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’s 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 does 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 advised 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. 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. Therefore the motif for structural realist, entity realist and scientific realist defense against Laudan’s charge is disconnected from the general basis of these

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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.