CHAPTER V

RESTUCTURING 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 fallacy17 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 predetermined 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	ontion 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 metaunderdetermination 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\textsuperscript{18}. However, the view that empirical success gives us an indication to truth is a very bold statement. 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

\textsuperscript{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 metaphysics 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.

The 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 domain 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.