

AN ACCOUNT OF REDUCTIONISM IN BIOLOGY; PRACTICES
OF MOLECULAR BIOLOGY AS INSTRUMENTS

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ABSTRACT

AN ACCOUNT OF REDUCTIONISM IN BIOLOGY; PRACTICES OF MOLECULAR BIOLOGY AS INSTRUMENTS

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This work primarily aims to clarify the subject of reduction and reductionism especially in the domain of reduction of classical biology to molecular biology. As a part of a larger project that tries to understand sciences as a manifold of practices. Reduction in science has been one of the most fundamental subjects in philosophy of science. This work brings a novel approach to reduction from a non-idealist perspective. The previous discussions on reduction has been debated under theory reduction, while this work tries to give account of reduction in science retaining a pragmatic posture that is centered around practices of science but not theories. Focusing philosophy of science on real scientific practices rather than solely theories is stance I share with Ian Hacking. Through this investigation, this work outlines the basic features of classical biology and molecular biology in general, and tries to point out the difficulties that the traditional, idealistic reductionist account creates, accusing them of being unable to account for real scientific progress in the sciences of biology and molecular biology. While doing that, this work also tries to sketch the metaphysical backgrounds of reductionist programmes, finally putting them in their proper places in scientific inquiries.

Keywords: reduction, reductionism, entity realism, invention of phenomena, philosophy of biology

ÖZ

BİYOLOJİDE İNDİRGEMECİLİK KONUSU ÜZERİNE BİR ÇALIŞMA; ARAÇLAR OLARAK MOLEKÜLER BİYOLOJİ PRATİKLERİ

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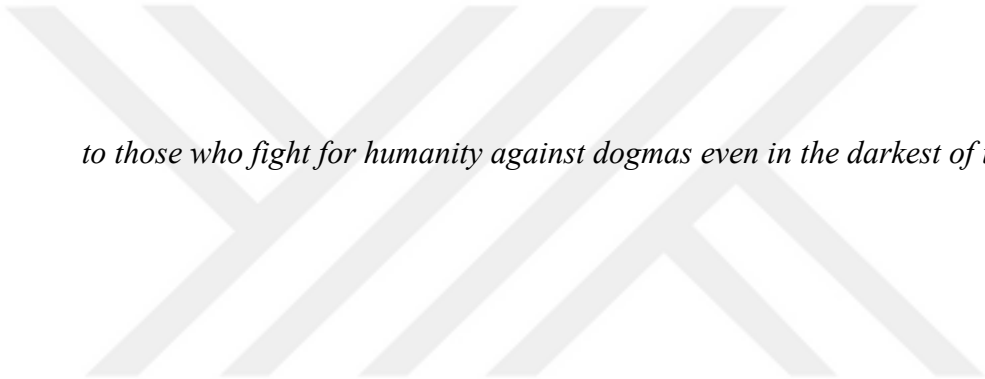
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Bu çalışmanın merkezi amacı klasik biyoloji ve moleküler biyoloji arasındaki ilişkiyi, daha geniş temelli bir bilim felsefesi, olguların izole edilip yaratılması ve varlıklara müdahale bağlamında inceleyerek bilimsel indirgeme konusunda yeni bir yaklaşım ortaya koymaktır. Bilimsel indirgeme, bilim felsefesinde önemli bir yerde bulunmakla birlikte şimdiye kadar süren tartışmalar genellikle idealist bir teorik düzlemde temellendirilmiş ve bilimsel aktivitenin araç çeşitliliğinden fazla faydalanılmamıştır. Bu çalışma bilimi teori merkezli değil pratik bir alanda ele alarak bilime faydacı bir bakış açısıyla bakar. Buna ek olarak bilimsel indirgemecilik tutumlarından başat olanlarını inceleyerek onların dayandığı metafizik arka planları inceler ve bu arka planları bilimsel araştırmada genel programlar olarak işlevsel yerlerine koyar. Bilimsel aktiviteyi çok katmanlı olarak ele alan bu çalışma aktivite içinde klasik biyoloji ve moleküler biyolojinin çalıştığı ve izole ettiği olguların farklılığını ve bu farklılığa sebep olan araçları inceler ve bu katmanlarda indirgemeci tutumları araçsal olarak inceler. Sonuç olarak klasik biyoloji ve moleküler biyoloji arasında alışıldık bir indirgemedен bahsedilemeyeceğini, indirgeme tartışmasının başka bir düzlemde sürdürülmesi gerektiğini iddia eder.

Anahtar Kelimeler: indirgeme, indirgemecilik, varlık gerçekçiliği, olguların icadı, biyoloji felsefesi



to those who fight for humanity against dogmas even in the darkest of times

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CHAPTER I

INTRODUCTION

My main aim in this thesis is to provide a novel account of reduction by emphasizing the importance and role of instrument use in biology, and after criticizing several attempts of reduction in science made so far – which, as I claim, generally are based on a crude materialistic conception of science, and mainly investigated as logical structures – in order to make place for the analysis of instrumental power of sciences. Afterwards, I will defend a pragmatic¹ general perspective for reduction in science, rather than crude materialistic prose which is the main position while approaching reduction in science. In the end, I will place the perspectives that I have criticized on reduction in their correct places in which they are useful. Therefore, I have two main aims, first is to assert that the preexisting definitions of reduction in science lack a very important experimental component to depict science, secondly, after problematizing that, I will draw new criteria for reduction in science that involves a more general framework to understand “science in totality”.

For many, philosophy is a discipline which is occupied primarily with ideas. And philosophers engage phenomena in order to apply or create new ideas or relations between ideas. Whereas science is the study of the natural world with experimentation, observation, measurement, theorizing, speculating, approximations and tools of different caliber. It is hard to distinguish the two disciplines and also call all the practices they are involved *a priori*, since while there are philosophers who are engaged directly with natural phenomena or the results of scientific experiments, at the same time there are many scientists who work mainly on theoretical grounds without referring much to material scientific work. However,

¹ I use pragmatic in a vague sense here. I call my method pragmatic mainly within the context of pragmatic theory of truth.

when conceived in a scale from theoretical to practical² we can see that the majority of philosophers get closer to theoretical while the scientists tend to be closer to practical. In this distinction philosophers' hands are weaker than scientists', since while scientists can work on empirical field, philosophers are bounded by tools such as conceptual analysis, thought experiments and intuitions. The methodological differences among sciences and philosophy seem to create a barrier which is difficult to penetrate. Still it is possible for philosophers to *use* these experimental results, and many philosophers, especially naturalists are in a close relation with the results of recent scientific development.

This hard-to-make distinction between domains of philosophy and science sets a difficult stage for philosophy of science. However, indistinguishability of science from philosophy and vice versa also provides philosophers and scientists alike a more diverse (not to say, more difficult) domain to work on. Not surprisingly, many influential philosophers of science were either scientists as well, or they were related to science intimately. As a general framework, philosophy of science is *philosophical investigation* of science. This involves, but is not limited to, philosophizing about scientific practices, presuppositions, entities and theories in science.

Western philosophy is a discipline divided into several subgroups. While there are many issues about which different sub-disciplines intersect, the main schools of thought are analytical, continental and naturalist. One might divide them further indeed; however, this distinction serves my purpose here. The main groups that are involved in philosophy of science are analytical and naturalist philosophers. The dominant view in philosophy is to bind these two schools together. Some philosophers do not even make a distinction between the two, however this work implicitly relies on the distinction between them, on the basis of their relationship to practical and theoretical knowledge. It is also important to note that there are people from the school of continental philosophy who have also contributed to the development of philosophy of science, however the main discussions under philosophy of science are mainly derived from analytic and naturalist philosophers.

² My usage of practical versus theoretical distinguishes between abstract and concrete.

Reduction and reductionism in science are established, yet still frequently discussed topics of philosophy of science. Contributors to these discussions are also generally analytic and naturalist philosophers. I believe that this variation, whether the analysis on reduction is made from analytic or naturalist philosophers make a big distinction about the answer to the question “What is reduction?” I think that the answer to this question determines approaches to reduction and reductionism, and the differences between these approaches lead philosophers to analyses which are quite distinct from each other. The differences in the approaches towards reduction make philosophers’ analysis very different domains of reduction. Given that, reduction is not a monolithic act, but rather *a collection of certain ways of various changes*³ in science. This is, of course, a fierce battlefield like many other definitions in philosophy. Some philosophers might feel uneasy about my claim about the definition of reduction. Because many philosophers of science have tried to define reduction their own way, and such definitions included in and excluded from certain changes in science. Of course, discussions regarding *the essence* of reduction and reductionism is crucial, however, the problems that occur while establishing a framework to understand reduction have suffered from relying on too narrow definitions of the term.⁴ My aim is more simplistic in this sense. I will look for a more inclusive definition of reduction. My definition is simple; *whenever a scientific theory steps out of per-established boundaries of that theory and works within the domain of another scientific theory*, I will call it a reduction.

Van Riel (2014) makes the following remark on definition of reduction:

Assume that any candidate for a full-blown characterization comes in the following form:

x reduces to y iff xRy . This is what I will call a *definition*. For what follows, every bi-conditional connecting open sentences will be regarded as a definition. Let us talk about kinds of definitions in order to distinguish

³ One of the most heated topic for debates on reduction is on containing Kuhnian paradigmatic theory changes in reduction or not.

⁴ This claim is in parallel with my method of analysis of reduction.

between different interpretations of how *a* definition should be approached. There are basically three kinds of definitions philosophers have offered for the concept of reduction: *stipulating* a meaning for ‘reduction’, tying a characterization of the notion to an investigation of *alleged cases of reductions*, and, thus, taking an empirical dimension into account, and, thirdly and only implicitly, by way of *explication*. An explication can basically be conceived of as a procedure that tries to illuminate a term’s pre-theoretic use by (i) reflecting upon criteria of adequacy this use imposes on a definition, without relying on the assumption that we can come up with a full-blown conceptual analysis of that notion, and (ii) by *sharpening* that concept. (Van Riel, 2014, p. 20-21)

The reduced theory can be theory of another science, as in the example of sociobiology, or within the same science as in Nagel's example of reduction of classical thermodynamics to statistical mechanics, or a more general theory change within a science such as reduction of Mendelian gene to molecular biological theory of gene. I am aware that this definition makes it difficult to draw criteria to distinguish other forms of theory change, be it replacement or outright revolution in science. Such indistinguishability may be problematic in some philosophical accounts of philosophy of science. However, I aim to give a novel account about reduction in this general basis, and try to discuss reduction together with *actual scientific practices* and will try to reach a more general perspective. And this perspective is not contradictory with the pragmatic and naturalistic accounts of philosophy of science.⁵ In addition to that, giving a detailed account of reduction that can be put to work with the diverging sub disciplines of philosophy of science – spare the whole domain of philosophy – is an impossible task to achieve. Stated that, I should also add that my analysis is compatible with naturalistic philosophy at best. By naturalism put to work as such, I want to downplay the role of armchair

⁵ Here my definition of pragmatic is not limited to notion of “reduced, or principally reducible, yet can be used in explanations of concept/entity”. Such a view within reduction is the most widespread one. No one claims that, given that water is reduced to H₂O, the concept of water will cease to exist. Almost all philosophers agree that because of pragmatism such concepts will not be eliminated. For this account, see Nagel (1961).

philosophy and conceptual analysis in favor of analysis of practices and instruments in science.

My understanding of instrumental analysis in this thesis rests on two pillars. The first is *entity realism* of Ian Hacking, advocated primarily in his book *Representing and Intervening* (1984). Hacking's claim is simply put by himself; "If you can spray them, they are real!" He uses this to argue the existence of electrons, and uses it as a general criterion for reality of theoretical entities. I will take one step back from Hacking's stance and advocate that the *tools that enable us to intervene* are the tools that give us the *real* analysis of scientific practice, and defend that, the case of reduction of traditional biology to molecular biology (or even better, cases of reduction in every scientific discipline) can be explained best within this framework. In this way, singular entities, theories, properties, etc. put aside, we can have a more general understanding about the *direction* of reduction and what the future will bring to science. This is the perspective I believe that is lacking in analysis of reduction.⁶

The second pillar is casualist account of Nancy Cartwright, particularly in her *How the Laws of Physics Lie* (1983). In her book she argues in favor of models and generalizations that are in the domain of disciplines such as engineering, biology and lower level physics over the more general fundamental laws such as Newton's second law of motion. She defends the notion that, such generalizations have more explanatory power and they are closer to truth compared to fundamentalism. In her ontology, she advocates that casual powers of entities grant their reality, rather than their property of being building blocks of the universe. Hence, my plan is to re-baptize the instruments; which are the tools that we intervene the world with, as the *truth makers* gains a stronger position. I plan to categorize the problem of reductionism not as a problem of concepts, but as a result of change in the usage of instruments and ways of intervention in biology. Given that, the pre molecular classical biology was devoid of such instrumentation which

⁶ Here I do not claim that reduction must be analyzed solely on this basis, I merely advocate that the driving force, *the empirical engine* of sciences in general, and molecular biology in particular is this, and without further analysis of instruments in biology, it would not be possible to make a comprehensive account of reduction in this discipline.

would help us intervene with biological entities, it should not be expected from that science to make better explanations than explanations of molecular biology, leading to eventual reduction of classical biology to molecular biology.⁷

Van Riel, makes an important analysis about some of the major philosophers of reduction, and presents their characterization of reduction as follows;

Here are some of these informal or pre-theoretic characterizations, which will lead to criteria of adequacy an explication of the notion of reduction has to fulfill:

(Kim) If Xs are reduced, or reducible, to Ys, there are no Xs over and above Ys. (cf. Kim 2006, 275 f., (given in a similar fashion by Smart 1959))

(Wimsatt) Inter-level reductions are compositional. They localize, identify, and articulate mechanisms that explain upper level phenomena, relationships and entities. (Wimsatt 2006, 449)

(Chalmers) [W]hen [in the context of a reductive explanation, $R \vee R$] we give an appropriate account of lower-level processes, an explanation of the higher-level phenomenon falls out. (Chalmers 1996, 42)¹⁴

(Nagel-1) [In reductions, A] set of distinctive traits of some subject matter is assimilated to what is patently a set of quite dissimilar traits. (Nagel 1961, 339 f.)

(Nagel-2) [The reduced] science deals with macroscopic phenomena, while the [reducing] science postulates a microscopic constitution for those macroscopic processes. (Nagel 1961, 340)

(Sarkar) The reduced theory is explained by a reducing theory which is

⁷ I do not claim that in the days of classical biology we were never able to intervene with the biological entities, however those which we could use to intervene them were generally from the domain of *chemistry*.

presumed to be more fundamental. (Sarkar 1992, 167)

These descriptions are often enriched by examples, which supposedly describe *paradigmatic cases* of reduction: (*human*) *pain* reduces to *C-fiber stimulation*, or *water* reduces to *H₂O* (the latter can be found in (Putnam 1975, 431) (Fodor 1981, 150) and (Kim 1992, 23)).¹⁵ Talk about levels, part-whole relations and explanation suggests *directionality towards*, or *priority of* the reducing item. (van Riel, 2014, p. 29)

I believe that all these accounts of reduction have advantages of their own within a given framework, and comprehensive work is required to explain any reduction relation. This is like a division of labor in philosophy of science. Logical models provide one perception, semantic analysis provides another, metaphysics another and so on. My account about instruments provides an additional and a more general perspective for science, yet indeed it cannot answer all the puzzles of reduction.

Chapter II outlines the general stances towards reduction and examines several important schools and their definitions of scientific reduction as well as identifying what their terms refer to and how they provide a general picture of science. In this chapter I draw one of the most fundamental distinctions in my analysis, between reduction and reductionism.

Chapter III deals with the formal models of reduction, particularly Nagel's and rehabilitations of his model in order to analyze it further. In this analysis, I do not claim that I examine all formal models, however, I believe that Nagel's model provides a good example of the limits of explanation and the narrow scope of formal models of reduction. The reason why I choose Nagel is discussed in the mentioned chapter. Towards the end of the chapter I show that formal models, even though they always mention empirical parts of the explanation, lack a method to make empirical content fit in their model. And I try to show that all formal models already invite the empirical component, yet they do not put it in any form that is compatible with the logical structures. Formlessness of empirical component make the general picture of scientific reduction blur.

Chapter IV discusses the *idea of scientific reductionism*. While doing so I

employ Sarkar's (1998) criteria of assumptions in reductionist explanations. I criticize these criteria in order to give a more general account of reduction, trying to distinguish between various doctrines of reduction. Within these doctrines, I try to show that all of them are tainted with metaphysics to a certain extent. And these metaphysical components do not suit the methods of science. Instead, I believe that a framework which is closer to – or continuous with if possible – sciences would make a better account of reduction.

Chapter V defends instrumental realism as a useful tool for explanation of reduction in science and compares it with the metaphysical backgrounds of other doctrines towards reduction. Eventually this chapter tries to put forwards a point of view of philosophy of science which is more consistent with the methodology of science.

CHAPTER II

CATEGORIZATIONS

“Reductionism constitutes, therefore, what Dewey (1929) viewed as an adolescent quest for absolute certainty at the expense of complex understanding.”

(Overton, 2002, p. 29)

Any problem in philosophy has a multitude of different answers depending largely on the perspectives of different philosophers, reduction in science is no exception. Due to background beliefs and various different points of view, a fundamental problem of a philosopher is not even a problem at all for some other philosophers. The notion of reduction itself is a controversial issue, and not every theory change is considered as reduction. In this chapter, I will lay out two basic categorizations that are employed in reduction in science. The first one is about the methodology of reduction in science, and it involves the discussion of layers of analysis. Which object should be under discussion when we talk about reduction, entities, theories or methods in science? Or how shall we reduce? The second issue that I will address in this chapter is about approaches to understand reduction. This is about the discussion of referent of reduction. How shall we define reduction?

In this sense, two different questions about categorization of reduction refer to two distinct acts. The first one is about the objects of reduction in science. What is to be reduced? The answer to that question will determine what is the important aspect of *reality*, and it has to do with our stance towards realism in science. Put in this way, the first one is more of *scientists'* work, about what they see fundamental in their research. The second question addresses the approaches to issues of reduction in science. After answering the first one, the entities of reduction, we ask, which among these entities or methods are applied to which specific cases, and what shall we expect to see as the outcome of such attempts. In this sense, the second

question addresses *philosophers'* work in reduction in science.

2.1 Types of Reduction in Science

Most common way to divide substances of reduction to three is through this schema:

2.1.1 Ontological Reduction

Ontological reduction is a general perspective which sees the world as constituted of different levels of complexity. This point of view has a number of aggregate ideas. The defining character of ontological reduction is commitment to fundamental particles.⁸ The background idea behind ontological reduction is a certain form of physicalism, namely materialism. According to this view, the world is made up of some⁹ elementary particles. These elementary particles are parts of higher structures while they have no parts in themselves. The complex entities that we come up in the world, such as biological organisms, are certain arrangements of these elementary particles. Therefore, the upper level complex entities are in principle reducible to more fundamental particles, since they are made of such building blocks. This view was supported by Smart (1959), Dawkins (1976, 1996), Pinker (2002) and other crude materialists. Sachse (2007), argues that, we need ontological reduction in order to create a base for epistemological reduction. However, the need to ground epistemological reduction does not by itself guarantee ontological reduction. And it is a requirement only as long as we try to hold onto a larger picture of reality, that of crude materialism.

For the most part, crude ontological reductionists do not try to establish a legitimate ground for their framework, and I believe this is the main reason why most of “popular philosophy of science” books, which are generally written by

⁸ Most of the ontological reductions also have a key element of the structure in which these particles are related, however it is not a necessary ingredient of ontological reduction, for a more detailed account, look at Chapter IV.

⁹ “Some” but not “certain” particles, because the quest for the most fundamental hasn’t ended, and might never end.

people with scientific backgrounds more than philosophical, follow this path. They simply look at ongoing reductions in science and urge to find a ground to establish legitimacy of it without questioning their right to do so. Maybe this is due to what Wimsatt (2003) says;

Scientists rarely do foundational work, save when their house come down about their ears. (Philosophers like to mess around with foundations, but usually when working on someone else's discipline!) (Wimsatt, 2003, p. 54)

Thus, many ontological reductionists do not provide an adequate basis for their belief. They are in this sense, like Hacking's (1984) description of realists, who pound their fists on the table and insist that it is real.¹⁰

Agazzi (1991) calls ontological reductionism as a research program rather than a genuine perspective towards science. I agree with him on this ground, since ontological realism proposes us a complete picture of the world, yet it is not yet – if ever it will be – established. They come up and tell us that “this is how things are”, yet the reduction that is proposed is not completed as a whole. There are still many cases (and maybe these cases increase in time) that reduction cannot still account for. In this manner ontological reductionism is like Lakatosian research programmes, where foundations, *hard cores* are not questioned, but individual cases of reduction, *the protective belts*, go under charge.

2.1.2 Methodological Reduction

Realism, which was put forward explicitly by ontological reduction is implicit in this perspective. Methodological reduction presupposes that there are fundamental particles as well. And the best way to understand the complex structures of higher level entities is by going to the less complex, lower levels. Scientific realism is implicit rather than explicit in this perspective since even they presuppose that there

¹⁰ I will discuss this under materialism in chapter four in detail.

are levels which go from fundamental to complex entities, such a structure is not presupposed as the end goal of the reduction attempt. The legitimacy claim of their approach is provided from a different source than that of ontological reductionists, and this constitutes their main difference between ontological reductionists.

Methodological reduction, thus, is compatible with accounts of the world other than materialistic perspective. However, generally it is simply a matter of level of commitment to materialism that distinguishes between methodological and ontological reductionisms. The simple difference is, while ontological reductionism claims that everything other than fundamental particles are appearances, the methodological reductionist is not obliged to think of complex structures as appearances, however she still believes that they are best investigated at lower levels. In this manner, while ontological reductionism is obviously ontological and has a firm belief in the ontological research program, methodological reductionism is less committed to this research program, but still follows it due to its success. Moreover, ontological reduction sees theories, fields and other non-material entities in science inferior to the fundamental particles. Therefore, theories are only instruments for reduction. On the contrary for methodological reductionist, theories can have the same reality value as the entities, and even more, since methodological reductionism works in more pragmatic grounds, and a methodological reductionist can see reduction as a matter of explanation, not reality.

Methodological reduction is thus a hybrid kind. It is rather a strategy that covers variety of endeavors in science, but not all. In sciences, investigation without the general examination of the system is also a common practice.

2.1.3 Epistemic Reduction

The main difference between epistemic reduction and the preceding two is their objects to be reduced. While all ontological reductionists and most methodological reductionists ¹¹ think about entities of science, epistemic reduction puts

¹¹ Methodological reduction is an amorph form, therefore it is not easy to distinguish which form uses which background

representations on focus. This is to say that, they believe that their objects of inquiry are not the entities, but representations of those entities. Such representations are conceived best in theories. It is quite obvious that theories are representations of certain entities and processes. Laws, models and theories are thus, subjected to reduction in epistemic approach, while in ontological approach they are used as secondary means. The metaphysical basis for epistemic account vary from anti realism to relativism and instrumentalism. Thus, epistemic reduction enables plurality of approaches in the area and it makes place for itself in philosophical domain mainly by attacking ontological reduction.

In a sense, in epistemic reduction, reductionism has no contingent connection with reduction. Epistemic reduction does not necessarily have a general formulation or world picture. It simply accepts the fact that we cannot know it all, but we must be content with what we can know, namely representations. And because not theory by itself claims that the fundamental particles that it speaks about are final nor best in explanation, the doctrine and method are separated. Agazzi, in his paper *Reductionism as Negation of Scientific Spirit* (1991), tells us that ontological reductionism is contradictory to scientific *spirit*. What he means by “spirit” is the way sciences work. And he characterizes the scientific knowledge as follows;

1. Knowledge of natural objects does not aim at the penetration and grasping of their intrinsic “essence”, but is satisfied to study a certain number of their properties, which lend themselves to being isolated and relatively simply described along with the help of mathematical language...
2. Knowledge of the universal is not the *precondition* for knowledge of the particular, it is not the source from which one is able by deduction or application to expose the actual properties manifested by concrete events. Hence the search for what is common appears to be of secondary importance with respect to the exact determination of that which can be described in detail.
3. An adequate knowledge of the parts does not necessitate knowledge

of the whole. ... One began to think rather the contrary, namely that the understanding of an event is found in a *deconstruction* of the event, in exposing the parts from which it “results” and their way of functioning.(E. Agazzi, 1991, pp. 5-6)

Simply, Agazzi tells us how the sciences work, and how this working is contradictory to ontological account of reduction. Further on, he defends epistemic reduction this way:

Our last affirmation permits us to say that epistemological reductionism, while not supportable as an absolute position, has the right to be applied *at the interior* of every discipline, in the following sense. Every discipline has the right to strive to read according to its criteria of objection any dimension of the real: the result of this effort will always be positive, for it will lead us to discover an even more rich quantity of aspects, of constituents, of factors relevant to this discipline which are effectively *present in the world*. What is essential is that this unlimited effort not be transformed into the pretension of having thus (eliminated the differences of having endorsed the other points of view and fields of objectivation which derive therefrom. If one gets beyond this limit, one falls within the anti-scientific attitude we have already illustrated, and which consists exactly in the disregarding of the conditions of delimitation founding the very notion of scientific objectivity. (Agazzi, 1991, pp. 14-15)

Agazzi advises us that, epistemological reduction, even when it cannot be an absolute position, is the most convenient way that we can pursue, since when we step out of it to create a great picture in which all sciences are organized, we lack plurality and violate scientific spirit. This perspective also grants all sciences their autonomy without reference to any higher metaphysical commitment.

I believe that the difference between epistemic and ontological reduction

constitute a division of labor. While ontological reduction is a general research project, one that transcends borders of any science, and has big and general claims, epistemic reduction resembles a minor project, a patchwork of perspectives of different sciences. In this sense, I believe that they are inseparable when science is accounted as knot of multiple aspects. Some scientists in human genome project (HGP) had strict ontological reductionist beliefs. One of the most famous of them is James Watson, who has been the head of the project from 1990 to 1992. He did not intend to have human genome sequenced because of aesthetic reasons, he believed that this would make us understand human nature (Meyer & Znemier, 2016). The driving force of the project was the firm belief that there is one gene directly related to one protein, or even better, a trait in some cases. However, close to its completion we have learned that this was not the case. While projected number of genes were around 150.000 in a human genome, it was merely around 25.000. Of course, this has sparked controversy, and one gene – one protein view has lost its weight. After HGP has provided fruitful results, biologists turned to other factors of molecular mechanisms for further investigation in order to understand heredity and biology in general.

Ontological reduction is still a major stance in biology, however it is a very general project. It is so general that it cannot be tested at once, and might never be completed. It is also a way to see the world for scientists and non-scientist alike. Just as some biologists are vitalist or supervenionists, some are materialists, thus they are committed to ontological reduction. However, this does not mean that all they do is just trying to elaborate or verify ontological reduction. “Normal”¹² biology is generally occupied with cases with smaller scope than the project to understand how the world is in its totality. And people like Agazzi (1991), do not believe that there will ever be such a complete picture of the world, while epistemic reduction is a necessary operation in some cases in biological research. While a certain model interacts with another model of biology, to what extend are we going to accept this explanation? This falls in the domain of epistemic reduction, and it is the one used more frequently in day to day scientific work. This is why I call this a

¹² Normal in the sense of Kuhnian normal science.

division of labor.¹³ My position is a simple one in this respect. I believe that the distinctions in this part are never complete by themselves and scientists employ all of them in some part of their work. Some as research programmes as in the example of ontological reduction, and some as research tactics of abstract entities in theoretical level as in the example of epistemic reduction and some in more experimental manner where a plurality is necessary like methodological reduction. Now let us move to another level, where does a reduction start or end?

2.2 Kinds of Definitions

The previous discussion was more about the attitudes on reduction, and the general picture of the world. Therefore, the previous case was a more general framework in which reductionism is seen as a clash between metaphysical backgrounds, and reduction as a matter of digging frontiers between materialist and idealist monism and various forms of pluralism. It was therefore my starting point to discuss the ways to conceive reduction. Now I will discuss the different attitudes to define reduction. Of course, there are also backgrounds of philosophers carried into the arena in these distinctions as well, however this area is more operational than classification of methodology of reduction as in the previous part.

Van Riel (2014) makes a distinction between attempts to define reduction as *stipulation*, *study of alleged cases of reduction* and *explication*. These are distinctions of linguistic operations.

In his distinction, he favors the method of explication as the best candidate to define reduction. However, even though I agree with him that it is indeed of great importance to work on definitions and conceptual analyses, my main concern is more about *alleged cases of reduction* in his distinction. I believe that in every case there is an important component of conceptual analysis in reductionist cases, however, these differences are not contradictory, on the contrary, I believe that it is again, more like a division of labor between philosopher's attitudes. If I use Kant's account liberally, the empirical dimension without conceptual analysis is blind,

¹³ A following discussion about levels of analysis of reduction is in the 4th chapter.

conceptual analyses without empirical components are empty.

There are also different tendencies which favor stipulations, such as that of Nagel (1961), and Dizadji-Bahmani (2014). Stipulations are also of great importance in reduction *in certain contexts*. It is impossible to mark every theory change as a reduction, without setting certain limits. Since science is a very dynamic activity, and its terms usually have multiple meanings, uses and future prospects, stipulations can be used in some very certain cases, where the terms to be stipulated has been devoid of its meaning and its use has been reduced to a very limited domain. One model cannot fit all the sciences and all reductions. One can still argue that, when stipulation is not possible in the very first place, we should not look for reduction, and instead focus on other forms of theory change. I believe that, this narrows our scope of investigation and creates a too fragmented image of science for us to study.

I will instead turn to investigation of *alleged cases of reductions*, because it is a richer domain in which scientific activity can be seen with all its parts. Following Hacking (1984) and Galison (1987), I believe that there is more in science than theories and representations, therefore, I think that we must try to study reduction in philosophy of science with actual practice of science, with experiments, failures and future prospects.

In this chapter I tried to lay out the most common categories and definitions of reduction in science. My sketch is too short to investigate everything about reduction in detail, however, I believe that I have summarized all the main trends. All kinds of methodologies and definitions of reduction are of some use in certain contexts. However, sticking solely to one definition and ignoring others makes us unable to understand the richness of science itself.

CHAPTER III

FORMAL MODELS OF REDUCTION

“...science is a unity, [such] that all empirical statements can be expressed in a single language, all states of affairs are of one kind and are known by the same method.” (Carnap, 1934, 32)

It is quite a commonplace practice among philosophers to construct or reconstruct the logical structures of certain relations. In this example, it is the reconstruction of the reductive relationships. It is of course of great importance to provide formal models of reduction in order to clarify what the criteria for reduction are, what the entities, properties or theories are about, what the essences of such entities are, etc. I will try to give a general perspective on these models, starting with Nagel.¹⁴ The reason I choose Nagel as the starting point for formal models is that, he provides a quite general structure of reduction, and also my aim in the thesis is that the importance of empirical components in reductionist attempts can be demonstrated better on the skeleton that was built on Nagel's model. The arguments in this chapter are, therefore meaningful within the logical framework. As a result, they fall into ontological reduction category.

3.1 Replacement or Reduction?

Before starting with the investigation of formal models of reduction, I would like to emphasize the distinction between reduction and replacement in science, because in order to define reduction properly, one shall also define what reduction is not.

¹⁴ Of course, there could be other starting places to begin the discussion, especially *Unity of Science as a Working Hypothesis* (Oppenheim and Putnam 1958), or even before that Kemeny, J.G., and P. Oppenheim (1956), “On reduction”.

However, before beginning I should emphasize that the proper definition of reduction, even an important subject, is not easily achievable without creating certain conceptualizations that would leave some cases aside. While doing so, I will also set aside the discussions about the distinctions between *stipulations* and investigation of *empirical definitions of reduction* for now, and I will discuss these distinctions in chapter IV, mainly because I want to keep the discussion at the level of applicability and strength of formal models in reduction for this chapter.

Kemeny & Oppenheim (1956), already classify theory replacement within theory reduction. Schaffner, a student of Nagel, and a practicing M.D. at the same time, who had seen the difficulty of satisfying a reductionist model strictly in Nagelian sense, also advocated that we should follow the footsteps of actual theory succession (that is to say, the undergoing theory change in practice of the sciences), and provide a reductionist account accordingly. This will inevitably force us to expand the reductionist account to include theory corrections and replacements as well. My stance is similar to Schaffner's in this respect. I do not claim that it is impossible to make distinctions between various forms of theory succession, it is possible to find reductions and replacements quite distinct from each other, however there are also cases in which these changes are almost the same. And of course, if a philosopher's aim is to develop formal models for each, and these models explain the past ongoing of sciences or future predictions, this is a worthy effort, because such models can help us by being exemplar to future cases. Yet, if our aim is the same as Kemeny & Oppenheim (1956), as they noted in the introduction of their paper "On Reduction";

The label "reduction" has been applied to a certain type of progress in science. As this process has been the subject of much philosophical controversy, it is the task of the philosopher of science to give a rational reconstruction of the *essential features of reduction*. (Kemeny& Oppenheim, 1956, p. 1, my emphasis)

we should clarify what the essential features of reduction are and are not. I exclude Kuhnian revolutions, where the successive theories are said to be incommensurable.

These theories, being incommensurable themselves make it impossible to put them in any kind of reduction relation where the reduced entity or theory has to bear some relationship with the reducing one. However, I maintain that analysis of instruments used in science also provides insight for these revolutionary situations. Furthermore, I believe that most of the philosophers of science have pointed to cases of reduction when *they were ripe*, where reduction is a relation between very close disciplines, and the empirical basis and main meaning changes of the respective entities were already done, such as Nagel's example of reduction of thermodynamics to statistical mechanics, as these essential features. On the other hand, people outside the area of philosophy of science like Skinner, Pinker or Dawkins, tend to make bigger steps and understand reduction as a relation bridging distant sciences. However even though formal models are fundamental components of any sort of reductionist attempt, we cannot say it is the whole story, and in my opinion, it is only the tip of the iceberg. Wimsatt (1976b), questions the reductionist endeavors' vainness in the cases where the model does not *fit the reality* of the ongoing scientific work. Therefore, I leave the important task of clarifying the distinctions between theory reduction and replacement to other scholars, and as it will be seen later in this work, the topic I would like to discuss is more about the instruments used in science and the sciences that they rely on rather than that. My definition is clear cut, given that two theories relate to each other, and are commensurable, and both make explanations, I will include all the theory changes as reductions.

3.2 Formal Models

Philosophy of science, or to put in a more relaxed terminology philosophizing on science, has been an important aspect of philosophy which can be traced back to Aristotle, who is sometimes referred as the father of sciences. In the enlightenment, many philosophers had vivid images of science in their minds. Francis Bacon investigated the sciences of his day with rigor and it is impossible not to note Kant's attempt to put metaphysics in the “secure path of science”. However, the investigation of inner workings of sciences as the analysis of theories has been done

by the logical positivists¹⁵ for the first time.¹⁶

The founding fathers of modern philosophy of science – the logical positivists – were thinking in quite formal terms. What I mean by formal here is the general attitude to study meaning, be it as its truth value, consistency, etc. with its syntactic and semantic aspects, which is a shared value for all logical positivists. Today's philosophers of science owe them a lot, both for the topics that they studied and the topics that they did not. I am aware of the fact that, it is not an easy task to distinguish formal from non-formal, since many of the philosophers who have constructed logical models to explain phenomena and concepts, also adhere to elements that I call content. This distinction is not always that clear cut. The issues that I call formal are about *a priori* studies of relations between theories. The formal issues also have a greater generality than the content of reduction. Formal models provide us with blueprints that can be applied to any accused reduction. Within this framework, what I call formal is closer to syntax, semantics, logic and mathematics. What I call content is more about individual cases, or the generalizations that are made with from these cases. Content refers to a more general analysis which takes history, empirical findings, the knowledge of individual cases into account. That is the empirical study of the sciences and the relations found between them.

One of the central aims of logical positivists was to make philosophy scientific through the usage of a verifiable and cognitively meaningful language – which, for them was the language of logic. Debates on reductionism were – and for some analytical philosophers still are – also under this influence of logical form and structure for the most philosophers of science.¹⁷ Two important *not solely formal* reductionists are Wimsatt (1976b) and Schaffner (1976). Wimsatt (1976b) tells us that the result of this logical program in reductionism is epiphenomenal, and advises

¹⁵ Many philosophers also dealt with sciences of their own day, - perhaps most notably Descartes – however, their accounts were generally not too permissive to the sciences content when content is seen as the manifold of methods that science employs.

¹⁶ Again, the great exception is probably Francis Bacon, who did not share much with logical positivists who will investigate “his domain”.

¹⁷ It is important to note the generally overlooked fact that, even he had contributed greatly in logical terms, one of the champions of formal reductionism in science, Nagel, also provided some crucial *nonformal* content to the topic, see, Nagel, 1961.

us not to pursue a solely logical model of reduction. His objection is not only limited to that, he also argues that (also this is the reason why he reached the previous conclusion), the *actual scientific activity* is not reflected in such strict logical structures.

I have chosen Nagel as the example of logical formal models of reduction, of course he was not the first and obviously not the only one who constructed logical models. Yet he is the cornerstone of any argument in reduction in science, and discussions are generally around his model or the successors of his model. In addition to that, as explained by Sarkar (1998), his model is – at least to logical empiricists and their followers – simple in logical form, quite similar and applicable together with deductive-nomological model of explanation defended by the same logical empiricists, and the model's emphasis is more on epistemological rather than ontological concerns, which is again compatible with the logical empiricist spirit.

3.2.1 Ernest Nagel's Model

One of the most prominent formal models of reductionist scheme is constructed by Ernest Nagel, whose *The Structure of Science* (1961) provides a detailed analysis on the reduction of scientific theories. His analysis is quite comprehensive, yet he is remembered mostly for his logical model. Nagel was educated as a mathematician and a philosopher, and he did not participate in the actual scientific research of his day but investigated sciences and the criteria for those sciences, including reduction. In his *The Structure of Science*, he tries to capture the concept of reduction. His examples are quite general ones, and he provides a logical model of reduction while discussing the *historical contexts* when these reductions occurred and also empirical requirements of reduction. However even though he makes sense of the *accused reductions* in the historical context, he does not go deeper into analysis of the elements of reduction other than logical models. His work also included analysis of the scientific works *preceding his own day*, he tried to give an account of the future sciences following the “reductionist success” of the previous sciences, notably, statistical mechanics.

Some post-Nagelian philosophers used his logical schema as sole answer to

problems of reductionism with various modifications in his model. Nagel himself did not believe that he provided an overall formal system for reduction. “The following schematic catalogue of such groups of statements is not intended to be exhaustive, but to list the more important types of statements that are relevant to the present discussion” (Nagel, 1961, p. 345). Furthermore, Nagel has written on *nonformal* conditions of reduction in his work. These *nonformal* conditions were about content and much more liberal than what the logical positivists of the day called form. Yet, his model was asymmetrical, the formal issues were much more important and they were the main inquiries he questioned in order to give the general account for scientific reduction, while nonformal conditions – the empirical components in his model – were not worthy of much deeper analysis, and even in such cases that they are of worth, they are in so far as they have a relation with the logical model.

Nagel's model is quite like a pipe system in this sense. The pipes – logical models – determine how will the liquid flow and what the direction of the flow will be. However, it does not answer how fast the flow will be, how much pressure will be applied, what the viscosity will be or even, what will constitute as liquid. There is a category of reducing science and one for the reduced science in Nagel's model, however there is no distinction between the sciences. There is no special analysis for the change of context among sciences in his analysis. He puts forward a system that is hardly able to account for any content. In this sense, mathematical models have the same relationship with engineering. The same mathematical model might be used to represent various different substances in different branches of engineering. However, in each individual case, the thing that is to be done will be different. One can use the operation of derivation in any equation, yet the equation might be of density, velocity or data accumulation. I believe that logical model of Nagel is that of a mathematician. My general stance is quite to the contrary, and will be discussed further in the thesis. However, before moving on to things that are more fundamental for me, the formal models (which are, for most of the philosophers, the knot to be untied in order to settle the issues in scientific reduction) will be investigated.

3.2.2 Nagel's formal structure

Ernest Nagel (1961), argues that there are both formal and non-formal conditions for reduction. However, he has been seen as an important figure for his contributions to the formal structure. One should also note that the purpose of his main arguments is to counter what he calls “doctrine of emergentism” or “holism”. Therefore, his willingness to analyze the facts by their constituents has been the central piece of his discussion of reduction.

Before starting a general discussion on his logical structure, let us take a look at one of his prime examples in order to see what he seeks in a reduction. In this example, he claims that the Boyle-Charles' law was reduced to statistical mechanics.

But our final result is that the Boyle-Charles' law is a logical consequence of the principles of mechanics, when these are supplemented by a hypothesis about the molecular constitution of a gas, a statistical assumption concerning the motions of the molecules, and a postulate connecting the (experimental) notion of temperature with the mean kinetic energy of the molecules. (Nagel, 1961, p. 345)

We can see three main components here, for the Boyle-Charles' law to be the logical consequence of the principles of mechanics. First, he assumes that the logical formulation and derivation¹⁸ of statistical mechanics to a more empirical law is possible. Secondly, additional hypotheses should aid this derivation, in this example, about molecular constitution of a gas and assumptions about motions of molecules.¹⁹ Thirdly, the experimental notion of temperature should relate to kinetic energy of the molecules. This looks like a rather rich understanding of reduction, so why have many of his successors emphasized the logical structures, rather than,

¹⁸ Derivation means outright logical consequence for Nagel.

¹⁹ This assertion by itself means that derivation is not sufficient for reduction in the very first place.

say the experimental components which connect notions in order to suffice for reduction? I believe the answer is in Nagel's own model. Even he writes about other factors, he always mentions them *in so far as they interact with a logical or at least semantic model*. Nagel probably believed that the empirical sciences were too disorganized to provide us with a structure of how the empirical knowledge will fit in any model. He held that logic, or at least philosophy of language could provide a more structured framework.

He sees reduction relation primarily as a logical relation. As cited below, his analysis is on the level of sentences.

Most sciences will also contain statements that are certifiable as logically true, such as those of logic and mathematics. Even if we ignore these, we have identified four major classes of statements that may occur in a science S, whether or not any degree of autonomy is claimed for it relative to other special disciplines: (a) the theoretical postulates of S, the theorems derivable from them, and the coordinating definitions associated with theoretical notions in the postulates or theorems; (b) the experimental laws of S; (c) the observation statements of S; and (d) the borrowed laws of S. (Nagel, 1961, p. 349)

This outlook forces him to abstain from further analysis on the differences between the structure of such laws. He sees all such laws, be them borrowed or experimental, with the addition of observation statements as well, compatible with the logical structure of the theoretical laws. This is due to the fact that, he believes translatability of all statements of sciences to first order predicate logic, at least in principle.

Furthermore, he believes that theoretical laws, or theory sentences occupy the highest place among others, such as empirical and experimental laws. This means, he sees experimental laws as extensions of and derivations of theoretical

laws.²⁰ This is the result of overemphasis of theories. As a general schema it can be seen that he has a deductive model of sciences in his mind.

Let us suppose that S has a theory capable of explaining all the experimental laws of the science; and let us designate as the “theoretical expressions” of S the descriptive expressions employed in the theoretical postulates (exclusive of the coordinating definitions) and the theorems formally derivable from them. Many explications aim at specifying the meanings of expressions by way of theoretical ones; and we shall call the set P_2 of theoretical expressions needed for explicating in this way the maximum number of expressions in D, the “theoretical primitives” of S. (Nagel, 1961, p. 350-351)

This view of logical basis providing primary science is also discussed by Nagel in 1949 in his “*The meaning of reduction in the natural sciences.*” (Dizadji-Bahmani, 2011). As Dizadji-Bahmani stated, Nagel “sets out to use the relation of thermodynamics and statistical mechanics as a ‘basis for generalization’”.

Nagel calls the science which is the source of reducing theory as “primary science”, and the science of which theories are reduced as “secondary science”. These will be referred as T_1 and T_2 respectively. On his account, reducing theory T_1 should provide adequate logical basis for T_2 to be inferred.²¹ In addition to that, both of the theories should be formalized in first order logic, which is again a very high standard for theories of science. This is the “condition of derivability” for Nagel.

Obviously, after providing this form of reduction Nagel must have seen one of the major difficulties with his model, the difficulty of applicability of his model to the sciences of his day. In order to cope with this difficulty, he makes another

²⁰ I believe that it is not easy to argue for this, I will discuss it in more detail in Chapter V.

²¹ When it is put this way, the whole story of reduction seems to require containment of the reduced science by the reducing one. Derivation is very rare in sciences – if possible – and only could be done in “complete science”.

distinction. If it is the case that T_1 covers every term that is covered by T_2 then such derivation can be done easily. This is easy to accept in principle, since as long as *the same reduced theory* can be derived from the reducing theory, it can be adhered that both theories are *about the same entities*. And it was previously assumed by Nagel that there is one *more fundamental or primary* theory. Then it seems logical to explain the same entities within a theoretical framework which is more fundamental and has better explanatory power. This form of deduction is *homogeneous* in Nagel's terminology.²²

If terms of T_2 are not directly derivable from the terms of T_1 , then it is called “*inhomogenous*” (or later, *heterogeneous*). At first sight it might seem that Nagel's whole model might only be working when there is direct derivability. In order to make *heterogeneous* cases fit his model²³, Nagel proposes “bridge laws” or “connectibility assumptions”. In the literature, these bridge laws are generally referred to as “property identities”. (Dizadji-Bahmani, 2011)

3.2.3 Rehabilitations of Nagel's Formal Structures

As I have mentioned above, Nagel's formal model works well in what he calls homogeneous reductions, where the terms of the reduced science are included in the reducing science. Here I should emphasize again that Nagel (1961) talks in strictly first order logic when he talks about reduction.²⁴ However when this relation is not already set – which is the case in almost all accused reductions in sciences, as claimed by Wimsatt, (1976a) –, and also, when the actual accused reductions need other dimensions of analysis, many philosophers were not content with Nagel's model. Yet since many of formal models regarded it as a starting point, instead of outright disposition, several rehabilitations have been put forward. I do not intend to analyze this deeper, however before passing, I will note that the most

²² Derivation that is done here is again a strict logical derivation of first order logic, which is the logic of Nagel's day.

²³ That is without appealing to the option of making T_1 *contain* every term that T_2 contains, or make T_2 *leave* the terms that are not contained by T_1 .

²⁴ He even calls derivations as logical conclusions at times.

important changes have been made by Schaffner (1967), who, according to Wimsatt (1976a), makes change in theory to be reduced possible. In Nagel's model, we have a solid **T₁** which has clear cut definitions and certain laws. As stated by Nagel;

It is an obvious requirement that the axioms, special hypotheses, and experimental laws of the sciences involved in a reduction must be available as explicitly formulated statements, whose various constituent terms have meanings unambiguously fixed by codified rules of usage or by established procedures appropriate to each discipline. To the extent that this elementary requirement is not satisfied, it is hardly possible to decide with assurance whether one science (or branch of science) has in fact been reduced to another. (Nagel, 1961, p. 345)

It is obvious that **T₁** and **T₂** are complete sciences.²⁵ However, as Schaffner (1967) tells us, there are cases when the reduced theory, **T₂**, is not directly derivable from the reducing **T₁**. In those cases, Schaffner tells us that we can recreate a new theory, **T₂***. **T₂*** should be a similar theory to **T₂**, which will do the same explanations, have similar entities, and is of the same domain as **T₂**. However, our creation **T₂*** shall have terms which can be derived from those of **T₁**. This turn overrules the conditions of derivation given by Nagel, since for Nagel, the definitions should be arranged to the last detail in both the reducing and the reduced theories. However, Schaffner sees the difficulty of application of this model to the various alleged cases of reduction, and thus tries to bypass it with his own form of rehabilitation. In addition to simple replacement, in Schaffner's (1967) model, **T₂*** corrects the mistakes of **T₂** and also specializes it to the given domain. In this sense, Schaffner (1967), sees reduction as a more comprehensive act, which also involves theory refinement.²⁶

²⁵ Of course, I do not claim that Nagel argues the sciences that are involved in reduction are complete in the sense that, there will not be any change in their structure at all. But he tells us that this is the basic requirement of any reduction.

²⁶ It is also important to note that, his emphasis is on making it similar to the more fundamental **T₁**,

In his later work in 1969, Schaffner includes changes in T_1 as well. He does the same thing he did in 1967 to the reducing theory this time. A new T_1^* should be constructed, which is better at explaining the domain of T_1 and also of which entities are closer in meaning to that of T_2^* . And of course, this is not a one-time business. Same operations can go on more than twice, and eventually we might end up a T_2^{*****} reducing to a T_1^{*****} .

Wimsatt (1976b), provides us a good grasp on the subject about the modifications on Nagel's model by Schaffner:

What would a new view of inter-level reduction look like? Schaffner's later move (1969) in allowing modifications to T_1 in order to affect the reduction (Figure 1b) is a step towards the picture I would draw: *Theoretical conceptions of entities at different levels coevolve and are mutually elaborated* (particularly at places where they 'touch' – where we come closest to having inter-level translations) *under the pressure of one another* and 'outside' influences. (p. 231)

Here Wimsatt (1976b) identifies the relation between T_1 and T_2 as dialectical. He tells us that both theories change during the *process* of reduction. Identifying reduction as a process is quite contrary to the spirit of Nagelian model. Since for Nagel, reduction is a timeless issue. It is more like a mathematical operation than a scientific endeavor. Yet, his student Schaffner thinks of it as a process as held by Wimsatt.

However different they are, they all follow the same logical lead. In this sense, whether there are alterations in theory or not is of little importance. Both Nagel (1961) and Schaffner (1969) take empirical dimension into account, yet they do not give a structure of it. In addition to that, even with Schaffner's modifications of Nagel's model, it is still difficult to connect the two terms of different sciences. Since even though theories get more similar to each other by modifications, having

which is “truer”.

the same term in both theories is a hard task to achieve. Now I will turn back to Nagel, and his efforts to *bridge* the gap between the meanings of the terms in reduced and reducing sciences.

3.2.4 Bridge Laws

There has been a lot of fierce discussions on the content and limits of bridge laws, and it is not easy to settle such a big dispute, but we know for sure what they do, that is to say, their function. Their main function is to provide the property identities that would connect the two theories; reducing and reduced. According to Nagel (1961), bridge laws are laws that are not part of either reducing or reduced sciences. They are not hybrids yet they can work in his reduction scheme. These laws, clearly are not from the domain of both sciences.²⁷ He does not expect any further relation with the reduced or reducing theories, yet he tells us that they are substantial parts of any reduction. Nagel tells us that there are three possible ways to construct bridge laws. (1) *Direct logical connections*. These are basically logical definitions that help us to match an entity to another. “Superman is Clark Kent” is one such logical connection. (2) *Conventions*. In these sort of bridge laws, the meaning of the concept of a previously defined entity is changed through *assigning* another meaning for it. An example for that would be naming Mendelian units of inheritance as genes. (3) *Linkages through material or factual knowledge*. This class of bridge laws is provided through empirical components. In such laws, there are no analytical definitions of any kind. Van Riel (2014), argues that these are *a posteriori* relations. These are similar to correlations where there can be no easy way to define the related terms yet it is possible to show different solid definitions between them.

Bangu (2011) tells us that there is no consensus among the philosophers about the role these bridge laws play, and believes that bridge laws, which are seen as the apple of reductionists' eye are in fact quite strong tools for anti reductionists.²⁸

²⁷ If they were, they could not have escaped from the wrath of Nagel's all covering theories.

²⁸ There are of course many different interpretations of bridge laws, such as *nomic correlation*, *correlation*, etc., however, I believe that the identity relation is the most coherent one with the logicist spirit of Nagel, and such a requirement seems only normal given the fact that Nagel's theories

3.3 Critique of Formal Models

So far in this chapter I tried to sketch the general role and structure of the logical constructs to express the reduction relations. My sketch is far from complete, however, I believe that I have given the spirit and the lack of consensus in establishing a general structure for reduction. I believe that in some cases always, and in every case at certain levels, constructing such models is of great importance. However, the level of explanation I seek, is not within these discussions. Nevertheless, let us move onto the difficulties that such formal models have to face.

3.3.1 Critique of Nagel's formal structure

The difficulties this model has to face are immense. Even though Nagel started many discussions in reduction, he could end nothing. There have been many revisions for the Nagel's model by himself and from other philosophers, notably, Schaffner (1967, 1969, 1976). In addition to revisions in his logical model, alternative models have been proposed to explain reduction in science, most notably by Wimsatt (1976a, 1976b). I believe that this is in parallel with the demise of spirit the of logical empiricism. Many other logical positivist attempts have faced similar difficulties in 70s and later on (and some shared the same fate with Nagel).

I do not intend to go into detail about the formal structure of Nagel's model, since even if it is possible to find weaknesses in the logical structure of the model, I believe that the stronger arguments against Nagel are at the level of content and its relevance with science, what he calls *nonformal conditions of reduction*. In other words, what Nagel talked about but under emphasized.²⁹ I will go into detail on nonformal conditions, yet before that, I will briefly demonstrate some basic critics of his structure.

to be used in his model are theories expressible in first order logic, where the only way to provide such a connection is through identity relation. In addition to that, "the other means of connection" rather than direct derivation share the common enough ground to be called as bridge laws.

²⁹It is also strange that many of his successors have solely mentioned his emphasis on logical structure, which was not in great detail in his work either.

First class of objections attack the notion of meaning. In Nagel's (and after Schaffer's) account it seems possible to adjust the meanings of terms according to our need. Nagel tells us that, the meanings of terms should be as precise as possible as a prerequisite of reduction.³⁰ Nagel does not discuss the obstacles for such definitions are, or how they are established. This is rather a common assumption of logical empiricists. Feyerabend's argument of incommensurability on the other hand, is one of the biggest problems that Nagel's model has to overcome. (Feyerabend, 1965) This is an objection about meaning of scientific terms. Since Nagel's model sees the terms in a scientific theory as sentences in first order logic, for him it is quite normal to use them as context independent objects which have their meaning settled already. However, as Feyerabend tells us that the terms in a scientific theory generally have their meaning from the context in which they are evolved. The *mass* for Newton's theory had quite different implications than the *mass* in Einstein's theory, and they referred to different relations. In addition to that, the vocabulary of the sciences, most of the time, do not have dictionaries. That is to say, the scientist “make” the meaning of the terms they employ through usage of them in scientific works. Thus, context dependency of terms makes it quite difficult for them to be torn apart from the framework that they are in and be reduced to another theory.

Yet it is important to note that Feyerabend spoke from a different perspective, a point of view which advocates the changes that happen in sciences are *radical changes*. Therefore, it is easy to capture Feyerabend's claim of incommensurability within Nagel's framework. The distinction on meaning is a simple one, as seen in Feyerabend's objection to Nagel, whether the context in which the word is positioned defines its meaning, or meaning of a term is inherent to that term.

Therefore, Feyerabend and Kuhn and many more later on, the *revolutionaries*,³¹ and the logical positivist as the *reformers* of science had quite different attitudes towards philosophy of science.³² Both parties have also

³⁰ The complete list of required criteria must be met for reduction in science in Nagel's model.

³¹ While Nagel's book is called *The Structure of Science*, Kuhn's best seller is *The Structure of Scientific Revolutions*.

³² Here naming Quine as *the conservative* in this trilogy does not hurt the metaphor, since while he

investigated the other aspects of sciences that they are engaged with. In this manner, it is not possible to find any form of revolutionary change in Nagel's model. A Kuhnian reinterpretation of Nagel would lead us to this conclusion; To use Kuhnian terminology, while logical positivists were investigating of *normal science* for the most part, the revolutionaries investigated *revolution* in science. Of course, they also included other elements in their work as well, however for both parties, it was their own emphasis that mattered the more to the philosophy of science.

3.3.2 Homogeneous Reductions

Homogeneous reductions are types of reductions that are fairly easier to set up by connecting two concepts. Nagel (1961), makes the distinction as *homogeneous* vs. *heterogeneous* reduction. In this distinction, the reduced science is derived from the reducing one. This logical derivation is reconstruction of the reduced theory from the reducing theory, without relying to the reduced theory. I believe that this is not how the reductions – which are generally referred to as reductions in the commonsensical or, even philosophical way – take place. Reductions in molecular biology, genetics, chemistry, all had many different aspects which were not derived from the more fundamental theory. Nagel (1961) in another part of his work also argues that the borrowed laws of S (reducing science) are also fundamental components in reduction. I argued before that, for Nagel these borrowed laws are not derivable laws from the reducing theory itself. Therefore, it is not easy to claim that homogeneous reductions are likely to occur. And even in cases when the criteria of Nagel are fulfilled, I believe that they become unimportant reductions, not the ones that magnet the heated discussions upon, since derivability requires a strong relevance in the field in the very first place.

Now let us move on to heterogeneous cases, where direct derivation is not an option in the very first place, and which is the case in almost all reductions.

claims that partial change in science does not happen in logical positivists' terms, he also does not mention the possibility of scientific or epistemic revolutions in science.

3.3.3 Critique of bridge laws

Another problem of Nagel's model is the bridge laws, their structure, content and more importantly, their origin. Nagel (1961) tells us that their grounding can be made as conventions, empirical connections and identity definitions. However, even though it seems this is somehow *liberal* in his model, when formalism of the central discussions in his book are considered, the problem still stays the same. Even though the *theory correction* approach of Schaffner (1969) proposes closer connections between reducing and reduced theories, there is still a fundamental gap to be filled, because the theories of different sciences, even though they get closer to each other, can never meet.³³ The three types of bridge laws as well as their critique is as follows.

(I) Definitions; It seems rather easy to take a term from the science to be reduced, define it in the reducing science's terminology and make it easier to be derived from the reducing science. Because terms in science can be quite ambiguous, especially when it is the case that they are employed in different scientific disciplines. In order to achieve this, two strategies can be followed: a) The term to be reduced can be deprived of some properties of its meaning, such that one domain will be unable to use that term. I will call this *negative definition*. b) it is possible to define a term, which is used vaguely in the science to be reduced by making it mean exactly one thing. This I will call, *positive definition*.

With positive definition, there seems to be no problem to intervene the reduced science, since the term was vague already, and there could be no harm in clarifying its meaning.³⁴ However, the context of the term, even vague, sets certain limits. One example for this is the definition of *trait* in biology. Indeed, it is one of the hardest tasks of a biologist to give a clear definition of a trait, yet a lot of biologists use the notion of trait in their research. However, just because it is a vague

³³ When definitions are set in an idealistic realm such as the realm of mathematics, just as in Zeno's arrow paradox, we can see two definitions getting closer to each other by every succeeding definition, yet never being able to meet.

³⁴ Rigid definitions are what Nagel puts as stipulations for reduction in the very first place.

term, it was not easy (and still is not) for genetic reductionists to define trait in genetic terms, and when they try they are faced with quite fierce criticism from the vague users of the term, since when defined strictly in genetic terms, this would make it impossible to fulfill their requirement in, say, morphology. For a molecular biologist, a trait is an end product of cellular processes, which makes quantitative analysis hard to achieve, while for a morphologist, arm length could count as a trait.

Wimsatt (1976b), argues that the reduced term and the reducing term have an asymmetrical relationship in reduction.³⁵

Another suggestive asymmetry is given by considering when we are likely to decide that a term has changed its meaning. ... Thus if we discover that genes are composed of DNA, this is held to involve or lead to a meaning change in the concept of the gene. But if we discover that an atom of iron occurs in the heme group that associates with hemoglobin molecule, we do not see a meaning change for “iron.” We say that we have just learned something more that it can be used for. (Wimsatt, 1976b, p. 225)

This simply means that, there is a negative definition in the concept to be reduced. However, what happens to reduced concept is not a negative definition.³⁶ The problem with negative definition is in the same ballpark with the positive one, except that, it deprives the term of some of its meaning (I am with Wittgenstein's *Philosophical Investigations* (1951) “meaning is use” in here) more explicitly.

Nagel himself tells us that there must be a rigid definition of terms for sciences to operate properly;

It is, however, of utmost importance to note that expressions

³⁵ It can be argued that this is a problem in biology, however I believe that, the multiple layers of analysis in biology force us to accept it as it is.

³⁶ I believe that this is due to the strict definition of iron in chemistry. If we use our daily life definition of iron, there would be an incompatibility, since when a blacksmith talks about iron, it has the property of being solid, which is not easy to find in hemoglobin or blood.

belonging to a science possess meanings that are fixed by its own procedures of explication. In particular, expressions distinctive of a given science (such as the word 'temperature' as employed in the science of heat) are intelligible in terms of the rules or habits of usage of that branch of inquiry; and when those expressions are used in that branch of study, they must be understood in the senses associated with them in that branch, whether or not the science has been reduced to some other discipline. (Nagel, 1961, p. 352)

Before passing to discuss identities as the second way to build bridge laws, I will briefly mention that, such conventions might be the final result of reduction, rather than an initial condition. Different operational definitions can be used in science. That is to say that we can have a plurality of definitions of a term which has one name in a single science. We can rename them as “molecular definition of gene”, Mendelian definition of gene”, etc. However, this is troublesome since all definitions are candidates that can reduce the other concept. For example, some phenomenon of heritability might be reduced to both definitions of gene. I believe that Nagel thinks the primary theories are always unique, and there are no such pluralisms in the reducing theory, while there might be such relations in the reduced one.

Still, in the long run we can see that some concepts lose part of their meanings and eventually become conventional. Since what makes the meaning unconventional is the very fact that it has meanings – that is to say usages – that cannot be limited to the realm of T_1 .³⁷ When Leibniz (1689) objected the existence of vacuum, directly by saying “There is no vacuum”, he did so because the other theories (metaphysical and religious so to speak) he believed in were contradictory with the idea of physical vacuum. The term vacuum in his day meant obviously more than how mechanistic philosophy of Newton used it. However today, if you ask any school children, they would tell you that vacuum exists in the universe

³⁷ I assume here that different properties of an entity indicate that the uses of the term in T_2 are various.

pretty much as Newton defined it. Therefore, convention has been brought to the meaning of the term through elimination of other meanings in time, yielding a convention. I believe that this was due to unpracticality of religion and metaphysics of Leibniz. The other “properties” of the word vacuum have become completely alienated from the usage of Newton. Therefore, I think, the conventions, and definitions as such, are results of scientific reduction, rather than some goals to be stipulated in science. And even during the inquiry sciences provide new meanings for their terms, they are seldom final definitions.³⁸

(II) Identities; Identities are much stronger relationships than definitions, since they can be used in derivations in logic. In addition to that, because of their structure which is logical rather than scientific, they are easier to establish for the philosopher. These sorts of bridge laws are the most discussed ones, since majority of post-Nagelians are in pursuit of establishing logical models, and they tend to dismiss Nagel's every argument but the logical structure. (This is in fact, the main reason of why Nagel is remembered as if he had solely constructed a logical model.) There are mainly two objections to identity relations as bridge laws. First is transitivity objection. It is first put forward by Fodor (1974), who claims that the bridge laws should be laws of identity (in order to carry the reductionist programme's heavy claims), and those identity laws should not be contained in neither the reducing nor the reduced sciences. Natal & Del Pinal (2014), make a good point on directionality of reduction.

For instance, suppose that we want to show that a law $LP: P_1x \rightarrow P_2x$, expressed in the language of theory P , can be reduced to—that is, derived from—a law $LN: N_1x \rightarrow N_2x$, expressed in the language of theory N . (For the sake of simplicity, let us assume that the languages of the two theories do not overlap, i.e., that the predicates of P do not also belong to N , and vice versa.) What we need is a series of *bridge*

³⁸ For a detailed discussion on naming of entities in science, see, Ian Hacking (1984, Chapter 8)

laws, that is, principles that govern the translation of the relevant *P*-predicates into *N*-predicates:

$$(R1) P1x \leftrightarrow N1x$$

$$(R2) P2x \leftrightarrow N2x$$

How should the ' $\leftrightarrow$ ' connective be interpreted, in order for *R1* and *R2* to play their role in Nagelian reduction? (Natal & Del Pinal, 2014, p. 639)

This leaves, *directionality* of reduction unexplained. One can say that water is H₂O, and H₂O is water. And even more, the relationship of cause and effect, which is one of the most commonplace assumptions in science is left unwarranted (Fodor, 1974). Logical derivation alone, supported by identity relation, does not tell us what is the cause of the other. Van Riel (2014), holds that, the symmetry in identity relation is important to note. I agree with him, as he states, "If water reduces to H₂O, then H₂O does not reduce to water! Reduction is directional" (p. 42). This is explanation begging. Identity relationship alone cannot solve the problem of directionality itself.

Secondly, there is the objection of multiple realization. Frequently, we observe that the same effect can be brought up by different causes. Ordinarily, we expect the blood types to be determined by genotypes with A, B or O. One might have A or B allele or neither in his genotype. The phenotype of an individual with respect to ABO blood groups is determined by these factors. Normally, an individual with A blood type produces A antigens on the surface of their red blood cells, and his blood serum has antibodies against B antigen. When a red blood cell with B antigens gets in his system, the antibodies against B antigen interact with red blood cells with B antigen, forming blood clots, which are undesirable. This is why people with A blood type can not receive B type of blood to their system, and vice versa. These phenotypes are determined by the alleles in genotype, A, B or O. Both A and B antigens are produced from an H antigen. This H antigen is produced by another gene, which is H gene. Most of the people have dominant H genes in their genotypes, the majority of the people have homozygous dominant genotype while some have heterozygous. The rare instance is homozygous recessive genotype, hh. These people are of Bombay phenotype. Since they lack the precursor

for both A and B antigen synthesis, such people produce antibodies against both A and B antigens. Therefore, the blood type phenotype of an individual is multiply realized. A person can have a blood type O due to more than one gene. This is the general case in molecular biology, a result seldom occurs due to a single identity connection, and a result is generally a combination of various effects.

(III) Empirical component; This is a rather interesting relationship, since being against causalism and in favor of correlation is something inherent to the logical positivist spirit. This seems like a gap in Nagel's model, since given that the general interpretation of the main structure of model is completely formal, it would be hard to fit any empirical data in it, since empirical findings will hardly be in first order predicate logical form. However, this is a necessity in terms of scientific practice, and as I mentioned earlier, Nagel's original model does not only talk about logical structure, but also mentions the nonformal conditions of reduction as well. In Nagel's model, such empirical components are generally in the form of correlations.³⁹ The terms of sciences to be reduced do not have the identity relation with the terms of the reducing sciences, yet there is a fact we call reduction. Nathan and del Pinal (2015) sees the bridge laws quite empirical in their study on the progress of neuroscience. However, when we let empirical data in, in the form of bridge laws, the main idea of Nagel's model, derivation, becomes impossible. Any attempt to reconstruct and rehabilitate the bridge laws has to add empirical content in it. And as long as there is any sort of empirical content in a non-formalizable manner within the model, direct derivation is not possible, at least within the limits of first order logic.

3.4 Rehabilitations, where empirical component kicks in

Nagel's model was not comprehensive enough to satisfy everybody, albeit it was the basis of many similar models, it was rehabilitated and rearranged continuously,

³⁹ Here it is also important to note that the logical positivists tend to discuss “observation reports” rather than the “full house” of scientific activity.

most notably by Schaffner. Here I will look at some recent rehabilitations in order to find where and how they fill the gap of missing link in Nagel's model, which is for me, empirical components.

Needham (2010), tells us that the identity claim is a very strong claim which did not even exist as a necessary condition of reduction in Nagel's original work. And although bridge laws, that are not identity relationships are weaker in relating the theories, it is the only way we can apply them. He also advocates there is also at least an amount of change in both of the theories, reduced and reducing, rather than a direct derivation. He adds empirical component in the approximation level of scientific work. While it is not quite possible to derive a direct consequence and call it a reduction thereafter, in order to fit it to the reality, there has to be some approximations to modify the laws.

Nathan and del Pinal (2015) write about *associative laws*, which connect the terms of science to be reduced to the terms of the reducing science. Their examples are from neuroscience, and they advocate that the *associative bridge laws* are not identity relations, but a different kind of relation that binds two theories. They provide a model for connection, which is probabilistic and pragmatic in an empirical way. They tell us that the associative bridge laws can act upon also not only completely established sciences, but upon sciences which are quite open ended as well. (Their example is the neuroscience, as previously noted.) Here again, I believe that the content is fixed empirically, in addition to that they despise what they call *reductionist bridge laws* in favor of their associative bridge laws.

Bangu's (2011) criticism about bridge laws comes at several levels. The idealization problem; for the bridge laws to function, they need to be idealized and thus, getting its feet off from reality.⁴⁰ Secondly, he tells us that the selection of the correlations is not strictly determined, and the bridge laws are some kind of “portable bridges” that can be used to connect a variety of points to the fundamental theory. And thirdly, he tells us that there are emergent properties which can not be contained within the reducing theory and bridge law alike, since they get their

⁴⁰ Some may argue that this is how sciences work, however I believe that non-ideal conditions are what we have in sciences in almost all the cases. For a detailed account of idealization problem in science, see: Cartwright, *How the Laws of Physics Lie* (1983).

meaning only from terms of the theory that is being reduced. Bangu's claim does not involve any sort of empirical support for the empirical nature of bridge laws, however, his argumentation implicitly contains remarks on counter idealism and he despises of idealizations in this manner.

Of course, I do not claim that in literature philosophers, implicitly or explicitly, have to use empirical support in order to establish bridge laws. There has been another form of attempt, an example of which can be seen in Lewis (2002), where he uses necessary a posteriori sentences of Kripke in order to create a basis for bridge laws, however, the necessity of an expression does not stop it from being a posteriori. Lewis takes his examples from water/H₂O identity, and provides arguments primarily from the domain of pure philosophy rather than science.⁴¹ Lewis's argumentation is dependent on the identity feature of the necessary a posteriori arguments. This is distinct from the correlation/association relation that was mentioned before, which is used to provide the legitimate grounds for bridge laws. However, establishing a posteriori arguments is a difficult task – if possible – in the majority of the cases in reduction.

In conclusion to this chapter, I believe that there is no way to avoid some form of empirical content when it comes to the analysis of bridge laws in Nagel's model, because science is more than the theories in science. The model bears some problems other than bridge laws themselves, yet when they are put aside, there are still a great deal of difficulties and a lack of consensus in the bridge laws themselves. However, I do not claim that Nagel's model is invalid or inapplicable, it still can be used, and there have been a tremendous labor invested on the rehabilitations of his model, fixing the loopholes. Maybe not through identity or correlation, but in a manner that invites certain forms of empirical work in. Despite all, I believe that there is nothing wrong with his model, but it is not explanatory enough for accused reductions in science. As put by Sklar (provided by Dizadji-Bahmani);

If we look at *actual cases of reduction* in science, we find that there

⁴¹I do not intend to go into detail about the discussions concerning the legitimacy of a priori necessity arguments, hereby all I want to emphasize is that, being a posteriori calls for empirical validation.

is always a close relationship between reduced and reducing theory at the level of theoretical structure. (Sklar, 1993, 335, my emphasis)

My opinion of the subject, thus, is the one that calls for plurality in the area of bridge laws as well as methods for reduction. As it seems from the various different analyses of these bridge laws, the way that various philosophers identify and defend them have a great diversity. The apparent result from this analysis does not demonstrate that the motto to be taken in reduction is, *to each his own* yet, a general frame that can be used to put every type of reduction together and make a meaningful explanation seems quite unlikely in Nagel's model, or within the framework of formalism. The most abstract way to express the general formula, without the need of further rehabilitation was given by Nagel himself, who also added empirical components in his own model, however, he did not say much about how and in which form the experimentation and empirical findings locate themselves in his model. However, the logical modeling is the *final result* of reduction. If we limit ourselves to stipulations as the only phase of reduction, we can not focus on the great variety about the directions of sciences, missing a comprehensive understanding of them.

CHAPTER IV

COMPONENTS OF REDUCTIONISM AS A DOCTRINE

“Philosophers have taken the 'linguistic turn' and talk about relations between linguistic entities, whereas biologists are more frequently unabashed (or sometimes abashed) realists, and talk about mechanisms, causal relations and phenomena.” (Wimsatt, 1976a, p. 1)

Previous chapter was about the general formal structures of reduction. In this chapter I will try to give a more general account of *reductionism*. I make this distinction, since scientific and philosophical work are not homogeneous attempts to construct one single thing, and there are certain divisions of labor in these works. Therefore, the question; “Should there be a formal model for reduction?” is answered with “Yes”. While the question, “Should there be a single model to explain all *accused* reductions throughout history of science?” is answered with “No”.

In addition to that, investigation on the *doctrines of reductionism* and study of *actual workings in science* are crucial to create a general account of reduction. I believe that the formal models fill this gap between the doctrines and the actual scientific work. This chapter will focus on the general *attitudes* about reductionism. That's why I use *reductionism* more frequently in this chapter, since the general stance towards reduction also creates the other category; that is *reductionism*. This could be perceived as a discussion about the *ideas* about reduction, which are of great importance when it comes to discuss reduction in a more general framework, since the ideas determine the bigger picture in which the scientific and philosophical discourses lead us into.

4.1 Why is there a doctrine?

In the previous chapter, I have discussed the formal models of reduction. As mentioned, I believe that, following Wimsatt (1976b), the major problems in scientific reduction of any kind has seen not be solved with a purely logical structure. I believe that for two reasons; First, the objections that are against logical reductionist schemes come mainly from an empirical basis. And the empirical objections are not easily compatible with logical models, thus I believe that another level of analysis, further from logical structures, is needed. Second, the *use* of reduction has transcended the logical limits which has been proposed by Nagel and others.

By saying *reductionist doctrine*, I may create the impression that there is only one kind of such doctrine. However, as I try to show in this chapter, there are many different ways and aspects of reduction in sciences. The logical reductionists themselves put no emphasis on the relation between facts and their models and seldom directly investigate the role of actual scientific work in the *act of reduction*. I call this as an act, since I believe that there is more than construction of models in what we call reduction.⁴²

While calling these attempts as doctrines, I would like to create a general frame. There can be many distinctions between reductions, yet I would like to emphasize here *the ideas of reduction*, and the components of these ideas.⁴³ Since obviously, there are a multitude of approaches towards reduction, and the definition given in the beginning, “reducing the terms, theories, laws and objects of one science to another” is too simplistic to explain these different attitudes towards the subject. In this respect, this chapter is about how different philosophers and scientists see the reduction themselves, how they interrelate different stances

⁴² I liberalize the notion of reduction following Wimsatt (1976a, 1976b), since talking only about logical structures and models limits our perception of the real future of science.

⁴³ I follow Ian Hacking’s distinction of idea and thing here. For further investigation of the difference between ideas of things and things themselves, see: Hacking’s *Social Construction of What*, (1999).

towards reduction and what the common and differentiating components of their perspectives on the issue are. I believe that this is fundamental to the analysis of reductionism in science, since the ambiguity of the term reduction is a result of the multitude of the perspective on this topic.

4.1.1 Explanation in Reduction

The main distinction between the study of logical models of reduction and the study of other components of reductionist approaches relies on ontology/epistemology distinction to an extent. Those who would put a stronger emphasis on formal models are “idealists”. If one assumes that the fundamental reality relies on a certain science (which is generally physics in this respect), then it is impossible to talk about representations of those models, since the “idealist” will insist that the reality of subatomic particles are unshakable while the models to be constructed from our everyday experience or lesser sciences would not be that “real” and further from the ideal. However, here one must make the distinction between what is *real* and what is *true*.⁴⁴⁴⁵

What I understand from *true* is true within a given context. For example, it is true that Friday is the best friend of Robinson Crusoe in the island. However, we know that Friday is not *real*. We use real in contrast with fictional in this case. This is the main objection I make to idealists in this respect. An idealist can tell us that, the “real nature” of matter is not to be understood by common sense. A certain theory of science gives us the *reality* to believe in, while our models of daily experience do not. When I look at a glass and say it is empty, the stubborn idealist might come and say it is full of air instead, since air fills the vacuum. He would insist that my model of perception of daily life does not reflect reality while microscopic entities and processes do show us what is real, in this sense, he would

⁴⁴ I am well aware of myself, walking in a minefield while talking about these two concepts, which are the most fundamental elements of many philosophical discussions, however, for the time being, let me use them in a way that is defined by myself and in their most basic form.

⁴⁵ The primary aim of this distinction is to discriminate ideal from real.

deny that the subatomic particles are observable only through models.⁴⁶ And I am with Wittgenstein and Austin on this matter, believing that meaning is use. Austin (1979), puts more emphasis on our daily use of the words than that of experts. This makes concepts like truth and reality context dependent themselves. And it is not possible to give a singular account of both concepts.⁴⁷ This dichotomy also shows itself in the logical positivist tradition.

The logical positivists tried to construct logical models to *explain* things, and they took those *things* as “observation sentences”. Such observation statements are emphasized heavily in Nagel's model.

Every positive science contains a large class of singular statements that either formulate the outcome of observations on the subject matter regarded as the province of the science or describe the overt procedures instituted in conducting some actual inquiry within that discipline. We shall call such singular statements "observation statements," but with the understanding that in using this label we are not committed to any special psychological or philosophical theory as to what are the “real” data of observation. (Nagel, 1961, p. 348)

On the other hand, Hacking (1984), tells us that, the emphasis on observation sentences rather than real observations and experiments create a mistaken image of science. And from such an image, it is not easy to establish a comprehensive philosophy of science. I believe that this distinction was inevitable for logical positivists. Because the raw empirical methods and the complex processes of science were not quite easy to translate into a logical form. Therefore, they took them as already established observation sentences. Logical positivists have looked at science, and told the scientists “Okay, do whatever you do, just give us a report

⁴⁶ This is a very long debate, and it is argued that the debate is between realists and instrumentalists, however this is a debate between *some* realists and *some* instrumentalist, one can still retain a realist stance, and accept that the subatomic particles and daily life experiences can be real simultaneously.

⁴⁷ As a matter of fact, no concepts at all.

we can understand in the end.” This created a magnificent loss of content in science, however, at the same time it made it much easier to be modified and worked on. The same problem can be seen in Nagel, who also discusses the possibility of empirical components of reductions in his model.⁴⁸ However, this leaves a gigantic area of study for his followers and critics alike, since the empirical part of the *act of reduction* is at least as important as the logical part.

Therefore, my scheme of conceptualization is primarily on the distinction between *what is real and what is true*. I believe that the logical positivists have saved what is true for the cost of what is real.⁴⁹ I agree with Hacking (1984), when he tells us that he believes philosophy of science has lost its focus since philosophy of science generally talked about scientific theories but not about *science itself*.⁵⁰ Indeed, one still can claim that the role of philosophers is to make conceptual analysis within a given framework, and not to intervene with the doings of sciences. However, I believe that without any general attempt to grasp what the sciences actually do, and simply waiting for “observation statements” to fill into the logical models and then to analyze the resultant scheme leaves us quite shorthanded to understand what is *real*.

4.2 Sarkar's Categories; the Elements of Reductionist doctrine

Sahotra Sarkar (1998), argues about reductive explanation under his definition of *substantive issues* mainly. My formal/empirical distinction owes a great deal to him and his distinction marks the same divide in Nagel (1961), in the shape of formal/nonformal. Sarkar also emphasizes the importance of making a distinction between ontology and epistemology of reductionist explanations, and he speaks in favor of epistemological reductions. He advocates that what we deal with when

⁴⁸ When empirical work is seen as not only observation reports, the whole process of experimental methods includes but not limited to, experimenting, stabilizing, phenomena identifying, making mathematical approximations.

⁴⁹ This is why most of their critics, e.g. critics of Nagel, focused their criticisms on Nagel's model's inability to show *actual workings of science*.

⁵⁰ The kernels of the same criticism can be found in Schaffner (1967), Wimsatt (1976), Sarkar (1998) and many others.

talking about reductions is not “things proper” but the representations of some entities, forces or alike.

The same attitude can be seen in Cartwright's book *How the Laws of Physics Lie* (1983), in which she tells us that we have better ground of belief for lower level laws, models, generalities and special sciences than the laws and entities of higher sciences. In this case, the reductionist attitudes are also attempts to establish relations between representations. I will assume this while I write about, at least as the traditional view of reduction. Now we move on to talk about *reductive explanation* (that is the explanation that is employed in reduction in science) rather than reduction as a whole.

Sarkar gives us three substantive criteria for reductionist explanation:

- I. *Fundamentalism*: the explanation of a feature of a system invokes factors from a different realm (from that of the system, as represented) and the feature to be explained is a result only of the rules operative in that realm.
- II. *Abstract hierarchy*: the representation of the system has an explicit hierarchical organization, with the hierarchy constructed according to some independent criterion (this is, independent of the particular putative explanation), and the explanatory factors refer only to properties of entities at lower levels of hierarchy.
- III. *Spatial Hierarchy*: the hierarchical structure referred to in (II) is a hierarchy in physical space; that is, entities at lower levels of the hierarchy are spatial parts of entities at higher levels of the hierarchy. The independent criterion invoked in now becomes spatial containment. (Sarkar, 1998, p. 43-44)

These three criteria are interesting to organize the attitudes of reductionisms.⁵¹ I believe that these criteria can be elaborated and criticized. For example, I find the

⁵¹ Reductionism in this sense is in much closer relationship with “the ideal”, explanation in science, rather than any other component of science.

third one only as an expansion of the second and the second criterion can be expanded on other dimensions as well. However, the general crude materialistic outlook towards science makes the third criterion perhaps the hottest topic. In the following part I will reevaluate Sarkar's criteria.

4.2.1 Fundamentalism

I believe that fundamentalism is a necessary criterion to be fulfilled for any sort of reductionist explanation, since the reducing theory-reduced theory relation has to be found upon such a basis. In this sense, fundamentalism is *the essence of reduction in science*. Without adhering to a theory more fundamental than the other, it would eventually lead us to a non-reducing, patchy picture of sciences, where each science is as good as any other and all of them are domain specific, where there can only be theory succession relationship through replacement.⁵²

Even though, some philosophers including Nagel (1961) tell us that the relation between reducing and reduced theory can be bi-conditionals, and such bi-conditionals are employed especially in bridge laws. I think this is a rather false picture of reductionist approaches, since although two theories or terms, etc. are connected to each other, the reduction goes only towards one direction, towards the direction of the reducing theory. In its logical formula, this is similar to mathematical operations. $2 + 2 = 4$ is an equation, if one is given 4, he can write it as $2 + 2$ as well, yet $3 + 1 = 4$ at the same time. However, the general stance is towards the result, the simpler, more unifying principle. Therefore, reductions are directional since it is the only way that they can be useful.

It would also be interesting to note a quote from Duhem here;

...If the science of motion ceases to be the first of the physical sciences in logical order, and becomes just a particular case of a more general science including in its formulas all the changes of bodies, the temptation will be less, we think, to reduce all physical

⁵² Compare this with *directionality* objection that I make in previous chapter on bridge laws 3.3.3.

phenomena to the study of motion. It will be better understood that change of position in space is not a more simple change than change of temperature or of any other physical quality. It will then be easier to get away from what has hitherto been the most dangerous stumbling block of theoretical physics, the search for a mechanical explanation of the universe. (Duhem, 1894, p. 285)

This passage has the most explicit definition of fundamentalism. After Duhem, as he expected, *the science of motion* ceased to be the first of the physical sciences, and was replaced by *more fundamental* sciences within physics.⁵³ In Duhem we can see that search for such a fundamental science has an old history.

In Sarkar's (1998) further classification, any kind of reductionist explanation has to use fundamentalism as a core component as well. His example on this part is “the attempt to explain phenotypic features of an organism from a genetic basis using the properties of heritability.” (Sarkar, 1998, p. 44) This was the attempt of population geneticists in 40s. And it involves a commitment to one gene-one trait idea. This basically means that a single gene of the organism is the direct cause of a given trait.⁵⁴

In addition, it should be borne in mind that fundamentalism is the relation between only two theories. For example, while biology can be seen as the fundamental theory when it comes to relation between biology and psychology, the same science, biology, is not the fundamental one between the relations of chemistry and psychology. Therefore, fundamentalism is the necessary condition of reduction, however, by itself, it can not give the whole picture of the sciences. I believe that there are many reductions of this kind. People who study only the relations between two domains may not be involved in the general picture of sciences. Now let us move on to the ones who would like to have a more general picture.

⁵³ Here I do not want to further discuss quantum mechanics being another form of “science of motion”, all I want to point out is the difference we observe in the fundamental theories.

⁵⁴ This approach has been largely abandoned, however, there are still many other fundamentalist approaches to reduction and this is not limited to the scope of genetics neither to that of biology.

4.2.2 Abstract Hierarchy

As defined by Sarkar, the representation of the system this time has strict hierarchical organization. I believe that this is one of the main components of most forms of reduction. His example of criteria I and II fulfilled is the relationship between genotype and phenotype. “The set of alleles and loci form a hierarchically structured genotype. The rules of genetics are assumed to be more fundamental than those governing the phenotype” (Sarkar, 1998, p. 44). The second sentence is the fundamentalism criterion, and the first sentence is assumed to be the criterion of abstract hierarchy. However, one may say that the first criterion alone can explain the whole reduction in genetics in this manner. I believe that the main difference to make here between the two criteria is the *structure* that Sarkar talks about.

In traditional one gene-one trait approach, only the rule of fundamentalism is employed. However, a structural genome has *other rules* than simply emphasizing the importance of rules in genotype over phenotype. These rules include for example; linkage of genes which is a direct contradiction to Mendelian independent assortment principle, and it states that some genes are connected to each other in the genome, and thus are likely to be inherited together. The alien element in this point of view is that it does not tell us how these rules are assembled, applied to phenotype or how they are related to the laws of genotype. This does not mean that we do not know about them. However, they are not *within* the reducing nor the reduced theory. This is a source of disunity in reductionism, however, for the time being, the important thing to note is, abstract hierarchy criterion presupposes a structure in reduction of which rules can not be directly reduced.

Adaptationism discussion in philosophy of biology is similar in this respect. While adaptationists try to explain most of the scenario in evolutionary history of organisms by emphasizing the role of natural selection and gene's eye view of Dawkins (1979). However, as Gould and Lewontin (1976) state, there are almost always other factors that shape a structure, other than the main force – natural selection in this instance – employed. Gould & Lewontin give the example of environmental factors as “alien factors” that should be considered as well, besides

the force of natural selection acting on organisms.

4.3 An Elaboration of Criteria for Reductionist Explanations

As mentioned earlier, I see spatial hierarchy as a contingent extension of abstract hierarchy. However, the story is not that simple. I believe that what Sarkar claims with his identification of abstract hierarchy is the invitation of other rules or laws either within the reducing science or ad hoc generalizations. Later on, he also states that spatial hierarchy becomes spatial containment in this version of reductionist criterion. However, as noted before, I think the main element in abstract hierarchy is the employment of rules and laws other than that of reducing science. Spatiality, even an important component of various reductionist programmes, is rather different. Of course, it is impossible to state that spatial components call for application of other laws or rules.

To exemplify it briefly, we should look at an example in molecular biology. In Mendelian genetics, the factors of heritability were the “units”, or “factors” of inheritance in Mendelian vocabulary. Today we call similar entities as genes.⁵⁵ One of the three famous principles of inheritance of Mendel was the principle of independent assortment. He postulated this principle, because he observed that the phenotypic features – the only features in his day to make any claims about “genotype” – in offsprings were carried from their parents independently. In his dihybrid cross, he matched a yellow (dominant), round shaped (dominant) pure breeding (meaning that the genotype was homogeneous) pea with a green (recessive), wrinkled, pure breeding pea. The result was yellow, round shaped peas. (Which is the expected result since one parent had the genotype AABB and the other was aabb, producing an offspring with AaBb genotype.) Then the offspring is self-pollinated to give F₂ generation. The result was, in statistical terms; 9:3:3:1. 9 individuals both yellow and round, 3 individuals green and round, 3 individuals yellow and wrinkled, and 1 individual both green and wrinkled. The conclusion he

⁵⁵ There is also a heated debate whether genes are the same things as the modern gene, here my account is mainly functional, what I try to define is “that which is inherited and is the cause of phenotype”.

drew from this experiment was, the traits greenness and roundness are being carried in different “units” since the statistical investigation of the results have showed that result.

However, we have seen with further analysis that this law is not applicable to all units of inheritance. In many organisms, at least some traits were not inherited independently, their inheritance was somehow linked. This led biology to develop linkage analysis. This analysis consisted of finding the linkage ratio of various units of inheritance (now they were genes), and it produced ratios of linkage which varies between 0 to 1. Some genes were completely linked to each other while some had no linkage whatsoever, and many others were in between. This created a puzzle for biology to solve, and the solution was provided by chromosomes, the structural carriers of genes. As Sarkar noted (1998, p. 44), this led us to the spatial expansion of the abstract hierarchy. Linkage without chromosomes – that is function without structure in this example – creates an abstract hierarchy while the function, linkage, is connected with a spatial structure, chromosome, it became spatial hierarchy.⁵⁶

I believe that the assumptions that are made about reduction require a *metaphysical commitment*, and such commitments are abundant in various reductionist schools. Thus I believe that the abstract hierarchy that is put forward by Sarkar needs to have sources for these *other laws*. Physical space and spatial hierarchy, being the most common attempt to construct this hierarchy is common because of commitment to materialism. Thus, my formulation about reductionism follows this scheme; fundamentalism plus metaphysical commitment. As a matter of fact, fundamentalism is what makes reductionism possible, and metaphysical commitment in this respect, determines the directionality of reductionist approaches.

4.3.1 Smart and the Materialist account of Reductionism

Materialist account of reductionism is the doctrine which is used frequently in a pejorative sense by anti-reductionists. This materialistic account can be found in

⁵⁶ Sarkar makes a more general analysis of what he calls Genetic Reductionism, which is the combination of fundamentalism and abstract hierarchy criteria, and adds several laws regarding to the abstract hierarchy in genetics, however, the essence of the claim, I presume, is the same.

Pinker (2002), Dawkins (1973, 1996), Smart (1959) and many other infamously called reductionists. I have chosen Smart as the main culprit because he is the one who openly states his position and also explains it in a greater detail compared to reliance on self-evidence by Dawkins or Pinker.⁵⁷

J.J.C. Smart, in his book *Philosophy and Scientific Realism* (1959), states his meta-perspective quite openly. He is a prominent advocate of reductionism, and argues for interdisciplinary reduction, especially reduction of biology to chemistry. He is a physicalist and he believes that while microscopic entities are real, other components of theories, such as lines of force are not. I prefer to call him a *materialist* since his main stance towards scientific entities, properties and forces are concentrated on the reality of material entities. In addition to that he does not place forces or properties on the same category with the entities. He states that, “The concept of a line of electric force is a useful theoretical fiction ...” (p. 34), while he welcomes the entities of such theories as the ultimate reality. “Now I wish to say that lines of force, unlike electrons *are* theoretical fictions. I wish to say that this table is composed of electrons, etc., just as this wall is composed of bricks” (p. 34). His perspective on reductionism applies to various topics. Be it philosophy of mind, biology, psychology etc., he always makes a clear stance that the microscopic entities of science provide us a better ground of belief. According to him, what is unchanging and true is the fundamental particles. He defends the following perspective:

Indeed, I would wish to go further than merely to defend the physicalist's picture of the world as an ontologically respectable one, I would wish to urge that the physicalist's language gives us a truer picture of the world than does the language of ordinary common sense. (Smart, 1959, p. 47)

This account clearly downplays the role of common sense and the formulations

⁵⁷ As a matter of fact, scientific realism in the most general sense is highly associated with materialism, while this relation is common, materialism is not a core component of scientific realism.

made via common sense. In his perspective, the building blocks of the universe are much better at explanation than our everyday objects. If something is explained by nervous system it is better than a solely psychological explanation, since the previous one has the secrets of the building blocks of the universe while the later hasn't. This perspective, when applied to scientific reduction, creates the form of reductionism I call the *materialistic account of reductionism*.

In the following parts of *Philosophy and Scientific Realism*, Smart further argues that the knowledge of building blocks provides us with an understanding of the universe which is much better than any other form of understanding. He is also against any form of emergence and he advocates that spatially lower level entities are better at explanation. However, this stance is not a strong one. Several objections can be raised. One is that while he believes that “the lower level the better”, he still advocates the power of chemistry and chemical objects in biological explanations, while spatially, there are still even lower level entities, e.g.; the entities of physics like subatomic particles. Yet, one can say that the difficulty in explanation is simply a matter of complexity, and even we need to use the entities of physics in order to make the excellent explanations, well defined chemical entities on physical ones can do the same work. This objection is a weak one. Wimsatt (1976b) faces this challenge already; he tells us that this doctrine leads us to “reduce everything to the most basic things, that is nothing”, since it is convenient to assume that the search for the *ultimate particle* in physics will not come to an end easily. The smaller spatial entities led us to even smaller ones. When Millikan tried to detect electrons, he was looking for a fundamental particle, now the particle physicists are looking for quarks by using electrons, and as long as we do not claim that the search for a final particle will find one single fundamental particle like Democritian atom, it is impossible to find the final and best explanation.

Secondly, even if we assume that the search for the final particles – even never ending – is still useful, and the current smallest particle has the strongest explanatory power within, this stance can still be objected. One objection is, while the lower level particles or entities are being newly discovered, they lack the *operational* power to explain. That is to say that even we know that there exist smaller, and in Smart's sense, also more fundamental particles, we might not know

how to use them in higher level explanations. Modern chemistry is a great example for this. Until electrons, or corpuscles as named back then, were discovered by J.J. Thompson in 1897, atoms were the spatially smallest, and thus the most fundamental particles that were known. No subatomic particle was known and the concept of atom did not mean much. Atom was used mainly in calculation of quantity of various chemical substances from the time of scientists such as Dalton and Avogadro. Thus, the behaviors of subatomic particles, nor the individual atoms as wholes were of no use. However, scientists knew a lot about behavior of various substances, or in this example, elements. There were laws of chemistry such as the law of multiple proportions. Microbiology of 19th century was largely blind to – if not completely ignorant about – developments in atomic theory, yet they have achieved great things. Great Pasteur – maybe the first and foremost reductionist in biology – made enormous contributions to biology by employing chemical and biochemical agents in his works. He controlled behavior of biological entities by modifying their physio-chemical environments. He has observed that enantiomer crystals in tartaric acid had different properties of reflecting light. And he passed away before electrons were discovered. With the assumption of “the more fundamental the better explanation”, we should conclude that he couldn't have gone this far! He used no subatomic particle, no atom, no smaller entity of his day, however, he made excellent contributions to science. And until much later, subatomic particles waited to be put to work to explain some of the biochemical processes.

Even if atoms were employed in explanation of biological phenomena back then, this would be a *wrong* picture of the atoms, or more fundamental particles. Today there is no working chemist who holds plum pudding, Rutherford or Bohr atom models as true depictions of the atom. If Smart's reductionism was held, we had to rely on even the most basic formulation of atoms instead of the most advanced chemistry of the day. However, this is not what *science does* and why science or any serious reductionist attempt does not employ this stance is more of a pragmatic concern. Scientists generally try the best explanation they have without such materialistic commitments. This is why Sarkar (1998) states that the classical genetics, linkage analysis and segregation were employed so commonly even when

we knew a lot about the structure of chromosome and structural molecular biology. This leads me to the second objection, which is made by Wimsatt (1976b).

All (or most) of the explanatory power of the lower level description is 'screened off' (Salmon, 1971, p. 55 but see Appendix below) by the success of the macro-regularity. The situation is different however for cases which are anomalies for or exceptions to the upper level regularities. Since an anomaly does not meet the macro regularity, the macro-regularity *cannot* 'screen off' the micro-level variables. If the class of macro-level cases within which exceptions occur is significantly non-homogeneous when described in micro-level terms, *then* going to a lower-level description can be significantly explanatory, in that it may be possible to find a micro-level description partitioning the cases into exceptional and non-exceptional ones at the macro-level." (p.10)

Here Wimsatt makes another objection to building blocks view of the world. His main objection is about explanatory power of the lower level. As he states, since the lower level regularities might be screened off in higher levels, it might not be a good idea to use them in explanations always. One might still hold the claim that lower level regularities can be seen as some kind of sum of total forces acting on an entity in classical mechanics. This point is made by J.S Mill as the composition of causes, which is today called as *residualism*.

This difference between the case in which the joint effect of causes is the sum of their separate effects, and the case in which it is heterogeneous to them; between laws which work together without alteration, and laws which, when called upon to work together, cease and give place to others; is one of the fundamental distinctions in nature. The former case, that of the Composition of Causes, is the general one; the other is always special and exceptional. (J. S. Mill, 1843).

Put bluntly; we know one of the basic laws in classical mechanics; $\mathbf{F}=\mathbf{m.a}$, however in order to find the efficient force in a system we add and subtract various forces that act on the entity in order to find the resulting force, and the motion of the entity we deal with. However, there are two objections to this stance as well. I used the ignorance of the fundamental argument previously as well. Since it is not easy to assume that we know everything possible about the behavior of the lower level entities, and not even claim that we know them all, this analysis will always be incomplete. We might still find a new property, behavior or even a new entity while we deal with the fundamental particles. Therefore, this claim never gives a complete picture of the explanation in higher levels. We might know a lot about molecular genetics, but in principle, since our knowledge can never be complete in this domain, this claim is a weak one. Instead, I believe that the usage of such laws and entities is much profound. Although never complete, we can know that our reductionist knowledge is sufficient when we can intervene higher level organizations from the lower level entities, this is the main stance of Hacking (1984), that I will elaborate in the following chapter.

Second objection to the compositionalist view is; even if we skip the particles and try to establish a theory centered point of view towards science, as it seems, there are still revolutions in sciences in Kuhnian sense. This is a quite strong objection against foundationalism towards theories. What we have observed by these revolutions is the unchanging character of higher level explanations. Be it plum pudding or Bohr atom model, the behavior of oxygen meant basically the same thing to a systems biologist.⁵⁸ All that mattered to some biologists was the effect of oxygen in the development of organisms, and its usage in respiration. Therefore, I believe that composition of causes is a useful approach, yet it does not always provide the answer due to reasons I listed above. In some sciences, such as today's molecular biology, the majority of phenomena within the discipline can be seen at fundamental theory's level. However, this does not give us the final answer

⁵⁸ Of course, when dealing with molecular biology, in some researches the quantum model of atom is of tremendous help, in the first half of 20th century these changes in the atomic structure did not mean anything to biology.

to every question in the domain of biology. Therefore, compositionism is a possible, yet not always favorable attitude in reduction in science.

Now let me briefly sketch the materialist reductionism. It is assumed that the world is made of building blocks. These building blocks are fundamental particles. These fundamental particles have much better explanatory power than non-fundamental ones. The fundamentality of the entities is gradual, meaning that there are not fundamental or non-fundamental particles, it all comes in degree, and the direction of fundamentality is spatial size.⁵⁹ Therefore, materialistic account of reductionism necessarily presupposes an ontological commitment towards reduction. This is the crudest, yet still meaningful summary of this doctrine. Reductionists such as Smart, Dawkins, Oppenheim & Kemeny support this position whereas Wimsatt, Hacking and Cartwright, while still retaining a scientific realist posture, object to such claims. I believe it is no surprise that many brilliant philosophers and scientists hold this position, since spatiality is a fundamental part of our thinking, and we observe that while dealing with sciences, there is such a discourse that tells us that the smaller causes the bigger. To cite Wimsatt (1976b) again;

Thus if we discover that genes are composed of DNA, this is held to involve or lead to a meaning change in the concept of the gene. But if we discover that an atom of iron occurs in the heme group that associates with hemoglobin molecule, we do not see a meaning change for 'iron.' We say that we have just learned something more that it can be used for. (Wimsatt, 1976b, p. 342)

However, the entities that science deals with are generally quite complex and they require multi-layer analyses, and without making a final picture of the world in that "layer" it is impossible to make this form of reductionism a strong position. Now

⁵⁹ Note that this perspective can not answer the success of physics and the laws of physics which operate over both entities smaller than the organisms, such as various molecules, and much larger than these organisms, such as planets and stars. This doctrine is thus problematic also in slicing the world to layers in strict spatial levels.

let us turn to Oppenheim & Putnam in order to understand another form of reductionism, where spatiality is no longer the ultimate determinant of the direction of fundamentalism.

4.3.2 Layered World Reductionism and Oppenheim & Putnam Model

Layered World reductionism is generally an extension of materialistic reductionism. The extension makes important distinctions between itself and the materialistic, fundamental particle seeking reductionism. We can see this in the quotation below that he is dedicated to the materialistic account, but his account lacks the search for fundamental to an extent. As stated by Dawkins;

The nonexistent reductionist – the sort that everybody is against, but who exists only in their imaginations – tries to explain complicated things directly in terms of the smallest parts, even, in some extreme versions of the myth, as the sum of the parts! The hierarchical reductionist, on the other hand, explains a complex entity at any particular level in the hierarchy of organization, in terms of entities only one level down the hierarchy; entities which, themselves, are likely to be complex enough to need further reducing to their own component parts; and so on. (Dawkins, p. 13, 1996).

Exemplified by Dennett (1995), Skinner falls in this category. Here, higher level entities or theories as fallen from the grace of materialistic philosophers. Dawkins' point is not novel, nor too different than that of Smart. Slicing the entities of science in spatial layers have been an important part of discussions of reduction. Oppenheim & Putnam (1958) were prominent defenders of such a layered world view.

In Oppenheim & Putnam's (1958) account, it is argued that the world should be divided into reductive levels in order to create a basis for reduction. They give us a number of criteria that should be fulfilled for an adequate reduction. The last criterion is the one which tells us that the levels provided by them are not ultimate

“The levels must be selected in a way which is 'natural' and justifiable from the standpoint of present-day empirical science.” (p. 9) To note here again, they seek unity of science, and see reduction as a tool to achieve this goal. Their six layers are as follows; elementary particles, atoms, molecules, cells, (multicellular) living things, social groups. They believe that analysis should be made separately with respect to every layer, “Nothing on any level should have a part on any higher level.” (p. 9) They also state that each level includes the higher levels (for example, the level of atoms also includes the level of cells), yet the thing at each level should be analyzed in its “proper level”, “Thus, each level includes all higher levels. However, the highest level to which a thing belongs will be considered the 'proper' level of that thing.” (p. 10)

Furthermore, they also talk about “aggregate things”, such as “man in a telephone booth” and tell that such “trivial” examples are no obstacle for their structure. Since aggregate things can be analyzed in isolation. The man (in level 5) and the telephone booth (level 3), can be separately investigated, and the concluding analysis can be made with the mitigation of psychological and molecular explanations. Therefore, their stance is not clear cut, since they support independence of each individual level while also holding that explanations for aggregate objects can have explanations from different domains.

The most important aspect of *layered world view* in reductionism is its emphasis on a preset hierarchy. Although Oppenheim & Putnam do not tell us that their slicing of the world is the only way to do that nor it is the best candidate for such division, they emphasize the importance of respecting the hierarchy. I do not claim that the proper way to reduce a science is by using the most fundamental science of the day as the reducing science. However, slicing the world in layers in a way that, these slices do not intersect requires a firm belief in *some form of* scientific realism. This form of scientific realism requires the belief that the entities that science deal with are likely to be *real* and generally of natural kinds, since there must be some way to organize the phenomena in order to set those levels. Yet still it is possible to have a pragmatical posture towards reality, and claim that all those layers are tools. Wimsatt (1976b), makes an excellent point about “cutting the nature at joints”.

“The aim of science is to cut Nature at joints.” This metaphor, in which Nature is compared to some mythical beast which science is to dismember or dissect tells us a great deal about levels of organization. The main point of the metaphor is that, among the many possible ways of cutting up the world, some are easier than others, just as it is easier to cut through cartilage at joints than through the bone and muscle in between. So also it implies that Nature *has* joints, or natural units of organization. If so, then explanatory ontological, systematic, and controlling aims of science all coincide at the importance of finding these “joints,” and of describing the units in between and how they articulate. Talk of joints also suggests that the units in between can be readily spatially delineated. (Wimsatt, 1976b, 237)

Here my interpretation coincides with Wimsatt’s. Referring to nature as something with joints presupposes that it has some soft and hard spots to cut through. This is what Oppenheim and Putnam speak out loudly. Wimsatt as well, advocates that one easy way to slice nature is through a spatial guide, from smaller to bigger.

I believe that spatial layers of the world do not coincide with the traditional divisions made by Oppenheim and Putnam. The starting point makes sense; the smaller particles and the science of such small particles are exact matches. Quantum particles are the smallest particles we know now and they are being investigated with quantum physics. The same rule can apply to one level higher, our knowledge of atoms is mainly from particle physics and chemistry, and they are all fundamental to all those higher level entities. Yet as we go higher in the list of Oppenheim and Putnam's slices, our work gets more difficult. Their example of “man in a telephone booth” is a good example, yet before going up there, we can exemplify from smaller organisms as well.

A lake is quite a large entity. It can be home for millions of different unicellular and multicellular organisms. There might be vertebrates, invertebrates, plants and many different types of organisms within. The scientists who work with

entities in the size of lakes are generally ecologists.⁶⁰ An ecologist works on very large cycles. Such as the nitrogen cycle or carbon cycle. Assume also that the ecologist not only investigates the lake, but also tries to stabilize the acidity of the lake in order to preserve the species living in it. Now, the ecologist measures pH of the lake, which is pH of the whole lake, yet a molecular indicator. He looks at diatoms (very small photosynthetic organisms that zooplanktons feed upon), their shape and abundance, even looks at the sediments of the lake to understand the changes in the population of diatoms to understand regime of the lake. Afterwards, he analyzes various molecular indicators of the lake, calculates biomasses and finally determines the trend of the lake. My outline of research is a brief sketch of what is actually conducted in Little Rock Lake by Frost, T., Montz, P., Gonzalez, M., Sanderson, B., & Arnott, S (1998). Yet we all know that there are also vertebrates in the lake, and they are also producing chemical outputs to their environments, resulting in the change of pH and biomass. Even they are spatially the biggest entities in the lake, they are not investigated separately, since pH, or diatom mass are much better at explanation, and they are also related to the fish population in the lake, the biggest ones are skipped. What we see here is, levels defined by Oppenheim & Putnam do not directly hold. In order to understand a larger entity, we skip the interlevels, where entities of in-between sizes are placed, and analyze it at lower levels. The science of geology does not account for what organisms lived above the plates when they deal with plate tectonics. The smaller entities have a greater impulse on the explanation of the larger even though there are entities closer to the size of the explained larger phenomena.⁶¹

Wimsatt (1976b), makes another objection to applicability of solid layers;

⁶⁰ Of course, if a dam is to be built by the lake, or if a channel for irrigation will be constructed, other scientists like physicists or engineers will also be working in the same lake. Yet for now, let us assume that this is not the case.

⁶¹ Brennan (1989) calls the type of reduction effected by Callicott ontological parasitism or ontological primitivism (pp. 76-88). To say that one kind of entity is ontologically parasitic upon (or primitive with respect to) another is to say that the one is nothing but the other (as when a table is said to be nothing but a swarm of molecules). The parasitic entity is then said either not to exist at all – reference to such entities is a mere convenience or convention – or they are less real than the entity upon which it is parasitic.

What natural selection does for organisms, other selection processes do for entities at other levels, both physical and mental. Atomic nuclei and molecules constitute two other levels of organization and foci of regularity. They are so because they are the *most probable* states of matter under certain ranges of conditions. They are not under other conditions. There are no or few molecules in a gas which has been heated sufficiently to become totally ionized, and there are no or few atomic nuclei in a neutron star. The relative probability of different states of matter, even in cases where no further mechanism for this can be found, is itself or represents the effects of a kind of selection mechanism. (Wimsatt, 1976b, p. 237)

Here Wimsatt relies on some sort of metaphysical claim; things have an organizational foci, and these foci constitute the levels for our analysis. This part seems the same with Dawkins's claim in *Selfish Gene*. Dawkins also advocates that there are selective processes for regularity in different levels. Dawkins simply states that, the more stable structures tend to stand the test of time, especially when they can copy themselves. However, there are two important distinctions between stances of Wimsatt and Dawkins. One is explicit in Wimsatt's writing. Wimsatt does not claim that such layers are non-temporal. For Wimsatt, such layers are applicable only within certain contexts. When the context or environment, or in Wimsatt's words, "under certain ranges of conditions" the "foci" changes, the structure of the layer under discussion changes as well. For Wimsatt, there is no "ultimate condition" or "ultimate context" that these foci shall be investigated.

Secondly, and rather implicitly, Wimsatt tells us that selection of these foci (since they are not "natural kinds", or ultimate particles) determine the organization in a level. I make this conclusion based on Wimsatt's context dependence of foci. In addition to that, he does not claim that these levels are unique. Different sciences, or different theories within the same science can use these levels as they see fit, since the context of the foci of organization is dependent on how different sciences interrelate different phenomena. The ecologist's perception of an elephant is not the same as anatomist's, since their concerns are different, the role of the "same" entity

in their theories is also different.

Almost the whole medical science can be examples for invalidation of such strict layers. Many commonplace drugs have histories before the birth of medicine. One such drug is aspirin. Jeffreys (2005) tells us that even the working mechanisms of aspirin were not investigated until 1958, people have been using it since at least 19th century. Its synthesis in the laboratory for the first time was conducted in 1853, yet even before that people were used to drink willow bark tea to cure fever and kill pain. Headache, being at the 5th level in Oppenheim & Putnam's list, were related to a level 3 entity, aspirin. Of course, people were aware that it had followed a path through the 4th, cellular level. Nobody claimed that aspirin did not pass through cells, or acted without cellular interaction, however they did not use cellular level in their explanations. People were *ontologically* committed to such layers, yet *epistemologically*, such layers had no place. Therefore, such layers are ontological beliefs, rather than epistemological pictures. It is a belief that almost all of us share, today it is impossible to find a scientist who claims that molecules are like spiritual entities and they do a trick to affect the whole organism. However, it is not what I claim. I merely try to show that Dawkins' reductionism model, which hopes to reduce everything step by step is not the way sciences work.

One can still claim that in the long run the intermediate mechanisms will be known to us by further research in science. We knew nothing about how gravity worked for hundreds of years, and we still do not have a comprehensive account of it, yet we discover more and more. However, sciences generally do not work as Dawkins hopes them to work. While constructing a building one builds layers on layers. The roof can only be completed after the other floors are finished. This might work in construction, yet not in sciences that we know of. A science keeps discovering new entities, new relations and new explanations on its own level while, another works all the same. Chemistry will keep working on *changeable* theories, while biology will do the same. Thus a level will never be complete unless the science of that level becomes history, and no more development is ongoing on that level. Therefore, ontological and epistemological reduction do not coincide in the explanatory level of sciences.

In conclusion of this part, I believe that I have shown three things. First; so

called layers of organization are not ultimate levels, they are highly context dependent and different sciences use different levels to organize their explanation of phenomena. Second; ontological and epistemological reduction are separate issues on layered world reductionism. While one can be an ontological reductionist, he might not be epistemological reductionist in the same sense while reverse is not really the case. Thirdly, even such layers are organized in this fashion, the explanation of phenomena might not follow the levels one by one all the same, with only exception of dead sciences. One can explain a layer 6 case with explanations from layer 3.

In this chapter, I tried to lay out some substantial issues in reduction, and investigated reductionisms as the metaphysical beliefs that try to give us a world picture. These pictures constitute explanation in reduction. I believe that I have given the account of various weaknesses of general attempts to give a large picture of the world. Of course, the doctrines are not limited to the ones that I have pointed out. There can be, and also there are different trends in general explanations. However, the two I highlighted – materialistic and layered – are the ones which are the most common among reduction debates. As I demonstrated, scientific work is much more than general beliefs and metaphysics of any sort. In the next chapter, I will try to shed light on an area, long neglected by philosophers of science, in order to give a more general account of reduction.

Chapter V

A New Approach to Reduction in Science Through Instrumental Realism⁶²

Kant and Fichte soar to heavens blue
Seeking for some distant land,
I but seek to grasp profound and true
That which—in the street I find.
(Karl Marx, 1975, p. 531)

My argumentation on reduction in science have been negative so far. I have tried to downplay various general pictures of reduction, and pointed out their weaknesses or their lack of explanation in some important domains as well as their incomprehensiveness. In the most general manner, I have accused many of these attempts in reduction either as incomplete or not reflecting the real picture of science.

In this chapter I will try to sketch a guideline to be followed in discussing reduction, and more generally, philosophy of science. The perspective I will defend is not novel, but it has been largely overlooked in philosophy of science and has not been applied to reduction in science. I will try to put together Hacking's (1984) *entity realism* with Cartwright's (1983) *emphasis of lower level laws* and Galison's (1987) *reliance on experimental analysis* in order to give an account of reduction in science. Their claims have been touched upon by various philosophers before themselves, for instance Wimsatt's (1976a, 1976b) critique on Nagel (1961) and Schaffner (1967) already employs some points used by Hacking and Cartwright. However, it was Hacking and Cartwright mainly, who put forward a more general

⁶² My definition of instrumental realism shall not be confused with instrumentalism – the anti-realist stance.

metaphysical framework in which a general account of reduction can be given.

My analysis is by no means a complete analysis of reduction. However, I want to point out that, instrumental traditions and the relationship between theory and experiment is a level, largely missed by the philosophers of science.

5.1 How to approach reality in science?

What is real, and how do sciences work? These are two fundamental questions philosophy should answer. I cannot give a complete account of such questions, but I will try to lay out two different perspectives, theory based perspective of Nagel and experiment based views of Hacking and Cartwright.

5.1.1 Nagel and Theory Centrist View

I have argued mainly by accepting presuppositions made in Nagel's (1961) work in the 3rd chapter, now I will try to examine his presuppositions and refuse some of them. This will lead me to another form of realism in science than that of Nagel. As a short reminder, let us turn back to Nagel's (1961) book about how formalist discussions shape the debate on reduction.

Most sciences will also contain statements that are certifiable as logically true, such as those of logic and mathematics. Even if we ignore these, we have identified four major classes of statements that may occur in a science S, whether or not any degree of autonomy is claimed for it relative to other special disciplines: (a) the theoretical postulates of S, the theorems derivable from them, and the coordinating definitions associated with theoretical notions in the postulates or theorems; (b) the experimental laws of S; (c) the observation statements of S; and (d) the borrowed laws of S. (Nagel, 1961, p. 349)

These lay out four main components of a science according to Nagel. He explicates

the relationship between them as follows;

Let us suppose that S has a theory capable of explaining all the experimental laws of the science; and let us designate as the “theoretical expressions” of S the descriptive expressions employed in the theoretical postulates (exclusive of the coordinating definitions) and the theorems formally derivable from them. Many explications aim at specifying the meanings of expressions by way of theoretical ones; and we shall call the set P_2 of theoretical expressions needed for explicating in this way the maximum number of expressions in D, the “theoretical primitives” of S. (Nagel, 1961, p. 350-351)

Therefore, according to Nagel's perception of science, we can tell two things; *theory is everything* or more precisely, *everything is theory*. That is to say that, all the experimental laws, lower level theoretical postulates, observation statements, etc. are results of some theory of a science.⁶³ Secondly, the more general a theory is, the greater its explanatory power. This is the very first reason that we want to reduce a more fundamental science to another. Since the laws of physics are more general than laws of chemistry, chemistry *should be an under-laborer of physics*, according to this perspective.⁶⁴

A general objection to this view, the comprehensiveness of theory, is given by van Brakel.

One thing is clear: the context and environment is not part of the reducing theory and restrict the range of a particular bridge law. Applying so called universal laws to concrete cases requires an appeal to initial and boundary conditions. Because fixing these

⁶³ Of course, result here indicates logical consequence from the higher theory.

⁶⁴ Compare this with materialistic claims of reduction in 2nd and 4th chapters. While the materialist held that reduction is meaningful because we must get rid of appearances and turn to real and fundamental particles, Nagel argues in the same direction because of explanatory power.

requires measurement, prediction (or retrodiction) is never strict, even before we enter the realm of quantum mechanics or quantum field theory. More generally one might argue that any application of theory requires that the location is specified in pre-theoretical terms (van Fraassen 1992). This background understanding of what one is doing involves many discourses including ordinary language (the universal meta-language). (Van Brakel, 2010, p. 131)

Here van Brakel tells us that, contexts that are needed to apply scientific theories are not themselves in the theory, and they are not less important than it. Therefore, theory in isolation can not account for much and the context in which theory is employed isn't derivable from the theory. After criticizing Nagel's perspective shortly, now let us turn to another account of realism in science.

5.1.2 Hacking – Cartwright Ground for Realism

Cartwright, in her elegant book *How the Laws of Physics Lie* (1983), tells us that the general laws of physics are very far from reality. They are idealizations that need various models, approximations, calculations and many other fine tuning to fit reality. On the other hand, phenomenological generalizations, laws of engineering and lower-level laws are much closer to “reality”. She makes her stance clear by referring Duhem:

Pierre Duhem distinguished two kinds of thinkers: the deep but narrow minds of the French, and the broad but shallow minds of the English. The French mind sees things in an elegant, unified way. It takes Newton's three laws of motion and turns them into the beautiful, abstract mathematics of Lagrangian mechanics. The English mind, says Duhem, is an exact contrast. It engineers bits of gears, and pulleys, and keeps the strings from tangling up. It holds a thousand different details all at once, without imposing much abstract order or organization. The difference between the realist and me is almost

theological. The realist thinks that the creator of the universe worked like a French mathematician. But I think that God has the untidy mind of the English. (Cartwright, 1983, p. 19)

Her depiction – in fact Duhem's depiction – of the French mind, is the mind of Nagel. In which theory and the first principles determine the lower-level laws and regularities as well as “experimental laws”. Her mind is that of the English, and she tells us that “things are not organized that way in the world”. Hacking goes further to advocate a rather “Argentinian fantasy”. According to him, things are ordered in the world in a way that resembles a Borgesian library. In this library, there are various books in different shelves. In each shelf, there are relevant books of another discipline. In this library laws and generalizations – even those which are contradictory – are coexistent, within the library, and this is possible due to separability of the subdivisions of the library.⁶⁵ The ancient Roman god Mars and the planet Mars do not create any confusion even when they share the same name, since their respective shelves are different. And there is no such “Book of Nature” but rather books of different disciplines separately.

Neither Cartwright nor Hacking believe such metaphysics because of simple commitment. This metaphysical difference between Borgesianism and building blocks perspective has its roots in methodology. Within the scientific community – and hopefully philosophy of science community – neither the materialist nor a pragmatist will support their standpoint due to a theological belief or for some first principles. They believe what they believe because of the methodologies that they employ. In the common materialistic outlook, the emphasis is on the structure of the world. A materialist asks; the world is made of fundamental particles and building blocks made from such particles towards more complex systems, otherwise, how could we better analyze them when we investigate them by explaining their parts? One of the (perhaps the foremost) introductory textbooks in biology in university level, Life's first part is named “The science and building

⁶⁵ It is also important to note that in Nagel's account, the theories of a given science are also necessarily non contradictory and preferably interrelated, which is seldom the case in science, especially in biological sciences, if ever possible.

blocks of life” (Sadva et. al. 2014). In the majority of textbooks of chemistry, the first thing they teach is the structure of the atom.⁶⁶ Therefore, in sciences like biology and chemistry, both for pedagogical and structural reasons understanding an area starts with the building blocks of the domain. In return, a Borgesian (one who follows Hacking and Cartwright's lines of thought on scientific realism) emphasizes the difficulty of application of more general laws. And he asks, the higher-level generalities do not apply to the phenomena in the world, we need to use approximations, models, calculations, laws of other sciences or other laws in the same domain, therefore, how can they depict reality?

Many of the answers given to the question of reduction and reductionism are from materialistic perspectives. Now I will try to build a form of reduction on an account that promotes experiment and lower level laws and models over general rules.

5.2 Intervening the Phenomena

Hacking's entity realism generally goes with the battle cry “if you can spray them, then they are real” (1984, p. 24). He tells us that when we can use entities to manipulate phenomena, we can be sure that the entity used to manipulate is real. He elaborates his stance by exemplifying that he has embraced that electrons exist when he has learned that physicists use electrons to detect quarks. Furthermore, he does not give the theories that employ such entities as much credit as he gives to the entities themselves.

Now within this framework of entity realism I will try to give an account of success of molecular biology to reduce classical biology. While doing so, I will refer molecular biology as the end product of an attempt that tried to reduce biology to chemistry. This success was credited to building block point of view generally.⁶⁷

⁶⁶ However, in physics as well as sociology, the textbooks do not start from the “building blocks” of the related area, they rather start with showing us regularities and methodology.

⁶⁷ See, Sachse 2007

5.2.1 Completeness of Physics over Biology Argument⁶⁸

This argument is quite easy to capture. Sachse (2007) tells us that, there are three dimensions of completeness of physics over special sciences; *causal*, *nomological* and *explanatory*. Even these three refer to different aspects of physics as a science – causal means, physicists always search for causes within physics, nomological means, a physical phenomenon can be a result of a physical law only and explanatory, a physical phenomenon can only be explained through the laws of physics – they all refer to one fundamental thing; in physics, there are, for instance no biological causes, entities or explanations which are from the special sciences. On the other hand, he argues that, in special sciences this is not the case. A biological event or a law can be explained by the fundamental sciences like physics or chemistry.⁶⁹

He uses this argument to support his claim of ontological reduction, which will be later on used by him to provide a basis for epistemological reductionism, in the end leading to a stance which is, in essence, my definition of materialistic reduction.⁷⁰

However, if we seek causal relationship without any structure this also creates misinterpretation of the world. Terrance Brown gives a good example of such situations.

For example, diuretic drugs have been found to be useful in treating congestive heart failure even though their effect is on the kidneys,

⁶⁸ This theme also has been debated under the topic of causal closure.

⁶⁹ Hacking (1984) writes about Morley's experiment in which he tried to eliminate the "noise" that is caused by the passing of horses in a horse ranch close to his laboratory in his calculations. One might argue that this noise is due to a biological property of the horses, making them easy to be tamed instead of physical causes, however this fact "speaks in the language of physics" as we assume, even we can not follow the exact molecules causing certain vibrations, we know that it is a physical effect. Secondly, physics can eliminate such circumstances through isolation, as a matter of fact, this is one of the most prominent features of physics, to isolate the system from externalities.

⁷⁰ Of course, he also uses supplementary arguments to support his claim, however, this is the backbone of the argument

which is normal: Their therapeutic efficacy is due to their ability to increase the output of water and electrolytes, thereby decreasing the functional burden on the weakened heart. (Brown, 2002, p. 6)

Therefore, direct causation in explanation means something, it might be useful to treat some patients by these drugs, however, it does not give us a general picture of the world. Thus, my criticism is twofold. First, we shall accept the causal advantage of physics in explanations. Second, we should appreciate that it does not tell the whole truth about a case. I take the argument of completeness from Sachse, however, I reach a different conclusion. While it is impossible not to agree that physics does not employ biology at all in its high theories, I believe that we should take a different lesson from this argument.

5.2.2 Invention of Phenomena or Emergence of Phenomena

In the previous part, I have indicated that I agree with Sachse on the grounds that high theories of physics are complete, compared to special sciences. However, in his book he uses a meta-narrative to connect reducing and reduced domains. This is a necessary move for him, since given that physics contains no entities or laws of biology within, there needs to be a bridge to connect the reducing and reduced science.

The flower, he says, can be seen as a biological and a physical entity. Yet how do we know that it is the same entity that biology and physics describe, and how are they connected? It is achieved by our daily language. This also presupposes that the environment and other factors that shape our theories at a certain level stay the same. For example, flowers need light to grow, while mitochondria in a certain cell in the root of the flower does not. Hence the relationship is indirect. This situation makes it difficult for us to establish an identity relationship between the flower of physics and the flower of biology.

One of the prime difficulties that ontological research program of reductionism based on materialism is, thus, the emergence of phenomena. If we assume that physics is complete in the sense described above, then how can

biological phenomena be handled by physics? If the inventory of physics does not capture biological entities or phenomena, how can physics reduce them in the very first place? Sachse's answer seems to be the vague common sense. If both a biologist and a physicist point to the same flower, then the bridge is established and the physicist might start to work on reducing the properties of the flower to physical terms. If the physicist will use his preset terminology, then emergence is hard to account for. The phenomena observed at the biological level will be nonexistent in the reduced physical theory because the phenomena themselves will not be in the toolkit of the physicist. For example, the physicist will never find the terms to explain inheritance. Once he tries to reduce any concept related to inheritance, he must make his science incomplete. Since as soon as the concept of inheritance is introduced, and it is not solely defined by the preexisting laws of physics, the domain of physics itself will have an entity (in this case inheritance) that is caused by biological laws, factors or entities.

The concept of wind is something that is not within the domain of physics per se. However, it is a concept that we can investigate in physical terms. It has to do with the atmosphere, the Sun, daily rotation of the Earth, pressure differences, geography and much more. However, we can not say that it is a reduced phenomenon. On the contrary, very different branches of physics come together to investigate the phenomena, and it is not directly reduced to any of them. Tümkaya makes the following note on reduction:

In one sentence, reductionism is not about phenomena, but about the theory that is to explain the relevant phenomena. That is, theories are reduced, not phenomena. It means that for example if a neurophilosopher argue for the reduction of psychology to neuroscience, it does not mean that psychology will be eliminated. Elimination and reduction are largely non-overlapping. (Tümkaya, 2014, p. 61)

If we accept this view, how can we account for the phenomena? If they are never reducible, are they things that exist in nature without reference to anything else?

Hacking has a different answer to these questions. He tells us that we “invent” phenomena. They do not lie out there in the world, waiting to be discovered. After opening up a discussion in Francis Bacon's philosophy, he supports that, phenomena are invented by us through isolation. He gives the example of electrical and magnetic phenomena, which exist in the world, but are meaningful only when we study them in isolation. The electrical, magnetic, biological, and many other phenomena are discovered only after laborious work of experimentation and isolation. I follow Hacking's idea that phenomena are inventions, they are isolation and observation of very amusing parts of the world.

5.2.2.1 Biology as an Observatory Science to Detect Phenomena

It is indeed very difficult to prescribe a role for any science. Sciences do a great variety of things, they explain, experiment, manipulate, observe, calculate, measure, approximate, theorize etc. However, discussing any of these roles does not downplay the importance of the other roles of science. Here I will investigate the observatory character of biology, which *invents phenomena*. Before moving into details, we shall remind ourselves the following:

Majority of the anti-reductionist fury come from the fact that, most of reductionist programs try to explain all the complexity in higher level sciences without referring to biological phenomena. Instead of trying to apply some form of reduction, some reductionists omit biological phenomena completely, which results in a fierce opposition. The discussion then turns into a rejection – acceptance fight, without being able to argue on the same ground.

This borne in mind, let us rethink biology. Like any other natural science, it relies heavily on observation. Hacking (1984) tells us that most physical phenomena, like the famous “effects” – Faraday effect, Compton effect, Josephson effect, etc. (p. 224) – are all phenomena that can only be observed in certain conditions. His examples are mainly from the domain of physics, I believe that the same applies to biology, whether classical or molecular, even though the phenomena of biology are assumed to be “over there”. Phenomena like reproduction, mitosis, meiosis, evolution, etc. are invented as solely biological

phenomena by observation and these phenomena can only be invented when certain limitations, such as temporal limitations to observe a certain cell phase such as meiosis are drawn.

The phenomenon of units of inheritance was one of the most remarkable works in biology. Before Mendel, the dominant view in biology about hereditary characteristics was that, they were blending. This was called blending inheritance and it is the form that we observe the most. When a dark skinned individual mates with one with lighter skin color, their offspring was expected to be in between. And this observation was true in the majority of inherited characteristics that people could measure. What Mendel did was to isolate certain traits, such as being wrinkled or being green, so much from the rest of the traits of the individual that, they were obviously seen as qualitative differences. This led him to invent the phenomenon of units of inheritance. Since when isolated, such traits showed a discreet variety, their inheritance should have been carried by certain discreet mechanisms, namely, units of inheritance. These units are “invented” because Mendel was patient and cautious enough to isolate the traits that lead to such a breakthrough.

Another story from genetics is that of “variegated plants”. Jim Endersby (2007) tell us that, it was a known phenomenon amongst corn. Many corn farmers in the USA observed that some patterns on the leaves or corn kernels were disappearing and reappearing from generation to generation. This phenomenon occurred in other plants as well and it has been an important concern for the planters of decorative plants. De Vries made the first scientific research on this area. He expected to find a pattern that could be explained by the concept of “mutation” of his day. However, they were not “settling in”. The changes in patterns were not inherited to the offspring generations. This was quite contradictory with the Mendelian understanding of mutations. Later Barbara McClintock also studied this phenomenon and she observed that within the chromosomal level – that was what she was researching – some certain “beads” on the chromosomes were disappearing, reappearing and even changing places. In addition to that, such fragments also changed the function of the gene that they were inserted close to. Upon closer investigation on another level, this time on the organismal level, she

has observed that while the corn leaf grew, it was not the case that the color is failing to appear, it appears, disappears and reappears during growth. (Endersby, 2007, p. 327) She was awarded much later with Nobel Prize for her discovery of “mobile genetic elements”. However, what she thought to discover was *controlling* elements, not genes themselves. These genes are called transposons today and they are very central to genetic studies. What she isolated, “the beads that change place on the chromosome” was the phenomenon that she isolated in a chromosomal level and it is what remained as the phenomenon to be explained. Variegated plants were just another phenomenon that was isolated at another level.

I believe that all sciences have a phenomena creating activity within. Be it isolation of electrical currents of physics, foundation of acidity in chemistry, observation of evolution in biology, the postulation of free will in psychology, etc. all the sciences need to create phenomena to investigate. The great discoveries in physics were possible through invention of new phenomena in the late 19th century. The “effects” of physics led physicists to investigate new entities and theories, construct models about things that were not previously discovered. Within the same framework, I believe that it is essential for biology to create new phenomena to study. In this sense, when the *greedy reductionist* or the *materialist* try to explain biology, they tend to omit these phenomena. This neglect makes more sense if we regard phenomena as things created within certain circumstances. Wimsatt (1976b) tells us something quite similar.

I also think that this is unlikely – indeed, virtually impossible – because neurophysiology cannot make progress at the level of higher units of functional organization without appealing to the mental realm for guidance. The task would be like asking a molecular biologist to give a molecular reconstruction of elephant physiology from what he knows, together with photographs of elephants taken in the wild at a conservatively safe distance. Constant reference to minute details of the upper-level descriptions of elephant anatomy and physiology are at least necessary (though not yet sufficient) for the task. The rebirth of cognitive psychology and an increasing

interest in development, while in part reactions to behaviorism with its emphasis on the contemporaneous external environment are also absolutely necessary for further neurophysiological progress. We must use our current and future intuitions and theories of the mental realm to help to develop our neurophysiological accounts. (Wimsatt, 1976a, p. 236)

Here what Wimsatt advises us to use with caution is the phenomena in the domain of higher level sciences. The requirement that Wimsatt asks for is the requirement of both additional laws and the phenomena for reduction to take place.

As Shwartz (2015) tells us; Pasteur believed in germ theory of disease (that is, diseases are caused by microbial creatures), however, the phenomena of germs were not tangible things back in his day. In addition to that microscopes were very inferior in the days of his major discoveries, and they could hardly detect microbial life.⁷¹ Even if they could be observed through microscope, this was by no means something helpful to biology because most of the image was blurred, the coloring was terrible, etc. Yet still, Pasteur was able to work on biological phenomenon of germs, by physical means. He did not have a strong theory of biology. For him, germs had some biological properties for sure, but he was not interested so much in “opening the box”, he rather regarded them as black boxes that can be “pushed” around and manipulated. This means, his work was on manipulation of such biological phenomena rather than direct investigation of it, investigation of germs was left to biologists.

5.3 Intervening Biology

Smart (1963) did not like biology much. It was the reason he put that much emphasis on reduction of biology to chemistry. In this part I want to clarify that biology is also a science of manipulation of phenomena, and any science is reliable

⁷¹ Hacking tells us that, until 1860s, the development of microscopes was very poor, and Pasteur put forward major discoveries before the “microscope revolution”.

as much as its ability to intervene. Yet Smart states that:

I do not, of course, deny that much of biology is pursued primarily on account of intellectual satisfaction, not on account of its applications. (Smart, 1963, p. 53)

Well, this could be a point perhaps back in his day, but even then, it would be a bad point. Everyday practices such as fermentation, agricultural studies, artificial selection, vaccination etc. were quite commonplace in his day. Was he ignorant of these practices? I think he rather classified these practices in chemistry or maybe in applied sciences. However, it is indeed impossible to say that today's biology aims intellectual satisfaction more than everything. Biology has become one of the well-funded sciences, and it had tremendous results that shape our lives. Medical research has become fruitful, genetic modifications and sequencing became commonplace applications, tremendous projects like Human Genome Project have been completed, being a major concern, ecological research has aided us to preserve wildlife reserves and on a larger scale, the earth. One important thing to add is, the great projects, unlike those mentioned by Nagel (1961), do not stem from a single science, and many tools and equipment used for the discovery are invented through the project itself.

In this case the marginalization is all the more inexcusable given Cantor's prominent role in the history of the HGP. Cantor, former director of the Department of Energy Human Genome Project and co-author with Berkeley colleague Cassandra Smith of one of the first textbooks on genomics (Cantor and Smith 1999), has been granted 54 US patents. (Suárez-Díaz, 2010, p. 7)

Here Suárez-Díaz tells us that a fraction of a major project pioneered the invention of many tools. It was not biology that invented these instruments. Almost none of them have direct reference to biological phenomena. However, in order to “isolate” important phenomena to be explained, the project constructed many different tools.

I believe that what Smart tries to point out is the absence of laws and law like systems in biology. For example, he obviously does not see agriculture as a domain of biology, and it is impossible to say that agriculture is purely biology, at least not in the sense of quantum physics being in the area of physics.⁷² I believe that is a false categorization for biology. Special areas such as astronomy, geology, neurology, etc. can not be accounted for solely by one basic science, when the sciences are limited to standard B.S. diploma programs in universities. The entities of biology tend to be different than those of physics. They have multiple domains; the completeness argument works well here because any biological entity is also a physical one. However, this does not mean that it is solely physical per se.

Intervention to biological phenomena or entities is basically this. The domains of agriculture, medicine, psychology, ecology all take into account biological phenomena. However, the problem is, such domains are not isolated. The “pure science of biology” is thus non-intervening; One can say just the same about the big bang theory. Being a theory alone it is mostly an intellectual pursuit, like the theory of evolution. However, if the basis of this theory and the evidence leading to that is investigated, it is impossible to turn the blind eye on the great scientific effort put to work, and many other theories and instruments that are employed. I believe anti historicism led Smart to tell us biology is pursued for intellectual reasons. Function of the heart is biological, yet it does not constitute a big theory, still was crucial for medicine. Hunters were aware of the behavior of their preys for thousands of years at least; people could produce medicine from herbs; wine, beer and vinegar making was quite commonplace practices. The same applies to chemistry. A lot about the behavior of substances had been known before the knowledge about the nature of substance or the matter itself.⁷³ Smart (1961) praises Boyle's law as a scientific law that depicts reality while he despises laws of biology.

Even though “pure laws of biology” seem to be absent, I believe the same applies to chemistry before 20th century. Yet if we follow Cartwright's path and rely

⁷² This refers to Sache's completeness argument again.

⁷³ I believe that this point is similar that of Foucault in his analysis on the relationship and absence of relationship between things and their categorizations in *The Order of Things*.

on lower level generalizations more than higher levels, we can claim that such generalities are indeed a core part of most scientific practice. As she states

We want laws that unify but what happens may well be varied and diverse. We are lucky that we can organize phenomena at all. That is no reason to think that the principles that best organize will be true nor that the principles that are true will organize much. (Cartwright, 1983, p. 53)

Biological phenomena have been handled in a variety of forms. And each and every generalization had some explanatory power, be it farmers' knowledge of plants or doctors' knowledge of treatment of the wounds.

However, if we look into the “theoretical” biology until the 20th century we see a lack of instruments employed. Of course, there are those that Pasteur appealed to contributed greatly to the investigation of biological phenomena, yet they generally created more phenomena than they explained. This created such diverse areas close to biology. The manipulation of physio-chemical factors of biological entities, thus understanding of biological properties of certain biological phenomena led to creation of bio-chemistry. I believe that the reduction of “theoretical biology” – or whatever meant by it – to molecular biology has occurred the same way.

Of course, it is not easy to distinguish bio-chemistry and molecular biology, especially while some people even classify molecular biology within bio-chemistry. I believe that the main distinction is temporality. Biochemistry is older and thus more covering than molecular biology (by the way, where to place biophysics in this distinction?) The difference between two do not touch upon a crucial distinction. What they have in common is, they all are used to intervene biological phenomena. If you consider classical biology's main task as “dissecting animals to understand their anatomy” or “investigating various parts of plants”, “creating taxonomic trees” or providing models for population genetics, it is only natural to see it as an intellectual pursuit. However, there has always been much more in biology than these. Focusing only on some of the end products of biology solely on the

theoretical grounds – and they are not all the end products of course – has been criticized by Garcia-Sanco.

By combining the tools of history and the sociology of science, Fortun has concluded that ‘the Human Genome Project does not exist.’ He describes a number of dispersed and disconnected initiatives, aimed at detailing the structure of the genomes of different organisms, such as viruses, yeast, the worm *C. elegans* and the *Drosophila* fly, initiatives that preceded the initial discussions about sequencing the human genome in the mid-1980s. The main transformation that human genome sequencing introduced was the ‘acceleration’ of genomic projects and the creation of an apparently coherent and unified entity – the HGP – which was easier to present to political actors and funding agencies (quotes from Fortun, 1999, pp. 26–7; see also id., 1993, 1998). (Garcia-Sanco, 2012, p. 9)

Now let us see the instruments that help us to intervene and create phenomena in biology.

5.3.1 Instruments in Biology – Building Bridges

The classical depiction of a biologist is someone with a lab coat, looking through the microscope, adjusting it and then writing notes of her observations. Indeed, this is a part of her work, yet it is far from complete. In the previous part, I have mentioned the observatory role of biology to create phenomena. If finer adjustments were not made by Mendel, biologists would still hold blending inheritance view, which is closer to truth if your main analysis is on skin colors of people.⁷⁴ However, there are a lot more things that biologists do.

I have a book; it is a very commonplace book for techniques in molecular

⁷⁴ Which was very useful for authorities during 1930s, which aided them with the discriminatory policies.

biology. It is of introductory value. Most of the procedures described in the book can be found in most modern laboratories of molecular biology. In most of these laboratories there are much more instruments than the ones described in this book, with the addition of chemical tools of analysis. This book is *Molecular Biology Techniques: An Intensive Laboratory Course* (Ream & Field, 1998). In the preface of the work, they make the following introduction:

We designed this intensive laboratory course to teach incoming graduate students the basic skills of molecular biology, so that students with varied research experience would understand the fundamental principles. To do this, we created an intensive 2-week course that includes techniques commonly used in molecular biology – methods that provide the foundation for most other procedures. In this course students learn how to prepare and analyze DNA, RNA, and proteins. (Ream & Field, 1998, p. IX)

What do we learn from here? First, people of varied research experience can understand fundamental principles of molecular biology, *within 2 weeks*! If we have seen experimental laws as the outcome of theoretical laws of sciences, this would be impossible, because in order to understand and apply such practical operations they would need a theoretical adequacy. Within 2 weeks, a zoologist, a botanist and a biochemist can learn the fundamental methods of molecular biology. Second; students learn about biological entities which are very different from each other. RNA, proteins, DNA, each constitute very different sub-domains, and even in their most general form they are very different from each other.

In majority of the cases biologists know the end product of their work. Some of the work is about observation; by this I do not mean staring at the cells, observation in molecular biology includes extraction, purification, sorting of different biological molecules. Some procedures last for days and are composed of tens of steps, employing many different instruments. Let's see a very common procedure for isolation of proteins.

We use SDS-PAGE to separate proteins primarily according to their size. The anionic detergent SDS (sodium dodecyl sulfate) binds to the proteins, causes the polypeptides to denature, and imparts a large negative charge on the proteins. This SDS charge masks any charge normally present and equalizes the charge along the protein molecules. When an electric current is applied during SDS-PAGE, each protein in a sample migrates toward the anode at a rate inversely proportional to its molecular weight. For highly positively charged proteins, molecular weights calculated from migration during SDS-PAGE may differ significantly from the true molecular weight. (Reid & Field, 1998, p. 81)

To which science does this procedure belong? It employs very basic laws of physics – the laws of electrical attraction – and it is the main reason that proteins migrate on a plain surface. It uses organic chemistry – the chemical agents bind to proteins in a known manner – and it employs molecular biology – the structure of biomolecules is considered because we know that they exhibit a 3D structures. Now if we look at the gel, which is a polymer in which the charged proteins migrate with respect to their sizes, we still see multiple laws or generalizations. We know that larger “things” – in this instance the proteins – move slower in a porous structure. If our observation is simply that, biological molecules are molecules and they have chemical properties such as size and charge, and they obey the electrical laws of physics, we might think that biology is eventually reduced to chemistry. However, even though this claim has some truth, it misses an important point. Even in such a standard procedure lies a new biological phenomenon; three-dimensional structure of proteins. Denaturation and three-dimensional structure are relatively new to the vocabulary of biology. They are created when it was seen that proteins show a very interesting feature. When they are physically or chemically moved out of their “natural” environments, like increase in heat and pH or through usage of detergents, they change structure. This change further degrades their function. This phenomenon is employed in protein determination. If we did not consider this biological phenomenon during protein purification and use a detergent to make the

proteins similar in shape, it will be impossible to correctly isolate proteins to further work on them. In addition to that, molecular biology now has to (and also has the opportunity to) use these new phenomena in its theories, alter its explanations and create a new understanding of the living beings.

The aim of another experiment in the manual is the following.

The goal of this experiment is to fuse virD2 (a virulence gene from *Agrobacterium tumefaciens*) to *gst* (glutathione-S-transferase gene) to express and purify the fusion protein. To accomplish this, you need to create restriction sites on either side of virD2. You will use polymerase chain reaction (PCR) to make copies of virD2 flanked by the required restriction sites. These restriction sites will allow you to insert virD2 into the expression vector pGEX2. To start this and subsequent experiments, you need several different plasmids, which you will purify. (Reid & Field, 1998, p. 1)

Plasmids, gene expression, purification of protein, fusing genes, these are all biological phenomena. However, unlike the common belief that biology will reduce to chemistry in a way that the biological phenomena like “life”, “reproduction”, etc. will be reduced, what we see in molecular biology is creation of even more phenomena. And we know that such phenomena exist, because it is us who isolate them, who manipulate them, who intervene them. With the phenomenon of gene expression, we can explain the relationship between a coding gene and its products – mostly proteins. Even the claim that molecular biology made the distinction between living and non living blur (since within its vocabulary *in vivo* meant living, however this was merely an operational distinction against the artificial environment *in vitro*), new instruments of chemistry and physics provided a novel dimension to biology with many different phenomena.

The way we intervene it is quite physio-chemical. The instruments that biologists use employ laws of chemistry and physics. An instrument such as X-ray crystallograph help us determine the molecular structures of certain molecules. It is the instrument that was used to determine the structure of DNA by Watson and

Crick in 1953. Even this discovery can be seen as a reductive attempt, it created even more problems to biology. If the structure of DNA is double helical, how can gene expression – the transfer of knowledge of the genes to the other parts – is possible? X-ray crystallograph, an instrument based solely on physical principles, has been used in other sciences as well. However, usage of the instrument merely created new phenomena to be explained.

The difference between molecular biology and classical biology is thus, a matter of intervening. While classical biology, caricatured as such, did not intervene with the phenomena it has observed and isolated, left these phenomena to be investigated by other disciplines, molecular biology is equipped with very powerful instruments to manipulate the phenomena. Therefore, the success of molecular biology over classical biology is the usage of such instruments which enable us to manipulate the phenomena. Thus, reduction of classical biology to molecular biology is not a surprise. Yet this is not a reduction in Nagelian sense, where everything is derivable and interconnected with one to one matching of concepts. The theories of classical biology have become redundant as the main driving forces of science – intervention and manipulation – are investigated at another level. However, the phenomena that classical biology isolated remained even with the fierce theory changes.

Latour and Woolgar (1986) investigate the role of instrument use in neuroendocrinology. They count several instruments which are operated within the same research and coin their introduction years to research. The majority of the equipment was introduced in 60s and the fields of origin of the aparati are; physics (isotopes), physics (spin), protein chemistry – analytic, biochemistry – synthetic, nuclear physics etc. This manifold of instrumental origins also make it quite difficult to explain one thing within one single science and one theoretical framework. They add;

It is clear, then, that some items of equipment are more crucial to the research than others. Indeed the strength of the laboratory depends not so much on the availability of apparatus, but on the presence of a particular configuration of machines specifically tailored to a

particular task. (Latour & Woolgar, 1986, p. 65)

This configuration shows us again the importance of biological phenomena. They are responsible for the certain configurations; they shape and sort the instruments as the instruments isolated and shaped the phenomena in the very first place.

Thus, I believe that molecular biology, when its concepts and phenomena it has created investigated, is not directly linked to phenomena and concepts of classical biology, which was expected to be reduced to molecular biology. However, this is more like a work on a new domain rather than different domains. Maybe the state of high theory is like Foucault noted; “the new episteme takes place where classical was silent”. I prefer to downplay the role of high theory in sciences and instead focus on experimental structures, systems and methods of intervention.

5.3.2 A Story of Two Theories and one Method

I have discussed earlier that Smart’s perception of biology, a science which is pursued solely for intellectual purposes can be tracked in old “natural history.” Natural history has been the forefather of biology in many aspects. Theories of evolution, phylogenetic trees, etc. has been ground within the topic of natural history for most of the course of 19th century. Put this way, preparing specimens in vats, discussing species in classes and many other features of classical biology has implicitly belonged to natural history. This was the time when biology was the “observatory science”. Natural history detected many phenomena, such as evolution, species and respiration.⁷⁵ Arguments concerning evolutionary theory have been in central place of natural history at its peak.

The major theories of evolution, that of Lamarck and that of Darwin were in quite a heated race, as well as other theories of evolution. Eventually Darwinism prevailed over Lamarckism, and many theories of biology have been reinterpreted in Darwinian evolutionary sense pretty much in Kuhnian post paradigm change case.

⁷⁵ Of course, most of the phenomena of natural history predates natural history itself, however, natural history enabled us to investigate such phenomena in greater detail by isolation of them through instruments such as microscopes.

Darwinian theory of evolution (even it has undergone major changes) was the central piece of the modern synthesis – the theory of evolution for most of the 20th century. An overwhelming majority of biologists as well as mathematicians that made mathematical models for evolution, with the addition of philosophers of biology have been doing their research and arguments in a Darwinian manner.

In 2005, Eva Jablonka and M. J. Lamb have been the spokes people of something that has been gaining power in biology. The practices that they were involved in were called *epigenetics*. It is a discipline that works both in genetic domain and proteomic⁷⁶ level. This was a proclamation that indicated the biggest change in molecular biology from the day “genetics” was coined in 1905. In 1905, biologists were in the high spirit of Mendelian genetics, they were armed with Darwinian evolutionary theory that fits it the most, and they directed their attention mostly on what they called “genes”. This emphasis led them to the study of genome, and later to DNA as the instruments have progressed, in order to understand major concerns in biology.

In this sense, the high theories of evolution, and the other high theories of biology that are related to evolutionary theory were in line with Darwinian evolutionary theory. The instruments, calculations etc. all followed gene centric approaches.⁷⁷ The objections did not find much public or intellectual audience. Although what they marked as the weaknesses of the modern synthesis were true, biologists kept using the explanations and instruments of manipulation they had for quite some time.

The change that Jablonka and Lamb (2005) called for was epigenetics, upon which long neglected theory of evolution, Lamarckism, found the opportunity to come back to the intellectual stage. Epigenetics, by empirical support, modelling and theories proposed that genes and DNA can not account for inheritance in totality. There are a lot of different parameters and mechanisms that are being inherited. In the very beginning of their book, they make the following marks.

⁷⁶ The discipline that is concerned with structure, function and modification of proteins.

⁷⁷ Of course, there were objections against those, for example Richard Lewontin and Stephan J. Gould.

The conceptual changes that are taking place are based on knowledge from almost all branches of biology, but our focus in this book will be on heredity. We will be arguing that

there is more to heredity than genes;
some hereditary variations are nonrandom in origin;
some acquired information is inherited;
evolutionary change can result from instruction as well as selection.

These statements may sound heretical to anyone who has been taught the usual version of Darwin's theory of evolution, which is that adaptation occurs through natural selection of chance genetic variations. (Jablonka & Lamb, 2005, p.1)

These are quite self-explanatory in terms of their significance. After their book, many universities started opening programs and courses on this new topic, epigenetics and joined the few that were already operating. However, how could a long fortified theory such as Darwinism could lose its strength and a long abandoned theory such as Lamarckism could be back on the stage?

If we look for an answer in the theoretical level, it is indeed very difficult to find it. However, in the experimental domain things were undergoing tremendous changes already. Epigenetics is a discipline that uses extra nucleic acid accounts in addition to preexisting genetic accounts for explaining inheritance. Extra genetic account is proteins for the most part, of which manipulation was quite alien to us for a very long time.

Proteomics is the general study of proteins, one of the most functional parts in the life of a cell. The term 'proteomics' was coined in 1997, making it a very new area of study. Today it is almost impossible to find a molecular biology laboratory that does not use the techniques of proteomics, there are many textbooks on the subject, and an increasing amount of funding goes to that direction. Eisenacher et. al. (2012) published one of the laboratory manuals for proteomics, *Quantitative Methods in Proteomics*. This is a very straight forward book that includes tens of

methods to be employed in protein research and hundreds of different instruments that are employed. Some of these instruments are fairly new and are used for identification of amino acids and proteins.

One of the most frequently used words in the manual is *isolation*, because through isolation we are able to detect separate proteins that we could use to observe new phenomena, such as new patterns of inheritance. Through isolation of proteins we were able to modify them and understand cellular mechanisms together with the genetic knowledge we had previously to alter cellular mechanisms. The instruments that made us able to isolate and manipulate proteins helped us to have reliable knowledge, because the working mechanisms of such instruments were relying heavily on chemical and physical manipulation in the first place.

Throughout the manual, it is impossible to find even one word for Lamarck and there is only one Darwin, which is the surname of one of the authors whose research is used in the manual. The laboratory instruments, the instruments of isolation and manipulation paved the way even for a long abandoned theory like Lamarckism. Furthermore, through these experimental means we were able to create new phenomena such as biomarkers tryptic cleavage sites and many others. Meanwhile in the manual of Eisenacher et. al. (2012) there is no reference to “life” except for phrases such as “lifespan”, “life sciences” or “real-life examples”. Therefore, as it seems molecular biology tends to omit the phenomena of classical biology and create its own rather than engaging them with the tools of molecular biology directly resulting in reduction in the classical sense such as Nagel’s.

The two grand theories, Darwinian theory of evolution and Lamarckian theory of evolution did not say anything about epigenetics nor about proteomics, however in the end the driving power of sciences, phenomena creation, isolation and manipulation marked the direction of biology. And while doing so, the chemical instrumentation had no relationship with the theories of evolution yet in the end, bending the theories in their own terms, making ends of the “intellectual pursuit” of classical biology redundant largely.

In conclusion to this chapter, I defended several things. First, phenomena are created through laborious work within different sciences, yielding a variety. Second, the phenomena of classical biology and the phenomena of molecular

biology, even intersecting at times, are largely distinct and they have different origins. Third, the difference of the origins of such phenomena indicate one thing, the distinct methods to isolate and manipulate separate entities and separate phenomena. Fourth, the instruments that are used to create different phenomena and intervene different entities do not belong to one single area of science, on the contrary, both in classical biology and molecular biology they are from a multitude of areas. In conclusion of all these assertions, I reach the conclusion that, the instrumental power of molecular biology and its success in intervention make it a great science, enabling it to surpass classical biology in many respects, however, by no means it is a classical, idealist scheme of reduction that happens in the domain of biologies.

Chapter VI

Conclusion

The mystical substance becomes the real subject and the real subject appears to be something else, namely a moment of the mystical substance. Precisely because Hegel starts from the predicates of universal determination instead of from the real *Ens* (*hypokimenou*, subject), and because there must be a bearer of this determination, the mystical idea becomes this bearer.

(Karl Marx, *Critique of Hegel's Philosophy of Right*, 1977)

There are various philosophical problems. One way to deal with them is to take sides and start defending a position through examples, thought experiments, conceptual analysis, new arguments etc. Another way is to look into the problem, and try to understand different points of view that *create* the problem and then try to solve it through providing a new area of research. I have taken the second path.

I have laid out four main chapters in this work for that reason. I believe that the definitions I have mentioned and perspectives of reduction taken into account have their places in philosophy of science. However, contrary to logical positivists' belief in which the mission of the science is to provide “observation statements” and “theories”, science is a very broad area which is composed of many different entities and very different activities. Galison (1986), goes even further to conclude that theoretical physicists and experimental physicists have been educated very differently from each other, such that, it is hardly possible for them to communicate. In classical biology, where the research is theoretical rather than practical, things were quite steady until it came to a time – roughly the time of biochemistry – when biological entities could directly be manipulated by “outside forces”, such as temperature and pH and new phenomena emerged through new tools of isolation and manipulation. This was the milestone for the possibility of entertaining the idea

of reduction of biology to chemistry or to physics. When new instruments came in – instruments of which working principles were physio-chemical – this resulted in the birth of molecular biology. This event connected the chemical with biological once more, this time stronger, since practical manipulation of genes and proteins was much more selective than the manipulation of more general parameters such as temperature.

Within the general framework of science, every point of view should be put on its proper place. While metaphysical claims, such as atomism and materialism can be general perspectives and research programs in science, they cannot explain the great diversity in sciences today. Therefore, ontological reductionism is a research strategy rather than a method to approach sciences.

On the other hand, when we put aside the visible ontological commitments, we still have to face with the application of background beliefs on explanatory aspect of reduction. I have explained briefly logical models and in which respects they can be applicable. I believe that seeing reduction as solely a matter of logical modeling creates a form of idealism which blinds one about the ongoing practices of science. Furthermore, even if the logical models of reduction in science can provide us strict limits and definitions, when they are seen as the complete depiction of reduction, we miss the driving force of reduction in science; that is the manipulative success of sciences. Yet there can be times when the manipulative success of a science or a discipline becomes almost completely obsolete and it rests solely on metaphysics, these are the times when such logical reductions are possible. Wimsatt (1976b) tells such a story about religion.

If correspondence seems unlikely, what about wholesale elimination of the mental realm? Suppose for example, that dualistic prejudices isolated mental talk sufficiently from progress in neurophysiology (much as religion has become isolated from its earlier task of explaining nature) that in the ultimate confrontation, the mental realm would just “slough off” as neurophysiological results explained more and more. (Wimsatt, 1976b, p. 32)

Classical biology or psychology might share the same fate with religion one day, when all of their phenomena can be explained by other sciences. Yet it is seldom that only one science is employed to explain such complex phenomena like those of biology and psychology. The terms used to explain phenomena of nature has had multiple meanings.

I believe that when practices of science are conceived as the driving forces of theories, phenomena, explanation and reduction, we can get a closer grip of reality. This work is thus about sketching another perspective of reduction, where theories are not in the center of everything in science. In every individual case of investigation, the instruments, theories used, sciences involved, models constructed will vary. I do not claim by any means that my work is a complete depiction of reduction of classical biology to molecular biology – to an extent, I believe that classical biology was not reduced to molecular biology, but molecular biology has sprung out from another level. However, the method I employed is of help to understand all alleged cases of reduction.

In philosophy of science, we shall look more to what is being done and the whole processes, rather than what is being told and seen as the final product. This calls for analysis of more than theories in science. I could not touch upon the creation of phenomena in physics. For a more comprehensive account of sciences perceived from the perspective of entity realism, investigation of physics is a necessary factor to be adhered. One of the important things to note as the method I used in this work is, *rendering unto Caesar*. In subjects that have turned into battlefields like reduction, the discussion is seldom reached to conclusion based on one perspective. Yet still, every perspective has something to put forward but *as what?* Some as a research program, some as a function, some as a minor part of a whole process. In our daily lives, almost nothing is one dimensional, everything comes in multiple aspects. When we have a cup of coffee, we can talk about its price, its taste, its effects on our hearts, the people that we drink coffee with, etc. I believe this is a fundamental feature of reality. Any real analysis has to include different dimensions of the issue to compare them, and put them in their proper places.

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APPENDICES

APPENDIX A – TURKISH SUMMARY/TÜRKÇE ÖZET

BİYOLOJİDE İNDİRGEMECİLİK KONUSU ÜZERİNE BİR ÇALIŞMA; ARAÇLAR OLARAK MOLEKÜLER BİYOLOJİ PRATİKLERİ

Bu tez, bilim felsefesinin temel sorunlarından biri olan bilimsel indirgeme sorununu, geniş kapsamlı bir bilim felsefesi yaklaşımı olan varlık gerçekçiliği ve olguların yaratılması bakış açıları içinden temellendirerek meseleye daha iyi bir kavrayış getirmeye çalışır. Bilim ve felsefe çalışma alanları belli ölçüde kesişse de yöntemsel olarak farklı alanlardır. Elinde deneysel pek çok araç olan bilimciyi felsefe yapmaktan ya da kavramsal tartışmalardan alıkoyan bir şey olmamasına rağmen, felsefeci bilimsel araçlardan çoğu çalışmasında tamamen, kimi çalışmalarındaysa büyük ölçüde yoksundur. Bu bilim felsefesini çalışması zor bir alan kılmaktadır. Buna rağmen büyük bilim felsefecilerinin pek çoğu ya doğrudan bilimsel araştırma içinde bulunmuşlar, ya da bilimsel araştırmaların sonuçlarını kendi çalışmalarında kullanmışlardır. Bu açıdan bilim felsefesi zorlayıcı olsa da ilerleme sağlayabilmiş bir alandır.

Modern anlamıyla bilim felsefesi, – eğer hatalı da olsa bir başlangıç noktası bulmak gerekirse – mantıkçı pozitivist başlangıcından beri, büyük bakış açısı değişiklikleri göstermiştir. Bu değişiklikler temelde bilimi bir kapalı kutu olarak gören ve bilimin zenginliği yerine “gözlem cümleleri” üzerine konuşmayı uygun bulan mantıkçı pozitivist bakış açısının baskınlığı, bilimi daha geniş anlamda – bilimin tarihini, sosyal ağlarını, kullandığı araçları, deneyleri, gözlemleri vs. – ele alan, dönüşebilen bir “bilim imgesini” savunan Kuhn, Lakatos, Hacking, Cartwright, van Fraassen gibi felsefeciler tarafından eleştirilmiş ve bu felsefecilerin

savundukları eksenlerde genişlemiştir. Bu genişleme genel anlamda tartışılmış, fakat bilim felsefesinin özel uygulamaları – bu örnekte bilimsel indirgeme – bu tip yeni bakış açılarıyla sistematik olarak çok fazla incelenmemiştir. Bilimsel indirgeme konusunu felsefedeki her konu gibi daha geniş çerçevede incelemek büyük katkılar sağlayacaktır. Ben burada, Wimsatt'ın (1976) bilimsel indirgemedeki mantıksal modellerden ibaret bir şemaya olan itirazını ve onun üzerine eklenen Ian Hacking (1983) ve Nancy Cartwright'ın (1984) bilimsel gerçekçiliğe yönelik tutumlarını bilimin gerçek işleyişine daha yakın buluyorum. Onların bakış açısını merkeze alıp Galison'ın (1987) fizikte araçlara ve deneysel geleneklere yönelik vurgusunu klasik biyolojinin moleküler biyolojiye indirgenmesindeki epigenetik açıklamaların güç kazanmasıyla ek olarak örneklendirip, bilimsel indirgemedeki köprü yasalar ya da köprü kurallarla özdeşlik ilişkileri ve bunların uygulanması sorununun bilimin kullandığı, etkilediği, müdahale edebildiği ve nedensel etkileri bulunan varlıkların gerçekliği üzerine kurulduğunda çözülebileceğini iddia ediyorum. Bununla birlikte bilimin asıl kullandığı felsefi konunun genel anlamda pragmatizm olduğunu vurgulayıp, bilimde indirgemenin de ancak pragmatik temeldeki bir yaklaşımla daha geniş çerçevede bilgi sunabileceğini ve mantıksal modellemeci bilimsel indirgeme tutumlarının bilimdeki gelişmeleri miyop görmeye sebep olduğunu, yani uzun vadede ve geniş çerçevede bilimin incelemesini zorlaştırdığını iddia ediyorum. Kapsamlı bir inceleme yapmak zor olduğu için, bu çalışmayı genel bir yol haritası çizme aracı olarak görüyorum.

Bilimin başarısı hem gündelik hayatta hem de felsefe çevreleri içinde yaygın olarak dillendirilen bir kabuldür. Bu tutumun belki de en üst noktası kendisini Kant'ta gösteren, “metafiziği bilimin güvenli yoluna sokma” çabasıdır. Felsefede bilimsel gerçekçilik olarak adlandırılan temel pozisyon da büyük ölçüde kendisini bilimin başarısı kabulüne borçludur. Her ne kadar bilimin başarısının sebepleri çeşitli şekillerde tartışılmış ve bu nedenler örneğin Feyerabend'in bakış açısında belirli bir forma sokulmaya karşı çıkmış olsa da bilimsel gerçekçiliğin başta gelen savunucularından materyalistler bu durumun temel sebebi olarak kendi metafizik bakış açılarını görmüşlerdir. Özellikle Smart (1963) gibi 20. yy. materyalistleri, bilimin başarısını büyük ölçüde bilimsel bilgi nesnelerinin gündelik hayat ya da diğer alanlardaki nesnelerden *ontolojik olarak daha üstün* ve bilimsel açıklamanın

gündelik hayattaki açıklamalardan daha iyi olduğunu açıkça söylemişlerdir. Bu tip açıklamalar Demokritos'un atomculuğuna benzer bir temel varlık arayışına girmişler ve gerçeğin uzamsal olarak daha altta yatan temel nesneler tarafından oluşturulduğuna ve bunlar tarafından oluşturulan nesnelerin daha az gerçek, ya da açıkça yanılısma olduğuna inanmışlardır. Bu materyalist tutum bilim felsefesinin bilimin resmini yanlış görmesine ve bunun sonucunda da mantıksal çelişkiler ya da yetersizlikleri tartışmaya gerektiğinden fazla gömülerek bir odak kaymasına sebep olmuştur. Ben bilimin kendi yönteminin pragmatik olduğunu ve bu pragmatik yöntemin Cartwright (1984), Hacking (1983) ekseninde bilim felsefesine katkıda bulunacağını düşünüyorum. Bu yaklaşım için Quine'ın felsefenin bilimle sürekliliği argümanına başvuruyorum. Bilimde pragmatizmden kastımsa bilimsel yöntemde elde edilen bilgilerin, bilimin anladığı haliyle nedenselliğin ve gözlemlenemeyen varlıkların varlığına fayda ölçüsünde inancı içeriyor. Benim kullandığım haliyle bu inanç, bilimin varsaydığı varlıkların içinde bulundukları mantıksal yapı içinde temellendirmelerinin yanında bu varlıkların nasıl elimizdeki araçlarla etkileşime geçtiği ve bu etkilerin kendisinin doğruluk sağladığı anlamına gelmektedir.

Bilimsel indirgeme genellikle bilimsel gerçekçilikle belli ölçüde ilişkide olan ve felsefede tanımlanması oldukça zor bir alandır. Bu alanda yapılan tanımlamalar temelde Oppenheim & Putnam (1958), Nagel (1963), Schaffner (1967), Wimsatt (1976) ekseninde şekillenmiş, fakat nihai bir tanımlama bulmakta güçlükler şimdiye kadar yaşanmaya devam etmiştir. Putnam, Nagel ve Schaffner bilimsel indirgemeyi temelde mantıksal modeller üzerinden tanımlarken, asıl amaçları *indirgemeci açıklamaya* meşru bir zemin kazandırmaktır. Öte yandan Wimsatt (1976) ve sonrasında, içlerinde Paul Churchland (1986) ve Bickle'in (1998) olduğu *Yeni Dalga İndirgemeciler* bilimsel indirgemedeki mantıksal modellerin ötesindeki unsurları da değerlendirmiş ve bilimsel indirgeme konusunu daha geniş bir ekseninde incelemeye başlamışlardır. Bu yaklaşım farkı Nagel'in köprü-yasaları ya da köprü-ilkeleri yerine daha az formal yöntemler kullanarak teorileri birbirine bağlama çabalarında kendisini göstermektedir. Bilimsel indirgeme konusunda karşımıza çıkan en büyük zorluklar da temelde köprü yasaların niteliği ve uygulanabilirliği temellidir. Bilimsel indirgeme üzerine yapılan çalışmalara tarihsel olarak bakıldığında, zamanla bu yaklaşıma daha geniş bir çerçeveden bakılmaya

başlandığı ve bu genişlemenin *bilimin gerçek işleyişini* daha iyi bir şekilde açıkladığı görülecektir.

Ben burada hem mantıksal modellemeci indirgemeci tutumların büyük ölçüde materyalizme dayanan bir metafiziği açık ya da kapalı şekilde kabul ettiğini iddia ediyorum. Benim buna alternatif olarak önerdiğim yöntemse, bilimsel gerçekçiliğin daha pragmatik bir temele dayandırılmasıdır. Pragmatizm hem bilimsel araştırmaların resmini bize daha gerçekçi olarak sunabilecek, hem de bu konuda bilim felsefesinin kimi dallarının bilimden kopukluğunu gidererek bilim felsefesindeki tartışmaları daha anlamlı hale getirecektir. Bilimsel indirgemenin bir yapı blokları birleşimindense, bilimsel araştırmalardaki çeşitliliği incelenmesi bilimde bu pragmatist tutumla mümkündür. Bilimsel indirgemedede pragmatist tutumu temelde bilimin kullandığı araçlar ve bu araçların sağladığı bilimsel bulgular üzerinden inceleyerek, bilimsel pratiğin indirgemeci yaklaşımda aldığı rolü araştırmak gereklidir. Bu gerekliliğin karşılanmadığı durumda bilimsel indirgemecilikle ilgili yapılan araştırmaların, özellikle klasik biyolojinin moleküler biyolojiye indirgenmesi konusunda, bilimin araçlarından bahsetmeden geniş çerçevede bilgi vermesinin mümkün olmadığını savunuyorum.

Bilimin başarısı hâlâ belli ölçüde açıklamaya ihtiyaç duyan bir durumdur. Bilimsel pratiğin zenginliği ve bilim insanlarının yaklaşımlarının çeşitliliği göz önünde bulundurulduğunda, belki de bilimin tümünü yansıtan bir bilim felsefesi asla mümkün olmayacaktır. Ancak daha geniş bir bakış açısı, bilimin başarısını açıklamada da daha büyük başarı elde edecektir. Bu açıklamanın gerekliliğiye sadece bu açıklamanın tatmin ediciliği yüzünden değil, aynı zamanda – eğer varsa – bilimin özsel ve başarıyı sağlayan unsurlarını bulmak hem gelecek bilimsel üretimde hem de diğer insan etkinliklerinde bize yol gösterici bir anlam taşıyabilecektir. Bilimsel indirgeme de bilimin başarısının bir alt başlığı olarak ele alındığı zaman, bilimin asıl başarı sırrı, *bakmaktansa yapmak*, kendisini bilimsel indirgeme konusunda da önemli bir unsur olarak gösterebilecektir.

1- Sınıflandırmalar

En geniş çerçevede bilimsel indirgeme konusu iki temel yöntemle

sınıflandırılmıştır. Bu sınıflandırmalardan ilk tip, indirgemenin nesnesinin nasıl ele alındığına dair bir sorudur. İndirgenen şey nedir? Buna verilen cevaplar sıklıkla üç kategoride karşımıza çıkar;

- a) Ontolojik indirgeme: Ontolojik indirgeme dünyanın farklı karmaşıklıklarda nesnelerden oluştuğunu kabul edip, daha karmaşık olan üst seviye nesnelerin daha anlamlı olabilmesi için daha temel olan bileşenleriyle açıklanması gerektiğini savunur. Bu yaklaşım indirgenecek şeylerin bilimin varlıkları olduğunu ve bu nesnelerin de kendilerini oluşturan yapıtaşlarının özelliklerini taşıdığını, ya da en azından bu yapıtaşlarının özelliklerinden yola çıkılarak daha karmaşık nesnelerin anlaşılabilceğini savunur. Meşru zemininin ispatını yöntemden alan bu tutum belirli bir yöntemden çok daha geniş bir bakış açısı, ya da bir araştırma programına benzemektedir.
- b) Yöntemsel indirgeme: Yöntemsel indirgeme bakış açısı, ontolojik indirgemedeki gerçekçiliği açıktan değil ama programına içkin olarak kabul eder. Bu yaklaşımda da varlıklar daha temel parçalar yoluyla açıklanır. Ancak ontolojik indirgemeye aralarındaki fark, bu tutumda temel parçacıklarla açıklama yönteminin kendinde bir özelliği olmadığıdır. Yani karmaşık yapıları daha basit araçlarla inceleme sebebimiz onların daha temel yapıtaşları olması değil, bu tutumun yöntemsel başarısıdır. Bu yüzden ontolojik indirgemedan farklı bir gerçekçi tutumu vardır.
- c) Epistemik indirgeme: Epistemik indirgeme indirgemedede indirgenen ve indirgeyen şeylerin varlıklar değil yasalar olduğu kabul edilir. Bilimin nesneler, üzerine konuşabildiğimiz ve kavramsal tartışmalar yürütebildiğimiz düzlemi bir temsiller alanı olduğu için, bizim indirgeme tartışmamızın da bu temsiller arasındaki ilişkilerden kaynaklandığını düşünür. Bu yüzden epistemik indirgemenin gerçekçilikle ilişkisi daha esnektir ve bu epistemik indirgemedeki tutumların daha çeşitli olabilmesine olanak sağlar. Buna ek olarak epistemik indirgemenin bilime yönelik tutumu diğer ikisinden oldukça farklıdır. Temel nesnesini temsiller olarak kabul ettiği içi, daha geniş bir

dünya resmine ihtiyaç duymaz, onun temel bakış açısı, en başta, temsiller üzerine tartışmayı kabul ettiğinde oluşmuştur

Bahsettiğim ilk ayırmadan farklı şekilde, bu alandaki felsefi çalışmanın merkezine oturacak bir diğer ayırım neyin indirgeme olarak nitelendirildiği üzerindir. Bu sınıflandırmada da indirgemeye yönelik yaklaşımlar üç kategoride incelenebilir. Bunlar *koşullu indirgeme*, *açıklaştırma* ve *indirgeme olduğu iddia edilen durumların incelenmesidir*. Koşullu indirgeme temelde Ernest Nagel tarafından oluşturulmuş bir yöntemdir. Bu yöntem indirgemenin yapılabilmesi için kesin ve açık koşullar öne sürer. Temel amaçlarından biri teori değişimlerinin hangilerinin indirgeme olarak adlandırılacağı konusunda karar vermektir. İndirgemenin gerçekleşebilmesi için kurulan koşullar incelenir ve indirgeme yapıp yapılamayacağına bakılır. Uygun olduğu durumda indirgeme mantıksal bir işlemler bütünü halinde gerçekleştirilir. Bu tutum fazla idealist bir tutumdur ve indirgeme genel bir süreç olarak ele alındığında, onun sadece son kısmını açıklar. Bu açıdan koşullu indirgeme bilimsel indirgemeyi açıklaması açısından kapsayıcılıktan uzaktır.

Açıklaştırma yöntemi indirgenen kavramların (ve indirgeyen kavramların da) daha açık hale getirilmesi gerektiğini, indirgemenin bu şekilde uygun bir işlem olabileceğini öne sürer. İndirgenen kavramın tam olarak neyi işaret ettiği, hangi varlığı temsil ettiği, eğer anlam karışıklığı ve çok anlamlılığı varsa bunların giderilmesi açıklaştırmada başat yöntemdir. Bu yöntem de dilsel bir operasyon üzerinden temellendirildiği için idealisttir. Buna ek olarak, bilimsel kavramların anlamları üzerinde oynamak, açıklaştırmak mümkün olsa da bilimcilerin kendileri dahi çoğu zaman bu anlam çokluğu ve belirsizliğini devam eden araştırmalarında kendileri lehine kullanırlar. Anlamanın sabitlenmesi ve indirgenen kavramla indirgeyen kavram arasında net bir ilişki oluşması, bilimde yine bir yöntemden çok sonuç olarak karşımıza çıkar. Bu yüzden açıklaştırma faydalı bir yöntem olabilse de kapsayıcılıktan uzaktır.

İddia edilen indirgemelerin incelenmesi sınırları daha belirsiz bir yöntemdir. Bilim ve felsefe alanlarında indirgeme sıkça kullanılan bir kavramdır. Bu kavram bazen bir işlemi bazen genel bir programı bazen iki bilim arasında olması beklenen

ilişkiyi vs. temsil eder. Bu tutum tüm iddia edilen indirgemeleri incelemeye değer görür. Buna ek olarak kapsayıcılığı daha fazla olduğu için kendisini dilsel araştırmalarla kısıtlamaz. Bilimde indirgeme adına olan mantıklı her iddia bu alan içinde yer alabilir. Bu genişlik, bilimi bir pratikler bütünü halinde görmeye alan sağlar. Bu sebeple benim de temelde takip edeceğim ve indirgeme sürecinin geneline hitap eden tutum budur.

2- Mantıksal İndirgeme Modelleri

Bilim felsefesinin modern öncülerinin en çekirdek grubu olan mantıkçı pozitivistler tüm felsefi sorunları dil sorunları olarak gördüler. Bu yüzden indirgemenin de ilk olarak mantıksal bir modelleme üzerinden ele alınması tesadüf değildir. Ernest Nagel tarafından net bir şekilde biçimlendirilen bu tutum, indirgemeyi temelde bir mantıksal işlem olarak görür. Teorilerdeki kavramlar ve bu kavramların benzerleriyle olan ilişkiyi mantıksal bir biçimde ele aldıktan sonra indirgemenin imkanını inceler. Bu incelemede Nagel gerekli koşulları öne sürdükten sonra özünde şu çıkarımı yapar. İndirgeme temelde mantıksal bir işlemdir. Bu bağlamda mantıktan kasıt formel mantıktır. İndirgeme imkânı indirgenen teorinin indirgeyen teorinin kabullerinden ve cümlelerinden mantıksal işlemler yoluyla yeniden üretilebilmesine bağlıdır. Bu imkân ancak kavramların sabitlendiği ve kavramlar arası ilişkinin net bir mantıksal hat içinde ele alınabildiği durumda mümkündür. Nagel'dan sonra bu tip mantıksal modellemeler üzerine çalışmalar devam etmiştir, bu çalışmalardan belki en önemlisi, Nagel'in öğrencisi olan Schaffner tarafından ortaya konmuştur. Schaffner indirgeme sürecinde indirgeyen ve indirgenen teorilerde bilimin kendi dinamiği içindeki değişiklikler yoluyla dönüşümler olacağını ve bu dönüşümlerin sonunda iki teoriyi birbirine bağlayacak kadar yaklaştıracığını söyler.

Nagel'in kalıbının uygulanmasının zorluğunu öngören Nagel indirgemeyi ikiye ayırır. Bunlardan ilki *homojen* indirgemedir. Homojen indirgemedede mantıksal işlemler doğrudan yapılabilir ve indirgenen teori indirgeyenin kavramları üzerinden sorun çıkmadan yeniden üretilebilir. Fakat bu durumun uygulamadaki zorluğu ikinci bir kategoriyi, *heterojen* indirgemeyi gerekli kılar. Heterojen indirgemedede

doğrudan mantıksal bir çıkarım yapmak mümkün değildir. İki teorinin kavramları birbirlerine *köprü yasalarla* bağlanır. Nagel bu köprü yasaların üç farklı şekilde kurulabileceğini söyler. İlki uzlaşma yoludur. Uzlaşma yolu indirgeyen ve indirgenen teorilerdeki kavramların anlamlarına müdahale ederek yapılır. Bu müdahale bilim alanındaki insanların kavramın anlamını belirli bir şekilde sabitlemesine dayanır. Örneğin indirgenen teorideki kavramın bazı anlamları yitirilir ya da ona yeni anlamlar kazandırılır. İkinci olarak önerdiği yöntem doğrudan mantıksal bağlantılardır. Bunlar basit şekilde Halikarnas Balıkcısı Cevat Şakir Karaağaçlıdır gibi teorilerin doğrudan içinde olmayan dışsal bağlantılardır. Bu bağlantılar özdeşlik özelliği sağladığı için kurulabildikleri durumda mantıksal indirgemeyi oldukça kolaylaştıran durumlardır. Üçüncü olarak maddi ya da olgusal bilgi üzerinden kurulan bağlantılar ele alınır. Bu tür bağlantılar empirik korelasyon gibi yöntemlerle iki kavram arasında mantıksal olmayan ilişkiler kurar. Bu yapının mantıksal olmayışı Nagel'in yaklaşımının merkezi yapısına uygun olmamakla birlikte, genel mantıksal yapılar ele alındığında bize çok önemli bir meseleyi gösterir. Bu durum mantıksal yapıların kendi başlarına bilimin zenginliğini kapsamak konusunda büyük zorluklar yaşadığıdır. Neredeyse her modelde empirik içeriğin girdiği bir alan vardır. Empirik içeriğin olmadığı şablonlarsa bilim alanında karşılaştığımız indirgeme durumlarının neredeyse hiçbirine uymamakta, ancak son durumu açıklayabilmektedir. Bilim felsefesindeki pek çok alanda olduğu gibi bu alanda da idealist bir tutum bilimin pratiğini görmemizi ve daha anlamlı analizler yapmamızı zorlaştırmaktadır.

3- Bir Doktrin Olarak İndirgemeciliğin Bileşenleri

İndirgeme temelde bir işlem olarak ele alınsa da günümüzde karşılaştığımız indirgeme tartışmaları genellikle daha geniş bir metafizik arka planın, yani bilimsel *indirgemeciliğin* içinde tartışılmaktadır. İndirgemecilik bilimsel açıklama yöntemiyle ilgili bir tutumdur. Gerçeğin ne olduğunu gerçeğin içinde söyler. Bu alanda Sarkar indirgemeci açıklamayı üç temel bileşenle özetler ve daha sonra onların kombinasyonları üzerinden farklı indirgemeci tutumları açıklar. Bu üç bileşenden ilkti *köktenciliktir*. Bu her indirgemeci açıklamada bulunması gereken

bir özelliktir, çünkü indirgeme ilişkisi temelde bir teoriyi diğerine önelemeye dayanır. İndirgeyen teori çeşitli özellikleriyle indirgenen teoriden daha temel olmalıdır ki indirgemeden bahsedilebilsin. İndirgemenin yönünü belirleyen unsur da bu daha temel olma ilişkisidir. İkinci olarak Sarkar *soyut hiyerarşi* bileşenini ortaya koyar. Bu bileşen indirgemenin yapısını açıklar. Bu her indirgemedede olması gereken bir özellik değildir. Temsil edilen sistemin nasıl bir soyut hiyerarşi içinde düzenlendiği indirgemeci açıklamaların çoğunda önemlidir. Üçüncü bileşense *uzamsal hiyerarşidir*. Aslında Sarkar'ın da belirttiği üzere, uzamsal hiyerarşi soyut hiyerarşinin yapısının uzam tarafından belirlenmesinin tezahürüdür. Burada varlıkların tabi olduğu hiyerarşi onların uzamsal büyüklüğüdür. Daha temel olan varlıklar uzamsal olarak daha küçük olan, kompleks yapıları oluşturan parçalardır. Ben üçüncü bileşeni ikincinin bir uzanımı olarak gördüğüm için bunu ele almayacağım.

Benim yaptığım sınıflandırma bu yüzden indirgemeci açıklamayı ikiye ayırır. Bu ayırmda ilk grup Smart'ın en açık şekilde belirttiği, indirgemeciliğin maddeci bir şekilde ele alınışıdır. Bu metafizik tutum varlıkların temel yapıtaşlarından oluştuklarını ve yapacağımız açıklamalarda gerçeği yansıtan açıklamaların da bu yapıtaşlarından yola çıkılarak verilebileceğidir. Bu görüşte maddenin en temel parçaları hem daha karmaşık toplamlardan hem de kuvvetler gibi teorik olgulardan daha açıklayıcıdır. Bu yaklaşımın çeşitli sorunları vardır. Bu sorunlardan ilki temel parçacık arayışının hiç bitmeyen bir uğraş olmasıdır. Bilim tarihi bize daha temel parçacıkları bulmak için uğraşmanın hiç değilse yüzyıllardır sürdüğünü ve hala sürmekte olduğunu gösterir. Buna ek olarak hali hazırda elimizdeki en temel parçacıklar genellikle hakkında pek az şey bildiğimiz parçacıklardır ve bu yüzden açıklama yapacak operasyonel güce sahip değildirler.

İkinci gruptaysa Oppenheim & Putnam'ın modelinde görebileceğimiz *katmanlı dünya* tipi indirgemeci açıklamalar vardır. Bu tipteki açıklamaların temellendiği unsur yapıtaşları değil varlıkların organize olduğu şemadır. Bu görüş varlıkların çeşitli organizasyonel birimlere ayrılmasıyla bu ayırım seviyeleri arasındaki bağı kademe kademe tekrar kurulmasına dayalıdır. Örneğin bir hücre incelenecekse, bu hücre öncelikle organel seviyesine, sonra organeller moleküler seviyeye, sonrasındaysa moleküller atom altı seviyeye indirgenmelidir. Fakat

bilimin yaygın olarak kullandığı yöntem bu da değildir. Bilimsel araştırmalar böyle bir hiyerarşiyi gözetmekle birlikte, asıl olarak indirgeme işlemini yapmaya çalışmazlar ve bu yüzden ürettikleri bilgi genellikle seviyeleri atlayarak ifade edilir. İlaçlar üzerine araştırmalar ya da moleküler biyoloji bu konuda güzel örneklerdir.

4- Araçsal Gerçekçilik Yöntemiyle İndirgemeciliğe Yeni Bir Yaklaşım

Şimdiye kadar olan tartışma genellikle açıklamacı ve olumsuz bir şekilde oldu. Yani bu kısma kadar temel yaklaşımları ortaya koyarak onların zayıf yönlerini göstermeye çalıştım. Bunu yaparken yaklaşımları genelde gerçek bilimsel araştırmayı yansıtmamakla suçladım. Eleştirdiğim tutumlar ya idealist bir yapıda oldukları için bilimsel çalışmaların tümünü yansıtmak yerine sadece teoriler üzerinden açıklamalar yapıyordu ya da metafizik arka planlarına çok bağlı oldukları için sadece araştırma programlarından ibaret kalıyorlardı. Bu durumda yapılması gereken şey teoriden uzaklaşarak bilimi bütün pratikleriyle, yani ölçme, deney yapma, tahminde bulunma hesaplama, mühendislik faaliyetleri gibi pratikleri bilimin içinde tutarak daha geniş bir çerçeveden görmektir. Ian Hacking varlık gerçekçiliği yaklaşımıyla bunun bir ayağını oluşturuyor. Hacking, başka varlıkları maniple edebildiğimiz varlıkların gerçekliğini temel gerçeklik olarak alıyor. Bunun yanı sıra, doğa olgularının doğada bizim toplamamız için açık şekilde görülmediğini, bizim uzun uğraş ve emek sonucunda çoğu olguyu izole edip sabitleyebildiğimizi anlatıyor. Ben bu görüşe klasik biyoloji ve moleküler biyoloji alanlarından örnekler veriyorum. Moleküler biyolojinin araçlarının farklılığı onun izole ettiği olguların ve ele aldığı varlıkların temelde klasik biyolojide olandan farklı olduğunu gösteriyor. Klasik biyolojinin olgularına moleküler biyolojik yöntemlerle müdahale edilebilse bile çoğu durumda iki bilimin olguları arasında birebir eşleşmeyle karşılaşmak mümkün görünmüyor. Aralarındaki ilişki temelinde, daha çok kullanılan araçlar açısından farklılaştığı için bu iki bilim teorik düzlemde birbirlerinden yakın olsalar da pratik alanda aynı şeye farklı seviyelerde müdahale ederler. Bunun sonucu olarak bu alanda karşılaştığımız indirgeme idealist indirgemeden oldukça farklıdır. Buna ek olarak moleküler biyoloji alanında kullanılan araçlar çok farklı bilimlerden gelmektedir ve bu yüzden tek bir bilimle

olan ilişkiden ya da tek bir bilime doğru indirgemedен söz edebilmek mümkün değildir. Moleküler biyoloji fiziğın ve kimyanın pek çok farklı alanından deneysel araçları aynı prosedür içinde kullanır ve bunu yaparken pek çok yeni olgu ortaya koyar. Bu durum da indirgemenin alışıldık anlamda tek bir bilime doğru yapılacak şekilde ele alınmasını zorlaştırır.

Hacking'in tutumuna ek olarak Nancy Cartwright'ın düşük seviyedeki yasaları temel yasaların önüne koyma argümanı da bu noktada çok önemlidir. Nancy Cartwright temel yasalar olarak addedilen soyutlamanın çok yüksek olduđu yasaların kendi başlarına somut pratiğe uygulanamayacağını söyler. Soyutlanmış temel yasalar bilimsel uygulamalarda her zaman ek yasalara, başka bilimden gelen kanunlara veya yardımcı genellemelere ihtiyaç duyar. Genel yasaların uygulamadaki bu zorluğu onları aynı zamanda bahsettikleri nesnelerden ve gerçeklikten de uzaklaştırır. Bu yüzden Cartwright basit genellemelerin ve dar alanlar için geçerli olan yasaların gerçeğı yansıtırma konusunda temel yasalardan daha başarılı olduğunu söyler. Cartwright'ın bu argümanları ontolojik indirgemenin temelini oluşturan teorik yasaların yukarıdan aşağı örgütlenmesini bekleyen tutumuna karşı çıkar. Bu yüzden indirgemenin düzlemi temel teoriler üzerinden değil pratik alanda gerçekleşmelidir.

Tüm bu yaklaşımlar ele alındığı zaman yapılması gereken ilk iş indirgeme üzerine olan tutumları doğru yerlere yerleştirmektir. Kaba maddeci araştırma programları her ne kadar araştırma programları olarak faydalı olsalar da bilimsel indirgemeyi tam olarak ele alamazlar. Onlar daha geniş araştırma projeleri için temel fikir vermekle yetineceklerdir. İdealist, mantık merkezci tutumlarsa indirgemenin ancak belli durumlarda, son halinde işlevsel olarak bize işlevsel sonuçlar sunabilmektedirler. Bilimi genel bir pratikler bütünü ve bir çokluk olarak ele aldığımız zaman hem bilim felsefesinde genel anlamda kuvvetli tutum takınabilmek mümkün olur, hem de bilimsel indirgemeyi anlamak ve çözebilmek için daha olumlu adımlar atmış oluruz. Örneklerden görüldüğü kadarıyla klasik biyolojinin moleküler biyolojiye indirgenmesi ve moleküler biyolojinin kimyaya indirgenmesi olayları, alışıldık indirgeme şemasıyla açıklanamayacaktır.

APPENDIX B – TEZ FOTOKOPİSİ İZİN FORMU

ENSTİTÜ

Fen Bilimleri Enstitüsü ☐

Sosyal Bilimler Enstitüsü ☒

Uygulamalı Matematik Enstitüsü ☐

Enformatik Enstitüsü ☐

Deniz Bilimleri Enstitüsü ☐

YAZARIN

Soyadı : ALTINOK
Adı : OZAN ALTAN
Bölümü : FELSEFE

TEZİN ADI (İngilizce) : AN ACCOUNT OF REDUCTIONISM IN BIOLOGY; PRACTICES OF MOLECULAR BIOLOGY AS INSTRUMENTS

TEZİN TÜRÜ : Yüksek Lisans ☒ Doktora ☐

1. Tezimin tamamından kaynak gösterilmek şartıyla fotokopi alınabilir. ☒
2. Tezimin içindekiler sayfası, özet, indeks sayfalarından ve/veya bir bölümünden kaynak gösterilmek şartıyla fotokopi alınabilir. ☐
3. Tezimden bir (1) yıl süreyle fotokopi alınamaz. ☐

TEZİN KÜTÜPHANEYE TESLİM TARİHİ: