory over others which also explain current evidence. (1970: 12)

Maxwell's hint is that Realism is the best of the lot when it comes to the explanation of the success of science. However, the nature of explanation when a theory explains an evidence is completely different from the nature of explanation when realists explain the success of science. It is already shown by the use of some examples from science. The reasons for accepting a scientific theory over another cannot be the same as

the reasons for accepting scientific realism over other positions. The realist is unnecessarily conflating the two issues. Philosophical rationale and scientific rationale are two different notions. In fact van Fraassen's later projects were all devoted to showing that how constructive empiricism is equally philosophically persuasive and compelling.

According to van Fraassen, truth entails empirical adequacy and empirical adequacy entails empirical success. This is because empirical adequacy is lesser to truth in its epistemic force. Therefore the success of science can be explained without in anyway invoking the notion of truth. When we can explain something with a simpler notion, it should be given preference over the other. First of all, he makes it clear that we do not have to look for a causal explanation when we attempt at the explanation of the success of science. He also criticizes the scientific realists for comparing inferences in science to that of an inference about the success of science. He also believes that abduction/IBE might help us lead to apt explanations in mundane phenomena as shown in the mice example, but it does not have the bite when it comes to unobservable phenomena.

van Fraassen concedes the point that it may be the case that we use abduction for all sorts of mundane cases involving observable phenomena, but does it lead us to belief in unobservables, or the truth of theories? He doubts whether we actually use abduction. What does it even mean to say that we follow a rule of inference? Certainly not that we follow it explicitly and consciously. But we also can't say that a rule is followed implicitly just when our conclusions happen to be permitted by that rule. This is because a rule of inference under- determines the conclusion that may be drawn and a sufficiently relaxed rule permits any conclusion.

Therefore, the claim that we follow a particular rule of inference is an empirical hypothesis. Specifically, the realist explanation by recourse to abduction and truth is just a hypothesis, and empirical adequacy is a rival hypothesis that works just as well. Evidence for abduction is not evidence against empirical adequacy. There is another objection too, that is, even if we grant that abduction is the method of science, how do we know that it leads to truth as the best explanation of the success of science? Historically,

we've inferred nothing but literally false theories. The track record of science is agonizingly awful in terms of unsuccessful inferences. Therefore we cannot really say that truth is the reason for the success of science just because we used the same method (abduction) used in science. Can the realist avert this problem by appealing to 'approximate truth'? I discussed certain attempts by realists to bring in epistemic humility by invoking the idea of approximate truth in the first chapter. But this brings more problems as we have already seen.

van Fraassen believes that history of science is the best witness for the fact that we select our theories on the basis of empirical adequacy. Theories that did not fit the data either evolved or went extinct. If empirically inadequate theories were preserved, it was for pragmatic reasons. And if we have a number of empirically adequate and equivalent alternatives to choose from, the choice will be based again on pragmatic considerations, which themselves are independent of truth. I believe that the rationale behind van Fraassen's moves can be given sympathetic considerations.Truth still is an idea which demands clarity and we need something more to be understood about truth rather than the Tarskian treatment of it.

To sum up van Fraassen's views, the best explanation for the success of science and its empirical reliability is the fact that scientific theories are empirically adequate. Empirical adequacy is the reason why we accept a theory. We do not accept a theory on the basis that the theory is true, neither we have to believe in the truth of the theory to accept it.

Van Fraassen accepts that Laudan's challenge is a fair one. He believes that it can be shown in the history of science that all abandoned theories which were once empirically reliable were empirically adequate too. Empirical adequacy does not compel us to maintain that the new theory (post-abandonment) cannot be also empirically adequate. In the realist sense, a theory cannot be true at one time and false at another. But a theory can be empirically adequate at one time and empirically inadequate at another time. It is just a matter of new data and failure to adapt and save the new set of data. Therefore, van Fraassen acknowledges Laudan's charge against scientific realism but uses it into his own advantage. He builds this new image of science where the hallmark

of science is empirical adequacy and uses Laudan's criticism to show how it does not apply to his position.

The common thread across the three sections in this chapter as challenges to scientific realism can be coined in a nutshell. The traditional antirealisms did not face the challenge from history of science and neither did they report such a challenge against realism, except Poincare. The traditional antirealist positions such as descriptivism, instrumentalism and conventionalism nevertheless acknowledged the empirical reliability of science. They were able to see why science is empirically successful. They were able to answer this by invoking pragmatic reasons similar to the answer given by van Frassen. However, their unsanctioned approach towards truth in the realist sense makes them closer to van Fraassen and other contemporary antirealists. Similarly, in both the works of Laudan and van Fraassen, we can see the ploy against scientific realist. The severity of the challenge from history of science is clearly uncovered by van Fraassen and Laudan. Van Fraassen showed that the scientific realist in a conceited manner claim that our current scientific theories are truth like. Laudan showed that this presumptuous and conceited claim pay the price if we thoroughly study the history of science. This simple intuition led him to systematically criticize the realist's defense itself. I would like to evaluate the responses (both by realists and non realists) to the challenge from history of science in the next chapter.

CHAPTER III

RESPONSES TO THE CHALLENGE

III.1 Preliminaries

In this chapter, firstly, I discuss three major alternatives to scientific realism which fall in the externalist faction of realism. These are entity realism, structural realism and semirealism. These are also called selective skepticisms or selective realisms. Secondly, I discuss internal realist positions of Putnam, Kuhn and Ellis. These positions are realistic but differ quite a lot in the way they are construed. All these six positions are significant in giving shape to the scientific realism debate. They respond broadly to the presumptuous and modest claims of scientific realism, namely the epistemological and metaphysical claims. However, in the remaining chapters, I focus exclusively on the externalist positions and I draw certain meta-philosophical observations about their agenda in the debate.

The epistemic optimism of the scientific realist is aptly expressed by Gaston Bachelard that "it is a pure impossibility to hit on the exact knowledge of some reality, even by chance, for a coincidence between thought and reality is a genuine epistemological monster" (1928: 276). This 'epistemological monster' is addressed by the internal realists and selective skeptics in different ways.

Proponents of selective skepticisms are externalists who stick to the idea of knowing the world partially in a mind-independent manner. They believe that everything the theory says to be real need not be in fact real, but certain things are. These positions differ in what is knowable about the mind-independent unobservable world, but they agree that there is a mind independent world and we can know at least some part of it as it is. Entity realism falls in a single-tier model of interpreting scientific realism, whereas semirealism and structural realism fall in the two-tier model. Realist presumptuousness about the epistemology of science and challenges from the history of science were fertile grounds for these piecemeal realist positions. They are called 'piecemeal realists' because they agree on the fact that scientific theories miss out on something about

reality, but in a piecemeal way, that is, scientific theories do get something right about the mind-independent world. In other words, they agree that scientific representations do not stand in relation to the world in toto. This strategy helps them to tackle pessimistic induction and also to come up with their own versions of the no miracle argument. I discuss each of these views differently.

I said in the first chapter that avoiding presumptuousness will bring in epistemic humility. These selective skepticisms are aimed at carrying out exactly this task. Epistemic humility adds the idea that our best representations and the mind-independent world have some amount of lack of correspondence/ match. But this gap does not force us to completely drop our realist spirit. The selective skeptics have the opinion that the gap between representation and the world is necessary to answer the challenge from history of science. There is definitely a gap, a lack of correspondence/match, but only between certain theoretical constituents of the theory and the world. Certain other theoretical constituents do correspond/match the world. This seems to be the pivotal idea underlying all selective skepticisms. This allows them to avoid the challenge because certain constituents in the theory which are about the world do go wrong. Certain other constituents represent the world in an accurate way. The former is responsible for failure of the theory and the latter, responsible for the success of the theory. So, in a single shot, they kill two birds. In a single move, they fetch two results in their favor. The selective skeptics answer the challenge from history of science and also give an explanation for the success of science. This seems to be a very smart strategy. I argue in the next chapter that dissecting scientific theory into several parts and saying that some of them represent the world and some not, doesn't bring any goods for the selective skeptics. One can easily pose advance criticisms against these positions.

On the other hand, internal realism is a position which replaces scientific realism in a rather interesting way. Most thinkers engage with internal realism at the level of semantic notions like truth and reference. The metaphysical position of internal realism is largely a neglected area of concern for contemporary analytic thinkers. Here, I take up this task, namely to elaborate the metaphysical stand of internal realism and to explicate the intricacies of this position by incorporating their semantic as well as epistemic

concerns. I consider the thoughts of later Putnam as comprising a moderate internal realism and classify the position of Ellis as an extreme version of internal realism which is closer to scientific realism. I regard Kuhn as an internal realist but having lesser than moderate internal realist colors as compared to Putnam and Ellis. Internal realism is expressed in different ways by these three thinkers, and it is a daunting task to assimilate the similarities and differences.

Ellis, Kuhn and Putnam acknowledge the idea that there exists a mind-independent external world. However, they maintain that our knowledge of the world is not a map of the world as it really is. The idea of *mapping* or *copying* a mind-independent world is flawed according to internal realists. The physicist Niels Bohr's words capture the idea metaphorically. He says, "when it comes to atoms, language can be used only as in poetry. The poet, too, is not nearly so concerned with describing facts as with creating images." (1933: 11). Knowledge of the world is arbitrated or mediated by a necessary set of categories imposed by the human mind. One can see the Kantian flavor in this idea. Internal realist believes that reality as it is in itself and a copy or map of it is the result of an absurd and misguided epistemic optimism inherent in scientific realism. The internal realist entertains the view that reality as it is in itself is compromised in our representations. This is a fatal compromise which leads to the inaccessibility of the mind-independent world to us other than through a mind-mediated manner. The fatality is ingrained in an inevitable consequence, which the internal realist is forced to draw, namely the notion of mind-dependence of the known. The ontology of the world is therefore dependent on our minds. In fact, for the internal realist, the world is given to us in an already compromised way. As a result, the internal realist avoids non-epistemic conceptions of truth. They altogether dismiss the notion of correspondence, as there is nothing independent of our minds to which representations correspond. I wind up the discussion on internal realism in this chapter and consider them as essential responses to the constitutive views of the externalist positions. They are not responding to the debate over history of science in the context of the thesis. However, I try to show some possible positions the internal realist may subscribe to, in the light of the challenge from history of science. Even then, one may argue that they do not comprehensively answer Laudan's charges. This is also because Laudan's PI is not hitting the internal realist's assumptions.

Internal realists are only realist about the existence of a world. Other than this crude realism, they are largely antirealists. Therefore, I take forward the externalist positions to the next chapter for a detailed analysis and offer some meta-philosophical findings.

III.2 Entity Realism

Entity realism was initially developed by Ian Hacking and recently held by several thinkers who considered experiments seriously. This position is also known as experimental realism. Nancy Cartwright and Hacking are considered to be the pioneers of this position. In simple terms, it endorses the view that entities exist in the mind-independent world. This is indeed a unique position. The uniqueness is not in this simple metaphysical claim, but in the assertion that the warrant for such a belief in the existence of entities comes not from confidence in the truth of our best representations of the world, but from the interventions we make with the world. Entity realism is the view that one has good reasons to believe that entities exist in the mind-independent world. This can be said to be the positive thesis of entity realism, but there is also a negative thesis. While endorsing certain existential claims, entity realism is skeptical about theories by which these entities are described. Entity realists here come with a bolder claim. They are skeptical about theoretical descriptions of these entities (which they believe to exist). They doubt the credibility of theoretical descriptions and also the truth of them. On the one hand, they are realists about entities, but on the other hand they are skeptical about the truth of these theoretical assertions. This very well reinforces the view that entity realism is a selective skepticism. An entity realist thus is optimistic about the existence of entities in the unobservable world, but he/she is pessimistic about the truth of theoretical assertions about them. This combination of a positive and a negative thesis allows entity realism to evade the challenge from history of science and at the same time be realist in its spirit. In other words, entity realism blames the theories for the failure of science and at the same time endorses the view that success of science is in the fact that entities are manipulated through causal contact.

Entity realism is a peculiar form of realism because it endorses knowledge of unobservable entities, but by equally holding an antirealism about theories. Entity realist doesn't hesitate to claim that much of the theories have changed over time and may

change again in the future, but entities survive these changes. Entity realists are not worried about the truth of our best scientific assertions. They don't entertain or subscribe to any theory of truth, rather they are interested to speak about the manipulation of causal powers of entities which exist in the world. Psillos puts their spirit in the right words,

> A major motivation for entity-realism comes from laboratory life. For both Hacking and Cartwright, experimenters have good reasons to believe in specific unobservable entities, not because they accept the relevant theories but because they *do* things with these entities. They manipulate them, they jiggle them in order to produce several effects, they use them to interact with other entities. These phenomena of laboratory life would be inexplicable if these entities did not exist. (1999: 247)

More than the idea that experimenters can successfully jiggle with the causal powers of entities in establishing their existence, supporters of entity realism takes experimentalism to be the most convincing and right notion about scientific knowledge. Hacking fittingly enforces his confidence in the experimental method of science in warranting realism by saying that,

> [T]he experimental argument about scientific realism about entities is based on *doings*, not *sayings*…. Realism and anti-realism scurry about, try to latch on to something in the nature of representation that will vanquish the other. There is nothing there. That is why I turn from representing to intervening.……Experimental work provides the strongest evidence for scientific realism. This is not because we test hypotheses about entities. It is because entities that in principle cannot be 'observed' are regularly manipulated to produce a new phenomena and to investigate other aspects of nature. . . . The more we come to understand some of the causal powers of electrons, the more we can build devices that achieve well-understood effects in other parts of nature. By the time that we can use the electron to manipulate other parts of nature in a systematic way, the electron has ceased to be something hypothetical, something inferred. (1983: 61)

He asserts that the move from representing to intervening is inevitable for an experimental realist. The cost of such a move is the notion of 'truth' which is an unavoidable ingredient in the representational (two-tier model) account of realism. The laboratory life of entities is explained with reference to entity realism. Many thinkers believe that the existence of several laboratory devices and instrumental set-ups are best explained with reference to entity realism. This laboratory phenomenon necessitates the existence of entities. In the laboratory, instruments function by relying on the causal structure of the world, they can track the causal properties of entities, and in fact manipulate their properties. Proponents of entity realism say that our causal contact with entities assures us of their existence.

> When one can manipulate them so as to intervene in other things effectively using them as tools for scientific investigation by exploiting their causal powers, one cannot doubt their existence. (Ibid: 23)

As an example, one can consider the use of electrons to study weak neutral current interactions among subatomic particles. Chakravartty sums up the discussion on this example beautifully in his recent work. (2007: 31). Entity realist considers intervention as an all important epistemic virtue. If one believes that one can use a laser to ionize an atom (knock out one or more of its electrons, leaving it with a positive electrostatic charge), one must believe in the reality of the cause, that is, the lased light. Let me also say here that this conception of asserting the cause is a strictly non-Humean exercise. According to Hume, only the regularities of nature are real, the underlying causes are not. Hacking reverses this theme and says that the causes are real and the regularities are not, regularities come with a great baggage of theoretical descriptions. Regularities of nature need to be accounted and made sense of with theories. Proponents of entity realism are not in favor of theory realism, they are in favor of realism about entities and their causal properties. Moreover, good experimenters can employ their skills to interact with and manipulate the causes in the world. This interaction allows us to track and pin-point the causal properties of entities. According to the thesis of entity realism,

this sort of causal contact is enough to believe in the reality of these entities. Hacking says,

> [A] good many theoretical entities do really exist.....but one can believe in some entities without believing in any particular theory in which they are embedded. One can even hold that no general deep theory about the entities could possibly be true, for there is no such truth. (1983: 27)

This dislike of theories stem from the idea that we can have a non-representationalist account of realism about entities. The epistemic virtue of causal contact is enough to vindicate the existence of entities. Theoretical descriptions are considered as a dispensable baggage in the path to establish the existence of entities.

One example that the proponents of entity realism use so often to show that entities do exist even in episodes of theory- changes, is the case of electron. Hacking seems to be too much pre-occupied with electron. It is a 'real' unobservable entity according to him. He believes that the history of science about this entity alone can legitimately support the case of entity realism. Many theories about the nature of electrons appeared and disappeared since J.J Thomson speculated that the 'cathode rays' he was experimenting on in1987 might be composed of a stream of 'corpuscles', but the entity itself still has a place in theory. There is a sense in which one can believe the case that starting with Thomson, Millikan, Rutherford and throughout the twentieth century, a long line of experimentalists interacted with the same entity. There is also a sense in which it is fair to say that all of them were talking about the same entity.

Let us now move to the challenge from history of science and the explanation of the empirical reliability of science. The thesis of entity realism has a definite answer to the challenge and a distinctive explanation of empirical reliability to support their position. It is obvious that entity realism evokes the idea that entities do survive theory-change. Hacking subscribes to the causal theory of reference of Putnam to elucidate his position. In his work 'Experimentation and Scientific Realism' (1991), he elaborates the support his view invited from causal theory of reference under the sub-heading 'Our debt to Hilary Putnam'. He seems to be completely sure about the correctness of the causal

theory of reference and adds it as a supplementary thesis which will help the entity realist to respond to the challenge from history of science.

> Putnam saved us from such questions (questions about incommensurability and theory-change) by inventing a referential model of meaning. Says that meaning is a vector, refreshingly like a dictionary entry. First comes the syntactic marker (parts of speech), next the semantic marker (general category of thing signified by the word), then the stereotype (clichés about the natural kind, standard examples of its use, and present-day associations. The stereotype is subject to change as opinions about the kind are modified). Finally there is the actual referent of the word, the very stuff, or thing it denotes, if it denotes anything. (Hacking, I. 1991: 157)

Putnam believes that it is the referent, or the stuff that is denoted which survives the theory-change. For convenience, let us set aside the intricacies involved in the semantic and syntactic markers along with the stereotype. Let us simply take these as certain descriptions associated with a word and its sense. These three are independent of the referent of a term. What Putnam claims here is that the change in the sense of a term does not affect or cause change in its referent. The sense (stereotype and semantic marker) of the term 'electron' according to Hacking, changed quite a lot, from clouds and liquids to particles, so on and so forth. But the stuff or thing that is referred when one utters the term 'electron' remains the same. This is by virtue of a causal chain that starts from the naming of a particular entity through the voyage it undertakes in change of meaning to the present day. Putnam roughly conveys the idea that even though our beliefs and understandings about certain entity might change overtime, the actual thing remains the same through these changes. The strongest cases in support for the causal theory of Putnam come from the history of science. This may be the reason why Hacking is interested in Putnam's thesis. The idea that reference is retained across theory-change is the perfect view to be employed by entity realists in responding to the challenge from history of science.

The classic example is the historical episode where the syntactic marker, semantic marker and stereotype, all changed but the stuff which was referred to is identified as same in a radical theory-change. It is the case of 'de-phlogisticated air' and 'oxygen' in the seventeenth century chemical revolution. It is believed that Stahl and Lavoisier were both referring to the same entity (oxygen in the present day nomenclature), when they spoke about 'de-phlogisticated air' and 'oxygen' respectively. And there is a sense in which one can say that this is the case. One must also notice that the challenge in the form described in the second chapter does not apply to entity realism. The challenge has to be modified slightly to be answered by entity realists. It is not the theories which were true in history of science, but statements about certain predictive processes. These predictive processes were successful in the past but nevertheless were part of false theories. How is it that false theories can generate successful predictions? This is because experimenters were successful in feeling the presence of certain entities and in designing instruments which capture their causal behavior. A theory asserts more than the observations. Therefore the theory has excess story to tell about the entity and its behavior which invariably goes wrong. This is precisely the reason why certain theories were abandoned later on with the emergence of better evidence and also new theories in the area. The predictive processes rely heavily on causal properties of entities, and not on the speculative stories the theories tell. The instruments track some of the causal properties of entities relevant for the prediction of certain process, and this leads to successful prediction. The entities survive these changes at the level of causal interaction or intervention, not at the level of theories.

Let us carry on the discussion with a thought experiment. Consider two cases, one is an episode of theory-change involving a particle 'zeon'. The name of the particle remains the same in both the predecessor and successor theories. Another is an episode of radical theory change where the name also changes from 'xeon'(in the predecessor theory) to 'zeon(in the successor theory). Let us consider that 'zeon' in its present day character is identified with three causal properties (may be something charge, mass etc.). They are c_1, c_2 and c_3.

Let us consider case 1, where a theory having theoretical descriptions of zeon, T changes to another theory T^* with certain other theoretical descriptions of zeon. The name remains the same, the issue is that experimenters were predictively successful when they used theory T even though the theory itself was later on abandoned. The entity realist's response to this would be that the above mentioned past experimenters would have been yet engaging in a predictive process involving one or the other causal properties (or all) of zeon, namely, c_1, c_2 and c_3. This act made the predictive process successful. These causal properties are alone responsible for the entrenchment of the theory T. The theoretical descriptions about zeon are not relevant in this act (we must keep in mind the non-Humean position about regularities of unobservable world. They need not be true, but what is true is the cause). Similarly, in case 1, what played a role in the predictive success was the causal property/properties of zeon.

Case 2 is an extreme case where theory T and T^* differ even in the naming of the entity. 'Xeon' is associated with T whereas 'zeon' is associated with T^*. Here too, the issue to be dealt with is the seemingly inexplicable notion that theory T was predictively successful but later on abandoned. However, the present day predictively successful theory T^* explains the same domain (and more) which T explained in the past. The difference is that one contains the descriptions of a particle xeon, the other contains that of zeon. To make sense of this, entity realist would say that xeon and zeon are referring to the same stuff/thing comprising the causal properties c_1, c_2 and c_3. The reason why theory T was once successful is because the experimenters were using some or all of these three causal properties while engaging in a predictive process, but their theories described the process differently and identified the entity with a different name. Roughly, their claim is that past predictions were successful not by virtue of theoretical descriptions, but by virtue of getting the causes correct in the predictive process.

Entity realists respond to the challenge from history of science by invoking the theory/entity distinction. One important move to be noted here is that they are responding to the epistemic stance of scientific realists when they answer the challenge. The theory/entity division brings the much needed epistemic humility which lacked in scientific realism. From their response to the challenge, we can easily infer their version

of explanation of empirical reliability of science. They maintain that there is a link between empirical success of science and the reality of causes, unlike the scientific realists who try to establish the link between success and truth. But this success has to be interpreted according to a single-tier model of realism. There is no role for theories and hence, empirical success is explained away by the causal powers of entities and experimenters' processes to arrive at predictions. The reality of causes or the reality of causal properties of entities is responsible for the empirical success of science. Not the truth of these claims. Here, empirical reliability is to be taken in a qualified way so as to match the ideas put forth by entity realists. Entity realist explanation of success thus is that, the success of science is to be considered as a miracle if there are no real entities in the mind-independent world. The existence of entities alone is sufficient for the entity realist to explain away the empirical success of science. That is, there is a necessary connection between the empirical success of science and the existence of entities together with their causal properties.

Let us recapitulate the relevant aspects of entity realism. Entity realists are realists about entities, not theories. Their position invites a necessary division between theory and entities. Entity realism has no room for any notion of truth. Entity realism responds to the challenge of Laudan by bringing in the non-Humean idea of reality of causes. Past success of science is explained by the view that these past theories were able to track at least some of the causal properties of entities. Thus it is possible to warrantedly believe that entities at the level of referents are retained across theory change. Theoretical descriptions may not be retained, even the names would change, but referents remain the same. The major stroke of the entity realist is in the claim that past predictive processes employed some/all of the causal properties of this referent. The entity realists further deploy their own version of explanation of empirical reliability of science by saying that the success of science can be explained with reference to the reality of entities. Let us now look closely the position of structural realism.

III.3 Structural Realism

Structural realism came to the forefront of the scientific realism debate in the mid nineties with the much celebrated article 'Structural Realism: The Best of Both Worlds'

by John Worrall (1989). However, in the history of philosophy of science, there were thinkers who upheld similar positions. We can see glimpses of structuralist ideas in Poincare though it was not developed further. Worrall refers to Poincare often in elucidating his structural realism. Russell, Maxwell and Ramsey had much developed and sophisticated structuralist positions. I will not elaborate on these positions. What I am intending to do is to give a bird's- eye view of the current version of structuralist position and their conceptual background. As his title says, Worrall takes his position to be the best of both worlds, namely realism and empiricism. In a nutshell, structural realism is the view that structural claims about the unobservable world are true and that we can know only these structural features of the world. As I pointed out in the first chapter, this position definitely falls in the two-tier model of articulating realism. Worrall believes that his structural realism avoids the charges raised by Laudan and also upholds a version of explanation of ERS. Following are Pierre Cruse's words about Worrall's ideas. These statements give the general reflection on structural realism among contemporary philosophers of science:

> Structural realists argue that their position can provide a synthesis of both scientific realism and anti-realist empiricism, and retains the advantages of each view. (Cruse, P. 2005: 556)

To avoid the charges leveled by Laudan, Worrall claims that certain things are retained across theory change (conceptual change), and these are structures. He mentions that certain mathematical-logical structures alone are 'hooked-on to the world'. The rest of the assertions in the theory cannot be justifiably believed to be rightly representing the world. In other words, Worrall says that structural assertions are true of the world and they are eventually responsible for the success of science. Worrall substantiates his structural realism by bringing in the case study of conceptual-change from Fresnel's theory of light to that of Maxwell.

> It is true—and importantly true—that many of the mathematical equations supplied by the wave theory of light still live on in science; and it is true—and importantly (if rather obviously) true—that repeatable (and repeated)

> experiments do not change their results, so that all the correct empirical consequences of the wave theory are still, of course, correct. Nonetheless, at the theoretical level there has been radical, ineliminable change. (Worrall, J. 1990: 342)

Worrall here point out that structure can be retained across theory-change, and structure can be known. This is an intricate position and needs further scrutiny. First of all, Worrall has a metaphysical idea in his mind when he speaks about structural realism.

> On the structuralist view what Newton *really* discovered are the relationships between phenomena expressed in the mathematical equations of his theory. (1989: 122)

James Ladyman, who is a contemporary pioneer in structural realism, believes that this metaphysical idea is evident in the above quote. Worrall often is talking about structures in the world. Ladyman claims that Worrall is hinting here towards a much bolder claim, that structures in the theory are indeed about real structures in the mind-independent world. Moreover, these structures are known, and hence carry an epistemic idea too, that is, we can know these structures. Structural realism according to Worrall avoids the challenge from history of science because it maintains that structures preserve truth (across theory-changes) and hence success. Past theories were successful because of the structures they inherit from their predecessors. Theories were successful because of such structural retentions. According to structural realists, the content or non-structural aspects of these theories were responsible for the eventual downfall of them. One can see that structural realism is not like scientific realism. Structural realism does not have the presumptuous force. It does not say that representations map reality in toto like the realists. This is one of the reasons why structural realists successfully avoid Laudan's challenge. They say that scientific theories miss out on something about reality. They set up a nice gap between reality and representation. This is done by maintaining a structure-nature distinction or structure-content distinction. Structures are real whereas natures are not. Similarly, structures can be known whereas natures cannot be. Structural realists also have an explanation of the success of science. They claim that scientific theories are

successful because structures in them are hooked-on to the world. Because of this, structures bring about truth. They believe that the best explanation for the predictive success of scientific theories is given by the notion that structural features of the world are correctly represented by structures in theories.

The combination of the ideas, 'structures are real' and 'structures can be known', in fact leads to two very interesting and unique theses of the structuralist view. Ladyman names them as 'ontic structural realism' and 'epistemic structural realism' respectively. Ontic structural realism is the position that all that is there in the world are structures, and natures can be characterized in terms of structures. Epistemic structural realism is the position that all that we can know about the unobservable world are structures i.e. the nature is not known. Ontic structural realism is in fact a full-blown realist position, which translates conent/nature into structuralist terms. Epistemic structural realism keeps the gap between reality and representation. I will be discussing epistemic structural realism here (not ontic structural realism) in detail as my intention is to pick out positions which speak about such a gap between representation and the world. Psillos believes that epistemic structural realism employs a restrictive strategy, which is that we can reach till the structural features of reality and ontic structural realism is an eliminative strategy which suggests that there is no nature/content in the unobservable world. According to them, even if there is nature/content, they can be translated to structuralist terms. Epistemic structural realism restricts or constraints our epistemic abilities whereas ontic structural realism eliminates the non-structural dimension of unobservable world from our metaphysics. Let us discuss epistemic structural realism in detail.

III.3.1 Epistemic Structural Realism

In short, epistemic structural realism is the view that allows only the knowability of structural features of the unobservable world. It restricts scientific knowledge to the structure and hence is agnostic or even skeptical about the nature of the unobservable world. It is realisic because of the assertion that structures of the mind independent world exist and can be known. It is a skeptical position because we cannot know the nature of the unobservable world. Worrall and Elie Zahar are considered to be thinkers of contemporary importance in epistemic structural realism. Worrall initially did not

develop a reasonable epistemic structural realism, but recently he and Zahar and many others try to support the Ramsey-sentence version of epistemic structural realism by subscribing to a modified Russellian structuralism.

Let us now be very clear about the meaning of the term 'structure' employed by the proponents of epistemic structural realism. This can't be a concrete structure mapping first order relations. It has to be definitely a second/higher order abstract structure because the first order relations after all constitute the nature of the unobservable world, and according to epistemic structural realism, this nature cannot be known. Structure for epistemic structural realism supporters is an abstract entity. It is a relation drawn between a set of objects. But these objects do not in any way constitute the structure. Anjan Chakravartty who himself roots for a much concrete notion of structure states what could be the idea of structure laid in epistemic structural realism. Chakravartty mentions that Russell spoke about structural identity and structure in the following manner,

> [W]e shall say that a class A ordered by the relation R has the same structure as a class B ordered by the relation S, if to every term in A some one term in B corresponds, and vice versa, and if when two terms in A have the relation R, then the corresponding terms in B have the relation S, and vice versa. (Russell, B. 1948: 271)

Russell here is talking about an abstract structure and structural identity, a non-concrete non-first-order structure which doesn't specify the objects it encompasses. Let us call this simply abstract structure. Epistemic structural realism subscribes to such abstract structures while talking about structures. The members of A and B need not bear any qualitative similarity to one another. A formal similarity is the only requirement. Structural identity does not depend on what sorts of relations these objects are tied down in their respective classes. Psillos inspired by Shapiro calls these abstract structures *ante rem* structures.

> Following Shapiro, we might distinguish between two versions of structuralism. *Ante rem* structuralism has it that structures are abstract,

> freestanding, entities: they exist independently of systems, if any, that exemplify them. In a sense, they are like universals (more like Platonic universals than Aristotelian ones). *In re* structuralism takes systems as being ontically prior to structures: it denies that structures are freestanding entities. Structures are abstractions out of particular systems and claims about the structure are, in effect, to be understood in one of the following ways. Talk, say, about the natural-number structure is talk about *any* system structured in a certain way, namely, having an infinite domain, a distinguished element *e* in it and a successor function *s* on it such that the conditions specified by the Peano axioms are satisfied. Or, talk about the natural-number structure is talk about all systems structured in the above way. (Psillos, S. 2009: 139)

I am not going to the details of *in re* structuralism, which is physical-system-specific or nature-specific. They shed light on the nature of the unobservable world and thus not subscribed by supporters of epistemic structural realism. It is obvious from Russell's version of structural identity mentioned above that the supporters of epistemic structural realism are more inclined towards the *ante rem* structures. They are just interested in the logico-mathematical properties of certain relations. Maxwell modifies epistemic structural realism in such a way as to accommodate Ramsey-sentence strategy of treating theoretical terms. Here, the referential status of theoretical terms is irrelevant. This interpretation claims that the cognitive content of a scientific theory lies in its Ramsey-sentence. A Ramsey-sentence of a theory replaces all theoretical constants with distinct variables, and then binds these variables by placing an equal number of existential quantifiers in front of the resulting formulae. By staking the explanation of the success of a theory on the approximate truth of its Ramsey sentence, epistemic structural realists think that scientific realism is no longer hostage to any particular theory of reference. That is, according to standard interpretation of scientific realism, predictive success legitimates claims to correct reference, and correct reference is necessary for a theory to be even approximately true. If one can show that accepting this link between reference and approximate truth is not necessary for scientific realism, then it might be

possible to overcome the pessimistic meta-induction. Chakravartty neatly sums up such a move:

> An unobservable entity whose place is held by a predicate variable in a Ramsey sentence is ‘‘whatever it is’’ that satisfies the relations specified by the sentence. This ‘‘indirect’’ reference is achieved by ‘‘purely logical terms (variables, quantifiers, etc.) plus terms whose direct referents are items of acquaintance [ie. Unobservables]
> …….We do not know what the natures of theoretical entities are, but we can assert that they exist and stand in certain relations. (2003: 869)

The motivation for turning into Ramsey-sentence strategy for elucidating structural realism is the idea that abstract structures provide no knowledge of the objects. But at the same time, we can know the kind of relations these objects or physical systems draw. Russell believed that we can never know the nature of the unobservable world; nevertheless, we can know the structural characteristics i.e. purely logico-mathematical properties of certain relations. Our best scientific theories can provide us with knowledge about certain properties of concrete relations between objects, but we can never know these first order relations between entities, or entities themselves. This is precisely because both the entities and their first order relations constitute the nature of the unobservable world. Epistemic structural realist recognizes only the knowledge of mere abstract structures. Ladyman gives a clear sense of the Ramsey-sentence strategy and the corresponding version structural realism.

> If we formalize a theory in a first-order language: Φ (O_1……O_n: T_1……T_m), then the corresponding Ramsey-sentence is $\exists T_1$……$\exists T_m$ (O_1……O_n: T_1……T_m). Thus the Ramsey sentence only asserts that there are some objects, properties and relations that have certain logical features, satisfying certain implicit definitions. It is a higher-order description, but ultimately connects the theoretical content of the theory with observable behavior. However, it is a mistake to think that the Ramsey-sentence allows us to eliminate theoretical entities, for it still

> states that they exist. It is just that they are referred to not directly, with theoretical terms, but by description or by logical form, that is via variables, connectives, quantifiers and predicate terms whose direct referents are known by acquaintance. (1998: 412)

Ladyman further describes that Maxwell and Russell take scientific knowledge to be knowledge of the unobservable world, but structural properties of this world is known rather than intrinsic properties. It is also said that knowledge of the unobservable world is limited to its second-order properties.

Let us now move to the issue of how epistemic structural realism proponents are able to counter Laudan's challenge and come up with their own version of explanation of empirical reliability of science. So far, we have two fundamental conceptions of epistemic structural realism.

1) The structure we know of the unobservable world is abstract structure.
2) Abstract structures are retained across theory-change.

From these two constitutive theses of epistemic structural realism, we can guess their response to Laudan's challenge. History of science is full of abandoned theories which were formerly believed to be true. However, certain features of the theories are retained, and they are abstract structures. The theories were successful once not because of the non-abstract structural features in them, but because of the role played by abstract structures. In other words, the nature of the theory is responsible for the failure, and since abstract structures are retained, truth is somehow preserved. Abstract structures have the capacity to generate successful predictions. It is however, important to analyze the role played by abstract structures, devoid of any physical interpretation, to be playing a role in any predictive processes. The realists counter attack by invoking this claim. I will discuss this later in the chapter. Epistemic structural realism has its own version of explanation for success. Here again, they rely on the role of abstract structures. The proponents of epistemic structural realism agree that there is a necessary link between predictive success and truth. Moreover, predictive success is acquired by the role played by abstract

structures. These logico-mathematical structures preserve whatever little element of truth that was there in past theories and carry it forward to newer versions of the theories.

Epistemic structural realism thus invited a more than required degree of epistemic humility. They say that we can only know structural features of the world, abstract logico-mathematical structures, not concrete first-order structures. Scientific theories according to epistemic structural realism miss out a great deal about the unobservable world. Theories hit only a small fraction of the characteristics of the mind-independent world, that too with certain abstract structures which are silent about the nature of the world. The structure/nature division keeps structural realism a great deal distanced from scientific realism. Moreover, the abstract structure/concrete structure division pushes them further towards anti-realist empiricism.

III.4 Semirealism

Anjan Chakravartty has recently come up with an idea that scientific realism can be modified in such a way as to only accommodate our commitments to the detectable features of the unobservable world. Rather than classifying the whole debate on the traditional observable/unobservable distinction, Chakravartty evaluates the debate along the lines of a distinction between detectable/non-detectable aspects of the world. In doing this, he brings in a fine blend of entity realism and epistemic structural realism, but only after modifying both of them to an extent. Entity realism suggests that entities exist because we can manipulate them and intervene in the causal processes involving them, and epistemic structural realism is the view that we can know the structural features of the unobservable world. Chakravartty proposes that what we know is a blend of both entities and structures. We know them together, as 'concrete structures'. We know nature through these concrete structures (the structure is not abstract as proponents of epistemic structural realism would maintain, but concrete). These concrete structures presuppose the knowledge of entities. Psillos and Dimitris Papayannakos puts Chakravartty's project in appropriate sentences.

> [Chakravartty aims] to develop a viable realist position which capitalizes on insights offered by entity realism and structural realism, while

> transgressing them. Semirealism, as Chakravararty calls it, comes out as a form of selective skepticism (or selective optimism, if you wish) which restricts epistemic commitment only to those parts of theories that can be interpreted as describing aspects of the world with which scientists have managed to be in causal contact. Semirealism adopts the epistemic optimism of entity realism (which is grounded on cases of experimental manipulation of unobservable entities), but adds that knowledge of causal interactions presupposes knowledge of causal properties of particulars and relations between them. Semirealism also adopts the epistemic optimism of structural realism (which is based on structural invariance in theory-change), but adds that the operative notion of structure should be concrete and not abstract. (Psillos, S and Dimitris, P. 2009: 204)

Let us now evaluate the notion of 'concrete structure' and, what Chakravartty captures by employing this term. He says that these are relations between first-order causal properties (these properties are responsible for causal interactions). These properties are to be understood as having a disposition to engage in causal processes. In other words, they have dispositional identities based on their causal properties. Scientists are able to detect these properties most of the times. Chakravartty comes up with a heavily loaded metaphysics of properties and relations in elucidating his position. He brings in the notion of detectable/non-detectable properties, causal/non-causal properties and auxiliary or fictional properties. The unobservable world can have both detectable and undetectable elements. Detectable properties of the entities in the unobservable world participate in certain causal processes and form these detectable parts, which can be causally contacted by our best epistemic abilities. Further, Chakravartty says that there could be fictional properties of entities which are developed for the overall knitting of the theory, or for explanatory reasons, things like the 'ether'. These fictional properties may not exist at all. Auxiliary properties are recognized as either undetected properties (these might be detected later), or fictional properties. The interesting stance that Chakravartty takes is that when we change the conception of structures in epistemic structural realism from abstract to concrete, it invites the knowledge of nature of entities. Relational properties of entities will consist of knowledge of the entities. The structure consisting of

these relational properties is not higher-order abstract structure, but concrete. Dean Rickles neatly sums up the position of semirealism.

> He [Chakravartty] argues that entity realism and structural realism both lead to semirealism when pressure is applied. The vital link is provided by detection properties': causal properties leading entity realists to believe in their entities. (These are contrasted with auxiliary properties that might be causal but are not detectable and so ought not to be given definite ontological credence). But the relations that structural realists will claim define their structures are, says Chakravartty, nothing but relations between these selfsame detection properties. Knowledge of detection properties, however, cannot help but involve knowledge of the particulars that have them, for particulars are (minimally) compresent properties found together according to certain regularities. (2009: 262)

The following illustration[12] makes the metaphysics of properties proposed by Chakravartty comprehensible. The auxiliary properties of entities play a pivotal role in semirealism. They are undetectable according to the current standards of scientific investigation, or are merely fictitious.

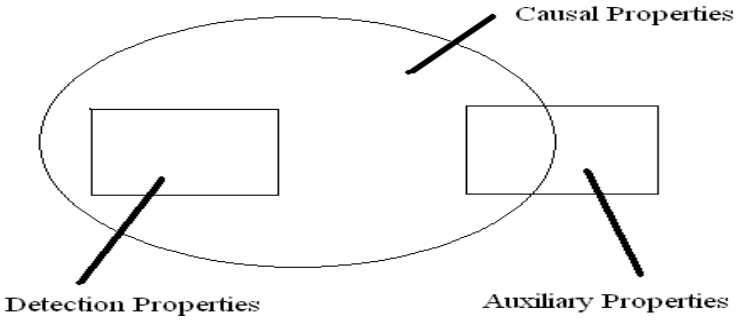


Figure 2. Chakravartty's Classification of Properties of entities.

[12] Illustration taken from, Chakravartty, A. (2007) A *Metaphysics for Scientific Realism: Knowing the Unobservable*, Cambridge: Cambridge University Press. p 48.

We have to also note that Chakravartty here is speaking about 'detection' in very similar terms as the entity realists. We can causally jiggle with certain entities. However, we don't have to buy a theory/entity division to make sense of semirealism. He endorses a two-tier model of realism, consisting of representation and reality, unlike Hacking's position, which asserts reality without relying on representation. Hacking dispenses with the notion of representation and truth altogether. Chakravartty reloads entity realism with the notion of representation by subscribing to a semantic or model-theoretic notion of theories. In addition, he brings in a notion of approximate truth which replaces issues relating to truth and correspondence. I am not discussing these aspects of semirealism here. I would only evaluate the position of semirealism as a response to the epistemic stance of the scientific realists, and as an answer to PI. Semirealism is motivated by the need that our epistemic commitment should be restricted to those parts of theories that are more likely to resist future revisions. Future revisions are not possible in the case of concrete structures according to Chakravartty. The goal of scientific investigation is to convert or transform as many auxiliary properties to detection properties. Here lies an incredible way of articulating the aim of science. I believe this aspect of semirealism has to be applauded because Chakravartty here is amongst a handful of thinkers who brought the notion of aim of science in a comprehensible way after Popper. This is achieved by employing a metaphysical thesis at the level of properties and relations.

Let us now ponder over the issue of how semirealism is different from entity realism. In the late nineties and early eighties, the criticism over entity realism mounted to newer heights by targeting the theory realism/entity realism division. The general sense amongst thinkers was that some extent of the idea of truth of theories needs to be added to the position of entity realism. One can see from a closer look, that this is precisely what Chakravartty does, but by also taking cues from epistemic structural realism. He suggests that epistemic structural realism is wrong in subscribing to the notion of abstract structures. A fusion of entity realism and epistemic structural realism is possible if one appeals to the notion of concrete structures. Concrete structures are first-order relations between entities, or these are in a sense representation of causal structure between entities, because entities have causal properties with dispositional identities. In other words, concrete structures can reveal the structure and nature of an entity, but both

in a minimal manner. Structure thus revealed is not abstract logico-mathematical structures, but first-order relations between entities. Nature of entities unraveled by means of concrete structure does not shed light on every property of that particular entity, but only their causal properties which are detectable. There could be causal properties of this entity which are undetectable. So for the time being, they remain as auxiliary properties. Further, chakravartty claims that the knowledge of causal properties of the entity constitutes the knowledge of its intrinsic properties too. Psillos puts this clearly:

> It [i.e. the notion of concrete structure] implies that one cannot have knowledge of the structures without also having knowledge of the intrinsic natures of things that make up the structure. Thus, Chakravartty claims, knowledge of concrete causal structures contains 'unavoidably' knowledge of intrinsic natures of particulars, and vice versa. It implies that one cannot have knowledge of the structures without also having knowledge of the intrinsic natures of things that make up the structure. Thus, Chakravartty claims, knowledge of concrete causal structures contains 'unavoidably' knowledge of intrinsic natures of particulars, and vice versa. (Psillos, S and Dimitris, P. 2009: 204)

In Chakravartty's conception, there are no properties which are idle such that they do not participate in at least one causal process in the mind-independent world. There can nevertheless be fictitious properties, which exist but not known through detectable means. Chakravartty does not have such a category. For him, idle/auxiliary properties are either non-detectable at present or totally fictitious. They are provided for explanatory reasons. Chakravartty doesn't have a notion of idle properties which do not participate in any causal processes. He employs a primal set of causal properties. The set of detection properties is a subset of this set. The set of auxiliary properties are spread across both causal properties and fictional properties. That is, either they exist as causal properties or they do not exist (but are framed for the knitting of the theory). That is, these auxiliary properties can also be fictional properties, which do not exist, but can never be idle properties. Every property an entity has is detectable and contributes to the nature of that

entity. What is non-detectable at present are taken to be auxiliary, but in the future, they will be abandoned or get transformed into detection properties.

Let us now move on to the semirealist's response to PI and its own version of EDR. The much needed epistemic humility is accommodated by semirealism by bringing in the notion of auxiliary properties. The gap between representation and reality is invoked by these auxiliary properties, the revision-prone characteristic of a theory. They may go wrong when new evidences are accumulated. But the detection-properties are not revision-prone. They are retained and are responsible for the explanatory success of the theory. The interesting issue that emerges here is, how is it that one can conformingly pick/demarcate a given mathematical structure to be that of detection properties and relations from that of auxiliary properties and relations. This poses a serious threat to the theses of semirealism. How can we interpret a structure to be referring to the detectable aspects of the world or to the auxiliary aspects? Chakravartty has a solution for this. He calls for a 'minimal interpretation' of mathematical equations which will differentiate auxiliary from detectable. Psillos ably sums up this strategy:

> Chakravartty appeals to what he calls a 'minimal interpretation' of the mathematical equations that make up a physical theory in order to demarcate the concrete causal structures associated with the detection properties from those associated with auxiliary ones. A minimal interpretation interprets realistically only those parts of equations that, in the context of a specific detection process, are indispensable for describing the (corresponding to that detection) concrete causal structures. (Ibid)

Having made this move, one can say that what get retained across theory-change are these minimally interpreted mathematical equations capturing concrete structures. These concrete structures represent the detectable features of the unobservable world. The response to PI is triggered along this direction, that is, certain theories in the past were successful because of the role played by these concrete structures in predictive processes. The failure of these theories is accounted by auxiliary properties which turned out to be non-existent, according to new evidences. Chakravartty says that,

> [Current] theories do not retain all of the structures described by their predecessors. But not all structures are causally connected to our practices of detection. The realist should expect to retain only those structures required to give a minimal interpretation of the mathematical equations used to describe well-established practices of detection, intervention, manipulation, and so on. (2007: 50)

One can also develop a version of EDR or NMA by taking the cue from semirealists. There is a necessary link between empirical success of scientific theories and truth, but this is obtained by the exact reference of certain minimally interpreted mathematical equations, or concrete structures. Success can be explained with reference to these concrete structures and causal properties which are detected.

III.5 Scientific Realist Responses to Entity Realism, Epistemic Structural Realism and Semirealism

There are several criticisms of selective skepticisms from a scientific realist point of view. However, I will be brief and choosy here in picking certain relevant attacks from this rather vast list. First of all, let us look into the major criticism against entity realism. The theory/entity distinction is targeted straight away by the scientific realist as it is a contentious claim. Psillos puts the point bluntly:

> [The] distinction between being realist about entities and being realist about theories is misconceived. It may well be the case that electrons exist, even though some (or most) of our descriptions associated with the term 'electron' are false. But the issue at stake is different. It is this: can we assert that electrons are real, i.e. that such entities exist as part and parcel of the furniture of the world, without also asserting that they have *some* of the properties attributed to them by our best scientific theories? I take it that the two assertions stand or fall together. Experimenters do not know what exactly it is that they manipulate, although they can know that they

> are manipulating something, unless they adopt some theoretical description of the entities they manipulate. (1999: 248)

Psillos opines that it is impossible to intervene and manipulate the unobservable world without also believing in at least some of the theoretical descriptions about them (if not all). The distinction between entity and theoretical descriptions of these entities makes no sense to the realist. Psillos further adds that if we are to feel the existence of electrons, and not neutrinos, then we must at least know some properties of electron and its difference between the properties of neutrinos. In addition, we must also know the laws that electrons obey and the laws that neutrinos obey, and importantly again, the difference between them. Psillos suggests that the justification for accepting the existence of entities and accepting the correctness of theoretical descriptions is the same. There is no epistemic difference in both these acts. We must admit that at least some theoretical descriptions about the causal properties of entities are true. The semirealist makes this move as we already saw. Psillos bluntly puts his criticism in the following sentences.

> Can we assert that electrons are real, i.e. that such entities exist as part and parcel of the furniture of the world, without also asserting that they have *some* of the properties attributed to them by our best scientific theories? I take it that the two assertions stand or fall together. (Ibid)

It is quite embarrassing for the realist to have a claim that experimenters do not know what exactly it is that they are manipulating, although they are manipulating something which exists. This doesn't make sense unless they also have knowledge of certain (minimal) theoretical descriptions which they believe to be true. I take this charge by the realists to be very serious and it might take a great effort from entity realists to avoid it. Another issue that the supporters of scientific realism and also some of the selective skeptics raise is that it doesn't make any sense to think of an entity devoid of its properties. Entity realism doesn't claim that we can know the properties of entities because these assertions would be part of the theoretical descriptions. Strictly speaking, we know that entities exist and nothing else. But what are these entities without these

properties? Either one has to say that entities are bundles of properties or that they are properties upon some substrata, may be some bare substrata. Is this what the entity realism supporters claiming to be existing in this world, bare substrata? But this doesn't seem to make any sense without the properties. How can such a substrata be causally active without the properties? Let us now move to the attacks against epistemic structural realism.

I briefly discussed scientific realist's position of structure-nature continuum in chapter I. According to scientific realism, it is impossible to have a distinction between structural and natural features of the unobservable world. Worrall, nevertheless entertains such a distinction, especially when he says,

> [I]t seems right to say that Fresnel completely misidentified the *nature* of light, but nonetheless it is no miracle that his theory enjoyed the empirical predictive success that it did; it is no miracle because Fresnel's theory, as science later on saw it, attributed to light the right *structure*. (Worrall, J. 1989: 117)

Psillos wants to hinge on the issue that Worrall and other structural realists of the epistemic version believe in the difference between structure and nature, and this distinction is uncalled for. Psillos intensely questions the structural realist presupposition, when he says:

> The question is this: can we draw a distinction between the *nature* of an entity and its *structure* such that we can claim to know its structure but not its nature? …..When scientists talk about the nature of an entity, what they normally do —apart from positing a causal agent—is to ascribe to this entity a grouping of basic properties and relations. They then describe its law-like behaviour by means of a set of equations. In other words, they endow this causal agent with a certain causal structure, and they talk about the way in which this entity is structured. I think that talk of 'nature' over and above this structural description (physical and mathematical) of a

> causal agent is to hark back to the medieval discourse of 'forms' and 'substances'. Such talk has been overthrown by the scientific revolution of the seventeenth century. (1999: 148)

Psillos is trying to establish the view that the structure and nature of an entity form an inseparable continuum. He argues in a similar way to the manner he challenged the stand of entity realism viz that knowing what an entity is, goes beyond its structural features.

The second and much severe criticism comes from the notion that abstract structures are incapable of constituting any physical system about which it is thought to be structured. Since the response to PI by epistemic structural realist lies on the notion of abstract structures, one has to also show that how these abstract structures are representing any physical phenomena. To say that abstract structure alone is enough to answer PI by claiming that they participate in predictive processes is not a viable option, precisely because a predictive process has auxiliary hypotheses and other theoretical descriptions which are non-(abstract)-structural. Psillos' concern here is that, how is it that these *ante rem* structures are privileged such that they alone are responsible for the predictive success of scientific theories.

Finally, the realists attack semirealism. Semirealists and scientific realists engage in a rather interesting metaphysical debate, which reflects the question of whether just causal properties of entities are responsible for a structure or that there are something else too which contribute to it. The talk of concrete structure nevertheless is appreciated by the realist, but when it comes to finer and subtler metaphysics of individuals, properties and relations, they disagree severely over certain issues. The semirealist says that causal properties of entities along with its relation to that of other entities are what are captured by the concrete structure. These causal properties are intrinsic properties and also detection properties, that we can causally interact with them and thus be sure of their existence. The influence of entity realism is quite visible here, that one can causally play or jiggle with them in experiments and assert their existence. Semirealists work with a minimal account of the notion of nature-structure continuum unlike the realists. Realists find it hard to conceive that one can blame just auxiliary properties and flee the scene of

the charges leveled by the thesis of PI, because auxiliary properties are unknown properties (undetected), maintained by the theory. They could be either causal or simply fictitious. So what if these auxiliary properties are contributing mostly to the behavior of an entity and there are only a few detection properties? Semirealists also thus feel the pressure and claim that they are also realists but of a sophisticated type and they never want to undermine the thesis of realism, but just that they are interested to fine-tune the realist metaphysics and thus strengthen it against the thesis of PI, as well as against charges leveled by other selective skeptics.

Dean Rickles mentions that semirealism face the same charges that are leveled against entity realism. One of them is that the domain of astronomy is not regarded as science proper if we take Hacking's view seriously. Such domains lie outside the reach of our experimenters, and thus they evade all manipulation and intervention.

> [….if] we tie our realism to manipulation and intervention in this [semirealist] way then we face the impoverished account that Hacking's bare entity-realism faces, rendering much that is of interest in modern physics and cosmology out of bounds for realists. (Rickles, D. 2009: 264)

This is indeed a grave consequence, invited by keeping the detectable/non-detectable distinction. What about the domains of science which lie outside these detectable features of the world? There are a lot of areas of science where we are not able to manipulate or intervene with the causal properties of entities. However, the major criticism comes from Psillos. He together with Papayannakos, accuse that the notion of concrete structure is not clearly elucidated by the semirealist:

> Isn't there a tension here? If detection properties are specified independently of the theory, there is no need to interpret the theory *minimally* to get to them. If, however, they are specified in a theory-dependent way, this theory should be already interpreted prior to fixing the detection properties—and in all probability more than a minimal

> interpretation will be required to specify which properties are detection and which are auxiliaries. (Psillos, S and Dimitris, P. 2009: 205)

Psillos and others targets the notion of structure employed by Chakravartty and the means of individuation employed therein to support his position. Psillos thinks that it is in some sense impossible to sort out non-abstract structures and abstract structures within a scientific theory. Further, a 'minimal interpretation' of mathematical equations in picking concrete structures asks for more trouble.

It is obvious that the selective skeptics are challenged by the scientific realists on various grounds. Till now, in this thesis, we have mapped the externalist positions and their constitutive features. We have also seen the criticisms and counter criticisms amongst them. In the next section, I move to the internalist positions. The emphasis is to see how they differ from the externalist positions like scientific realism by accommodating a conceptual-scheme-dependency of reality. I do not try to drag them into the debate because Laudan's PI does not affect them. However, their views are pivotal in this thesis because of the contrast it brings out with scientific realism. This contrast helps us to see the other available concepts than the typical scientific realist ones concerning 'truth', 'real' etc.

III.6 Is *Being* Dependent on *Knowing?*: Putnam's Internal Realism

The question 'Is *being* dependent on *knowing*?' roughly captures the worry that almost all internal realists have. Of course, the term 'knowing' here conveys a complex set of ideas regarding human knowledge. Representations, descriptions and being able to causally interact with the world are all components of this. To know that the table in front of me has four legs, I need to perceive its four legs and also have in mind conceptions of objects and their parts such as tables, legs etc. Perhaps, to avoid visual illusions, I should go and touch these legs and feel that they are for real. 'Knowing that something is' and 'knowing what something is' comprise thus, a vast array of complexities which includes the functioning of our mind and language. This is the picture that the internal realist is invoking. According to this picture, knowing is a

combined causal-linguistic procedure. 'Causal' in the sense that we (our minds) need to interact with the world through our sensations and that there is no causal closure between our mind and the world. Linguistic component of knowing is the most controversial element in the discussion of internal realism. The idea that we organize and categorize the world is captured by this linguistic component. For the internal realist, the world does not have pre-established joints or boundaries in structuring its objects, the mind structures it. Here is the bite of the linguistic component. A conceptual scheme organizes or categorizes the world. Therefore, most internal realists convey the idea that the ontology of objects and its structure depends on a conceptual scheme. Our being able to describe or represent the world in some way necessarily compromises the ontology of the world as *it is in itself*. This is diagonally opposed to the scientific realist position. However both internal realists as well as the scientific realists allow causal independence of the world. This means that the world or objects in the world are not caused by the mind. In other words, causal component of knowing does not toy with reality. The onus of the idea that our representations compromise the real (as *it is in itself*) rests with the linguistic component. All internal realists agree on the conceptual-scheme dependency of the 'real'. The idea of ontological dependence (or independence) is a rather difficult idea to sketch out as compared to the notion of causal dependence (or independence). Let us consider the following example aptly explained by Gabor Forrai:

> [An] umbrella is causally dependent on the mind of its designer. It has come about partly because someone has designed it. At the same time, it is ontologically independent of the mind of its designer. It may continue to exist even after its designer's death. Moreover, it could have been designed by someone else. On the other hand, the color of the umbrella is ontologically dependent on its being extended. Were it not extended, it could not have color. But its color is not causally dependent on its being extended. (2001: 12)

Forrai captures the idea of ontological dependence with this analogy. The ontological dependence of the world needs to be taken only as dependence at the level of structure (organization of objects in the world). The world is not in any way dependent on

the human mind for its existence. But the classification or order of objects, namely the structure is dependent on the human mind. The umbrella analogy is slightly misleading. The absence of extension forbids the happening of color. Or being extended is a pre-condition for color to exist on it. But the absence of mind does not erase the existence of the world. Rather it leaves out the possibility of an organized and categorized world. It would definitely leave the world in a brute state.[13]

The world does not depend on the mind to exist, but the ontology of the world (its objects and classification) is dependent on the mind. The ontology of things in the world depends on the mind, not for its existence, but for its character. This is a serious claim which needs further elaboration. This metaphysical claim is the starting point for a meaningful discourse on internal realism. Putnam metaphorically hints that "the mind and the world jointly make up the mind and the world"(1981: p xi). This metaphorical sentence contains both the linguistic as well as the causal component of knowing. The world causally reacts to our interactions which is picked, classified and given proper character by the human mind. Luca Moretti elaborates Putnam's metaphor as follows:

> [The] metaphor "the mind and the world jointly make up the mind and the world" says – in my opinion – that the formal structures provided by language-users' minds (the mind) and the independent [causal] content provided by the world (the world) jointly make up the world as conceptualized by language-users' minds but distinct from the latter, as not constituted or caused by them (the mind and the world). (2008: 44)

Putnam did not elaborate on the metaphor himself, as he must have believed that it would bring down the beauty and force of the idea that is to be conveyed by the metaphor. The causal component is the only realist thrust that can be felt in Putnamian internal realism. That is, the role of 'the world' in the metaphor. It is an ontological realism about the existence of the world alone. One should be cautious here not to compare it with ontological realism implied in scientific realism. Internal realism only

[13] I feel that the umbrella analogy is perhaps best suited for elaborating idealism. However, the analogy captures causal dependence; which it is not a charge against both internal realism and scientific realism. Both these positions don't subscribe to causal-dependence.

subscribes to the existence of a mind-independent world. It does not endorse the mind-independent nature/character of the things and its classifications in the world. Let us now get into the details of the metaphysical stance of internal realism and analyze it in the light of the metaphysical stance of scientific realists discussed in chapter I.

The metaphysical stance of scientific realism comprises two different expressions.

A. There exists a mind-independent world (the attribute of mind-independence roughly means that the world is not caused by our minds.)
B. Mind-independence of the known (things we talk about, or our best representations are part of the world *as it is in itself*, they are not created by us.)

Scientific realists usually don't provide serious arguments in justifying A. They take it to be a claim that is widely accepted on its own merits. They mention it as their starting point and move on to B. In all realist versions, be it Boyd's or Psillos', one can see that the effort is minimum with respect to the issue of supplementing arguments for the first component in the metaphysical stance. However, B is much better elaborated by scientific realists than A. One can see that A is held by even non-realist thinkers as it does not have serious realist colors to it. B, of course, carries more load and realist spirit. Antirealist thinkers like Bas van Fraassen entertain A (but not B) as it does no harm to their position. Putnam too proposes A, (but not B). I presume that subscribing to A does not make a position more realistic. However, subscribing to B does improve one's position to be more realistic. One doesn't become a realist just by opting for A, choosing B is a more serious deal. It endorses the claim that our representations are about the mind-independent world without in anyway affecting its nature, that is, these representations are about an already structured/organized world. Our minds don't structure/organize the world. One has to also contemplate whether A implies B. Many realists take it to be so. But I doubt the persuasiveness of this line of thinking. The notion that there exists a mind-independent world doesn't imply that our representations are free of any projections of our minds. Bas van Fraassen's and Putnam's positions justify this doubt.

> The concept of, and any claim about, how things are (conceiver-independently) is unintelligible, i.e., makes no sense. (Putnam, H. 1987: 36)

It is clear that Putnam's annoyance is with B, not with A. He entertains the existence of a mind-independent world. But he is skeptic about the possibility of B. Putnam suggests that scientific theories are not representations of the mind-independent world. Our language organizes, selects, categorizes, and classifies things. Therefore, the things we talk about are not part of the world *as it is* in itself, but rather are contaminated by our thinking and knowing.

> 'Objects' do not exist independently of conceptual schemes. We cut up the world into objects when we introduce one or another scheme of description. Since the objects *and* the signs alike are *internal* to the scheme of description, it is possible to say what matches what. (Putnam, H. 1981: 52)

This suggests that if we did not "cut up the world into objects", there would *be* no objects, so that we bring objects into *being* by organizing the world, or roughly, in the process of knowing them. Putnam here denies the role of truth-makers as they are independent of our knowing or thinking (as the realist would believe). For him, truth makers and truth bearers are internal to the conceptual scheme of things. It is however not clear whether Putnam agrees for conceptual-closure as in the case of pure idealism. The reason for the hesitancy of Putnam is because he subscribes to A, the existence of a mind-independent reality. Many thinkers draw a similarity between Putnam and Kant in this regard.

> Both [Putnam and Kant] consider, somewhat uncomfortably, the view that there is an unknowable noumenal world behind the phenomena. Both are motivated in part by the threat of scepticism: Kant by scepticism about our ability to *know* the external world, Putnam by scepticism about our ability to *refer* to it. Both Kant and Putnam hold that the world we know and talk

about is empirically real, but both hold also that it is mind-dependent. (Brown, C. 1988: 146)

Curtis Brown here mentions that Putnam's skepticism about the possibility of knowledge of the mind-independent world is motivated by our failure to refer to the world. The Kantian skepticism about our ability to know the external world is streamlined with the notion of reference. This is done by neutralizing the role of truth-makers. Reality for Putnam is a notion that is necessarily dependent on the methods of enquiry and ways of representation. Roughly, reality *as it is*, is compromised in the act knowing and in the language we employ. Realists on the other hand, endorse a view where 'knowing' is dependent on 'being' and, not the other way around. The role of external world in the knowledge that science puts forth, is of at most importance for the realists. The view that terms in scientific theories do not refer to the items in external world is Putnam's major argument in disagreeing with scientific realists. According to Putnam, our language cannot reach out to the external world.

> Elements of what we call "language" or "mind" penetrate so deeply into what we call "reality" that the very project of representing ourselves as being "mappers" of something "language independent" is fatally compromised from the very start. (1990: 28)

Putnam here is challenging B, the realist claim that we can come up with exact representations of the unobservable world *as it is*. Scientific theories are descriptions of a reality very different from the actual one. Putnam asserts that our use of language is one of the reasons why we are distanced from the metaphysical world, the external mind-independent world. It is because we use language to know and organize the world that we are unable to describe the world correctly *as it is*. In other words, reality is language-dependent.

Realists however challenge Putnam's suggestions. They believe that the role of language in knowing does not in any way negates the claim that our representations cannot describe reality *as it is*. According to them, Putnam's understanding of B is flawed. From the claim of independence of the world from human cognitive activity, it

does not follow either that human inquirers are cognitively closed to this world because of the use of language, or that when they come to know it, they somehow constitute it as the object of their investigation. Psillos claims that the notion of knowledge of the mind-independent world *as it is* cannot be compromised just because language plays a role in theorizing and knowing.

> It [mind-independence] should be taken to assert the logical-conceptual independence of the world: there is no conceptual or logical link between the truth of a statement and our ability to recognise it, assert it, superassert it and the like. The entities that science studies and finds truths about are deemed independent of us (or of mind in general) not in any causal sense, but only in a logical sense: they are not the outcome of, nor are they constituted by (whatever that means), our conceptualizations [or our language] and theorising. (Psillos, S. 1999: 10)

According to the realists, unless our knowing and employing of language causally contaminates the characteristics of the external world, one cannot in any way argue that we are incapable of describing the external world. This, however, is an unlikely prospect, that our knowing causally alters the features of reality. One can nevertheless argue that knowing contains a causal aspect unlike describing, that we causally act on the world, and that this may affect the *real* character of things in the external world. This is a persuasive argument but Putnam is not going after this. He is suggesting that our language can never capture reality *as it is*. It is not the causal aspect in knowing, but the use of language in the act of *describing* that compromises the real character of the external world. We organize objects internally to a scheme and thus we are forced to be skeptics about knowing the *real* world. Putnam does not hold the view that employing language affects causally upon the world, which is an absurd claim. Yet he argues that it alters the real character of things in some way. This however doesn't make sense to the realists. They object to this view of Putnam. They believe that the only way something can have an effect on reality while knowing it, is by causally altering its character (but which also is further knowable or inferable).

Realists argue that our language is apt for describing reality. It can capture the natural kind structure of the world. The world too participates in the process of knowing. According to them, the world, and not thinkers, sort things into natural-kinds. But for Putnam, thinkers sort things into kinds by employing a language and, in fact, there is no pre-classified natural-kind structure to the external world. Perhaps this idea is reflected when he says that "objects and reference arise out of discourse rather than being prior to discourse." (1981: 53-54). The mind-independent world exists, but the descriptions in scientific theories are not *really* about it, but about an altered reality, or a discourse-generated reality. The idea that we can represent the external world *as it is,* is a myth. It is an absolute impossibility. The external world may exist, but any representation of it is discourse-driven or language dependent. Therefore, it is possible to interpret Putnam's views as consisting of a crude ontological realism.

Let us now sum up the different views discussed so far. The metaphysical stance of realists is constituted by both A and B. The claim that there exists a mind-independent reality together with the thesis of the mind-independence of the known. All realists subscribe to both A and B. Thinkers like Putnam deviate at this juncture and negate B. However, subscribing to A does not make one a realist as certain empiricist positions (such as that of Bas van Fraassen) too entertain A. Therefore Putnam's position cannot be taken to be a realist one just because he entertains A. Moreover, realists doubt the persuasiveness of Putnam's argument against the possibility of knowledge of the world as *it is in itself*. Realists suggest that, unless the world is not *caused* by the use of our language, it is hard to accept the idea that its constitution depends on language, because there is always a role for the external world in shaping our descriptions of it. But Putnam denies this role of external world because it is also discourse driven and internal to a scheme. His claim that reality is language-dependent rests on the assumption that both the terms (signs) and its references are internal to a scheme. This is the non-realist move he made, to treat the notion of reference as something that does not belong to the realm of the external world, but internal to the realm of discourse. Let us now move to the versions of internal realism maintained by Brian Ellis and Thomas Kuhn.

III.7 Essentialism and Internal Realism: Ellis and Kuhn

Let us get into the metaphysical issue which makes internal realism more realistic. The issue is that whether accepting A and the causal component of knowing, demands us to accept certain quiddity of entities. If yes, then this leads to the acceptance of a classification of things immanent in nature. Ellis entertained internal realism in his early works and most recently, tilted towards a more realistic position, namely essentialism, the view that the world is comprised of natural kinds with intrinsic properties. I regard Ellis' position to be an extended version of entity realism having an internalist theory realism. Ellis comes up with the idea of a minimal classificatory scheme which is common to all human beings and which capture objective similarities. The idea of objective similarity leads to the notion of the necessity of laws in nature.

> Scientific essentialism is a theory about natural necessity and also about laws of nature. It is the thesis that there are natural necessities in the world that find their expression in cause and effect relationships and the laws of nature. If this claim is correct, then it follows that laws of nature are immanent in the world. (Ellis, B. 2001: 261)

One can see why Ellis moved to a more essentialistic position. To grasp it, we need to comprehend the idea of quiddity. Entities have quiddity if they are intrinsically different from another type of entities. For example, electrons have quiddity because they are not protons. They can be identified differently. There is another concept, haecceity, which is normally used in discourses related to the metaphysics of individuality of an object. An electron doesn't have haecceity because there is nothing intrinsic to electrons to single one out from all others of the same type. One particular electron is not intrinsically different from another. Quiddity is applied to single out one type of object from another type, whereas haecceity is applied to single out one object from another one of its own kind. However, these notions may change when we focus on the biological sciences or even climb up the ladder to physics of massive bodies. All planets, stars and the like possess individuality and thus haecceity. Similarly an amoeba is different from another one of its own kind and hence possesses haecceity.

The issue to be explained by the internal realist is that, if we are able to subscribe to straight forward ontological realism about the existence of world, and also use causal interaction as a justification for this claim, one must also allow the idea that quiddities exist in nature. The view that certain kind of entities exist which are different from certain other kinds of entities. At this level, one is not tempted to include theoretical assertions about these entities to be true or reliable. One can subscribe to a quiddity-based view of entity realism which speaks about a rough natural-kind structure immanent in the world. The idea here is to push the entity realist thesis further to accommodate belief in the existence of certain properties of entities that will intrinsically provide the required quiddity.

As I said before, one can be a single-tier model realist here by not invoking either the notion of representation or truth and still be a sophisticated entity realist. The attempt here is to see whether we can interpret internal realism in the light of entity realism (or even a crude ontological realism with quiddities). However, Ellis would claim that all that we know are nothing but internal to a scheme and he always entertains an epistemic notion of truth.

I argue that if the thesis of internal realism allows causal interaction and an ontological realism, then it can extend its realist spirit by allowing some knowledge of the world consisting of a pre-established classification of things. Quiddities are identified as a result of interacting with such pre-established natural joints of the world. The ontological dependence thesis of internal realism slightly collapses here. Ontological dependence arises only at the level of representations, or when we represent the knowledge of the causally found natural joints of the world. This has grave consequences for the thesis of internal realism. Internal realism here might lead to essentialism and to some form of realism. Realists would be happy to welcome this position of internal realism as it bridges the gap between them. For the realist, entity realism is much more acceptable than internal realism. It seems that the metaphysical stance of internal realism needs to be reformulated in the light of these issues.

	Single-tier model	Two-tier model
	Hacking/Cartwright/Devitt	Ellis/Putnam/Kuhn
Existence of a mind-independent world (A).	Yes	Yes
Mind-independence of the Known(B)	Yes	No

Table 3. Internal Realism and Metaphysical Attitude

When the internal realist accepts A, he or she does not invoke the idea of a possibility of B. The internal realist subscribes to the two-tier framework discussed in chapter I while accepting A. The internal realist wants to say that the world exists independently of our minds, but nature/character of things in the world is dependent on our use of language, and thus dependent on mind. Therefore, there is an inherent tension, or a split, evident in the internal realist position if we invoke the idea of quiddities and causal knowledge.

One may attempt to avoid the tension by bringing in the distinction between the notions 'existence' and s'real'. Existence can be affirmed without in any way presupposing notions of representation or truth, whereas to say that something is real, the role of representations and truth are inevitable. Further clarity is required in this direction. One has to appreciate here the brilliance of Hacking where he does not say that theoretical assertions about entities are true, or that the properties assigned to entities by theories are indeed real. He merely says that entities exist. To say this, one does not need the aid of representation or truth. Plain causal interaction is enough to justify the claim of existence. Once existence of entities is established, one may go ahead and say that therefore these are real entities. But there ends the story of entity realism. They don't argue for the truth of theoretical assertions because when we attribute 'truth' at the level of representations, we are forced to also attribute 'real' at the level of world. Mere existence is a very category, it just says that something is, but one is not sure what that entity is or what role it has in the larger picture of nature's causal chain captured by theories. We rely on theories to get this larger picture, and since these theories are not

needed to affirm the existence of entities, we can be skeptical about the claims provided by these theories and the reality which it endorses.

Stretching internal realism to accommodate some kind of a crude entity realism is a radically new position. I did this for a specific purpose. I wanted to reveal that, in most philosophical formulations about scientific knowledge, there is room for more than one epistemic attitude to be present. The upshot of this idea is elaborated more in chapters V and VI.

Internal realism can accommodate the following: a crude entity realism (by elaborating on A) and an internal theory realism (by adopting the causal component of knowledge, quiddities of entities and a concept of existence).

However, I assume that Putnam would not agree with the above characterization of internal realism. Putnam claims that what we have in front of us is an already described world, described by the human mind, and therefore negates the idea of a ready-made world with natural joints. To understand this idea better, it is useful to bring into play the ideas of Kant here, the world is given to us in an already mind-mediated way. The scheme of entities and their quiddities we take to be a pre-organized by nature is comprehended by us, but the organization that the world has, is given to us through our categories of knowing. Hence, reality as *it is in itself* is already compromised by our categories on knowing. There is no way that we can comprehend the categorization of things in the world as *it is in itself*. We already compromised the character of entities and their joints as they appear to us in a certain way, because the mind participates in showing it to us in a that way. There is no way that we can get out of our skins and see how things really are. Therefore, internal realists like Ellis claim that essentialism, when incorporated with internal realism will be an internalized version of it. Esssentialism need not always be an externalist position and thus, our reading that it accommodates a crude ontological realism is false. This is precisely because the ontology of things in the world is internal to a scheme. The world might be having its own classification or 'the world has natural boundaries', but we don't have any epistemic access to them. Representational success needs to be understood in such a way that the world's natural joints are interpreted by us by the very moment we start to engage with it. Knowledge of

the world is a pre-described (by our minds) description (by scientific theories) of the world's natural joints and objects. By the time, it is represented by theories, the reality *as it is*, is twice compromised. Firstly by our minds and secondly, by the scheme of things embedded in language. In fact, this interpretation of internal realism doesn't allow Essentialism to break the shell of internal scheme-dependency and mind-dependency. This is a possible response to our previous interpretation which identifies internal realism to be a more realistic-essentialistic externalist position.

Let us now move on to Kuhn's position which is internal realist without essentialist commitments. I am not discussing issues related to incommensurability. Here, I directsly discuss his metaphysical position. There is no consensus among thinkers about what his exact position is. Thinkers had regarded Kuhn as a relativist and as an idealist, and sometimes as a Kantian. I will be selective here in picking some of the recent thinkers' comments and then proceed to classify his position as a version of internal realism. Kuhn's position is least realistic of all internal realisms. Many thinkers take the views of Kuhn to be of a Herderian or perhaps a Humboldtian lineage, the second-generatison Kantians. They maintained that there could be many different conceptual schemes. Each of these conceptual schemes is a hermeneutic device on its own merit. A person who is within one scheme and its description of the world might not even capture the intelligibility of another scheme and its description of the world. Kuhn also denied the idea that there is something called pure experience. Conceptual schemes permeate everything. One can make sense of this by following the IR version of essentialism I mentioned before. The world and its objects are already described by our minds. Forrai draws parallels between Kuhn and Kant.

> [According to Kuhn] whatever we perceive is already structured by our conceptual schemes. This feature reminds one of the Kantian notions. In the Kantian view, the whole world as it appears to us is within our conceptual scheme. Whatever we have access to is inside. But there is an important dissimilarity here between Kant and Kuhn. [Firstly], the Kantian view does not [imply] that there are many worlds. The Kantian view has just one conceptual scheme, which is shared by the whole of humankind.

> [Secondly, Kant has the *phenomena-noumena* distinction]. Even if the world appeared differently to people with different conceptual schemes, there would still be an underlying world, which is the same for everyone- Kuhn lacks both these features. (2001: 7-8)

These characteristics of the Kuhnian metaphysics (according to Forrai) alienate his position further from even Putnam's internal realism, which invokes the existence of a mind-independent world. Kuhn says that "after a revolution, scientists are responding to a different world" (1970: p 211). But many commentators of the later works of Kuhn opine that these words need not be taken literally. They say that Kuhn wanted to retain the common sense idea that there is just one world. Kuhn also maintained that the external world imposed certain constraints in the form of anomalies, which restored the idea of a mind-independent world in his thoughts. Howard Sankey says that Kuhn had a clear cut position in his later works, which is some kind of an ontological relativism.

> [In] recent years Kuhn has retreated from many of the claims which were responsible for earlier reactions to his position. In his later works, Kuhn presents instead an ontological form of relativism, which involves an anti realist denial of natural kinds. (1997: 305)

I don't subscribe to Sankey's presumption that Kuhn entertained some kind of ontological relativism. However I draw some conclusions from Sankey's exposition of Kuhn's philosophy.

> According to the new Kuhnian position, which has lately begun to emerge, scientific theories are the source of alternative sets of taxonomic categories which are imposed by theories on the world. A set of such categories constitutes a localized complex of interconnected concepts, such that terms for such categories are unable to be translated from one set of categories to another such set. Rather than reflecting reality, these categories constitute, at most, ways of ordering experience; such categories do not reflect reality because it is not possible to do so. (Ibid)

Given this picture of Kuhn's later works, Sankey's contention is that there is no right way to represent reality, and since the sets of categories change from theory to theory, there is a sense in which, as theories change, the world changes with them. One can clearly feel the ontological dependence of world to the mind evident in Kuhn's ideas. Kuhn however throughout his works assumed the existence of an independent reality.[14] This makes him a qualified Kantian. The admittance of a reality independent of human mind can be roughly taken to be the Kantian *noumena*. A Kantian reading of Kuhn would render the idea that the *phenomenal* worlds keep on changing (as theories change) and are constituted by sets of categories within the theories. The *noumena* stays firm and unchanging, because our theories are unable to represent it *as it is*. Several such interpretations are possible and Kuukkanen rhetorically puts this elusiveness of Kuhnian philosophy:

> The old irrationalist Kuhn who 'killed logical empiricism' received company two decades ago from the Kantian Kuhn (Hoyningen-Huene), but has subsequently been supplemented by at least the conservative Kuhn (Fuller), the last logical empiricist Kuhn (Bird, Friedman, Irzik and Grünberg, Earman, also Reisch), the cognitive science Kuhn (Andersen, Barker and Chen) and the Wittgensteinian Kuhn (Sharrock and Read). And, of course, there has been the sociological Kuhn (Barnes) all along too. (2009: 217)

Many of these characterizations read different metaphysical commitments in the writings of Kuhn. I believe that Sankey's interpretation of Kuhn is profound and provides much insight into his later works. Within this untidiness nonetheless, there is a rather unnoticed reconstruction of Kuhn's philosophy by Hacking. He, in his 1979 review of *The Essential Tension*, claims to have found a new character in Kuhn's thoughts, which he calls *revolutionary transcendental nominalism.* Sankey also mentions Hacking's stand on Kuhn in the following words:

[14] Devitt and Mandelbaum had this suggestion that Kuhn assumed a mind-independent reality in all his works.

> Kuhn is not to be read as an idealist who denies that there is a reality existing independently of human thought. Rather Kuhn [according to Hacking] denies that the kinds to which individual things belong have any existence prior to thought. (1997: 305)

I regard the Hackingian interpretation of Kuhn as a version of internal realism. Names apart, transcendental nominalism reflects the position of internal realism.

> [According to] transcendental nominalism, there is not some uniquely right conceptualization of the world. Nor is the world itself constituted by more than merely superficial 'kinds of things'. The 'kinds' that enter our theoretical speculations are man-made…. (Hacking, I. 1979: 230)

Hacking says that according to Kuhn, the world as *it is in itself* cannot be taken to have a natural kind structure. In other words, Hackingian Kuhn denies essentialism in the realist sense. Yet we can say that the phenomenal world is comprised of kinds which are imposed or projected by us. And this ontology at the phenomenal level is subject to change as theory changes. One can see an internal-essentialism as in the previous discussion (with respect to Ellis) here. Internalized version of essentialism presupposes a kind-structure of phenomenal world, which Hacking calls empirical-kinds at the phenomenal level. The Hackingian Kuhn might say that, as theory changes, the world itself may not change, but the world of kinds (empirical-kinds) change.

> The world does not change, but we work in a new world. The world that does not change is a world of individuals. The world in and with which we work is a world of kinds. The latter changes, the former does not. After a scientific revolution, the scientist works in a world of new kinds. (Hacking, I. 1993: 306)

This is indeed a new interpretation of Kuhn. Hacking's insights are clear. The world as *it is in itself* might have individuals in it, but its classification *as it is*, is epistemically inaccessible by human minds. All that we have is a mind-mediated and dependent ontology of empirical kinds. The individuals in nature can be regarded as in a brute state, where we must be skeptical about the knowledge of a classification of the

world as *it is in itself*. Hacking internalizes essentialism, or to some, provides an empiricist reading of essentialism together with Kuhn's position. The world of individuals is unaltered by theory change, and this notion supplements the idea of a mind-independent world in a robust sense. We think and interact with a world that has our own categorization which is subject to change. In fact there is no other way in which we can interact with the world. We are trapped in this epistemically difficult situation. The moment we interact, our categories starts its function. Kuhn at a later stage said that Hackingian interpretation does not impinge on the problems with which he was concerned, but still, many think that Hacking is right in his interpretation. The key idea of Hackingian interpretation is that the world itself at the trans-empirical level does not organize or divide individuals to natural kinds, but we do, at an empirical level, and there is no way that we could be sure of representational success, because there is no such notion of an organized world *in itself*.

One can however push this argument and say that human minds can categorize the world at both the trans-empirical level and also at the empirical level. This will make Kuhn's position more realistic. But here too, Kuhn would maintain that the world as *it is in itself* does not have any pre-categorized structure which humans can copy, even though there exists an external world. Whatever structure or organization of objects are provided by us, be it at any level, is mediated by the mind and imposes categories of particular conceptual schemes. Kuhn often suggests that the external world is ineffable or indescribable. This draws his position closer to the Putnamian internal realism. But according to Sankey, Kuhn's position is also Kantian. In his later works, Kuhn suggests that lexicon provides a structure that constraints our experience.

> Both [lexical structure and Kant's *a priori categories*] are constitutive of possible experience of the world. But neither dictates what that experience must be. (1993: 331)

We can definitely regard the lexicon structure to be supplementing preconditions of knowledge similar to the Kantian categories. In all these different interpretations of the metaphysical stand of Kuhn, there are certain similarities. Most commentators ascribe that Kuhn maintained the existence of a mind-independent world, therefore we can be

sure that he accepted A. In addition, all the commentators opine that, according to Kuhn, we impose certain organization and order into the world, be it at an empirical level or at a trans-empirical level. Therefore we can be sure that he denies B like Putnam. But the justification he provides for accepting A is very week. The external mind-independent world is largely ineffable and only negatively responds to certain conceptual-scheme projections. It never dictates terms with us in deciding the correct map of the world. Because to map something, there must be a target, and here we don't have access to any such world. Kuhn de-structures the external world, he de-essentializes it too. Moreover, to some, he structures the phenomenal world and essentializes it by bringing in the idea of taxonomical structure or lexicon. Kuhn had himself claimed in an interview almost towards the end of his life that "I am a Kantian with movable categories" (2000: p 264). This suggests that he entertains an ineffable mind-independent world which is epistemically inaccessible; nevertheless we may have our own ordering of the world. The categories of knowing are not fixed in the Kantian sense, but might change from one theory to another as they are provided by the lexical structure.

Having gone through the different versions of internal realism and their characteristics, let us now move on to the issue of truth and its relation to empirical success from the internal realists' point of view.

III.8 Truth and the Explanation of Empirical Success

Notions of a non-epistemic account of truth and a corresponding notion of reference are the key ingredients of a realist EDR. According to realists, there is representational-success because there is correspondence or a mapping between our representations and the world. This mapping is assured by the notion of a non-epistemic (verification-transcendent) account of truth. The truth-makers which are at the level of reality are accountable for truth. This idea has a greater semantic import, namely that empirically successful scientific theories do refer to the features of unobservable world. In other words, theoretical terms of successful scientific theories have reference. The notion of empirical success, representational success, truth and reference are all linked to each other in the realist repertoire. EDR endorses the view that the best explanation for the success of scientific theories rest in the fact that they are true. In addition, since they

are true, the theoretical terms in them do have reference. Let us now evaluate what would be the internal realist response to EDR as they don't subscribe to the non-epistemic notion of truth. Let us start with Putnam, because the original version of EDR, the 'no miracle argument' is to be found in his early philosophy.

Putnam disagreed with an absolute conception of 'real', and thus shifted towards an epistemic conception of truth. Putnam takes truth to be an idealization of rational acceptability.

> What this shows, in my opinion, is not that the externalist (metaphysical realist) view is right after all [in holding truth to be a radically non-epistemic notion], but that truth is an idealization of rational acceptability. We speak as if there were such things as epistemically ideal conditions, and we call a statement true if it would be justified under such conditions. Epistemically ideal conditions are of course like frictionless planes. We cannot really attain epistemically ideal conditions, or even be absolutely certain that we have come sufficiently close to them. But frictionless planes cannot really be attained either, and yet talk of frictionless planes has cash value because we can approximate them to a very high degree of approximation. (1981: 55)

Putnam takes truth to be stable and timeless, but admits that it is to be regarded as an epistemic notion exclusively. First of all, according to Putnam, we can never be sure about representational success. I discussed this view briefly in the previous sections, as involving in all the three thinkers I considered so far. We are never sure about when our language is hooked-on to the world, because truth and reference are decided internally by a conceptual scheme (Putnam) or a lexical structure (Kuhn). Nicholas Jardine and Brian Ellis endorse similar epistemic views on the notion of truth, Ellis says,

> Truth is what we should believe, if our knowledge were perfected, if it were based on total evidence, was internally coherent and was theoretically integrated in the best possible way. (1985: 68)

Scientific theories represent true descriptions of the world, but such a truth is not attained with the help of truth-makers in the mind-independent world. Truth is what is rationally acceptable, in an ideal knowledge seeking scenario The notion of truth that Ellis figures out is not contextual, or relative. It has to satisfy the ideal conditions to be termed as true. According to Ellis, scientific realism has only one option as far as the issue of truth is concerned. He elaborates it in the following way:

> [Scientific] realism can only accept a pragmatic theory of truth, i.e., a theory on which truth is what it is epistemically right to believe. But the combination of realism with such a theory of truth is a form of internal realism; therefore, a scientific realist should be an internal realist….. The only way I can see of explaining why the truth matters for a scientific realist is to argue that truth is really an evaluative concept, like 'rightness' in ethics, e.g., that the truth is what it is right epistemically to believe. But any such concept of truth is an internalist one. (1988: 409-10)

It is clear that both Putnam and Ellis are moving in the same direction, even though it is clear that further elaboration of 'idealized truth is' needed. At this juncture, let us also have a look at Kuhn's notion of truth. His notion is slightly complex in the sense that he believes that a given claim may be true or false within the context of a given lexicon, the categorical system embedded in the lexicon system itself is not capable of being true or false. Kuhn in the following passage provides insights into his notion of truth.

> ….lexicons are not…the sorts of things that can be true or false. A lexicon or lexical-structure is the long-term product of tribal experience in the natural or social worlds, but its logical status, like that of word-meanings in general, is that of convention. Each lexicon makes possible a corresponding form of life within which the truth or falsity of propositions may be both claimed and rationally justified, but the justification of lexicons or of lexical-change can only be pragmatic. With the Aristotelian

> lexicon in place it does not make sense to speak of truth or falsity of Aristotelian assertions in which terms like 'force' or 'void' play an essential role, but the truth values arrived at need have no bearing on the truth or falsity of apparently similar assertions made with the Newtonian lexicon. (1993: 330-31)

One can clearly see the ideas of incommensurability or in-translatability surfacing in his expressions. Kuhn believes that the notion of truth/falsity appear only within the context of particular lexicons. Since the world (i.e. its ontology) varies with lexicon-change, we can't say that he has a relativized notion of truth. He merely suggests that truth is internal to the lexicon. Having drawn these brief elaborations of notion of truth entertained by the main three proponents of internal realism, let us now turn to a possible response to the empirical success of science and PI by them.

It is evident that by subscribing to an epistemic conception of truth, internal realists change the overall outlook of both the explanation of ERS and the issue of PI. I have the hunch that the notion of empirical success as a global concept must be dropped if it is understood in the light of internal realism. Success makes sense only internal to a scheme or lexicon. It would be useful to evaluate the following passage by Kuhn with reference to his project of reading Aristotle where he was initially struggling to grasp the overall intelligibility of Aristotle's views. But once he continually concentrated and immersed in the texts, things started to appear differently.

> Suddenly the fragments in my head sorted themselves out in a new way, and fell into place together. My jaw dropped, for all at once Aristotle seemed a very good physicist indeed, but of a sort I'd never dreamed possible. Now I could understand why he had said what he'd said, and what his authority had been. Statements that had previously seemed egregious mistakes now seemed at worst near misses within a powerful and generally successful tradition. (2003: 16)

This revealing carries the hint to a Kuhnian understanding of the notion of success. He would say that evaluation of empirical success is possible only within a

lexical-structure. Aristotle's claims fit well within his phenomenal world and so is Priestley's or Lavoisier's. Their theories were predictively successful not because they are true in the realist sense, rather because they are true within their scheme of organization of the world. Internal realist's version of the explanation of success internalizes the notion of truth and success. A theoretical claim might be successful as it is true within a context. This explanation of the success of science is justified by PI. PI does not pose a threat for the internal realist. In fact internal realists would agree with Laudan and use it for their advantage. The very fact that there are theories which were once successful (and later abandoned) justifies the idea that we have to abandon global conception of success and an absolute-non-epistemic conception of truth.

I presume that both Ellis and Putnam would maintain similar views regarding empirical success and PI. I am merely exploring a possible explanation which the internal realist would favor. In the literature on internal realism, there are no specifically given answers which can be used in answering the issues which the externalist positions engage with.

Since PI is not a charge against internal realism, I wind up the discussion on internal realism and take forward (to the following chapters) only the views with regard to the externalist positions and their engagement with PI and ERS. I discussed internal realism in detail because of a specific purpose. I tried to see the possibility of interpreting internal realism as having a split epistemic attitude, having a crude realism about the world on the one hand and an internalist theory realism on the other. However, many also believe conversely. My agenda was to see whether a realist and antirealist position go hand in hand in internal realism. This is to supplement my arguments in the last two chapters. I would not like elaborate on those arguments here. Therefore I move on to the next chapter where meta-philosophical concerns about the scientific realism debate are addressed.

CHAPTER IV

NUANCES OF THE DEBATE

IV.1 Preliminaries

There are a number of historical narratives that go along with the different positions in the debate. These narratives are used to provide justifications to the positions on the one hand and make them plausible on the other. History of science is used in an effort to vindicate the philosophical positions. In this chapter, the attempt is to see the intricacies of the use of history of science in relation to the scientific realism debate. It is argued that the use of history of science fuels philosophical over-generalization. I scrutinize the four externalist positions namely entity realism, structural realism, semirealism and scientific realism and the way these are posed in the overall agenda of the debate.

I analyze the strategies employed by the externalist positions in evading Laudan's challenge and argue that there are three different levels of discourse, in which the thinkers participate. Firstly, thinkers talk about particular historical episodes of theory change and pick out the revision defiant constituents in them. They point to certain entities with properties or certain equations. Secondly, the talk climbs to a level where general terms such as 'structure', 'entity', 'concrete structure' etc are employed. Thinkers at this level argue for revision-defiance in terms of such general terms rather than historical examples involving concrete cases. Thirdly, they elevate their discussion to the metaphysics of properties and relations. The debate's focus here is about the property, intrinsic or relational, that has a stake in the architecture of reality and the ontological priority of one over the other. I argue that these three discourses are connected in an interesting way and that all the positions in the debate face a peculiar epistemological weakness in trying to evade the challenge they face in providing solutions originating out of history of science. The passage from the talk at the level of history of science from the talk at the two subsequent philosophical levels point the finger of suspicion to the charge of overgeneralization leveled by many contemporary thinkers like Saatsi (2011). What follows in the first section is the realist involvement in history of science followed by other externalist positions. In the second section, I discuss trans-theory talk, the debate at

the level of wholesale arguments concerning theory change and predictive success. In the third section, I discuss the metaphysical implications associated with the first two levels of discourse.

IV.2 Theory-Talk: Discourse at the Level of History of Science

Doing justice to the idea that 'something is retained across theory-change' is the primary aim of all the externalist positions in the debate. No matter what happens at the level of 'sense', there is continuity at the level of 'reference'. This is the philosophical underpinning of such moves. However, the effort then is to pick and interpret revision defiant 'stuff' occurring in consequent theories even though their 'senses' in individual theories vary. Scientific realists also try to show that many theories in Laudan's list (of past failed theories) were not as entrenched in a research tradition as Laudan actually claims. Theory-talk is thus narratives from history of science judged to be lending support to particular philosophical positions. Theory-talk may be from any particular phase of history of science. Many thinkers believe the view that history of science can adjudicate between philosophical positions, especially with regard to the challenge from Laudan.

Let me make clear what 'theory-talk' pertains to in the context of this thesis. As said earlier, it has been the tendency of philosophers of science to rely on history of science in explaining their positions. Both the externalist positions as well as the internalist positions depend on specific narratives from history of science to sharpen their views. It could be an example, a case study or a narrative on a lengthy historical phase which can all be termed as theory talk. In theory-talk, the emphasis normally is to do justice to the philosophical position rather than narrating history. Theory-talk is therefore not driven by an interest in history of science, but an interest in history of science motivated to justify a philosophical stance. This indeed is the pivotal act where narratives from history of science are taken to be tools in explaining philosophical positions. One has to bear in mind that theory-talk itself is not history in the strict and objective sense of the word; in fact the narratives are loaded with interpretations favoring a particular side. Most case studies and examples cited by philosophers of science are biased and can be taken to be interpreted in one's advantage. This is evident by the selective treatment of

certain episodes in the history of science rather than disparate phases. The scientific realism debate perhaps contains the most number of historical narratives where thinkers try to establish individual philosophical positions on the basis of them. Theory-talk, in general can be formulated in terms of the following dictums.

- Any narrative from history of science can be endorsed (or used) in explaining or defending a particular philosophical position (e.g. using the case of electron in entity realism or using the Fresnel-Maxwell theory change to validate structural realism).
- The narratives include those provided by scientists on the details of scientific knowledge and scientific practice (e.g. Laplace and Lavoisier's claims on caloric theory of heat which realists like Psillos use to develop (and generalize towards) a philosophical position).

Having mapped the broad contours of theory-talk, let us start with the scientific realists' engagements in the seventeenth and eighteenth century theories of heat.

IV.2.1 Caloric Theory versus the Dynamic theory of Heat: History of Science Narrated by a Realist

Psillos launches himself into theory-talk by explaining the realist views associated with the Caloric theory of heat. Psillos' initial strategy is to see whether scientists of a particular phase warrantedly believe in a theory. Secondly, he elaborates whether the theory at stake has any role in providing successful predictions. Thirdly, he claims that even if predictive success is attained, the credit need not go to the whole theory. Instead we can find certain components of the theory which fuelled success. Scientific realists usually employ a differentiated epistemic attitude towards past theories. They claim that not all components of the theory need be right representations of the world. Some components in the theory are (is) responsible for representational success and truth, and thus predictive success. We need to therefore only focus at those components in the theory which fuelled empirical success. These components are the revision defiant stuff contained in the theory. The scientific realist case study related to such a strategy starts with the problem whether heat is a liquid substance in the bodies or heat is a result of

motion of molecules of matter. Antoine Lavoisier, Joseph Black and Pierre-Simon Laplace stand at the former end of the debate whereas Count Rumford and Davy belong to the latter end.

Caloric was understood as a theoretical entity and a material substance which is nothing but an indestructible fluid of fine particles. When a body absorbs caloric, it will have a rise in temperature. The cause of the rise and fall of temperature was an important explanation pursued especially in the eighteenth and nineteenth century chemistry. Therefore, heat was taken to be in a sense, an observable effect of the transference of caloric from a cold body to the hot body. However, several thinkers believed that the caloric account of heat was just a fictitious tale. The reason for this is that, if caloric is a substance, and if heat increase is nothing but absorbing caloric into a body, then there must be weight increase associated with increase in temperature. Count Rumford conducted a series of experiments where the aim was to calculate the increase of weight in heated bodies compared to their normal states. All the results obtained were negative but still the caloric theory was not replaced conclusively.

Running parallel to the caloric theory was the dynamic theory of heat. According to this theory, the cause of heat is not to be explained in terms of any material substance. Heat was the result of the particles (in motion) which constitute a substance. In other words, the dynamic theory explains heat as nothing over and above the result of the motion of the molecules of a body. One of the main reasons why the dynamic theory had several followers is because it could explain the cause of heat due to friction. When two bodies are rubbed together, heat is produced. Most of the times, when we pursue the rubbing a little more, the bodies seem to run out of matter. If heat was a material substance, as the caloric theorists would argue, then there must be weight increase and the bodies must not lose its matter. However, in friction bodies lose matter. Davy conducted another series of experiments like Count Rumford, which showed that there is no addition of matter when two things produced (or exchanged) heat by rubbing or otherwise. Heat could be produced by friction until the two bodies were finally rubbed away. This means that as long as there is matter in the two bodies, heat can be produced constantly by friction.

Even though experiments continued to run against the Caloric theory, many were unmoved. This is because it is impossible to experimentally show that friction can produce heat in a consistent manner and thus validate the dynamic theory. The reason for this is that the bodies will eventually get rubbed away. So we never know, further down the line, that friction would cease to produce heat. An experimental set up where matter is indestructible (or we may need massive bodies) due to friction is needed to prove the dynamic theory. Therefore, according to Psillos, in a certain sense, indecision and uncertainty clouded eighteenth century scientific community as to whether vote for one theory rather than another. An advantage the dynamic theory held compared to the caloric theory was that according to which, heat would not imply weight increase in bodies. In friction, it is possible that two bodies get heated up until they are rubbed away because of the vibration of molecules in them. Heat is not transference of a material liquid. Even after facing several negative experiments in the early eighteenth century, especially due to the efforts of Davy and Count Rumfod, it is believed that the caloric theory of heat was still the more accepted one among the scientific community. Psillos says that even though the caloric theory of heat was entrenched in the research tradition of early eighteenth century, scientists were cautious in affirming that it was a right representation of the world. Here, Psillos does some sort of a cognitive historical research and cites certain statements by scientists where they are in doubt about the representative success of the caloric theory. He especially points to the following statements of Lavoisier and Laplace, from their combined work in 1780 *Memoire sur la Chaleur* (*Treatise on Heat*),

> We will not decide at all between the two foregoing hypotheses [material v. dynamical theory of heat]. Several phenomena seem favorable to the second [the dynamical theory of heat], such as the heat produced by friction of two solid bodies, for example; but there are others which are explained more simple by other [material theory of heat]- perhaps they both hold at the same time. (1780: 152-53)

Psillos says that the scientists themselves were undecided as to whether to believe in the caloric theory or the dynamic theory. In fact he quotes time and again both these

thinkers to show that the idea of uncertainty with regard to belief in either of the theories was not an idiosyncratic feature or unsympathetic interpretation. What follows are the words of Black in his *Lectures on the Elements of Chemistry* from 1803.

> Our knowledge of heat is not brought to the state of perfection that might enable us to propose with confidence a theory of heat or to assign an immediate cause of it. (1803: 42)

Psillos gives us a completely different picture of the three scientists. Laplace, Lavoisier and Black were suspicious about the very theory they were attributed to be followers of in the history of science. This is indeed a miraculously novel interpretation running counter to the claims of Laudan. Laudan claims that the caloric theory of heat is an established theory held by many scientists of that period. However, there is a missing link in Psillos' efforts. He was successful in identifying and quoting unsure minds of the scientists who held on to the caloric theory whereas he never comes up with such uncertainty in decisions associated with thinkers associated with the dynamic theory. All the experimental results were against the caloric theory and, therefore, one can imagine the possibility of the suspicion entertained by the scientists. However, what Psillos tries to establish is something very interesting. According to him,

> Most of the eminent proponents of the caloric theory were aware of the difficulties that this theory faced. They knew the advantages of the alternative representation of heat, especially in explaining the production of heat by friction. They were aware also of the shaky experimental evidence, and of the inaccuracy of most of the experimental results available. (1999: 117)

These views endorsed by Psillos could be due to the projection of realist spirits to history of science. One can see that Psillos' theory-talk is very selective. Psillos does not discuss the views of supporters of dynamic theory as to whether they were also suspicious about the representational success of it. Psillos further tries to show that most of the work in experimental calorimetry was conducted independently of any theory of heat. Delinking the false theory from predictive processes is a major strategy employed

by the realist. He goes on to claim that Laplace'e prediction of speed of light in air and Carnot's work on the heat cycle are in no way connected to the assumptions that heat is a material fluid.Having briefly elaborated the scientific realist's historical project, let us now move on to entity realists' endeavors in history of science.

IV.2.2 Talk about Electrons: Entity Realist Lenses

Gelfert summarizes the conviction of Hacking drawn from the story of electron.

> In *Representing and Intervening* (1983), Hacking recalls vividly how he witnessed a Stanford University experiment for the detection of fractional charges that convinced him of the reality of electrons and positrons. The experiment was based on Millikan's old idea that small charges can be detected by observing the movement of a macroscopic superconducting metal sphere in an electric field. The Stanford experiment required neutralizing any initial surplus charges present on the sphere. This charge neutralization was achieved by transmitting electrons and positrons onto the sphere. During this process of "spraying", the sphere's behavior in a magnetic field changed—much like stripping oil droplets of an electron in the Millikan experiment altered their behavior in a static electric field. The success in using electrons and positrons, thereby manipulating the behavior of the sphere, should, according to Hacking, suffice to convince us of the reality of electrons and positrons.(2003: 246)

Hacking's major claim is that if we can use something as a 'tool' for doing something else, then this 'tool' cannot be unreal. Manipulative success will make us believe in the reality of the thing that is manipulated. Hacking claims that we may hold that all our scientific theories about electron are strictly speaking false and yet we can believe that electrons exist, on the grounds that we have manipulative success about the interactions of electrons with other aspects of the world. The general idea underlying entity realism is that there is continuity at the level of entities whereas there is a severe discontinuity at the level of theories (about these entities). The most common stand of entity realist can be inferred from Cartwright's words that "electron is not an entity of

any particular theory" (1983: 92). This statement reflects the idea that entities stand independent of any theory even if the so called entity was proposed by one (or many) of the theories. Thinkers in the likes of Resnik had long raised their suspicion to the theory talk of entity realists. They claim that recognizing manipulative success might require already the involvement of substantial amount of theories. In fact, many critics of Hacking think that there is no theoretical vacuum in an experiment as he would claims. They say that existence of entities also can be inferred from explanatory success rather than manipulative success or intervention. The notion of explanatory success destroys the delinking between theories and entities. If we use an entity in an explanatory schema, then that entity can be termed real according to theory realists. However, Hacking again leans to the story of electron, he says

> Once upon a time the best reason for thinking that there are electrons might have been success in explanation [theory]…. Luckily we no longer have to pretend to infer from explanatory success (i.e. from what makes our minds feel good). (1983: 271)

What he hints at here is that explanatory success is epistemically inferior to manipulative success. This tone is frequent in Hacking's works. Hacking shows that in the case of electron, the initial bunch of experimenters had least causal knowledge of it, even though experimental success was met. However, by the time of Rutherford's experiments, the experimenters had not only a belief in the existence of such an entity but were aware of most of the causal properties of it. So starting from Thomson to Millikan and Rutherford, a transition can be seen- a transition from a knowledge of electron based on explanatory success (in terms of theoretical knowledge) to knowledge of electron based on manipulative success. In fact, it can be argued that all scientific investigations begin with a vague explanatory understanding and later on after several experimental works, manipulative understanding (causal knowledge) takes its place. Does this mean that when electron was posited by Thomson, we will have to resort to explanatory success mostly and become theory realists? Further, later on, when all the causal properties of electron became evident in the course of experiments, then alone we can be entity realists justified in terms of manipulative success?

Hacking's theory-talk on electron leaves way for so many speculations. He roughly hints that theory realism is a precondition to entity realism. The understanding of electron in the early years was all based on explanatory success, and explanatory success links rather than delinks an entity and a theory. Sometimes it feels as if Hacking is proposing a passage from theory realism to entity realism rather than obliterating the relation between theory and entity as many of his commentators would hold. The criticism that is leveled against Psillos, that of selective theory-talk can be leveled against Hacking too. Hacking is obsessed with the tale of electron. He never speaks about the recent developments in physics where electron as an entity loses its individuality and subsumes into a field as a set of particles. Gelfert (2003) charges Hacking with the idea that it is extremely difficult to hold even the conception of an entity in the recent developments in physics. The identity conditions of electrons 'vaporize' in the recent scientific knowledge. However, Hacking's condition for existence of a process is justified here too, there is a large amount of manipulative success associated with field theories. Does this mean that Hacking's entity realism sounds meaningful within a particular phase in the history of science?

IV.2.3 Phlogiston versus Oxygen: A Structural Realist Version

As a third instance of theory-talk, let us move on to the discussion in history of science by the structural realists. Firstly, let us take a look into the phlogiston-oxygen theory talk and see how the structural realists' version shapes up. Psillos (1999) makes an outright comment that the term 'phlogiston' is a non-referring term. He says that "a phlogiston-based taxonomy is wrong because no natural kind has the kind-constitutive properties attributed to phlogiston". However, Ladyman (2008) suggests that from a structuralist perspective, this need not be the case.

> The argument from theory change threatens scientific realism because if what science now says is right, then the ontologies of past scientific theories are far from accurate accounts of the furniture of the world, even though they were predictively successful. It follows that the empirical success of our best current theories does not imply that they have got the nature of the world right either. The structural realist solution to this

> problem is to reject the claim that the nature of unobservable entities is successfully described by science, and to argue instead that successful scientific theories give increasingly accurate descriptions of the structure of the world. Theories can be very different and yet share all kinds of structure. The task of providing an adequate theory of approximate truth that fits the history of science and directly addresses the problem of ontological continuity has hitherto defeated realists, but it is easily possible to display the structural commonalities between different theories. Hence, a form of realism that is committed only to the structure of theories might not be undermined by theory change. (2011: 21-22)

The crux of the above passage can be summarized in terms of the phlogiston-oxygen case thus: the entity phlogiston and the entity oxygen dos not refer to the same referent but the structure in which the two theories on the whole describe certain phenomenon of the world has massive resemblance (or perhaps it's a carry-over). Let us now briefly sketch the two theories.

Two pivotal conceptions which drove the phlogiston theory were those of phlogistication and dephlogistication. These two are reciprocal processes. Combustion and calcination involve the dephlogistication of the fuel or metal and the phlogistication of the air, and the addition of an acid to a metal involved the dephlogistication of the metal to leave the base. It must be noted that contemporary chemistry also contains similar processes which are oxidation and reduction. Many believe that these processes are a carry-over of phlogistication and dephlogistication. The main empirical success of the phlogiston theory according to Ladyman are as follows: firstly, the phlogiston theory identifies charcoal as pure phlogiston since it leaves no calx and burns out completely. It was believed that phlogiston is the key ingredient that is absent in calx and present in metals. So phlogiston was supposed to have a metallic quality which explains what all metals have in common namely, being shiny, malleable and so on despite their calces lacking these qualities. In such case, if we burn a metal calx with charcoal, it must acquire more metallic character. Ladyman noticed that Stahl had adviced those extracting copper from copper ore to make sure to add enough charcoal and this advice worked. In

general, the addition of charcoal (conceived as a source of phlogiston) is necessary for the extraction of metals from ores in most cases. Secondly, Combustion in a closed chamber ends in a matter of time. This was explained in terms of the saturation of the air with phlogiston. Thirdly, animals in a sealed chamber eventually cause the air to be unable to support combustion (it is believed that this was known to Robert Boyle). On the other hand, air in which plants are grown is better able to support combustion.

It was generally believed at that time that dephlogisticated air is worthy of respiration. However, Cavendish's discovery of inflammable air ('hydrogen' in the current use) complicated things for the phlogiston supporters. The nature of air took over the hot debates in circles of chemistry. Ladyman beautifully sums up this scenario.

> The discovery of so-called 'inflammable air' [hydrogen] (by Henry Cavendish (1731- 1810) in 1766) led to renewed controversy about the nature of phlogiston because inflammable air is obviously not just ordinary air with phlogiston in higher concentration, since the latter would not burn or support combustion (it contains a lot of what we now think of as carbon dioxide which was dubbed 'fixed air' by Priestley). Nonetheless, Cavendish thought that inflammable air was pure phlogiston. He produced dephlogisticated air and inflammable air from water (1783) (and vice versa), showing that it is a compound substance. Priestley also heated some metal oxides in inflammable air to make pure metals (and water) (this works for some oxides, for example, that of lead, but not all, for example, that of iron). Priestley found that some of what he thought of as phlogisticated air dissolves in water (carbon dioxide) and some does not (mostly nitrogen). Neither supports ordinary combustion (like oxygen) or reduction (like hydrogen). (2011: 7)

Ladyman criticizes the general attitude among historians of science towards the phlogiston theory. He asserts that many thinkers believed that it is not proper chemistry, and the theory contained terrible use of human mind by speculating more than what systematic science would amount in its contemporary sense. Many believed that phlogiston theory was "qualitative, and still related to renaissance and Aristotelian ideas

of principles, qualities and virtues" (Ladyman 1998). Some historians of science believed that chemistry, in a certain sense, was on the right track after the phlogiston theory's demise. Ladyman suggests that such conceptions are not correct. He says that Lavoisier's theory is nothing but a structural retention of phlogiston theory.

Running contrary to the phlogiston theory was the oxygen theory proposed by Lavoisier. Ladyman claims that even though the phlogiston theory faced several challenges with the advent of pneumatic analysis, a certain amount of it is evident in Lavoisier's oxygen theory, namely, that combustion, respiration and calcification are all the same kind of reaction (oxidization). Reduction can be termed as an inverse relation to oxidation.

Worrall narrates the lengthy historical phase with conviction. Worrall believes that Lavoisier was convinced that mass is conserved in chemical reactions as he constantly noticed the mass of some residues from combustion. Lavoisier understood the idea of a chemical element as 'the endpoint of the chemical process of analysis' in 1789 and concluded that metals were elements. However, from Stahl's theory, this was impossible because metals contained phlogiston. Worrall claims that,

> According to Lavoisier, oxygen is a component making up the compound ordinary air, and those processes such as burning, respiration and the rusting of iron previously categorized in terms of the release of phlogiston are all oxidization reactions (note however that Lavoisier did not think gaseous oxygen was elemental). Lavoisier's theory was not without its problems. The least of which is that he thought that oxygen was the principle of acidity (phlogiston theorists had thought that earths such as carbon and sulphur which had been dephlogisticated were acidic), and thus he could not account for acids like hydrochloric acid which do not contain oxygen. Recall also that prior to Lavoisier's theory, the supposed presence of phlogiston explained what all metals have in common, whereas Lavoisier could offer no explanation and indeed none was forthcoming until the emergence of the theory of electronic orbits and free electrons in the twentieth century. (2011: 8)

The major structural retention which structural realist proponents ascribe to this theory change is about the categorization structure under the names phlogistication or dephlogistication reactions in phlogiston theory and the reactions oxidation and reduction in later theories. It was believed by the phlogiston theorists that dephlogistication and phlogistication are inverse relations. This is same in the case of oxidation and reduction. Ladyman says that,

> This is a prime example of a relation among the phenomena which is preserved in subsequent science even though the ontology of the theory is not; the inverse chemical reactions of reduction and oxygenation….The empirical success of the theory was retained in subsequent chemistry since the latter agrees that combustion, calcification and respiration are all the same kind of reaction, and that this kind of reaction has an inverse reaction, and there is a cycle between plants and animals such that animals change the properties of the air in one way and plants in the opposite way. Furthermore, it is worth noting that inflammable air [Hydrogen] really is considered metallic by contemporary chemistry. (2011: 26)

We may say that phlogiston theory identified a number of real patterns in nature and that it correctly described aspects of the causal/nomological structure of the world as expressed in terms of reactions into phlogistication and dephlogistication.

In a certain way, it is philosophically appealing to say that there is retention in the phlogiston-oxygen theory talk. However, I argue in the next chapter, taking clues from the landscape of historical narratives that no case study determines epistemological theories of science in toto. I argue for the view that history of science underdetermines philosophy of science in general and epistemology of science in particular. But before that, let us have a look at a fourth instance of theory talk, the much talked about Fresnel-Maxwell theory change which seems to have lent support to structural realism, scientific realism and semirealism.

IV. 2. 4 Fresnel-Maxwell Theory Change: A Habitat of Interpretations

It is doubtless that Augustin Fresnel and James Clerk Maxwell had quite different accounts of the propagation of light. Fresnel developed a set of equations which capture the intensities of the incident, reflected as well as refracted light when its beam passes from one medium to another having different optical density (e.g. passing from water to air, or glass to water). The scientists prior to Fresnel believed that light was composed of a stream of particles, the corpuscles. This view has its roots in the Newtonian corpuscular theories of light. Fresnel, on the contrary believed that light is not composed of corpuscles, rather it is a wave like disturbance in the ether, which is a mechanical medium thought to have real existence. This belief is rational and sensible in the historical context. Waves usually travel in a medium, for example waves in water. Fresnel thus came up with an account of medium on which light travels namely, luminiferous ether. Therefore, believing that light is nothing but waves which are disturbances in luminiferous ether sounds a very levelheaded account considering the historical context.

Maxwell, on the other hand, thought that light was just one of the several forms of electromagnetic radiation. Maxwell wanted to make his theory consistent with the ether theories. However several experimental results which casted shadows of doubt on the existence of ether made him sway away from such attempts. Maxwell's theory was eventually accepted by many. It construes light in terms of oscillating electromagnetic field vectors. However, Fresnel's equations appear in Maxwell's theory. This is a classic case in the history of science where the nature of an entity changes drastically from theory to theory whereas the structure in terms of mathematical equations remains the same.

Let us have a brief look at Fresnel's equations. Let I^2, R^2, X^2 be the intensities of the components polarized in the plane of reflection of the incident, reflected and refracted beams respectively and I'^2, R'^2, X'^2 the intensities of the components polarized at right angles to the plane of reflection of the incident, reflected and refracted beams respectively, then Fresnel's equations state that these variables will always be related by

$R/I = \tan(i-r)/\tan(i+r)$

$R'/I' = \sin(i-r)/\sin(i+r)$

$X/I = (2\sin r.\cos i)/(\sin(i+r)\cos(i-r))$

$X'/I' = 2\sin r.\cos i/\sin(i+r)$

where 'i' is the angle at which the light is incident on the glass (and therefore also reflected from it) while 'r' is the angle at which the light is refracted into the glass. These equations are retained entirely intact within Maxwell's theory. Of course, the latter theory radically 'reinterprets' the variables. In Fresnel's theory, the I, R, X, I', R' and X', which are the square roots of the intensities of the various beams, measure the maximum distance by which a particle of the elastic ether is displaced from its position of equilibrium by the passage of the wave. In Maxwell's theory (in its 'mature' form) there is no such medium and those variables instead measure forced variations in the electromagnetic field strengths. Worrall comments that,

> From the vantage point of Maxwell's theory, Fresnel was as wrong as he could be about what waves are (particles subject to elastic restoring forces and electromagnetic field strengths really do have nothing in common beyond the fact that they oscillate according to the same equations), but the retention of his equations (together of course with the fact that the terms of those equations continue to relate to the phenomena in the same way) shows that, from that vantage point, Fresnel's theory was nonetheless structurally correct: it is correct that optical effects depend on something or other that oscillates at right angles to the direction of transmission of the light, where the form of that dependence is given by the above and other equations within the theory. (1989: 119)

The main point that Worrall draws out of this historical narrative is that we may not know the nature of entities or processes which these variables represent, but we can be more or less sure about the structure of the phenomena. Scientific realists object to

such an interpretation of this historical episode. Psillos registers his discontent in the following way.

> Is it correct to say that it was only 'structure' (i.e. uninterpreted mathematical equations) that was carried over in the transition from Fresnel to Maxwell? I shall now try to show that fundamentally correct theoretical principles about the propagation of light and some properties attributed to the carrier of light waves were also carried over. (1999: 157)

Psillos reexamines the case, and claims that not only structure, but also nature is carried across to the successor theory from the predecessor theory. If the equations of Fresnel were to be understood as correct representations (of the nature of phenomena, some of which he got wrong), it also contain certain presuppositions about the variables. Psillos says that Fresnel was right about the view that the velocity of the displacement of the molecules of ether is proportional to the amplitude of light. The view that the velocity of light is inversely proportional to the density of the medium is Fresnel's theory's core assumption. The theory also contains principle of conservation of energy during the propagation of light from one medium to another. Psillos claims that if we closely examine Fresnel's engagement in optics, we can grasp that the view that intensity of the light wave is a function of the square of it amplitude and similar views (together with corresponding equations) are all the result of a close reading of the nature of phenomenon of light. Psillos reinforces his view that the nature of a phenomena and the structure of a phenomena cannot be known in isolation from each other. He attacks the epistemology of the selective skeptics and reinstates the strategy he endorsed in most counter examples of Laudan, that if the continuity is referential, the two theories are stating about the same thing, otherwise they diverge (e.g phlogiston-oxygen case). Psillos, however, does not come up with scientists' own opinions on this episode about their lack of confidence in believing Fresnel's theory. Therefore, his understanding is that there is continuity from Fresnel's to Maxwell's theory, but it is at both levels, namely, structural as well as natural.

Chakravartty, taking clues from Psillos as well as Worrall fine tunes the realism that is required in addressing issues of theory change. In doing so, he also uses Hacking's ideas regarding manipulation (see chapter III). Chakravartty claims that structural realism is too weak a form of realism that it only entertains the view that abstract structures are carried over. This does not do any justice to realist spirits and tenets (explained in Chapter I). In the same way, standard scientific realism is too strong a view to retort to Laudan's challenge. It states that one has more than mere structural knowledge of external unobservable world. Chakravartty states the following views referring to Fresnel's equations described a little earlier.

> The existence of certain properties is minimally required to give a realist interpretation of these equations, viz., intensities, and directions of propagation. These are first-order, intrinsic properties of light, but what about the ether, or the electromagnetic field? In the very limited context of these specific equations, ethers and fields are auxiliary posits. Our theories incorporate such entities as important heuristic devices; they help to fill out one's conceptual pictures of the phenomena…. The advice semirealism gives is straightforward: believe in the relations of detection properties, as given in the minimal interpretation, and treat anything that exceeds these structures with caution. Furthermore, recall that these properties are dispositions. When light is subjected to certain forms of detection, certain concrete structures are manifested. (2007: 49)

The semirealist thus looks for structure as well as nature which get retained. However, the nature they look for consists of detectable and manipulative properties. This is the entity realist punch in semirealism. Certain properties can be shown to have existence based on experimental intervention. We have to believe selectively in those properties. The auxiliary entities and properties have to be disposed off from one's warranted beliefs. So, the structure and nature, in a certain sense, form a continuum for the semirealist too, and it is called concrete structure[15]. This is indeed a severe selective

[15] A detailed discussion of the idea of concrete structure and other semirealist ideas is carried out in chapter III.

realism, where standard scientific realism is made to shed its strong views regarding theory change and also made to incorporate the crucial features of structural realism and entity realism.

The Fresnel-Maxwell episode of theory change is unique in many ways. Most of the positions in the debate respond to it and interpret it in one's advantage. The standard scientific realist, the structural realist and the semirealist find this episode justifying their claims. Each position sees its views reflected in this historical episode. What follows is the discussion on the endorsement of these interpretations across the dynamics of scientific knowledge.

IV.3 Trans-Theory-Talk: Over-generalized Philosophies of Science

This level of discourse is the result of frequent overgeneralization seen in the scientific realism debate. Thinkers employ terms such as 'structure' and 'entities' (content/nature) and pick out the 'type' of tokens such as electron or oxidation-relation. However, it may be doubted that such types are available. Philosophy traditionally is in the business of coming up with accounts that apply to several or all cases, whatever be the issue. This tendency is at its peak in the scientific realism debate. I consider this phenomenon as trans- theory-talk. Selective skeptics and realists both engage in trans-theory talk (employing terminology such as 'structure', 'entity', and 'concrete structure' and 'structure-nature continuum') which applies over the board in defending their positions in the scientific realism debate. The type words such as 'structure', 'nature', 'concrete structure' are all taken to have tokens in the history of science. The type 'entity' picks 'electron', the type 'relation' or 'structure' is about tokens such as mathematical equations (the Fresnel-Maxwell theory change) or general structure of the theory (the oxidation-reduction structure in the phlogiston-oxygen theory change). As we already saw, Psillos metaphorically defends his position by saying that there is a structure-nature continuum in scientific theories, and that it is not possible to differentiate structure and nature from theoretical descriptions. This again is an instance of trans-theory talk. Semirealism is an appeal to the idea of concrete structure which contains knowledge of causal properties of particulars and relations between them. Here, the scientific realism-debate is all about the 'types' that are revision defiant, be it structure, entity, concrete

structure or some parts of the theory which are both (structure-nature continuum). We can see that the tokens are discussed in theory-talk. In trans-theory-talk, the discussion is about the type, the debate is not about the particular equation or entity in the specific theory in a particular historical context. It is about whether the upshot obtained from such examples in history that 'something is retained' can be stretched to all instances of theory change. Precisely for this reason, they employ trans-theoretical terms in order to climb to this higher level of discourse.

It must be admitted that the idea of combing for revision defiant constituents in past theories is something which most thinkers sympathize with, not because they favor the strategy, rather because they favor the effort and the careful attention paid to history of science. However, the activity of theory-talk itself is biased and contains a lot of interpretations which are devised to justify trans-theory-talk of each of these positions. What eventually constitutes a justification for theory-talk is trans-theory-talk (the philosophical basis) and what is considered as a justification for philosophy is the history of science. The likes of Hacking, Ladyman and Worrall rely on theory-talk for justifying trans-theory-talk: the case of electron, phlogiston and Fresnel-Maxwell theory change are used in justifying entity realism and structural realism respectively. On the other hand, the claim that structural realism, scientific realism, entity realism etc are really plausible philosophical positions because there are instances of retention (be it a particular equation or entities like electron) in history of science because. There are a lot of works which describe the nuances of the debate, but the most notable among them is that of Juha Saatsi's (2011) where charges of overgeneralization and supplying inadequate cases from history of science are leveled against all positions which move from one or two historical examples to more general and wholesale arguments at the level of trans-theory talk.

> I'm sympathetic to the rejection of wholesale arguments [scientific realism, structural realism, entity realism etc]…. I don't see reason to dismiss the use of historical evidence in the scientific realism debate altogether, but there are some hitherto unappreciated reasons to reconsider the way in which historical evidence bears on the realism debate. [They are] 1. Scantiness of case-studies: historical evidence has not been sought

> extensively, open-mindedly, and across the board, partly because it is not even clear exactly what kinds of historical case-studies matter. 2. Disparity of explanatory considerations: Realist strategies for dealing with historical evidence are often out of sync with their basic motivation (the Miracles argument). 3. Over generalisation: Positions in the realism debate have been construed too rigidly and in overly general terms, ignoring the potential for domain-specific (or'relativized') (anti-)realist theses.(Saatsi 2011: 2)

The previous chapter contained in fact nothing but a lot of examples of trans-theory-talk when I elaborated the individual positions, because the positions were understood as having import to scientific knowledge as a whole. Therefore I won't repeat all instances of it in this section. In response to Laudan's charges, all the externalist positions engage in trans-theory talk where we see thinkers trying to reinvent episodes of theory change in history of science and give their own twist to it. Structural retention, content/entity-retention, concrete-structural retention, retention of success fuelling constituents, are all projected on to these particular case studies. As Saatsi opines, the whole program has gone over the board so much that the realist spirit about the no miracle argument is completely overlooked.

The 'something is retained across theory change' dictum is perhaps the major driving force for going out of sync with matters at hand. The journey from theory-talk to trans-theory-talk is to be scrutinized to see whether the passage from one to other is philosophically plausible. For example, can we say that all (scientific) knowledge is structural knowledge because of the occurrence of structural retention in one or two particular historical examples? The charge of over-generalization arises because the positions' epistemic dependence is towards certain unique cases in history of science. However, not all cases seem to support one position convincingly. In other words, philosophical positions rely on takes on history of science as their defense, yet the story is mostly restricted in number.

I doubt whether the onus of proving/testing a position against history of science is necessary. In fact it is doubtful whether a philosophical position such as scientific realism

can be tested against history of science. This is because positions such as scientific realism, structural realism and entity realism are attitudes towards scientific knowledge. Even though historical cases can be employed in making sense of these positions, the requirement that each and every episodes of theory change are in line with their own position is too much of an ask. This can be understood if one looks carefully at the basis of these positions. For example, the philosophy that drove structuralism is different from the structuralism that is employed as a strategy against Laudan's charges. The former can be a general position which Russell (and to an extent Kant) maintained towards human knowledge. Here there is no theory-talk since what is said is plain philosophy. Russell believed that our senses give us only a structural representation of the world. The ontology of the world thus eludes our best senses. Similarly, Kant identified knowledge as accounting for the phenomenal world rather than the noumenal world. Both Russell and Kant might have used examples to make their positions more meaningful, but probing history of science or any other area of knowledge in order to confirm their position according to them is a futile and audacious attempt. This is because bodies of knowledge such as history of science are subject to a lot of interpretations which can support more than one philosophical positions. This point will be discussed in more detail in the next chapter.

In the same way, entity realism is the result of a philosophical view which purports to a rough ontology of the world, which it endorses in the absence of representation and truth. Looking for survived entities (referents) and failed theoretical terms (senses) all over history of science is not a bad idea, if conducted without overgeneralizations. One can easily pose a counter example from field theories that the idea of entity itself is misconceived and the search for such 'stuff' is on the wrong track. Gelfert (2003) shows that entity realism is really a thing of the past, in the real sense of the word[16]. Therefore the motif for structural realist, entity realist and scientific realist defense against Laudan's charge is disconnected from the general basis of these

[16] Many thinkers have argued in similar lines. Especially with the contemporary prominence of structural realism, the notion of 'entity' itself is problematic. The ontic structural realists dispense with the idea of entities and intrinsic properties. See Ladyman, French (2003). Some argue that the late nineteenth and early twentieth century history of science might do justice to entity realism but the later twentieth century lends its support to structural realism.

positions. The general bases are intended to serve purposes in providing philosophical attitudes to scientific knowledge, which are unnecessarily weighed against history of science in a wholesale manner. This is largely due to over-generalization and unfair scrutiny of history of science. Let us now see what would be the brief outline of trans-theory-talk.

- Any talk about general types such as 'structure', 'entity' which are taken to have tokens in history of science in a wholesale manner. That is, the use of general terms meant to cut across all instances of theory changes.
- Any talk which conjectures about the possibility of types such as 'structure' from tokens (instances of structural retention). In other words, trans-theory-talk is plainly over-generalization from particular instances in the history of science to general stances. The higher level of discourse gives us the impression that it applies to all instances of theory changes.

Having drawn the general features of trans-theory-talk, let us discuss how it is prevalent in contemporary debate over scientific realism.

IV.3.1 Theoretical Constituents and Success: Searching for 'Retention' in all the Wrong Places

Unlike selective skeptics, Psillos does not search for type-words (covering whole of science) which can be types of tokens that are revision defiant in the history of science. Yet, his approach is not different either. He does not use the term 'structure' or 'entity' in his defense-repertoire, but uses expressions like 'theoretical constituents that fuel or contribute to success' and 'structure-nature continuum'. One can argue that here he does not mean the locution 'success fuelling constituents' to be intelligible in the whole of science. But he definitely argues it to be the case in all the theory changes to which Laudan points to. If he concedes the claim that his strategy of picking theoretical constituents can be applied only to certain cases, he loses his place in the debate. He then will have to also concede the point that the realist does not have an over-the-board strategy in defending themselves against Laudan's charges.

However, one should admit that Psillos' theory-talk is always in tune with NMA of the realist that success is connected to representational success (referential success). This does not happen in selective skeptics' strategies. They are largely unsuccessful in showing that the success of certain theory is due to structural features or natural features.

Chakravartty charges Psillos' attempt as an instance of rationalization *post hoc* (chapter III). It is the view that if we are set to find the constituent that is responsible for success and retained across theory change, most probably we will consider retained constituents to be the ones responsible for success. That is, what is visible from our perspective of the present, is that retained constituents are the best candidates for success. However, I argue that the charge leveled against Psillos can be leveled against selective skepticisms too. The difference is that Psillos is combing history of science for retention (of all sorts), whereas selective skeptics are looking for retention of specific constituents (structural/natural features). Is not here a kind of rationalization post hoc happening as well? Psillos says that picking revision defiant terms is something scientists always perform.

> In response to this objection [rationalization *post hoc*], it should be pointed out that eminent scientists do the required identification all the time. It is not that realists come, as it were, from the future to identify the theoretical constituents of past theories that were responsible for their success. Scientists themselves tend to identify the constituents which they think were responsible for the success of their theories, and this is reflected in their attitude towards their own theories. (1999: 112)

Psillos' approach is a naturalistic in nature. In this way, however, he does not dodge under the charge of rationalization *post hoc*. He admits that this is a common activity in science. The scientists themselves in a research tradition are able to pick theoretical constituents which have a role in prediction from theories as they do this all the time. In contrast, the selective skeptics fail to show that success is related to the retained (and later identified) feature of the theory. The usage of history of science is justified if one shows that success of past theories is linked to the retained parts. Selective skeptics only show that there are retentions. They are silent or mostly unsuccessful in

their theory-talk showing that the retained things were responsible for success. Therefore, the charge of rationalization *post hoc* applies to them as suggested earlier but with certain qualifications. The revamped rationalization *post hoc* can be outlined as follows.

- Retentions across theory change are seen as reflecting instances of tokens of specific types (structure, entity etc.,). Rather than looking at past theories from the perspective of the present (as the realists do), the selective skeptics look at past historical episodes from the perspective of their own positions, which isn't good either. 'What is visible is what is retained' was the problem for scientific realists. I claim that 'what is retained is what is visible according to a philosophical formulation' is the problem for the selective skeptics.

Searching for retention in past theories imply that what appears as retention from the present are the anticipated types (structure, entity etc). Moreover, it is also not established that the retained constituents are responsible for the theories' empirical success. The realist can avoid the charge by showing that the retained constituents indeed are the ones responsible for success. But this is a strenuous task which needs to involve the effort of both scientists and philosophers, which has not found the limelight and its proposed result. In fact, Kitcher and Psillos have already tried such projects in vain. The realist can say that the perspective of the present is the best perspective (or rationality) we have (of course with a lot of practical difficulty in executing such projects). This goes in line with the general tenets of standard scientific realism. The current rationality of science is taken to be the best one available. The idea of cumulative progress of science is reflected in this attitude. Therefore, one can also say that the scientific realist is not troubled by the charge of rationalization *post hoc* because it is the very essence of their position.

The important question is that can we avoid rationalization *post hoc*? We commit this all the time. However, sometimes, it is used without caution. Philosophy contains a whole lot of rationalization *post hoc* instances than any other subject. Projecting Sankara's ideas in Hegel is perhaps the most impulsive example to point to. Looking for something in a past text is the activity sometimes performed too loosely. The trans-

theory-talk by selective skeptics contains similar but naive instances of rationalization *post hoc*, which may not be as presumptuous as some of the other examples in philosophy.

Let us have a look into how the structural realists can easily engage in trans-theory-talk by committing to rationalization *post hoc*. We already saw that structural realists discuss about structure in two distinct and disparate episodes of theory change. The phlogiston-oxygen case and the Fresnel-Maxwell case. The former one is about structure in the sense of structure of a theory. In the latter episode, structure is understood as mere abstract mathematical equations. In the Fresnel-Maxwell case, structures are more apparent than the phlogiston-oxygen case, in which Ladyman projects a structure- the similarity of certain reactions in Lavoisier‘s and Priestley’s account. The major advantage for the structural realists is that they already know what to look for in history. However, there is no unanimous agreement even within structural realist camp as to what constitute the notion of ‘structure’. We already saw the various digressions in providing a definition of structure in chapter III. Russell, Worrall, Ladyman, French and several others stand in a relatively naïve framework where structure is attributed to anything and everything. Structural realists in their trans-theory-talk, in a certain sense, structuralize the constitution of scientific theories. Even entities are interpreted structurally (ontic structural realism). In such a scene, it is not impossible to meddle with history of science, where the type ‘structure’ is itself not discerned properly. In fact, the structuralist can easily explain away all instances of theory change as cases of structural retention as one or the other connotation of structure will capture them.

The act of rationalizing *post hoc* is moderately easy for the structural realist to perform because there are plenty of interpretations of structure available to project onto history of science. The story of entity realists is not different either. If they have to capture the recent developments in science, they will have to bend their conceptions on the discernability of entities and also perform a lot of theory-talk. Notions such as field, processes, group of particles etc. have to be given a non-structural interpretation by the entity realists. The individuality of such entities can be ensured only by identifying the intrinsic properties in them. A ‘field’ can be understood as an entity, provided we

broaden our conceptions as to how we discern the individuality of entities called fields by identifying intrinsic properties of 'the field' as a whole.

Trans-theory-talk and rationalization *post hoc* happen simultaneously. This can be roughly understood as – projection of retainable-types into history of science arguing for implications across the sciences. However, the danger is not in both these cognitive acts. The danger is in conceding the point that the theory-talk justifies trans-theory-talk and thus the various philosophical positions. This tone is what is reflected in the scientific realism debate, which, needless to say, is accompanied so often with rationalizations *post hoc*.

History of science is thus a malleable territory and can be loaded with interpretations which support one's ideas. This is not a new tendency in philosophy of science. It has always been like this but in the scientific realism debate, thinkers have gone a little too bold and consider history of science as a testing ground. Selective skepticism is not only selective in proposing the view that we should be selectively realists; they were selective about history of science too. They searched and came up with certain unique cases which supported their claims, whereas one can easily show several counter examples. Most selective skeptics engage in trans-theory talk before considering historical cases. They are already clear about what to look for and where to look for.

However, the discourses involved in theory-talk and trans-theory-talk have their repercussions in the metaphysics pertaining to several issues. Most thinkers use theory-talk and trans-theory-talk to explain their metaphysical position. The following section discusses such attempts.

IV.4 Metaphysical-Talk

Tim Maudlin (2007) claims that science can reveal a great deal about the fundamental features of reality. This however rests with the matter of how we interpret scientific knowledge and the manner in which we take it to have an import to metaphysics. Maudlin is not so patient. He has the view that the Kantian idea, that metaphysics is just an enquiry into the conceptual system of humans and it cannot be validated by experience (the same line of thought is maintained by the positivists too) is

out of date. He believes that quantum theory and relativity 'annihilates' Kant's position. I am sympathetic to Maudlin's line of thinking that science can reveal a great deal for doing metaphysics. However I am suspicious about the claim that science can test, or in a certain way, adjudicate between metaphysical positions.

> Metaphysics is the most generic account of what exists, and since our knowledge of what exists in the physical world rests on empirical evidence, metaphysics must be informed by empirical science. As simple and transparent as this claim seems, it would be difficult to overestimate its significance for metaphysics.
>
> …..Empirical science has produced more astonishing suggestions about the fundamental structure of the world than philosophers have been able to invent, and we must attend to those suggestions. That our physical theories are supported by empirical evidence is no demerit, but rather provides us with grounds for believing that these extravagant accounts of what exists might be correct. (Maudlin 2007: 89-90)

There is a reason for casting shadow over Maudlin's bold claim of testing metaphysics by science, even though it is difficult to comprehend Kant's view (or anybody's view) in terms of quantum physics. The reason for doubting Maudlin's confidence is that, science does not test metaphysical positions in the same way as observations in science tests scientific theories. In the scientific realism debate, theory-talks over several past scientific knowledge are taken to have import to metaphysics. A much more detailed discussion as to whether the thesis of realism itself is testable is provided in the next chapter. The major point to be noted in this regard is that, scientific knowledge has to go through a lot of interpretations before taking them to be adjudicators of metaphysical knowledge. These interpretations are fairly frequent in philosophy of science. We saw in the section related to theory-talk as to how thinkers devise their own interpretations for an episode of theory change.

Let us now take the conceptions of structure and entity and see how most of the proponents understand the term itself. Almost all recent commentators agree that

different connotations are possible for this. This would lead us to the metaphysical aspects of these terms. Let us start with Chakravartty's views. He sets out to differentiate his view from structural realism and other positions by fine tuning the concept of concrete structure.

> Concrete structures are identified with specific relations between first-order properties of particulars, and first- order properties are what make up the natures of things. So on this view, to say that two sets have the same structure is ipso facto to say something about the intrinsic natures of their members. Furthermore, concrete structures arise as a consequence of the dispositions conferred by these first-order properties. Natures are thus intimately connected to the relations into which properties and particulars enter. Speaking rather loosely, one might say that while causal properties are intrinsic, they also have a ''relational'' quality. They are ''relational'' in that they confer dispositions, and dispositions determine the sorts of relations properties and particulars can enter into. (2007: 41-42)

Chakravartty claims that we should look for concrete structures in scientific knowledge and not abstract structures. For example, the relation and relata together connote concrete structure. But by relata, chakravartty means individuals with causal properties. Chakravartty provides us a very simple example, if the mass of one cake (individual with a causal property) is greater than the mass of another, then the former is heavier (an abstract relation) than the latter. He says that semirealist metaphysics asks us to pay attention to individuals with causal properties (something like the mass of cake) than the abstract structure (heavier) which normally the structural realist favor. One must notice that the metaphysics of science is developed here by focusing not only on day-to-day examples, but also by referring to theory-talk. Chakravartty brings in the case studies like the Fresnel-Maxwell theory change to justify his position, especially his metaphysical position.

Since there are several instances of metaphysical-talk already discussed, I would not repeat it here. Recall the debate over *in re* and *ante rem* structures which are provided

in the last chapter. This is a classic account of metaphysical-talk. In metaphysical-talk, thinkers reach the end point of the trajectory from theory-talk through trans-theory-talk to metaphysical-talk. History of science is taken to be a body of knowledge which reflects the metaphysical insights.

The interesting question is, what is the architectural feature of reality? Is it structural, individualist or a mix of both (as Chakravartty claims)? The answers to such questions are taken to be reflected in history of science. The archetypal metaphysical questions concerning individuals, properties and substrata are mostly understood as meaningful in the Newtonian phase until the emergence of quantum mechanics. However, there are efforts by metaphysicians in restructuring these archetypal concerns in tune with contemporary scientific knowledge. Ontic structural realism is the best example in this regard. Disposing of the idea of individuals is neatly carried out in ontic structural realism by pointing to the developments in quantum field theories. However, the metaphysicians painstakingly attempt to work out metaphysics that conforms to both classical as well as modern images of science.

> Philosophical reflection on the 'new' quantum mechanics was entwined with the development of the physics itself, with Born and Heisenberg, for example, suggesting that quantum statistics – both the Bose-Einstein and Fermi-Dirac varieties – implied that particles could no longer be regarded as individuals (see French and Krause 2006, pp. 94-115). For many years this was effectively the 'received' view of the matter, until it was argued that such particles could be regarded as individuals, subject to certain constraints (French 1999; van Fraassen 1989; French and Krause 2006). With the development of 'non-standard' logico-mathematical frameworks suitable for accommodating the 'Received' view's 'non-individuals' and a detailed understanding of the afore-mentioned constraints, two distinct metaphysical packages can be elaborated, consistent with physics: particles-as-non-individuals (described via quasi-set theory) and particles-as individuals (subject to certain state accessibility constraints). (French 2009: 213-14)

It is evident that the central issue is about the ontological priority of one feature of reality over other. Ontological priority of 'relations' over 'relata', thus takes the center stage in the debate among (ontic) structural realists and other selective skeptics. Having briefly gone through some narratives of metaphysical-talk, it can be outlined in the following way.

- Metaphysical-talk is narrative on metaphysical positions taken to have its grounds on the developments in science, or in the history of science (theory-talk). Sometimes metaphysical-talk also refers to narratives on metaphysics which are amended in line with scientific knowledge (e.g. ontic structural realism).

The thesis of scientific realism is not perhaps as sophisticated as structural realism or semirealism in providing intricate metaphysics. Splitting a theory in terms of structural and individual features does not do justice to the general spirit of scientific realism. There is however, a metaphysics of objects underlying the thesis of scientific realism. Chakravartty, wonderfully sums it up.

> At the thick end we have metaphysical theories that give high ontological priority to objects [metaphysics of entity realism, scientific realism], and relatively less ontological priority to relations in which they stand. In the limit at the thick end we have realism about substance: a metaphysical commitment to brute, primitive principles of objecthood. Typically on such views, objects are composed of bare substrata, the very concept of which defies further analysis. Properties inhere in or are instantiated by substrata, forming composites, and relations obtain between these composite entities, *inter alia*. (2011: 192)

This metaphysical position on objects is part and parcel of realism because of the genealogy it shares with classical debates on objecthood, discernability etc. The origin of these concerns goes back to the early Greek thinking. The issue of how a property is exemplified or instantiated in bare substrata was one of the main debates of those times.

The scientific realist's position contains these metaphysical residues which may not go along with contemporary scientific knowledge or knowledge from historical case studies.

To sum up, it is obvious that in the scientific realism debate, positions exhibit a dependence on historical knowledge- dependence on theory-talk. This dependence is exposed by invoking the other two levels of discourse, namely, trans-theory-talk and metaphysical-talk. However, can we say that theory-talk justify the other two levels of talk? If the answer is considered on the affirmative, then the whole debate rests on mere interpretations and scantiness.

The chapter also discussed how the proponents of the various positions select phases of history of science in an arbitrary manner. The charges of a qualified rationalization *post hoc* as well as overgeneralization stand in the light of the debate's dependence on theory-talk as well as arbitrariness in choosing case studies. However, the important question to ask is, will non-arbitrariness in selection of case studies improve the supremacy in justifying their positions on theory change? Non arbitrariness is what is called for by thinkers like Saatsi in dealing with case studies. But even then the problem of rationalization *post hoc* prevails. Even if we choose episodes in theory change in a random manner, we have already made up our mind as to what to be looked for.

In the next chapter, taking clues from this chapter, I argue for underdetermination of philosophical positions in the debate by history of science. After exposing the shortcomings of the debate as a whole and the individual positions, I argue for a split epistemic attitude towards scientific knowledge.

CHAPTER V

RESTRUCTURING THE REAL

V.1 Preliminaries

In this chapter, I continue with the task of explicating the shortcomings of the scientific realism debate and put forth a proposal to reshape our epistemic attitudes to scientific knowledge. The main objective is to discuss the relation between history and philosophy of science in the light of the views from the previous chapter. We saw how history of science is getting interpreted in accordance with philosophical formulations. The significance of history of science in characterizing the metaphysics and epistemology of various positions was also discussed. An interesting question immediately arises in light of the shortcomings- should we maintain a uniform epistemological and metaphysical view covering all of science? I answer this question in the negative. The reason is simple. If we can isolate our epistemic and metaphysical views on past science, then the same can be sanctioned towards current science. In the scientific realism debate, thinkers like Psillos claim that it is not necessary to believe in the ontology of an entire past theory. We can be selective in our beliefs. In several cases, there are only certain components of theories which get 'something right about the world'. The whole theory cannot be taken to be representing the world. Several theories vanished in the course of history of science without actually contributing even a single a revision-defiant component to scientific knowledge. Wouldn't such attitudes (of Psillos *et al*) lead to the idea of several disparate metaphysics and epistemologies of science? What is so unique about current science that it defies the characterization of past science? These are the questions we need to address in order to see the intellectual plausibility of a differentiated approach.

The debate over scientific realism itself gives us hints regarding the question about the right selection of metaphysics and epistemology of science. The differentiated approach roughly is the view that: based on the unique warrant we have with respect to a particular context of scientific knowledge, we can be selective in our epistemological and ontological attitudes. However, the question to consider here is- what would be such warrants? It is 'truth' in the case of scientific realism, 'intervention' in the case of entity

realism, 'detection' in the case of semirealism and 'structural retention' in the case of structural realism. I argue that, based on the unique characteristics of a subject area, there can be a set of such warrants or indicators, so that our metaphysics and epistemologies of science are context-dependent and malleable.

The first section of this chapter consists of views which make clear the intricacies of the shortcomings in the debate. The second section is largely a proposal, towards a split epistemic approach to scientific knowledge, which has its upshots in the shortcomings of the debate.

V.2 History of Science: Pre-determination and Under-determination

Let us provide more clarity into the shortcomings of the debate by building on Saatsi's primary concern, i.e. "historical evidence has not been sought extensively, open-mindedly, and across the board, partly because it is not even clear exactly what kinds of historical case-studies matter" (2011: 2). The shortcoming is visible when we look at the different narratives on history of science (theory-talk) discussed in the last chapter. But the important question is- is there a way we can find out whether an enquiry is sought openly? We already saw that rationalization *post hoc* is a severe charge applicable to all the externalist positions. In fact, rationalization *post hoc* is a historical fallacy where the objectivity of history itself is compromised. I also argue that rationalization *post hoc* brings down the openness in the historical investigations. The act of predetermining history of science is the characteristic of this historical fallacy[17] where the virtue of openness is entirely compromised. I take clues from ad-hocness of scientific theories in constructing an account of openness, which is clearly breached by the thinkers in the debate.

[17] This term was introduced by John Dewey. However he used it to denote to a psychological error, of determining a particular component to be part of a process (as an initial ingredient) where actually the component itself was the result of the process. For example, if we are to determine the ingredients of a loaf of bread, the historical fallacy would be something like identifying 'air' in the bread loaf to be an initial ingredient (component) in the production. However, air was in fact a result of the process of producing bread. I use the term 'historical fallacy' here to refer to premeditated investigations in history of science, something like looking for structural retention in historical contexts of theory-change.

Usually it is believed that if the knowledge of a phenomenon predicts another unexplained phenomenon, then there is, in a certain sense, an epistemic indication in that particular piece of knowledge. This is known as novel explanation. The best example often discussed in the circles in philosophy of science is that of the explanation of tidal waves and its causal connection to the motion of moon from Newton's work on gravitation and motion. However, there are cases where a theory is adjusted to answer certain concerns of an already known phenomenon. This is known as ad hocness wherein we use a theory to explain an already known phenomenon or event. In other words, the theory is constructed or made to adapt keeping the particular event (to be explained) in mind. Most thinkers in philosophy of science believe that novel prediction or novel explanation is to be given epistemic priority over ad hocness. Here, we need not have to go through the intricacies of ad hocness and novel prediction/explanation to undertake the task at hand. All we need to do is to see the resemblance of these concerns in historical investigations related to the scientific realism debate. The historical finding which is pre-determined would be epistemically inferior to one where the finding is novel. This is the case with any human investigation, not just history or science. Therefore, openness in a historical investigation is to be understood as having no predetermined formulation of 'what is to be looked for'. Rationalization *post hoc* runs counter to open historical investigations precisely because it is a predetermined intellectual exercise.

If we find structural retention in a particular historical phase without in anyway having prior intention of 'looking for structures', then it is an instance of open investigation. Worrall would claim this to be the case in his theory-talk, that he noticed structural retention in the Fresnel-Maxwell theory change accidently and without any premeditation. Even if we grant that a unique case of investigation in history of science was openly carried out, in order to defend structural realism, we will have to interpret several disparate cases in line with its formulation. This is also evident in Saatsi's worry regarding 'the extent' to which a position is checked against history of science. At this juncture, as he rightly claims, the openness is compromised.

Can we say that philosophical positions like structural realism or entity realism explain most of the historical phases of theory change in an open manner? To answer this

in the affirmative is highly problematic. The differences in the way structural realists treat the phlogiston-oxygen case and the Fresnel-Maxwell case is the best example. They had to modify the very notion of 'structure' in two different ways to account for both. Hacking also gives us an impression that he accidently stumbled on to the idea of entity realism and its connection to the case of electron. This does not mean that he was completely clueless as to what to look for in the examples he investigated. In addition, the entity realist later on gets into an unending struggle to fix their metaphysics in line with the quantum field theories.

In science, the interplay between theory and observation occasionally exhibits the virtue of novelty (as in the case of Newton). If a theory proposes a hitherto unknown observation, then it is definitely a theoretical virtue. But often there is a complementary relation between the two. Theory and observation complement each other and it is difficult to establish the exclusiveness of one from the other. This could also be the case between history and philosophy of science. I am not demanding that philosophers should be so open in their historical endeavors such that they completely suspend all predeterminations before venturing into history. This is perhaps not possible too. But a moderate level of openness can be called for in such contexts. The important point to remember at this stage is that notions in philosophy should not be frequently amended to suite history. Notions such as 'individuality', 'structure', etc. are constantly appropriated. This itself hampers the openness of historical pursuit.

The relation between theory and observation in science may not exactly resemble the relation between philosophy of science and history of science. However, I propose that the relation between the latter two domains is rather intellectually weak within the scientific realism debate. This is because philosophy of science predetermines history of science and history of science underdetermines philosophy of science in the debate. To predetermine, one must already have some knowledge of what to pursue in history. Also, one must construct the philosophy so as to account for history. Because of this predetermination, several positions easily explain the same historical context. This leads to a serious underdetermination as to a difficulty in choosing a position which is justified by the historical context in question. We are left with no epistemic tool to judge the

position that is best explained by history. The promising proposal is to work out the notion of openness in this regard. The following are at least some suggestions where we can avoid the historical fallacy of predetermination by strengthening openness.

- Pursue all historical episodes of theory change without amending the philosophical details in question frequently (i.e. analyze whether several historical phases conform to the same philosophical criteria; e.g. structure only as mathematical equation or entity only as substrata with properties). If we formulate our philosophy in order to predetermine our history, then openness is compromised.
- An extensive approach is the hallmark of openness. But if the philosophical formulation we search for do not reflect in most historical episodes, we must drop the hunt for extensiveness there. Such an act augments the openness.

It is not a difficult task to see that underdetermination in the scientific realism debate is the result of predetermination and lack of openness towards history of science. However, we need to see the characteristics of such an underdetermination. One must note that it has its ideological basis in the Duhem-Quine underdetermination, but it differs from it on various counts. It is not a feature in science, but in philosophy of science. The following subsection is an attempt to understand the kind of underdetermination evident in the scientific realism debate.

V. 2. 1. Historical Meta-Underdetermination

Huene and Oberheim (1997) claim that incommensurability is not only a feature of science but also of philosophy of science. They believe that the reason for the debate between external and internal realist positions to be a never ending endeavor is that both camps have, in a certain way, stuck with meta-incommensurable basic conceptions with respect to truth, reference and reality. It can be easily noted from the chapter III that the internalist adopts an epistemic account of truth which is incommensurable (at a meta level) with the non-epistemic conception of truth propounded by the externalists. The very fabric of 'real' itself is understood differently by the two camps. For the externalist, the epistemology consists of *mapping* an already existing mind-independent world.

However, the internalist routes for a *projection* of our constructs onto the world. The epistemic conditioning compromises the character of the 'real'. Thus, the kind of incommensurability we encounter in this context is acting at a meta-level. Huene names it 'meta-incommensurability'. Taking the hint from Huene, one can see that the underdetermination happening in the scientific realism debate is of a meta-level nature. It is about history underdetermining philosophy. The term 'meta' is used only to denote that the case of underdetermination is not in science, but in stances or in philosophies of science. The different theory-talks underdetermine the trans-theory-talks and subsequent metaphysical-talks. I call this: historical meta-underdetermination of philosophical positions. We are left in a state of indecision because several narratives arise out of a single episode in history. These narratives make us unable to decide which position is supported convincingly.

Unlike Duhem, Quine claimed that underdetermination in science is a logical issue. Duhem had the opinion that it cannot be understood independent of a historical context. That is, the question of what constitutes evidence for a hypothesis is historically ingrained for Duhem. Whichever version of underdetermination we subscribe to, one important condition it demands is the empirical equivalence of scientific theories. Without this feature, underdetermination would not happen. In other words, two theories must be empirically equivalent to trigger underdetermination. In the context of scientific realism debate, the competing candidates are not scientific theories but philosophical positions. One can however immediately notice a similarity. These philosophical positions in the scientific realism debate are all explanations for the success of science. Even though they are different in their epistemology and metaphysics, they trigger underdetermination because in this context, they are competing to unearth success-fueling constituents from history of science. Here, several positions provide us with different stories of the success of science, yet they all are perhaps axiologically equivalent. They all pose the same historical method, that of picking components of revision-defiance. In science, the same observational consequence underdetermines empirically equivalent theories. In the scientific realism debate, the same historical context meta-underdetermines more than one philosophical positions with axiological

equivalence. Let us briefly outline the notion of historical meta-underdetermination and then proceed further to make some more points clear.

- Alternative philosophical formulations are possible for the same historical episode of theory change. The historical episode does not uniquely provide us adjudicating conditions to single out one among these alternative formulations.

The complimentary relation between history and philosophy of science becomes visible in the context of historical meta-underdetermination. One can get at least a sensible picture of historical meta-underdetermination if we take it to be emphasizing the difficulty of arriving at a decision as to which philosophical position gets its support from history of science. In science, a theory usually is taken to be entailing a piece of evidence which underdetermines it. In fact, entailment is a necessary aspect in Quine's view of underdetermination. We need to ask whether there is a resembling counterpart in the context of historical meta-underdetermination. The relation between philosophy and history cannot be that of entailment. However, the scientific realism debate does give us an image where philosophical positions have their manifestation in the history of science (this relation need not be of entailment). Entity realism can be said to implicate referential stability across history of science. Similarly, structural realism might imply structural retention in theory changes. Even though these relations look like entailment, they are different from the relation between observation and theory in science. It is however a rationally forceful assumption, to say that a philosophical position implicates an observation in the history of science.

Rather than entailment, here, the relation is that of intellectually plausibility. Given, the thesis of a particular philosophical view, it is intellectually plausible to claim that history of science exhibits the assumptions in the thesis. Structural retention in a particular historical case can be said to be an intellectually plausible assumption, given the thesis of structural realism. Similarly, referential stability is an intellectually plausible aspect in any particular instance of history of science, given the thesis of scientific realism or even entity realism. Evidence and theory have a relation of entailment whereas philosophical formulation and instances of history have relation of intellectual plausibility. The conception of intellectual plausibility definitely needs further

clarity. But I would not venture into it now. It can be taken to be a likely description which is in line with a corresponding thought. For example, it is not intellectually plausible to say that elections are to be banned if one believes in democracy. Similarly it is not intellectually plausible to say that structures are retained across theory change if one believes in entity realism. In a certain metaphorical sense, intellectual plausibility is the guardrail of a particular view. It helps us to say whether one is in line with what one believes. This need not be a matter of deduction and entailment, but a matter of rational forecast.

Another important aspect where clarity is at stake with regard to historical meta-underdetermination is the implications it has to the debate. Usually, underdetermination in science poses a threat to 'truth' of theories. We are left with no clue as to believe in which among the two or more empirically equivalent theories (all implying the same observational consequence). Thinkers like Bas van Fraassen believe that in such contexts, we need to choose theories based on pragmatic considerations. Truth and other epistemic virtues have no role in theory choice. With regard to historical meta-underdetermination, we are in a slightly different situation. The scientific realism debate contains several positions, in which we are having more than one philosophical formulation concerning the same historical episode. How are we to decide the apt formulation? Here, like in science, the indecision of choice in our mind can be termed as the unavailability of any rational means to choose one formulation over the others. However, what is at stake is not truth, but good grounds. There is no meta-epistemic indication by which we can say that one position is better than the other. In science, there is always a possibility of testing a theory against an observational consequence, or collecting more evidence. In fact the underdetermination thesis makes its presence felt rarely in science. Firstly, hardly there are empirically equivalent theories other than the ones logically created using Craig's notation. Secondly, even if we have empirically equivalent theories occurring in science innately (not created logically), it is often not difficult to come up with a sequence of observational consequence by improving on the data (or by adding implications from another already established theory) where only one theory can be held to account for it. The adjudication is complex but not impossible. The practice of science ensures such adjudications.

However, the scientific realism debate consists of several axiologically equivalent philosophical formulations where the relation between the formulations and history of science is that of intellectual plausibility. Since there is no entailment, we cannot really go on and test a philosophical formulation against history of science. We can only say whether the formulation makes sense in a particular historical context. Rather than 'truth' we are looking for appropriate formulations which have the intellectual virtue of openness discussed earlier. In such a scenario, we must say that there is scope for antirealist positions as well. If we pursue each historical episode with openness, then it is possible to understand that unique philosophical formulations account for unique historical contexts. For example, given the formulation of 'individuality' (used to address early twentieth century physics) and the thesis of entity realism, it is highly unlikely that the same formulation can account for quantum field theories. If we say that it can account for both, then here is a breach of openness. Similarly, we may not be able to pick revision defiant constituents in all the cases of theory change. In some cases, there are simply no carry-overs. We do not gain in philosophy if we are to grapple with history and vehemently say that something is carried over by appropriating one's philosophical formulation.

Saatsi's idea of having domain specific epistemic and metaphysical attitude is relevant in this regard. If we pursue history of science openly, then we might end up with domain specific epistemologies and metaphysics of science. I discuss this concern later in the chapter. However, I believe that there is something unique in the culture of philosophizing such that we resist intellectual compulsions towards differentiated approaches. The history of philosophy itself is witness to the fact that we always were in need of overwhelming and all encompassing explanations. It was in a certain sense, not in the culture of mainstream analytic philosophy to back the claim that an issue like pessimistic induction can be collectively answered by different positions as it consists of disparate historical phases.

Now, let us have a look into the way the debate reached a stage where we get the impression that positions are tested against history of science. The reason for this can be traced back to NMA. The scientific realists embraced the argument so much that they

rested their thesis on the basis of this. The relation between empirical success and truth is perhaps not so vivid in the history of science. The realists were not expecting Laudan's challenge which squarely put the onus of a satisfactory explanation of the failure of past successful theories on themselves.

V.2.2 Can We Test a Philosophical Formulation against History of Science?

NMA begs the question because of what it imputes to history of science. If the explanation of the success of science is given in terms of truth, then, obviously we have not learned our history of science properly. Laudan is highlighting precisely this point that there is a sense in which we can evaluate the thesis of scientific realism against history of science. To add to the strength of Laudan's argument, recently Saatsi (2011a) showed that even visibly inconsistent theories can exhibit empirical success. Kirchoff's theory of diffraction is one of the best examples in this regard. The scientific realism debate became a territory full of arguments from history of science, consisting of, both for and against the thesis of scientific realism. I doubt whether a philosophical position can be tested, or in a sense, evaluated like this. Of course, any philosophical formulation should comply with the conjectures it foists on history. But the parameters here are different; it is one of intellectual plausibility rather than entailment.

NMA puts the scientific realist squarely against the view that success and truth are not related in any way. The simple reason for such an intuition is that, if truth is the reason for success, then truth must logically imply success; whereas the scientific realists argue backwards, that successful theories are true. This is nothing but the fallacy of affirming the consequence. Recently, many thinkers (e.g. Held, Carsten (2011)) have argued that it is not possible to defend such a fallacious intuition, and that NMA is simply wrong.

The notion of truth and its connection to empirical success is to be worked out in detail by the scientific realist to even have the possibility of foisting such a formulation on history of science. Before venturing into history of science, we need to make sure that a pragmatic feature like empirical success is either an epistemic virtue or at least a

constant epistemic indication that the theory is hitting reality[18]. However, the view that empirical success gives us an indication to truth is a very bold statemsnt. The scientific realist would agree with this because that's what is meant by NMA. However, pragmatic virtues are exhibited by several past theories which were simply false. Therefore, they cannot be said to provide indication to truth if we consider the track record of science. The notion of truth, as it is currently applied in the debate, is too cumbersome to be related to a scenario of assessment. In what way can we say that certain parts of the past theories were true, or even partially true? Even if we grant that certain parts of a particular past theory are true, in what way it drives empirical success?

May be the scientific realist should talk about an ideal science, where an ideal theory which is true drives empirical success. One can argue that, even though it is not the case that all empirically successful theories are true, ideally, a theory which is empirically successful must be true. However, in such an ideal scene, the empirical success of a theory would not be a miracle because it is simply an ideal case. Here, NMA loses its force. So the scientific realist cannot argue that NMA is referring to an ideal scientific scene.

The bottom line is as follows: NMA diverted our attention to history of science. Historical case studies were pursued rigorously, and in this effort, the objective of the debate was ignored. The intent of the debate consisted of establishing a link between at least one of the theoretical virtues and the pragmatic virtue of empirical success. Instead of this, the historical narratives were used to defend philosophical formulations, most particularly the epistemologies of science. Nowhere in the debate, except in a few instances from Psillos, can we see historical investigations aimed at establishing this link. One might want to propose that the notion of consistency is the right epistemic virtue at least to start with, in establishing this link. The reason for choosing the notion of consistency is very simple. It is the only available conception with a potential for some kind of operationality in history of science. Truth does not offer us any handy working principles as to how to understand the truth of a piece of knowledge other than the

[18] Theoretical virtues are usually of two types: pragmatic and epistemic. Notions such as truth and consistency are epistemic virtues whereas notions like scope and simplicity are pragmatic virtues.

semantic notion of 'correspondance'. We only know that some past theories are not true. But this assumption does not offer any assistance with respect to an analysis of how a false piece of knowledge can imply true predictions.

The positive aspect in the notion of consistency is that it allows the possibility of a very practical enquiry into history of science. Hitherto the notion of truth has failed in providing this practical advantage. The important shift that happens in such an approach is the difference in the structure of NMA in terms of consistency, that of checking the link between empirical success and consistency, rather than truth. Here the epistemic virtue of truth is replaced by consistency. We may look for inconsistency in past theories (set of sentences). Saatsi's example of Kirchoff's theory is an instance of an inconsistent theory having empirical success. An inconsistent theory would be obviously false. However an inconsistent theory can imply several valid true consequences. This intuition does not undercut our basic insight of logic. An inconsistent set of premises offer us a valid but unsound reasoning. We might hit a true conclusion with a mix of true and false premises (initial conditions and theoretical assumptions). Therefore, it is perfectly plausible for a false theory to have empirical success. But I believe, this approach of replacing truth with consistency is again another futile move.

It might appear as if the only hope at testing a philosophical formulation against history of science would be to work out an operational and effective concept like consistency rather than an intuitive and ineffectual concept like truth. I am not proposing any particular notion of consistency to be used in this regard. However, here again, one can ask about the meta-epistemicness or historical-neutrality of any such moves. The notion of consistency we demand from past theories would be completely alien to the notion of consistency (or similar notions) which those scientific cultures actually entertained. Therefore, my primary point is to step back from all such exercises which predetermine past scientific knowledge. This might sound as a statement completely denying the philosophical utility of history of science. However, the proposal is not as gloomy as it sounds. The openness I discussed earlier is the right intellectual virtue that reduces the predetermination in matters like this. If we openly pursue the historical cases

rather than be selective, then we can minimize the philosophical (over) generalization profusely happening in the scientific realism debate.

V.2.3 Surrogative Reasoning and Empirical Success

Mauricio Saurez (2004) advanced an inferential account of scientific representation which is suitable for the semantic conception of scientific theories. Gabriele Contessa (2009) recently developed an interpretative version of scientific representation taking cues from Saurez. The basic tenet of such approaches is the emphasis given to the activity of reasoning inherent in scientific practice and its import to the link between the world and representation. Hitherto, notions of correspondence (in the syntactic view of theories) and similarity (in the semantic view) held center stage. However, both these notions were relatively unsuccessful in capturing the link between representation and the world. The demise of logical positivism shifted focus from the syntactic view of theories to other views. The syntactic view consists of the idea that theories are axiomatic systems closed under deduction. This was slowly replaced by the view that theories are sets of models. The first chapter contained a little discussion on the nitty-gritty of these positions. The strength of the semantic view consists in its scope. It explains not only issues such as the nature of abstraction and idealization but also concerns like the range of scientific representation starting from images and graphs to the set theoretical models. Saurez pointed out an important feature of scientific representation viz that a theory represents the world in terms of the amount of inferences it makes towards the world.

> [On] the inferential conception, scientific representation, unlike linguistic reference, is not a matter of arbitrary stipulation by an agent, but requires the correct application of functional cognitive powers (valid reasoning) by means that are objectively appropriate for the tasks at hand (i.e. by models that are inferentially suited to their targets). (Saurez, M. 2004: 778)

The inferences a theory make about the world consists of not only deductive but also inductive and abductive inferences. It is like reading a map and inferring how to move in a particular direction using it. This kind of reasoning where several types of

inferences are drawn from a source to the target is known as 'Surrogative Reasoning'. Sometimes we use deduction inside an abduction or vice versa. Surrogative inferences consist of the set of all possible inferences drawn from a source representation to a target. Surrogative reasoning was developed initially by Chris Swoyer (1991). Saurez believes that a theory is like a map in many ways and the link between the world and the theory is not to be understood in terms of similarity or correspondence but surrogative inferences.

Contessa's interpretive approach is a modification of Saurez's view. A scientific theory is understood not only as a source of inference but also as a source of interpretation of the world. Explanatory power of theories is given due importance here. Now the interesting question to ask is- what is it that these views offer to the scientific realism debate? To come back to the task at hand, we were pursuing the link between empirical success and some unique epistemic indication that the theory is right about the world. One can easily see that explanations and predictions are the result of complex reasoning from a representation to the world. Scientific theories are inferentially suited to the world. Therefore, theories evolve in such an environment where they are constantly made to go through these highly demanding reasoning processes. It may be argued that the link between such pragmatic results and the epistemic virtues of the theory is provided by scientific (surrogative) reasoning. The theory cannot be said to be about a phenomenon in the sense of similarity or correspondence. It is made to pick a phenomenon by making it undergo severe reasoning. The pivotal idea is that the direction from a representation to the world is established in terms of reasoning. We can entertain the point that empirically successful theories are mostly epistemic representations. An epistemic representation is understood as follows

> The fact that a user performs a piece of surrogative inferences from something (a vehicle) to something else (a target) is the main symptom of the fact that the vehicle [i.e. scientific theory] is an epistemic representation of the target (for that user). (Contessa, 2011: 8)

An epistemic representation, unlike a denotation allows us to infer about the target. The example of a map can be considered. It is more than a denotation. Of course, denoting is a necessary criterion for a representation, but it is only an initial criterion.

Just by denoting, I can't consider a representation to be epistemic. Even if I denote that the tomato in front of me represents the Taj Mahal, it is difficult to establish the inferences from tomato to the characteristics of Taj Mahal. Now an interesting issue pops up. Are a past empirically successful theory and a current empirically successful theory both epistemic representations? The answer is 'yes', because the past proponents of the theory were able to perform surrogative reasoning similar to the current subscribers. Let us consider an analogy.

Imagine a fifty years old map of Hyderabad and the current map of Hyderabad. Can we say that both are epistemic representations? The old map surely would have helped a lot of people in finding places in the past similar to the new map. The old map must be an epistemic representation. I argue, as does Contessa, that past scientific theories are epistemic representations as current ones. Contessa says that the only difference between past and current maps is that the past ones *are* 'not faithful' whereas the current maps are 'faithful'. However, the past maps *were* faithful at that time. Now, imagine a person using a past map at present. Can he find his way through? This, again, is a matter of how a representation becomes epistemic. May be the person can use it if he interprets it well. Contessa claims that the notion of 'faithful' and 'non faithful' is a matter of degree in terms of the amount of correct surrogative reasoning. But, according to me, this is not an objective issue. We cannot, at a meta-level, find out the number of surrogative inferences in Aristotle and Newton. It depends on the human being who uses it as well as the representative success of the map as to whether it is epistemic or not. If I take the old map to be just as well faithful as the current one by bringing in my own interpretations to the target, then it can be used for producing correct surrogative inferences and it must then be faithful.

Contessa gets the act together until the notion of epistemic representation. He identifies both the past and current scientific theories as epistemic representations. In addition, his view is sensible when the past scientific theory is understood as less faithful (partially-faithful). But from here on, he is struggling to provide any philosophical progress. The partially-faithful/ faithful divide does not make any sense at a meta-historical level. How is it that an epistemic representation was faithful at the time of

Aristotle and partially faithful now? If we can provide a historically-neutral account to assess this change, we are in a better position. But such an account is not possible. We can clearly see that if we assess the partially-faithful/faithful distinction of two chronologically different epistemic representations, then we are predetermining history again by bringing into play our current perspective on 'faithfulness'. The notion of surrogative reasoning cannot be objectively used in all cases in history of science since the notion itself is not historically-neutral. The same problem we encountered with using the notion of consistency emerges here.

The map analogy is perhaps misleading. Maps change because the terrains and institutions in the real world change. The ontology-change in the world prompts a map change. In science, the view that a theory changes because of the change in ontology is not so appealing, except for the Kuhnians (with a lot of qualifications). The mark of an epistemic representation is the potential to trigger surrogative reasoning, but with respect to an interpretive schema of a particular scientific culture. We may be tempted to say that the old map of Hyderabad was a faithful epistemic representation and it is a partially faithful epistemic representation with respect to the current ontology. This is because the ontology of Hyderabad has undergone massive changes in the last fifty years. However, we cannot grant that Aristotle's theory of motion was a faithful epistemic representation at his time and a partially faithful epistemic representation now simply because we are concluding an epistemic representation as 'partial' on the basis of current interpretations and current ontology of the target.

The view that past theories are partially faithful epistemic representations with respect to the current scientific practice is a realist's response to Laudan. However, the view that both past theories as well as present theories are faithful epistemic representations with respect to particular scientific cultures is conceivably an internalist response to Laudan. The internalists deny the very possibility of a historically neutral evaluation of past theories. However, they too do not indicate the possibility of a domain specific differentiated approach to the metaphysiscs and epistemology of science. This is because, the conceptual-scheme dependency of our epistemology only compromises the character of 'real'. It does not imply that we should subscribe to a certain metaphysical

and epistemological attitudes at one context in history and certain other at another context.

To sum up, the notion of surrogative reasoning seemed promising in answering the concerns emerging out of the scientific realism debate. The positive aspect of this move is that it rests in the relation between the world and representation. Most positions in the scientific realism debate failed to tackle this link between the world and representation. However, after having a hopeful start, the notion of 'faithfulness' lands us in more problems. Firstly, the proposal of approaching history of science with a particular account of surrogative reasoning breaches the openness in question. Secondly, the view that past scientific theories are 'partially faithful epistemic representations' runs counter to a historically sensitive rationality.

V.3 Split Epistemic Attitudes to Past and Present Scientific Knowledge

My aim in this section is only to show a way forward, therefore by no means it is a well formulated philosophical position. It is a proposal that surfaced from the worries in the previous sections and chapter IV; the scantiness of historical investigations with respect to the debate over scientific realism. I ask the question- "Do the shortcomings of the debate point to a change in attitude in dealing with philosophical problems having historical significance?" The answer to this question seems to be in the affirmative. We already saw that predetermination and historical meta-underdetermination are roadblocks to an open engagement in the debate. Uskali Mäki's notion of a doubly local realism catches the eye here:

> I suggest that the issue of realism about science should be contextualized in terms of the peculiarities of particular disciplines and kinds of theories. Instead of any absolute and universal assertions for or against scientific realism we end up with a sort of relativization of realism. *This amounts to a concrete and local as against abstract and global philosophy of science.* (1996: 427, emphasis added)

There are several thinkers in the likes of Mäki who hints at a split approach or a differentiated approach to our epistemic attitudes. David Pappineau had claimed the following after pondering over the views of Cartwright in his book:

> [D]ifferent philosophical morals may apply in different areas of science. Perhaps we should be fundamentalists in physics but not biology. Or perhaps we should be theory realists in chemistry, entity realists in geology, and outright sceptics in paleobiology. ... Perhaps a more fine-grained approach would be worth the extra effort. Now that we are clear about the epistemological options on offer, there is no obvious reason why we should expect the same alternative to apply to every scientific discipline. (1996: 20)

Similarly, one can be a realist in ethics and an antirealist in mathematics. This is because we believe in the uniformity of method of a discipline and distinctiveness of a domain of knowledge. There is such a philosophical choice to be realist and antirealist in disparate domains of knowledge. But when it comes to a single domain, to say the least, natural science, a differentiated attitude is normally not considered hitherto plausible, or considered as philosophically unpalatable. The reason for this could be manifold, and I will get to such a discussion soon in this section.

I claim that the best way forward is to analyze the occurrences of differentiated or split epistemic attitudes in the realism debate itself, even though they were largely unnoticed as instantiating split approaches. To start with, let us go back to the scientific realists' stance on past scientific theories. As suggested in the chapter I, scientific realists bring in epistemic humility by claiming that we can believe in certain portions of theories which contributed to success. Their strategy is to single out certain parts of the theory on which we can warrantedly believe and disbelieve in certain other parts. Is not here a split attitude too? Does such a strategy mean scientific realists are antirealists about revision-prone parts of the theory? How do they entertain two apparently different positions on the same body of knowledge (i.e. two parts of past theories) having methodological sameness (e.g. both parts having been arrived at by IBE)?

I argue, taking the cues from realist themselves, that if one can entertain a differentiated epistemic attitude towards two parts of the same past knowledge, then certainly such an attitude can be endorsed against current knowledge too. Psillos claims that the variation in attitude towards past theories does not make us occasional antirealists such as instrumentalists. He claims that, perhaps, the differentiated approach makes us occasional semantic realists who hold the view that theoretical statements have truth-value. He claims using the intuition that scientists move backward to semantic realism and forward to epistemic and metaphysical tenets of scientific realism depending on the availability of evidence. However, Psillos is serious in asserting that the sanctioning of differentiated approach should mostly be towards past knowledge, not towards current ones.

> Epistemically, the scientists' attitude was one of cautious and differentiated belief. Their epistemic attitude was not an all-or-nothing matter, but rather was determined by the evidence which supported the several theoretical constituents of the theory. (Psillos 1999: 138)

Further,

> [R]ealism requires and suggests a differentiated attitude to, and differentiated degrees of belief in, the several constituents of a successful and mature scientific theory. The degree of belief one has in a theory is, in general, is a function of the extent of its support by the available evidence. Since different parts of a theory can be supported to different degrees, realists should place their bets on the truth of a theory accordingly.(Psillos 1999: 145-46)

My main point of contention is that if such an attitude towards past theories is intellectually compelling (to save realism from Laudan's attack), then why not implement it in our philosophical outlook towards current science as well? Why is it that the realist slogan of 'unobservables exist' applies over the board in current science, even though

most of the 'past unobservables' are shunted? What does Psillos mean by 'differentiated degree of belief' towards a past theory? Psillos does not elaborate more on this notion.

Psillos' historical endeavors were better than the other attempts, because he was seeking a connection between empirical success and truth. May be this is the reason why Psillos came to a point where he entertains a differentiated epistemic attitude at least towards past theories. The other thinkers in the debate were searching for mere retention of what they believed as revision-defiant components, rather than seeking the connection between these revision-defiant components and their role in empirical success.

The interesting question that arises here is – "What kind of an epistemic attitude should one hold regarding those aspects of knowledge where one is not a realist?" If I am a realist with regard to a particular piece of knowledge of the past, then what about other knitted areas? If I am realist with regard to a causal property of an entity within a past theory, then what about my epistemic attitude towards other given properties of the entity about which I have no grounds to believe in their existence (where I cannot find an epistemic indication in believing them)? Similarly, if I believe in entities by asserting intervention, what about those aspects of knowledge (representation) where intervention is not a hallmark? Hacking's position can be taken as an example. He was an antirealist about theories. He never specified what sort of an antirealist he was. The reason for Hacking's split approach is obviously different, but I wanted to show that it is not rare in philosophy of science to maintain a split approach. In chapter III, I interpreted internal realism as having an ontological realism as well as an internalist theory realism. I think such sophisticated split positions are possible where there is already room for filling the gaps. The structural realists are realists about structures, but are they antirealists about non-structural features of scientific theories? If they are antirealists about non-structural features, what kind of antirealism would they prefer? They can be agnostic, but it still delivers a split approach. The point is that split epistemic attitudes are not rare in philosophy of science. However the reason for such approaches could be different. We usually do not notice the 'split' because we are only interested in what aspect of scientific knowledge is warrentedly believed in by some proponents. Typically we ignore the position regarding those aspects where one is not warrentedly believing. A series of

options are possible in such situations. One can be a selective skeptic, instrumentalist, a descriptivist, a conventionalist etc. or even an agnostic.

Let us now briefly look at Mäki's view on the right epistemic attitude which makes realism flexible enough to capture the nitty-gritty of disparate domains of human knowledge.

Mäki believes that, in the statement 'realism R about X', where 'X' is a domain of knowledge and 'R' is the realism we maintain (or say, an epistemic attitude towards a particular knowledge), then both 'R' and 'X' should be taken to depend on each other. A subject area can trigger a kind of realism, and realism can be adhered to be about a subject area. In other words, the subject matter as well as the type of epistemic attitude both contributes to a proper philosophical position consisting of a split approach.

> [T]he adherence to realism is a function of kinds of scientific units and of kinds of realism….The two questions (of the units of science and of the contents of realism) and the answers to them are dependent on each other, both ways. One may begin with fixing the contents of one's realism, then check which units of science fall within its domain. For example, mind-independence or a certain physicalistic view of natural kinds may be included among the defining ingredients in realism, which will imply that those units of science that are about the mind-dependent parts of the world or do not capture physicalistic natural kinds are not to be interpreted along realist lines. One may also begin with fixing one's wish to interpret certain units of science in terms of realism, and then, in order to succeed in doing so, proceed to adjust, within limits, one's notion of realism so as to make it applicable. The latter is my doubly localist strategy here. (Mäki, 2005: 232-33)

Mäki believes that such a doubly local approach would enable us to go back to a global realism. That is, the characteristics of realisms availed with respect to different units of science would give us hints to the characteristics or at least a set of shared features of a local realisms. I believe that Mäki's intent of developing a differentiated

approach is to go back to global realism in full swing after mopping up the exact characteristics of realism from local versions. However, I think such an ambition is uncalled for. Mäki is a realist, a minimal ontological realist. He hardly would invite antirealist interpretation of scientific knowledge. His slogan is to 'reglobalize realism by going local'. The idea I favor is to deglobalize realism by staying local. This would set off the openness we discussed earlier in the chapter precisely because there is no intention to come up with any generic position. This would be especially useful in investigating history of science with regard to the scientific realism debate.

An interesting question needs to be addressed at this juncture. Does the split approach sanction different epistemic attitudes within a discipline, or even within a particular theory itself? If such is the case, how are we to understand its import to the methodology of science? A part of a theory about which we are antirealists and a part of a theory about which we are realists are both the results of similar method. If we presuppose methodological monism, then it seems as if our epistemic attitudes cannot be split. The reason why we believe in unobservables could not be assorted. The scientific realist believes in the existence of unobservables because they are justified by the truth of theories which posits them. The entity realist believes in the existence of unobservables because they are justified in the epistemic virtue of causal intervention. The semirealist's reason for belief in unobservables is the detectable features latent in the world. If we are methodological monists, then it seems we cannot be realist and entity realist in the same domein of knowledge. This is because it seems as if there is availability of a stable 'indication to believe' in most of these positions.

However, we need to be attentive to the fact that 'indication to believe' is discipline-specific. The formulation of the thesis of scientific realism and allied selective skepticisms are largely physics-specific. The idea of mind-independent ontology (existence of unobservables), reference of theoretical terms, belief in the truth of theories are all outcomes of paying special attention to physics. This is visible if we are keen students of history of philosophy of science. Mostly, philosophy of science has been a postscript to physics. When Laudan's charge was taken to task by the contemporary scientific realists, the physics-specific philosophical formulations were ill equipped to

satisfactorily answer most of his concerns, because the very character of some of the theories Laudan pointed out were drastically different from the current ones. Therefore, there is no doubt that the character of a discipline plays a role in developing a corresponding and fitting philosophical formulation about it. However, contrary to popular belief, I believe that the 'indication to what to believe' in a discipline need not be uniform. The idea that 'our belief need not be about the whole story a theory tells' requires it to be understood on the basis of the reasons for believing in 'bits and pieces'. Therefore, in maintaining realism in bits and pieces, we need to see whether we are warranted by causal detection, consistency, number of surrogative inferences etc. This can be easily done with regard to current science. The availability of a whole set of epistemic indicators is what I hint at. The approach might fail in the case of past science because of the lack of historical-neutrality in these indicators. Nevertheless, the approach would be definitely better than the current formulations of past scientific knowledge as seen especially in the scientific realism debate.

Let us sketch the broad contours of a split epistemic attitude to scientific knowledge and then proceed to see how it does justice to the disparities with respect to the historical investigations in the scientific realism debate.

- Split epistemic attitude to scientific knowledge- There is autonomy to adhere to any philosophical formulation about a particular portion of scientific knowledge by approaching it openly. This would require an attempt where the 'context of knowledge' in question provides us hints to what philosophical formulation one should maintain, what is the 'epistemic indicator', if at all it has any, that is instantiated by it. This also allows a pluralistic philosophical formulation of the context of scientific knowledge, if more than one epistemic indicator is at play. If there is not enough hints as to whether a particular context of knowledge can be interpreted realistically (any form of realism), then it ought to be interpreted antirealistically based on the pragmatic virtues.

Pappineau's remark that "perhaps a more fine-grained approach would be worth the extra effort" is what is reflected in the split epistemic attitude to scientific knowledge. Rather than going for an all inclusive strategy from the top, I propose to work with a

bottom-up tactic. This would allow us to see whether different epistemic standards are at work in different contexts of scientific knowledge.

The position of scientific realism presumes that the theoretical statements about unobservables are true. However, this is an inclusive statement. If we are to say this about geology and paleobiology in whose domain there are several causally undetectable entities at play, we are committing the gravest philosophical mistake. However, certain parts of physics can be taken to validate the above presumption of scientific realist. In the bottom-up strategy, of putting the 'extra effort' that Pappineau remarked, the openness we discussed play a massive role. The context of scientific knowledge leads us to the epistemic attitude in favor of it, rather than a particular form of epistemic attitude being projected onto it. Predetermination has to be avoided in our effort and this leads to openness.

Let us now go back to the question of why split epistemic attitudes were hitherto considered less interesting. It can also be answered by invoking its relation to the methodology of science. As discussed early, it is believed that methodological uniformity across a discipline sanctions only one particular epistemic attitude towards it. If it is the case, then we cannot be entity realist in classical particle physics and structural realist in field theories. May be it is the case that disputes in science itself drive one philosophical position rather than the other. But normally it is believed that it is not appealing that we can subscribe to different positions at different chronological phases of scientific knowledge, or different contexts of current scientific knowledge.

There are two options. Either we can claim that there is no methodological uniformity in science, or we can claim that our belief in the posits of science are not dependent on the method of science but on the epistemic indications science provides. I subscribe to the latter option. If we are to ask the question- "why is it that we believe in the existence of something and not on the existence of something else?" The answer would definitely touch upon the method of enquiry but more than that, the answer would hint at something uniquely epistemic about the knowledge and posits which we believe to exist. It could be mostly our ability to detect, or our observation that it is consistent with certain established theories etc. Thus, the method of science and the epistemic indication

(that scientific knowledge provides us) are two independent issues when it comes to the adjudication of philosophical positions. Hacking believes in the existence of electrons and not in the existence of non-causally-detectable astronomical bodies, even though both types of knowledge claims are provided by the same scientific method. He could have easily said that both are the result of same method of science and thus a belief in both is sanctioned. But he is looking for unique epistemic indications (manipulability) from the context in question, in order to believe in something's existence. In fact, the history of philosophy of science itself contains several such split attitudes. I propose that we make ourselves open to the availability of a set of epistemic indications in contexts of knowledge. The lack of such epistemic indications is to be interpreted antirealistically.

Having mapped the sensibleness of a split epistemic attitude, I move onto the next chapter where further explication of the details of the split approach is provided. In addition, I wind up the discussion in the scientific realism debate by highlighting the proposals I so far put forth.

CHAPTER VI

CONCLUSION

VI. 1 Preliminaries

In this concluding chapter, let us furnish some more details of the split epistemic approach. I distinguish between our reasons for believing in the existence of unobservables and our general epistemic attitude towards scientific knowledge. Scientific realism is normally comprises a general epistemic attitude that scientific theories are true. However, selective skepticisms comprise the reasons for believing in what a theory states. Scientific realism, unlike selective skepticisms does not provide us exact hints as to why we should believe in the existence of unobservables. Selective skepticisms are recent developments in the literature of philosophy of science. The supporters of selective skepticisms are keen in developing notions like 'detectability', 'manipulability', 'identification of structural features' etc. in order to supplement reasons for believing in selective parts of the theory. Scientific realism can be regarded as a general stand and a more idealized attitude towards scientific knowledge. Truth of theories is the reason for believing in the existence of unobservables, provided we know what truth is.

I now explain the notion of 'epistemic indicator' which I introduce in this thesis. We already saw that selective skeptics used a variety of 'indications to believe' like 'detection' or 'manipulation' to justify their beliefs about specific knowledge. These are epistemic indicators. I argue that epistemic indicators are domain specific. For example, we may not be able to causally interact with an entity stated in a theory in economics. This, of course is a sweeping statement but I believe it uncovers the issue. Physics and allied disciplines give a lot of importance to 'causal contact'; but there are other areas of knowledge where explanatory power and consistency with established theories yield more confidence in our knowledge claims than causal interaction. A lot needs to be clarified in this respect and I am afraid there are a lot of open ended issues at stake too. However, the aim is to leave scope for further development in this line of thinking having set up an initial direction.

VI.2 Epistemic Indicators

In order to make further inroads into the intricacies of a split epistemic approach, we need to distinguish between epistemic indicators and what is generally regarded as epistemic virtues. Usually we consider the following as theoretical virtues: consistency, unifying power, simplicity, coherence, fertility, scope, and even elegance and beauty. However, it was believed that some theoretical virtues are epistemic. Both Kuhn and Bas van Fraassen treated all theoretical virtues as pragmatic, but having inevitable roles in theory choice. Scientific realists believe that some theoretical virtues like consistency are epistemic. Typical examples of epistemic virtues include: consistency, truth, empirical strength, axiomatizability etc. However, such views are not well received within the ongoing scientific realism debate. This is because of the latent difficulty in defining these virtues as well as in applying them to disparate cases in science. Jones (2002) gives us the following hints regarding the nature of theoretical virtues:

> [A] desideratum X [is] a pragmatic virtue just in case X is relevant in theory choice due to person-or context-related reasons, that is, insofar as X's relevance concerns the use or usefulness of theories….A desideratum X [is] an epistemic virtue just in case X is relevant in theory choice due to the epistemic goals of (our) scientific theories, that is, insofar as X's relevance concerns either the relation between theory and the world, or purely logical properties and relations within the theory. Crudely restated, anything that satisfies or furthers an epistemic goal is an epistemic virtue. (2002: 1)

The discussions on theoretical virtues relate to theory choice. Pragmatic virtues are associated with the use of a theory and epistemic virtues are about the theory's representative success. For example, if we regard empirical success as a pragmatic virtue, then it means that, based on empirical success, we cannot claim that the theory is representatively successful. This was the whole allusion behind the scientific realism debate. Empirical success and explanatory power etc. are usually considered as pragmatic virtues, and they pertain to the usefulness of scientific theories, rather than denoting to the link between theory and world. Similarly, if we consider consistency as an epistemic

virtue, then the theory in question can be at least regarded, in a certain sense, a step closer towards representing the world (if not really representing the world).

The epistemic indicator is normally the aspect in a philosophical position which gives us reasons for what to believe about the world. This, of course is, a very vague formulation, but an example will bring in some clarity. Chakravartty believes that 'being able to detect' is a sufficient reason to believe in certain statements of the theory which are about the unobservable world. According to him, to come across a causal property of an unobservable aspect of the world shows us that it is real and it exists. There is an indication provided by 'causal contact' and 'detection' that what we believe about the phenomena is right. Therefore, these are epistemic indicators believed to have the potential to indicate that the representation is capturing the world. They indicate that we can be warranted in believing in the existence of a phenomenon. Most philosophical positions in the scientific realism debate consist of epistemic indicators. Bas van Fraassen believes in the truths of statements about observable domains of the world. This is because he believes in the epistemic indication provided by observability. The unobservable parts of the world are unreachable as far as the epistemic indication of observability is concerned. van Fraassen does not believe in indirect observability. However, he believes in the empirical adequacy of statements about the unobservable domains. In my reading, he maintains that truth is an epistemic virtue and, observability is an epistemic indicator in the observable domain. As far as the unobservable world is concerned, Bas van Fraassen regards empirical adequacy as a pragmatic virtue and thus there is a lack of epistemic indicator. Therefore, he remains an antirealist about the unobservable domains of the world.

The same is the case about Hacking, Worrall and many others. They are looking for unique epistemic indicators which function in specific contexts. The same epistemic indicator might not lead us to hints about what to believe in all cases. The scientific realist's notion of 'truth' is just an epistemic virtue. The realists would have to provide an epistemic indicator to warrantedly believe in unobservable parts of the world. They usually stick with 'extendable perception' as a strong contender for believing in unobservables. But soon we will see that this leads to more problems than gains.

Structural realists believe that structural retention is an epistemic indicator wherein they are warranted to claim that the theoretical components having structural features capture the world. I argue that, unlike most epistemic virtues, epistemic indicators are domain specific. If we are to look for an epistemic indicator in radio astronomy, then we must hold detection (indirect) as an indicator. Similarly, a life scientist working in molecular biology will be prioritizing certain epistemic indicators like consistency with Darwinian theory rather than indirect observation, precisely because the specific context generates and makes available such indicators. Further, a theoretical physicist in contemporary field theories will be believing that the recurring mathematical equations are the indication to what he believes to be the case in reality. The epistemic indicator for structural realism is contemporary physics-generated. Theoretical virtues like truth is not discipline specific, but epistemic indicators vary from one context of knowledge to other. Truth can be regarded as the epistemic virtue we look for in all the contexts of knowledge.

In matters related to the 'warrant' of our beliefs about the world, we are indicated by something's existence by these indicators. The notion of truth, even if considered as a supreme epistemic virtue is unproductive as far as our 'warrant' to beliefs is concerned. Truth prevails because epistemic indicators like observability, causal-contact and indirect detection allows us to validate our beliefs as true. I believe that epistemic indicators precede epistemic virtues. If we assess the epistemology of science from a meta-epsitemic perspective, we can understand that the precedence is conceptual and not temporal as in an intellectual activity. Without the epistemic indicators, we will not be in a position to even say that the theory is true or consistent. However, the major point to be noted in the discussion on epistemic indicators is that they are all domain specific.

It is worth paying attention to a set of epistemic indicators which are context-generated. Can we say that the usual theoretical virtues like consistency, unification, axiomatizability etc. are all epistemic indicators? I propose that the context in question would help us tackle such issues. In a severely mathematics-ingrained area inside theoretical physics, we need to consider axiomatizability and consistency as epistemic indicators. We may not be in a position to consider manipulation in such contexts (e.g.

cosmology) because they may not have the potential to indicate what to believe. The precedence with respect to identifying epistemic indicators rests with the context in question.

However, the question of whether axiomatizability and consistency are also indeed epistemic virtues is also relevant. Whether they are the ultimate benchmarks in issues regarding rational theory choice? However, my proposal is to set aside issues with respect to theory choice and epistemic virtues for the time being and focus on the issue of epistemic attitude and the role of epistemic indicators. The discussions of epistemic virtues are, in a certain sense, idealized talk about norms in theory choice. They help us to deal with choosing one scientific theory over the other. However, epistemic indicators help us to choose one epistemic attitude (philosophical formulation of science) rather than another.

I would like to argue that concepts like axiomatizability and consistency are just a few from a set of several epistemic indicators, out of which most would not be available in a particular context. In a context where there is more than one epistemic indicator, further clarity is called for. Observability is a very basic epistemic indicator. We may encounter observability as well as manipulability in the same scenario. In such contexts, it is always better to resort to the stronger epistemic indicator. But we may have differences in believing in one epistemic indicator over the other. I remain agnostic as to whether such meta-epistemic concepts are available in adjudicating between these indicators. Here, being pluralistic might just be the only option.

The major proposal that the split epistemic attitude puts forth is the following- rather than fixing what kind of an attitude and epistemic indicator we ascribe to a scientific knowledge context, let us see whether the scientific knowledge context provides us with the relevant epistemic attitude and epistemic indicator. Now, history of philosophy of science itself is witness to several split approaches which operate with different epistemic indicators. The following table will make it clear.

Philosophical Position	**Epistemic Indicator**	**Theoretical Virtue**	**Epistemic Attitude**	**Domain of Knowledge**
Constructive Empiricism	Observability	Observational Truth	Realist	Observable world
	None	Empirical Adequacy (Pragmatic)	Antirealist	Unobservable world
Semirealism	Detectability	Truth/Similarity	Realist	Detectable World
	None	None	Antirealist	Undetectable World
Structural Realism	Structural Features in Theories, Structural retention etc. (e.g. mathematical equations)	Truth/ Similarity	Realist	Structural Aspects of the Observable and Unobservable World
	None	None	Antirealist	Non-Structural Aspects of the Observable and Unobservable World
Entity Realism	Causal Contact	Intervention	Realist	Knowledge of Entities in both Observable and Unobservable worlds, where Intervention is Possible
	None	None	Antirealist	Theoretical Knowledge
Scientific Realism	Observability/ Extended Perception	Truth	Realist	Both Observable and Unobservable Worlds.

Table 4. Philosophical Formulation and Epistemic Indicators.

However, my proposal goes a step further. We need not have to believe in what a theory states by holding on to seemingly stable epistemic indicators in observable or unobservable domains. We may keep our options open and see if there is scope for an epistemic attitude on a domain of knowledge hinted by the domain itself.

Let us now move onto a derivative that emerged out of the discussion on epistemic indicators. As said before, the interesting fact is that scientific realists are not so clear about the epistemic indicators available to them. Perhaps the thesis of scientific realism is not as sophisticated as the various selective skepticisms. This is evident in their resoluteness that truth is the measure of scientific knowledge. They claim that our beliefs about unobservables are warranted because these beliefs are true.

The view that scientific theories correspond to the world by bringing in referential success is a credible philosophical view. The descriptions of truth-bearer and truth-maker are also components in such a tale. But this could be the case in an ideal scenario where proper epistemic indicators are at play together with the availability of meta-epistemic perspectives. The scientific realist often sounds similar to the story of 'Archimedes' lever' where it was believed that Archimedes had claimed that a lever long enough and a place to stand would enable him to move the earth. But simply there are no such place and lever.

The scientific realist's assessment of scientific knowledge and truth would demand us to 'jump out of our skins' to a meta-epistemic perspective. Also, the 'truth of theories' sounds rationally compelling in observable domains. Scientific realist's slogan of 'unobservables exist' cannot be guaranteed with the naïve conceptions of truth and referential success. In the thesis of scientific realism, there is an absence of proper epistemic indicator concerning the unobservable world- a fact which ably exploited and tackled by the selective skeptics. When our beliefs about certain features of the world are stated, the selective skeptics and constructive empiricists in the debate have justifications of these beliefs in terms of observability, detectability etc. That is, they have justifications in terms of epistemic indicators and not epistemic virtues. We may be tempted to say that the scientific realist believes in unobservables based on our ability of indirect perception to validate true statements. However, this would bring in an immediate worry. For the

scientific realist, the truth of theories is the hallmark of scientific knowledge. Truth is not attained by our ability to observe or detect; these are epistemic activities. Truth is independent to any epistemic activity. Therefore, the belief in unobservables is warranted by truth of theories, not our epistemic abilities. If such is the case, then it is impossible to adjudicate something as 'true' or 'false' based on our experience. The entire idea of a past theory becoming false rests on the belief that we adjudicate it to be false based on several epistemic considerations such as observability and detection. The point is that scientific realist thesis entertains a non-epistemic notion of truth, which gives us a picture of truth that is not contextually-assessable because in all contexts of truth assessment, we will have to rely on verification, assertability etc. which are epistemic in nature.

Therefore, the ineffectiveness of any epistemic indicator in the case of scientific realism is because of the non-epistemic account of truth they maintain. NMA amounts to saying that scientific theories are successful because they are true in a non-epistemic sense. The activity of searching for true theoretical components in past scientific theories is an epistemic activity which goes against the spirit of scientific realism. This is because we are looking for the link between certain components in those past theories and their connection to the world- some kind of a *post hoc* intellectual activity which is dependent on epistemic considerations like verification. If a past scientific theory was adjudicated as false based on epistemic considerations, then why do the scientific realists believe that past theories are indeed false? Truth and falsity are not epistemic matters but non-epistemic matters according to the scientific realist. Similarly, the view that past scientific theories are partially true cannot be established without shedding the non-epistemic account of truth by observing empirically what the case really is. So the scientific realist's endeavors into history of science have a second roadblock other than historical meta-underdetermination. The historical pursuit of the scientific realists itself goes against some of their core beliefs.

VI.3 Conclusion

We analyzed in the previous chapter whether concepts like consistency or surrogative reasoning can mend the shortcomings of the scientific realism debate by overcoming historical meta-underdetermination. However, the only useful proposal

which arose was to undertake historical investigations with openness. We saw that there are no historically neutral and meta-epistemic concepts which can be used in adjudicating one position's aptness over others.

Similarly, there is difficulty in fixing the priority of epistemic indicators in various knowledge contexts where there is more than one epistemic indicator at play. One may be tempted to say that we need to be flexible enough to entertain more than one epistemic indicator at the same time. In a context where we have the availability of both axiomatizability and detectability, then we need to be simply realist about both structures and detectable knowledge. We can be pluralists in our epistemic attitude too. Perhaps we are being too much flamboyant here. In most scenarios in science, there is no availability of an epistemic indicator. Mostly we need to stick with explanatory power, scope etc. These virtues do not give us sufficient reasons for believing in what a theory states. That is, in most contexts of knowledge, the context in question does not throw sufficient hints as to whether to be a realist or antirealist. However, my proposal is to keep the options open. A theory at a particular time may not be susceptible to a particular epistemic indicator but might invite one during the course of its life.

To come back to the initial issue, the unavailability of meta-epistemic concepts is the reason for our failure in answering historical-meta underdetermination. Thus, scientific realism cannot be tested against history of science (the difficulties I exposed in chapter V). Therefore, Laudan's challenge remains unanswered if we are attempting to answer it by involving in historical pursuits. The positive aspect emerged out of the discussion in this thesis is a direction of thinking, of keeping a split epistemic attitude to past and current scientific knowledge. Does a split epistemic attitude undercut the methodology of science? This is the perennial question that runs against my proposal. However I showed that most of the philosophical formulations in the history of philosophy of science are split. The only difference is that they all have stable epistemic indicators. I have further argued that, there is no need to maintain a stable epistemic indicator because the very concept itself is domain specific.

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Summary

of the PhD thesis titled **'Restructuring the Real: A Study in Scientific Realism-Debate'**

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Science as a cognitive enterprise displays a positive and a negative aspect. The former relates to predictive, explanatory and technological success of science. The latter pertains to the frequent theory-changes and abandonments of past theories. The positive aspect is offset to an extent by the fact that our best theories in the past were abandoned in the course of history of science which is a "graveyard of theories" as Poincare remarked. This thesis takes the positive aspect of science, namely its empirical success as its stalking horse. I elaborate various realist and antirealist positions and their takes regarding the empirical success of science. However, in doing so, I also discuss the constitutive features of these positions in detail. The scientific realism debate is the result of the arguments and counter arguments between these disparate positions.

In the early years, thinkers were interested to develop individual positions which were constitutive elements of the debate. Now, attempts have been made to firstly, evaluate concepts entrenched in particular positions of the debate and subsequently, evaluate the characteristics of the debate itself. This thesis falls in a category where both these trends in the history of philosophy of science are incorporated. I discuss the views of Stathis Psillos, Anjan Chakravartty, Ian Hacking, John Worrall, Thomas Kuhn, Brian Ellis, Hilary Putnam and many others. In addition, I come up with certain views about the debate itself, which can be considered as meta-philosophical perspectives on the debate. Cues from these meta-philosophical perspectives point to a differentiated or split epistemic attitude towards scientific knowledge. The following passage contains a very brief summary of the issues and views I discuss in each chapter.

In Chapter I, I elaborate the characteristics of the thesis of scientific realism and its defense in terms of the explanation for the success of science. I also explicate the metaphysical, epistemological and other stances of scientific realism. In chapter II, I articulate the major challenge for scientific realism, the challenge from history of science, advanced by Larry

Laudan. Also, I explain van Fraassen's criticisms of the general assumptions behind the thesis of scientific realism. Chapter III is an attempt to articulate the different responses available for the challenge. The externalist positions, namely, entity realism, structural realism and semirealism are discussed. The internalist positions of Putnam, Kuhn and Ellis are also discussed. In chapter IV, I locate the debate as the seat of three different levels of discourses. Starting from narratives about history of science, the thinkers climb to philosophical and metaphysical generalizations. I argue that these different levels of discourses expose the shortcomings of the debate. In chapter V, I develop the view of 'historical meta-underdetermination', which denotes to a state of indecision as to which philosophical formulation corresponds to cases in history of science. I also propose a split epistemic approach where, roughly, the context of knowledge in question provides the clues as to which epistemic attitude can be adopted. In the concluding chapter, I further clarify my position. I introduce the term 'epistemic indicator' which denotes to the specific signal from a knowledge context indicating whether to believe or not. I also argue that split attitudes are not new in the history of philosophy of science.

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