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ASPECTS OF SCIENTIFIC MODELING
A PHILOSOPHICAL ANALYSIS OF IDEALIZATION, ABSTRACTION AND REPRESENTATION

A THESIS SUBMITTED FOR THE DEGREE OF DOCTOR OF PHILOSOPHY

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BİLİMSEL MODELLEMENİN ASPEKTLERİ

İDEALİZASYON, SOYUTLAMA VE TEMSİL ÜZERİNE FELSEFİ BİR ANALİZ

ÖZET

Modelleme günümüzde bilim felsefesinin önemli meselelerinden biri olsa da, konuya ilişkin ilgi ancak 1960'ların sonlarına doğru zirveye ulaşmıştır. O zamana kadar pek çok bilim felsefecisi, daha ziyade bilimsel teorilerin yapısıyla ilgilenmektedir. Özellikle bazı Viyana Çevresi pozitivistleri ve onların sadık takipçileri entelektüel enerjilerini fazlasıyla bilim teorilerini aksiyomatik bir şekilde yeniden inşasına harcamışlardır. Teorilere yönelik alınan bu olumlu tavır modelleri kaçınılmaz olarak bilim felsefesinin bir yan konusu haline getirmiştir.

Öyle görünüyor ki, bu türden teori–merkezli tutum, bilimi karakterize etmek için artık geçerli bir yaklaşım değil. Teori–merkezli tutumun baskın statüsü uzunca süredir eleştirilmektedir ve neredeyse bütün çağdaş bilim felsefecileri, modern bilim pratiğinin modellemeye dayandığı görüşüne katılmaktadırlar. Modellerin çeşitli türleri olmasına rağmen, matematiksel olanları diğerlerinin arasında öne çıkmaktadır. Bu araştırmayı farklı açılara yönlendiren sebep matematiksel modellerin bilimdeki yaygınlığıdır. Bu tez, matematiksel bir modelin, daha spesifik söylemek gerekirse buzul çağlarının Milankovitch Modelinin, inşa ve uygulama süreçlerine ve ayrıca ilgilendiği fenomenleri açıklamadaki ve öndeyide bulunmadaki başarısına odaklanmaktadır. Bu vaka çalışması aynı zamanda matematiksel bir modelin bilimsel bir bulmacanın çözümünde nasıl çalıştığını da göstermektedir.

Vurgulamak gerekir ki, vaka çalışmasının amacı, yalnızca matematiksel bir modelin hedeflediği fenomeni nasıl başarılı bir şekilde açıklayıp onunla ilgili öndeyide bulunduğunu göstermekle sınırlı değildir. Bununla beraber, bu tezde, Milankovitch'in modeline dayanarak, ortaya konulacak herhangi bir bilimsel temsil görüşünün idealizasyonu ve soyutlamayı hesaba katması gerektiğini öne sürmekteyim. Ayrıca de-idealizasyonun, eğer mümkün ise, epistemik olarak verimli bir strateji olduğunu düşünmek için nedenler sunmaktayım. Bu nedenler ise idealizasyon ve soyutlama arasında yaptığım ayrımın temelini oluşturmaktadır. Bu bağlamda, idealizasyon ve soyutlamanın tersine çevrilme süreçlerinin bir stratejiyi diğerinden ayırt etmek için yeterli nedenler temin edeceğini savunmaktayım.

Anahtar Sözcükler: Modelleme, Modeller, İdealizasyon, Soyutlama, Temsil

ASPECTS OF SCIENTIFIC MODELING

A PHILOSOPHICAL ANALYSIS OF IDEALIZATION, ABSTRACTION AND REPRESENTATION

ABSTRACT

Modeling is currently a lively topic in philosophy of science, though interest in it peaked during the late nineteen sixties. Until then many philosophers of science had busied themselves for decades with the structure of scientific theories. Especially, some of the Vienna Circle positivists and their loyal followers devoted their intellectual energies primarily to the reconstruction of physical theories in an axiomatic way. With such a positive attitude towards theories, models were inevitably thought of as a side issue for the philosophy of science.

It appears that such a theory–centric attitude is no longer a valid approach to characterizing science. The dominant status of theory–centric view is long since challenged, and nearly all philosophers of science are inclined to agree with the view that the modern practice of science is based on modeling. Although there are various types of models, the mathematical ones stand out among the others. It is the pervasiveness of mathematical models that led this research into different aspects. This dissertation studies a mathematical model, specifically the Milankovitch Model of ice ages, by thoroughly focusing on its construction and application processes, and also its success in explaining and predicting the relevant phenomena. This case study also illustrates how a mathematical model works in solving a scientific puzzle.

It seems significant to emphasize that the purpose of the case study is not merely to demonstrate how a mathematical model can successfully explain and predict its target phenomenon. By relying on Milankovitch's model, I argue that any account of scientific representation should take idealization and abstraction into account. I also provide reasons for thinking that de–idealization, if possible, is epistemically a fruitful strategy. These reasons subsequently became the basis of my distinction between idealization and abstraction. In this context, I argue that the reverse processes of idealization and abstraction give adequate reasons to distinguish one strategy from the other.

Keywords: Modeling, Models, Idealization, Abstraction, Representation

To Sinan…

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ABBREVIATIONS

CM: Classical mechanics

$L_{TO:}$ Theoretical and observational language

$L_{T:}$ Theoretical language

$L_{O:}$ Observational language

Kyr: A period of one thousand years, kilo-years.

H-S: Hypothetic-structural explanations

I-S: Inductive-statistical model of explanation

D-N: Deductive-nomological model of explanation

TS: Target system

S: System

M: Model

D: Domain

HAE: How-actually explanation

HPE: How-possibly explanation

SV: Sub-vocabulary

MCF: Main causal factor

BP: Before present

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CHAPTER ONE

A FOCAL SHIFT IN PHILOSOPHY OF SCIENCE: FROM THEORIES TO MODELS

1.1 Introduction

Ludwig Boltzmann (1844–1906), the Austrian physicist who is known for his contributions to statistical mechanics, wrote an entry about 'models' for *Encyclopaedia Britannica* in 1902. It was an encyclopedia entry and as such it was simple, introductory, and descriptive. In some places, however, the entry was providing information beyond an encyclopedic scope. Especially, the parts where the indispensability of scientific modeling was emphasized, Boltzmann was suggesting to do more further research on the topic. In essence, his one purpose was to arouse interest in scientific modeling.

Boltzmann was a reputable scientist and his ideas on scientific models were expected to be acknowledged by many researchers, especially by philosophers who were scientifically oriented. But that did not happen; theories (but not models) remained a central issue among researchers. While the use of models is as old as science itself, philosophical study of them had to wait until the middle of the twentieth century.[1]

In this chapter, I attempt to give an overview, together with an explanation of why this is the case. Delving into the story behind the shift from theories toward models would hopefully offer insights into the story of how philosophy of science has evolved to its present 'model–centric' state. More specifically, the significance of the story is that it provides historical and terminological basis for the issues that will be covered in various chapters of this dissertation. In the following two sections, I present a very short overview of the situations of science and philosophy in the early twentieth century. Then, I briefly summarize the intellectual effect of Ludwig Wittgenstein (1889–1951) upon the Vienna Circle. In the last two sections, I outline syntactic and semantic (or model–centric) characterization of science.

[1] Actually, the number of books and articles on the models is *not* countless. D.M. Bailer-Jones (2009), for example, presented a bibliography of works on scientific models. Although this bibliography has no claim to exhaustiveness, its comprehensiveness is invaluable for the researchers who are interested in studying scientific models. It just takes a quick glance to see that the number of books and articles published seems to increase only after 1960s. If I did count right, prior to that year there had been seventeen publications had been issued.

1.2 Science in the Early Twentieth Century: A Very Short Overview

The first decades of the twentieth century were a pivotal period for the development of physics. At that period, two grand theories have been proposed almost simultaneously: General Relativity and Quantum Mechanics.[2] These theories have changed our way of understanding the nature in a revolutionary manner. On 7th of November 1919, a popular British newspaper, *The Times*, reported the story of this scientific development with a bold headline 'Revolution in Science'.

The revolution was needed because Classical Mechanics (CM) was partly insufficient for explaining the world both at macro and micro levels. For example, Mercury's anomalous precession was posing a problem for the CM. However, the theory of general relativity was providing "an explanation for the advance of Mercury's perihelion or, more precisely, the advance followed from [its] equations" (Eisenstaedt 2007, p.159). The photoelectric effect was another challenge for the CM. According to CM, the intensity of the light (not the kind of light) determines the degree of ejection of the electrons from the surface. Nevertheless, "all of the experiments showed that the frequency (*i.e.*, the color) of light, rather than the intensity, was responsible for the electron ejection" (Kimbrough 2005, p.5). Fortunately, scientists were "able to provide a completely satisfactory picture of the photoelectric effect by using the concept of the quantum nature of light" (Saleem 2015, p.3).

Both revolutionary approaches of these theories are of course deemed as a success for scientists. From that time onwards, the new goal for them was to test their theoretically derived predictions and in turn to see whether they are confirmed or not. On the other hand, these theories brought about at least two new issues. The first one was about the CM. As a well–established theory, CM was treated almost certain for centuries. Accordingly, its failure was crying out for a proper explanation. Of course, one might encounter such revolutionary moments in the history of science. For example, the domination of the Aristotelian paradigm came to an end nearly a thousand years after it was introduced.

[2] For some, Quantum Mechanics, perhaps, cannot be considered as a prosperous theory when compared to General Relativity or Classical Mechanics. Nevertheless, logical positivists' main tendency was to see Quantum Mechanics as a theory.

Although "Aristotelian [theory of] physics [was] correct, [it was] non–intuitive approximation of Newtonian physics", *i.e.,* CM (Rovelli 2015, p. 23). So, in a sense, its impact was not so enormous. Preeminently for historians of science, therefore, it was pretty different, as much as a hard task to explain this revolution. The second issue was related to the successes of General Relativity and Quantum Mechanics. The crux of the issue here is to explain how both theories were so insightful about natural phenomena. In detail, parts of the issue include: (a) how these two theories should be evaluated; (b) how their predictions are so accurate; and (c) how their explanations are better than the best competing (*i.e.* CM) explanations.

1.3 Philosophy in the Early Twentieth Century: A Very Short Overview

While science was in a state of revolutionary process; philosophy was contemporaneously going through an upheaval. Especially, reactions to the German Idealism divided philosophical movements into two main camps both with its opponents and its advocates (see, Disley 2013). The proponents of Neo–Hegelianism were on the latter side and have not entirely discarded German Idealism. Their main aim was to fill gaps in this idealistic philosophical system. In the following, neither Neo–Hegelianism nor its particular lines of thought will be discussed; instead the remaining sections are devoted to the opponents of German Idealism. To put it more specifically, the following sections will broadly cover Wittgenstein's works and their influence on other philosophical movements. Such an approach is not arbitrary; rather, as will be seen in the next section, Wittgenstein's works heavily influenced the Vienna Circle where the doctrine of Logical Positivism was born. Therefore, in this section, the situation of philosophy in the early twentieth century is limited to one aspect, *i.e.* to the philosophical reaction against German Idealism.[3]

On 19 August 1918, just three years before *Tractatus Logico–Philosophicus* was published, Wittgenstein wrote a letter to Bertrand Russell, indicating his main lines of philosophical thought as follows:

[3] In historical manner, one can trace back the criticism of German Idealism to Auguste Comte (1798-1857), Gottlob Frege (1848-1925). However, the validity of such historical approach is seemingly controversial. For example, Hayek (1952/2010) states that philosophies of Comte and Hegel share many similarities. Nikolay Milkov (2015) argues that "Frege actually integrated elements of German philosophical idealism into his logic" (p. 89). In this sense, Wittgenstein's criticisms against German Idealism are relatively crystal clear. Perhaps, Bertrand Russell (1872-1970) can be counted as an exception.

> I'm afraid you haven't really got hold of my main contention, to which the whole business of logical prop[osition]s is only a corollary. The main point is the theory of what can be expressed (*gesagt*) by prop[osition]s – *i.e.* by language – (and, which comes to the same, what can be *thought*) and what cannot be expressed by prop[osition]s, but only shown (*gezeigt*); which, I believe, is the cardinal problem of philosophy (Letter to Russell [19.08.1918] in McGuinness 2008, p. 98).

This quotation clearly manifests the project of Wittgenstein, in a nutshell. His main aim was to distinguish between saying and showing. Such an aim also points out that there is a significant difference between *what can be said* and *what can be shown*. "The main contention of the work" of Wittgenstein was to "fit together" these two themes (Landini 2007, p.77). To make more clear his idea, think of an oak tree. When I point it out, I only show it as a part of the external world. Nonetheless, if I utter the sentence, 'It is an oak tree', I am not at the level of *showing*; rather I am at the level of *saying*. The first situation of showing could not be described at the propositional level because the oak tree itself is not a unit of language; in the second situation, the level is not marked on the limits of external world, by contrast, this is the level of thought in which propositional expression is available. According to Wittgenstein, the cardinal problem of philosophy is to give a logical account of the link between these two levels, namely in its original form, the level of *gesagt* and the level of *gezeigt*.

A careful look would reveal that Wittgenstein is addressing the fundamental problem of the relationship between the language and the external world. I will not go into details about the issue here, but it is sufficient to think that the picture theory of representation in *Tractatus* was proposed as a solution. For my purposes, one aspect of Wittgenstein's approach is noteworthy. The sentences, like I gave in the example, are not problematic because the term (oak) corresponds to the object (the oak tree itself) that belongs to the external world. That sort of correspondence relation is not limited to the terms and objects; it also holds among the elementary propositions and the state of affairs. As Wittgenstein (1922/2002) puts it "the simplest kind of proposition, an elementary proposition, asserts the existence of a state of affairs" (p. 36).

Wittgenstein himself has always avoided giving a specific example for elementary proposition. Moreover, we also come across some dubious statements about this in his philosophical system. One of the scholars of Wittgenstein who drew attention to this issue was Brian F. McGuinness (2002):

> Briefly the difficulties with which this confronts us are the following: in the first part of the *Tractatus*, notably in the 3's and early 4's, we seem to be told that the essence of a proposition is to be a picture, while in the later parts we are told that its essence is to be a truth-function… The 'picture theory' requires further elaboration, and the truth-function account of what it is to be a proposition seems to involve a circularity by presupposing a prior understanding of what it is to be an elementary proposition. But a more serious difficulty is that the two accounts seem to be quite separate things, and, if this is so, cannot both be adequate accounts of what it is to be a proposition (pp. 65-66).

Perhaps, this quotation clearly indicates the so–called uncertainty. We may assume that even Wittgenstein was not sure about what an elementary proposition is. Nevertheless, my concern is quite something else. I aim at revealing how some of the Vienna Circle members have evaluated Wittgensteinian approach. To this end, the following remarks are of interest in connection with The Circle's general reception of Wittgenstein.

In this manner 'The oak tree on the right side of the river is diseased by Anthracnose' is an example of an elementary proposition that describes a state of affair. If the referenced object is actually an oak tree and is diseased by Anthracnose, then the proposition expressed is true and meaningful as well. On the other hand, the referenced tree might be olive instead of oak. In this case, the expressed proposition would be false, but would still be meaningful.

True and false sentences are unproblematic, but the meaningless ones are. According to Wittgenstein, philosophy, especially the version that the school of German Idealism adopts, is completely surrounded by the sentences of the latter sort. In his words, "most of the propositions and questions to be found in their philosophical works are not false but nonsensical" (Wittgenstein 1922/2002, p. 22).[4] To avoid

[4] I have to mention again the name of Frege here because "of all philosophers, it is Frege whom Wittgenstein held in greatest esteem" (Beaney 2016, p. 74). With the works of him, many philosophers have sought the meaning in sentences (or specifically in propositions) but not in ideas. In the introduction part of *The Foundations of Arithmetic* (1884/1960), Frege makes explicit the three fundamental principles that will guide his research. One of the principles he kept himself is "never to

nonsense, for Wittgenstein, a novel method should be employed. Once such method is appropriately employed, the never-ending philosophical problems, though perhaps not solved, but would be disappeared or at best shown to be pseudo-problems. The method, as far as it goes, is straightforward: distinguish metaphysical statements from the others because they are (namely, metaphysical statements) meaningfully unfounded and need to be eliminated.

Speaking of such a simple method seems an easy task; nevertheless application of it is not equally easy. In general, the propositions are complex, and decomposing them into elementary propositions is nearly an impossible task. As G. E. M. Anscombe (1965) similarly states "it is impossible to regard generalized propositions that relate to an infinitely numerous universe as truth–functions of elementary propositions" (p. 26). Furthermore, Wittgenstein did not develop any strategy to overcome this challenge. All we know is that elementary propositions express states of affairs and those propositions consist of names that denotes simple objects, like an oak tree. Norman Malcolm, in his personal communication, asks Wittgenstein, at least, to explain what generally the simple objects are. The unsatisfactory response he received is as follows:

> I asked Wittgenstein whether, when he wrote the *Tractatus*, he had ever decided upon anything as an *example* of a 'simple object'. His reply was that at that time his thought had been that he was a *logician*; and that it was not his business, as a logician, to decide whether this thing or that was a simple thing or a complex thing, that being a purely *empirical* matter! (Malcolm 2001, p. 70).

It seems that Wittgenstein left his promising project unaccomplished by withdrawing the issue at this specific point. He decided to leave this issue for empirical research and headed to a small Austrian village to spend the rest of his life until his sudden decision to return to Cambridge. Strictly speaking, he found it more interesting to be an elementary schoolteacher, than dealing with elementary propositions (see Monk 1990, pp. 192–204; and also Peters and Stickney 2018, pp. 59–85).

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ask for the meaning of a word in isolation, but only in the context of a proposition" (p. xxii). That is to say, the same concept might have different meanings in different sentences. So, the meaning of a concept should be evaluated within the context of the sentence in which it appears. Frege's perspective for the contextual definition of the concepts provided a fruitful ground, such that many philosophers avoided tackling a problematic issue that arose in classical epistemology. In sum, the message was plain and simple: concepts or ideas cannot take truth values, but sentences can.

During this period, a group of intellectuals, who call themselves Vienna Circle, were carefully following the works of Wittgenstein, especially the ones on the status of the elementary propositions. The Circle was established in the city of Vienna where intellectuals from various fields came together. They were incorporating science oriented targets into their philosophical views. In 1926, they decided to devote two semesters to an in-depth study of *Tractatus*. Donald Gillies (1993) comments on this study plan as follows:

> Wittgenstein's *Tractatus* theory of meaning is based on the notion of elementary (or atomic) propositions, and we might well expect that he would clarify this notion by giving some examples of elementary propositions. But he never in fact does this…Granted this obscurity in the *Tractatus* itself, it was up to the Vienna Circle to clarify the matter, and this they did by identifying elementary propositions with simple observation statements, or protocol statements (p. 167)

In 1929, Wittgenstein quits his notorious teaching career and decides to move back to Cambridge. Although, he learns that his views on philosophy were embraced at least by some of the members of Vienna Circle, he never felt belonging to them. A possible reason for this was the weaknesses of *Tractatus* because his further book *Philosophical Investigations* reflects the aim of closing any identified gaps of it. Conceivably, Wittgenstein was not convinced by his arguments on several matters, but his remarks certainly accomplished a philosophical breakthrough. From this point on, Vienna Circle, specifically Logical Positivists, took their turn and embarked on to complete relay race.

1.4 Vienna Circle, Logical Positivism, and the Philosophy of Science

The revolution in science and the breakthrough in philosophy have both affected their fields in a significant sense. Their traditions that prevailed for centuries have taken on new forms or abandoned partially. Both fields with their new theories shaped the future of technology, politics, and many other intellectual disciplines. However, due to its subject matter, philosophy of science was the discipline that was most devastatingly affected by this new era. By all means, this was not a huge surprise.

The reception of the new scientific and philosophical developments was not so difficult for the Vienna Circle. The reason is that the members of The Circle were

both scientists and philosophers. The founder of The Circle was Moritz Schlick (1882–1936). He was a philosopher and a trained physicist as well. Rudolf Carnap (1891–1970), Herbert Feigl (1902–1988), Kurt Gödel (1906–1978) and Otto Neurath (1882–1945) were other notable members, among others. In addition to philosophy, all these names were experts in many fields from mathematics to economy.

At the beginning, Vienna Circle has been launched under the name *Verein Ernst Mach, i.e.* Ernst Mach Association. Affiliating the name of Ernst Mach (1838–1916) actually gives us a clue about the philosophical and scientific tendency of the association. Mach's general approach to science is built upon sensations. For him, all scientific –theoretical– concepts can and should be should be reducible to those that can be verified trough direct sense experience. Science, according to Mach, is "a collection of statements about the connections among sense perceptions" and its aim should be to acquire the relations among the physical phenomena (Frank 1917/1941, p. 29). Therefore, if a term or statement goes "beyond the limits of sense experience", then it "could not have any scientific foundation" at all (Brush 1968, p. 199). In this manner, Mach was essentially an empiricist.

It will be a little exaggeration to say that Mach was the "real master of the Vienna Circle" because his thoughts deeply influenced the members of the association (Frank 1917/1941, p. 212). For this reason, they have been often called logical empiricists. Nonetheless, it is argued that logical empiricism is not an appropriate nomenclature for the Vienna Circle. Given this view, it would be more convenient to characterize the members of the Berlin Circle as logical empiricists. This seems to be a fair claim because as a pioneer of the Berlin Circle movement, Hans Reichenbach (1891–1953) himself clearly states the point where they differ from the logical positivists:

> [I]t seems to me to be at least doubtful whether this reduction to perceptual reports and pure logic exhausts everything we mean to include in our assertions about reality. These doubts are principally aroused when we consider the use of the concept of probability in the natural sciences, for if we accept [logical positivistic] reduction of scientific assertions, we forfeit the indisputable basic principle that such assertions are not merely reports of past perceptual experiences, but are also invariably predictions of future perceptual experiences. It is a puzzle to me just how [logical positivism] proposes to include assertions of probability in its system,

> and I am under the impression that this is not possible without an essential violation of its basic principles (Reichenbach 1933, p. 407)

The nuance between logical positivism and logical empiricism lies in the issue of scientific prediction and probability. Reichenbach and his followers assert that such logical positivistic approach does not reflect the actual scientific practice. For one reason: scientific statements are not all about immediately given phenomenal experience; they also make assertions about future cases that did not yet happen. Accordingly, logical positivists "could not develop a theory of propositions about the future", and thus Reichenbach and his "Berlin group could not accept positivism" (Reichenbach 1936, p. 152).

This subtle distinction drawn by Reichenbach, at least, reveals that the members of the Vienna Circle should be called as logical positivists –not as logical empiricists (see for a detailed explanation Uebel 2013). Nevertheless, it seems hard to exactly define what logical positivism movement is. Moreover, it is equally uneasy to identify its members as well. Several studies in the last couple of decades have already supported this view. A growing body of literature shows that logical positivism is at best misunderstood and at worst denigrated. Michael Friedman (1999), for example, highlighted that logical positivism is crudely represented in some of the works of philosophers of science. Other studies also found that the positivist philosophy of science is fabricated in particular contexts (Reisch 1991; Irzik and Grünberg 1995).

Keeping this context in mind, I limit myself to the issues which are sufficient for my purposes. Since my aim in this chapter is to trace back the historical roots of model-centric view, I will now move on to the structure of scientific theories. In the following, I present two views concerning how theories should be characterized, namely syntactic and semantic characterizations of science.

1.5 The Structure of Scientific Theories

To begin with, I need to make some terminological and conceptual remarks, and provide a bit of background information. According to the advocates of syntactic view, science consists of two distinct languages: theoretical and observational. To express it in a symbolic way, the language of science is $\mathbf{L_{TO}}$. The theoretical

language ($\mathbf{L_T}$) contains a vocabulary of unobservable terms, while the observational language ($\mathbf{L_O}$) involves a vocabulary of observable terms. Given the proponents of syntactic view, what needed to be done is to provide a dictionary that would succeed in defining the terms of $\mathbf{L_T}$ by means of the terms of $\mathbf{L_O}$. If such a dictionary allows us to present the intended definitions, then we can, for example, interpret the theoretical terms *i.e.,* we can provide "observational interpretation of theoretical terms and statements" (Suppe 1971, p. 57).

The idea of linking $\mathbf{L_T}$ to $\mathbf{L_O}$ is based fundamentally on Frank Ramsey's (1903–1930) formal method which later was called *'Ramsey Sentence'*. In his seminal work, Ramsey (1931) attempts to provide a "dictionary being in the form of equivalences" (p. 231). This attempt consists in suggesting a biconditional relation between the theoretical and observational sentences. As it is clear, the term 'dictionary' is not used arbitrarily because the literal dictionary definition of a word is its meaning, and vice versa. Ramsey's dictionary could be understood in a similar fashion. By means of such a dictionary, the terms belong to $\mathbf{L_T}$ (*e.g.* 'molecules', 'genes') are defined within the glossary of $\mathbf{L_O}$ with respect to the properties ($x_1, x_2, \ldots x_n$) they have.

From this point of view, 'magnetism', for instance, is apparently a theoretical term, but essentially it is only an abbreviation for a particular observation of a phenomenon. If magnetism is defined *merely* in terms of its physical behaviors, then not only magnetism but also all other theoretical terms would not cause any difficulties. For example, one might define a particular magnetic object as follows: "*def.* x is a magnet ≡ x attracts every iron object in its vicinity" (Hempel 1965, p. 182). As is seen, an equivalent definition of a magnetic object is given in terms of the $\mathbf{L_O}$. In logical sense, the right and left side of the equation means exactly the same thing in different languages. To express it differently, x is magnetic if and only if being x is nothing over and above attracting iron objects toward itself. The same goes with the term disease. When one utters the expression 'This oak tree is diseased by Anthracnose', she essentially means that the oak's foliage is malformed and its leaves have black spots on it. In sum, theoretical terms are appropriate for science, only if they are "explicitly defined in terms of phenomena –or phenomenal language–, and are nothing other than abbreviations for such phenomenal descriptions" (Suppe 1977, p. 11).

The syntactic view of scientific theories will be discussed in the following subsection **§1.5.1**. On the other hand, in **§1.5.2** a different approach to the scientific theories will be discussed. This is called the semantic characterization of scientific theories which first put forward in the late 1960s. Both views share the common idea that theories are structures, but they differ on what really the structure is. The main purpose of this section is to provide details about where the differences lie.

1.5.1 Syntactic view of scientific theories

In the preceding section, I have outlined the *Reichenbachian* nuance between logical positivism and logical empiricism. This nuance might be significant for different purposes, but beyond this there are some overlaps between the two approaches. One of them is the affinity between the terms 'context of justification' and 'rational reconstruction'.

As a logical empiricist, Reichenbach maintained that epistemology and psychology are distinct domains. In his view, the subject of epistemology is confined to the context of justification. If one can explicitly express her ideas in an external manner and also can specify how this idea is justified, then she remains in the domain of epistemology. On the other hand, if one deals how she came up with a particular idea, then she is in the domain of psychology. As the story goes, for example, Newton has discovered the laws of gravity with the help of an apple getting hit on his head. According to Reichenbach, this can certainly be a true story but it cannot be a genuine subject for epistemology. However, the logical relationship among the discovered laws or the logical interconnections between these laws and other scientific statements are proper subjects for epistemology. As he puts it:

> The way, for instance, in which a mathematician publishes a new demonstration, or a physicist his logical reasoning in the foundation of a new theory, would almost correspond to our concept of *rational reconstruction* [italics mine]; and the well–known difference between the thinker's way of finding his theorem and his way of presenting it before a public may illustrate the difference in question. I shall introduce the terms *context of discovery* and *context of justification* to mark this distinction (Reichenbach 1938, pp. 6–7)

The distinction of these contexts was crucial for Reichenbach. It was mainly because nearly all misunderstandings and confusions about epistemological topics lie in a lack of distinction between contexts of discovery and justification. Reichenbach was

paying great attention to the context of justification because he believed that only this way, our statements would be interpreted precisely. The field of epistemology, however, was lacking such precision. Moreover, such lack of precision was not unique to epistemology. Science, for example, was producing reliable knowledge but it was also including imprecise language. Therefore, science also requires "a bit of rational reconstruction" as well, and "only philosophers [hold] the tools needed to eliminate such imprecision" (Richardson 2006, p. 42).

According to Reichenbach, Carnap was the philosopher of science who has presented the most systematic approach to the issue. Even for this reason, in a footnote, he cites Carnap's main work *Der logische Aufbau der Welt*. As also can be seen from the quote above, the context of justification and the method of rational reconstruction are actually carrying the same message: 'make explicit the logical relations between statements'. In the following, I will try to interpret how Carnap embraced this message and used it in evaluating scientific theories.

Like any other language, the language of scientific theory has syntax. As Carnap (1942) puts it "we are in the field of (logical) *syntax*, if [w]e abstract from the designata and analyze only the relations between the expressions" (p. 9). Actually, the syntax of a language constitutes the grammar of language. Thus one needs to follow the grammatical rules while logically reconstructing a scientific theory. Carnap had the seed of this idea in his days in Vienna and supposed it as a useful formula for solving philosophical problems:

> The chief motivation for my development of the syntactical method, however, was the following. In our discussions in the Vienna Circle it had turned out that any attempt at formulating more precisely the philosophical problems in which we were interested ended up with problems of the logical analysis of language. Since in our view the issue in philosophical problems concerned the language, not the world, these problems should be formulated, not in the object language, but in the meta–language. Therefore it seemed to me that the development of a suitable meta–language would essentially contribute toward greater clarity in the formulation of philosophical problems and greater fruitfulness in their discussions (Carnap 1963, p.55).

Although Carnap argues that a proper meta–language should be developed to solve philosophical problems, it can further be argued that this meta–language can be appropriately applied to the problems of various scientific areas. At this point,

syntactic view seems inadequate because it does not seem to reflect scientific theories being commonly used 'in the wild'.[5] True, scientific theories are not represented by scientists as in syntactic or in axiomatic ways. However for the proponents of syntactic view "a theory is best viewed an axiomatic structure" and if a theory is scientific, it is in principle "could be axiomatized" (Bechtel 1988, p. 28). At first sight, it is likely to benefit from such a normative attitude. For example, by means of rational reconstruction, one can give a rational account of scientific knowledge by replacing "the concepts of science with logically precise counterparts" (Halvorson 2016, p. 588). However, such a normative attitude towards science gradually brought about undesirable results. Let me discuss two of them briefly.

Recall that Carnap found firm ground in *Ramsey Sentence* where he anticipated providing a translation between theoretical terms and observational terms. To put it roughly, theoretical terms cease to be a problem in this way, and this leads to a possible reconstruction of a scientific theory with the sentences that only contains observational terms. Elements that build bridge between theoretical and observational terms are called correspondence rules (or bridge principles). By means of these rules, the connection between $\mathbf{L_T}$ and $\mathbf{L_O}$ becomes possible. According to Carnap, it is vital to make this connection, because otherwise on a purely axiomatic or syntactic level, theories could not be linked up to the empirical domain. By all means, scientific theories are not purely mathematical. They are not devoid of physical meaning and thus they are about a specific domain that belongs to the external world. For this reason, a theory's logical syntax requires an interpretation.

I have mentioned before how sentences that contain theoretical terms ('magnetism', 'gene' *etc.*) are properly interpreted in terms of observational terms. Nevertheless, not all theoretical terms used in science are as unproblematic as they may appear to be. For example, there are dispositional terms that fall under the category of theoretical terms. In general sense, a dispositional term indicates a property which requires specific condition(s) for being empirically manifest. Popular examples for dispositional predicates include flammable, fragile and soluble. Take for instance a

[5] The point is to develop a philosophical account that explains scientific practice. So the question is not the one about history of science —how scientists actually represent their theories and understand them; but rather about philosophy of science —how scientific theories should be understood? So, one must keep in mind the distinctions between philosophy, psychology and history.

pinecone, or a lump sugar. It might, for sure, be possible to identify some of their properties by simple observations. But, no matter how many observations are made, one could not be in a position to state whether a pinecone is flammable or a lump sugar is soluble -unless she throws them into the fire and to the water, respectively. This seemingly raises a problem: how dispositional terms can be cast in terms of observational terms? According to Carnap, there is a way to figure out this problem by defining such dispositional-theoretical terms as conditional statements. For example:

def. x is soluble in water ≡ (t) [x is placed at the water at the time t ⊃ x dissolves at the time t]

In this case, the theoretical sentence that states 'x is soluble' is defined by means of 'if-then statement' as in the forms of observational sentence. As Carnap (1936) states, "we could define 'soluble in water' in the following way: 'x is soluble in water' is to mean 'whenever x is put into water, x dissolves'" (p. 440). Notice that theoretical sentences that include dispositional terms do not have explicit definitions as some other sentences do (such as the sentence that includes the term 'magnetism') because dispositions are manifested under particular conditions. Therefore, it is suggested that dispositional terms would be appropriately defined not by protocol sentences, but by conditional sentences.

To be clear, Carnap himself was also aware defining dispositional terms this way is somewhat problematic. He suggests, primarily, to symbolize the above conditional statement as P_1, P_2 and P_3 each respectively means '*soluble in water*', '*the body x is placed into water at the time t*' and '*the body x dissolves at the time t*'. Now the statement can also be expressed as follows:

$$P_1 \equiv (P_2 \supset P_3)$$

The problem is this: given predicate logic, a sentence in a conditional form is true when the antecedent is false. This means that if P_2 is false, then $(P_2 \supset P_3)$ is true. As a consequence $P_1 \equiv T$ *i.e.* P_1 is true. For example, whenever x is *not* placed at the water at the time *t*, x would be soluble. Following this logic, Carnap (1936) states that one can undeniably assert a particular pinecone which is "completely burnt yesterday"

would be “soluble in water” as well (p. 440). As a matter of fact, though, pinecones do not dissolve in water.

In order to avoid this unintended conclusion, Carnap introduces reduction sentences. These sentences take a different form than the above conditional statement:

$$P_2 \supset (P_1 \equiv P_3)$$

To express it in words: “if anything x is put into water at any time *t*, then, if x is soluble in water, x dissolves at the time *t*, and if x is not soluble in water, it does not” (Carnap 1936, p. 440–441). As it is understood, Carnap suggests a way out to exclude the objects that could/have never been placed at the water. In this manner, he presents a framework for defining dispositional terms on the basis of observational terms.

Carnap believed that an appealing solution to the issue would be possible with the help of reduction sentences. However, a logically similar (but conditionally different) problem appears. In this case, the new condition of being soluble becomes P_2 and this excludes the cases in which such conditions are not met. For example, the status of lump sugars that was not thrown into the water is uncertain, since they are not placed in the water at any time *t*.

This problem exhibits the inherent difficulty of reduction sentences because the new version of them cannot define dispositional terms in a complete manner either. To be clear, the condition of being soluble for any x depends on the antecedent of the original conditional statement. In other words, the biconditional statement $P_1 \equiv P_3$ tells us nothing if P_2 is not present. This, in turn, shows that Carnap’s framework does not account for defining dispositional terms on the basis of observational terms.

Actually, Carnap tried to figure out this problem. For this reason, he further suggested that dispositional terms could be defined partially by the reduction sentences. In other words, when the antecedent condition is present for any x, a reduction sentence can *possibly* define. In opposite conditions, however, it cannot. The way to express dispositional terms more sharply is to pair the reduction sentences. The more reduction sentences we pair, the closer we get to the meaning of theoretical terms. Carnap develops this idea as follows:

> A set of reduction pairs is a partial determination of meaning only and can therefore not be replaced by a definition. Only if we reach, by adding more and more reduction pairs, a stage in which all cases are determined, may we go over to the form of a definition (Carnap 1936, p. 449–450).

When different test conditions are identified, the number of partial definitions of particular object x increases. It means that as the number of relevant reduction sentences increases, we get closer to forming an explicit definition of x. For example, different solvents (S_1, S_2, S_3, …S_n), other than the water, could show that x is soluble. Similarly, a fictional device, call it SolDet (solubility detector) for instance, might detect x as soluble by means of signals, such that it can be understood when its monitor displays positive (+). Perhaps, one can expand the list of test conditions by adding different methods, but, I believe, our example would also suffice to explain Carnap's point. So, to this point, solubility for x is determined by the following tests:

(a) If x is put into water at *t*, then, x is soluble in water *iff* x dissolves at *t*.

(b) If x is put into S_1 at *t*, then, x is soluble in water *iff* x dissolves at *t*.

(c) If x is put into S_2 at *t*, then, x is soluble in water *iff* x dissolves at *t*.

(d) If x is put into S_3 at *t*, then, x is soluble in water *iff* x dissolves at *t*.

(e) If x is tested by SolDet as (+) at *t*, then, x is soluble in water *iff* x dissolves at *t*.

As it is seen, (a) is not the mere test condition for defining the disposition of solubility. In addition to this, we have (b), (c), (d) and (e). Thus the term 'soluble' is defined far better. According to Carnap, dispositional terms should be understood as open ended concepts. Such open–ended concepts would have better definitions when further researches, experimental developments and different testing methods are involved.

According to many interpretations, however, characterization of dispositional terms as open–ended concepts is seen as too weak or too liberal (see Scheffler 1963, Ch. 2, §10; Bechtel 1988; Halvorson 2016). The reason is that these terms are less likely to have any explicit definition. When we consider the different solvents or the fictional device SolDet, the interpretation seems acceptable. As a theoretical term, one could partially define any dispositional property in almost numerous ways. In this way, one could also obtain a more explicit definition of the dispositional property within the

observational terms. However, Carnap did not provide logical criteria that would make a definition of a dispositional term explicit enough. Accordingly, there seems no way to define a dispositional term as explicit as a non–dispositional theoretical term. Recall from the preceding section that the proponents of the syntactic view were aiming at creating a dictionary by means of *Ramsey Sentence*. On the face of it, this aim was not met due to the liberal status of the reduction sentences. If considered that the main purpose of the *Aufbau* was to create such a dictionary, one could conclude that one of the central tenets of the syntactic view is weakened to some extent.

It could be said that *Ramsey Sentence* is not a suitable logical tool for translating some theoretical terms into observational terms –especially with its compromising feature. This could be counted as a defect for the syntactic view of science. For one reason: correspondence rules play a vital role for rational reconstruction of science, but reduction sentences seem far from being such a rule (or at least a strict rule). Could another logical tool be more appropriate?

Adherents of the syntactic view gave it another try and once again attempted to answer this question. At the time, a similar approach to Ramsey's work was put forth by William Craig (1953). According to some proponents of the syntactic view, Craig's theorem might be of use.

Craig's theorem, in a nutshell, states that a sub–vocabulary (**SV**), that captures all vocabulary of an axiomatic recursive first order theory (**T**) could be used to construct another theory (**T***) whose theorems are no more different than the (**T**). The formulation of the theorem was containing significant consequences for the specific purposes of the syntactic view. As one of the developers of logical empiricism, Carl G. Hempel (1905–1997) discussed how, if possible, *Craig Theorem* can be used to eliminate the theoretical terms from scientific theories. In detail, his main aim was to explore whether "the systematization achieved by general principles containing theoretical terms can always be duplicated by means of general statements couched exclusively in observational terms" (Hempel 1965, p. 182). He accepts that if a sub–vocabulary of **T** is limited to observational terms, then it would be possible to construct a **T*** with that specific sub–vocabulary. This allows us to dispense with the

requirement of the original theory **T** which includes theoretical terms. However, the resultant set of all conditional statements of **T*** (*e.g.* x is a magnet *if and only if* x attracts every iron object in its vicinity) would be "extremely unwieldy", while, "as actually presented" **T** "provides a concise way of representing a disparate class of consequences' (Kitcher 2001, p. 153). Moreover, according to Hempel, one could argue that theoretical terms are redundant and can rationally be eliminated. However, for him, this is true only if the scientific theories are taken as deductive systems. Nevertheless, inductive systematization should also be taken into account. As he puts it:

> [If] the sole purpose of a theory [is] to establish deductive connections among observation sentences, [then] theoretical terms would indeed be unnecessary. But if it is recognized that a satisfactory theory should provide possibilities also for inductive explanatory and predictive use and that it should achieve systematic economy and heuristic fertility, then it is clear that theoretical formulations cannot be replaced by expressions in terms of observables only (Hempel 1965, p. 222).

Hempel draws attention to the inductive aspect of the theories. This ostensibly indicates that syntactic characterization of scientific theories is a dead end. Thus, the idea of rational reconstruction of scientific theories in a syntactic fashion is logically misleading. Neither *Ramsey Sentence* nor Craig's Theorem provide a logical means for translating theoretical terms into observational terms (*cf.* Hooker 1968). In other words, the problematic status of correspondence rules indicates that the alarm bells are ringing for the syntactic view of science.

1.5.2 Semantic view of scientific theories

There is no exact date as to when criticisms against the syntactic view (or the received view) of theories arose. Nevertheless, Frederick Suppe states when this view precisely came to an end, as follows:

> The Received View on Theories was the epistemic heart of Logical Positivism. Twelve hundred persons were in the audience the night it died. It was March 26, 1969 –opening night of the Illinois Symposium on the Structure of Scientific Theories. The Received View had been under sustained attack for a decade and a critical mass of main protagonists had been assembled to fight it out. Carl Hempel, a main developer of the Received View, was the opening speaker and was expected to present the Received View's latest revision. Instead he told us why he was abandoning both the Received View and reliance on syntactic

> axiomatizations. Suddenly we knew the war had been won, and the Symposium became an energized exploration of where to go now (Suppe 2000, pp. 102–103).

Suppe's expressions, which may seem dramatic to most of us, can be divided into two important parts: The first part is destructive, and the second is constructive. The destructive part of Suppe's claim could be interpreted as the deadlock of logical positivism. In other words, Suppe states that logical positivists were apparently not capable of maneuvering to sustain their syntactic view of theories. To be clearer, the problematic status of correspondence rules has already been discovered by Carnap and Hempel, who actually advocate the syntactic view of theories. That is why Hempel himself agreed to surrender and abandoned the program of the Vienna Circle, and thus the 'Received View'. On the other hand, the constructive part of Suppe's claim is about the possible research path to be followed. This part covers the need of focusing on scientific theories with a new perspective. In this section, I discuss the alternative of the syntactic view, *i.e.* the semantic view of scientific theories.

As Suppe states the war against the received view has been won. The upcoming task was to decide where to go now. Paul Feyerabend (1924–1994) claimed that the virtue of formalizing theories in science became questionable. For this reason, he suggested that the philosophical approaches that aim to formalize scientific theories should not be adopted from this time forth:

> What we must do is to replace the beautiful but useless formal castles in the air by a detailed study of primary sources in the history of science. This is the material to be analyzed, and this is the material from which philosophical problems should arise. And such problems should not at once be blown up into formalistic tumors which grow incessantly by feeding on their own juices but they should be kept in close contact with the process of science (Feyerabend 1970, p.183).

According to Feyerabend, the project of theory formalization was failed and thus insisting on this type of approach to theories would be pointless. True, abandoning the approach of theory formalization would mean a lot of uncertainty or low level precision. However the nature of science would be better understood by means of focusing mainly on the historical aspects of science. On the other hand, some philosophers of science were not inclined to agree with Feyerabend. They were

maintaining the *status quo*, such that most of them were thinking that the syntactic view has failed not because its commitment to the idea of formalization, but because its insistence on adopting the framework of logical syntax.

The first move of this project was to use model theory in mathematical logic. According to the proponents of the semantic view, model theory would better serve in the formalization of scientific theory. Model theory finds its roots in Alfred Tarski's works on set theory (1901–1983). According to Tarski truth can be defined in terms of satisfaction relation. To put it roughly, the open formulas (like *'α is a conductor'*) that belong to a particular language L are either satisfied or not in a particular domain. If a specific domain Dc contains conductors, then the formula is satisfied by silver, gold, copper, aluminum etc. So, the sentence 'silver is a conductor' is true in this specific domain Dc. Open formulas can be expressed as true sentences by means of satisfaction relation. If the true sentences of a particular theory T satisfy the model M, then M is a realization of T. As Tarski (1953) states, a model of T is "a possible realization in which all valid sentences of a theory T are satisfied" (p.11).

In this manner, models are not linguistic entities, but rather they are model structures. More specifically, open formulas are true in a given structure. This point somehow explains the semantic part of the semantic view of theories. Recall that syntactic view was dealing with relationships among words and sentences. On this view, it was possible to translate theoretical terms into observational terms because it was assumed that there was a linguistic equivalence between the two terms. However, semantic view deals with the relationship between words and objects. From this point of view, there is no such thing as linguistic equivalence because first and foremost, objects are extra-linguistic entities themselves. As Everett W. Hall (1944) asserts "extra–linguistic facts cannot be translations or translated" (p. 30). Therefore, in Tarskian terms, considering models as extra–linguistic entities would additionally explain why semantic view is sometimes called as model–theoretic view.

When we talk about the term 'model' in *Tarskian* sense, we often understand that the term refers to the term model which is used in mathematical logic. Instead, the term 'scientific model' seemingly refers to something entirely different. For example, the

model of the atomic structure, the rational choice model in game theory or the physical model of a bridge all seem refer quite different models from the Tarskian term of the model. However, according to one of the developers of semantic view, Patrick Suppes, the Tarskian term of the model captures all the different models in science:

> I claim that the concept of model in the sense of Tarski may be used without distortion and as a fundamental concept in all of the disciplines ... In this sense I would assert that the meaning of the concept of model is the same in mathematics and the empirical sciences (Suppes 1960, p. 289).

According to Suppes, the reason why the term model is seen differently in other disciplines is closely related to the use of the term itself. However, the meaning of the term is essentially the same with respect to the many branches of science. Therefore, if Suppe is right, the term model refers to the term model in mathematical logic and this context holds across many disciplines of the scientific research. Bas van Fraassen similarly, but more strictly, argues that the scientists' practical use of the term model is similar to the term used in mathematical logic:

> The use of the word 'model' in this discussion derives from logic and meta-mathematics. Scientists too speak of models, and even of models of a theory, and their usage is somewhat different. 'The Bohr model of the atom', for example, does not refer to a single structure. It refers rather to a type of structure, or class of structures, all sharing certain general characteristics. For in that usage, the Bohr model was intended to fit hydrogen atoms, helium atoms, and so forth. Thus in the scientists' use, 'model' denotes what I would call a model-type. [Therefore] the usages of 'model' in meta-mathematics and in the sciences are not as far apart as has sometimes been said (van Fraassen 1980, p. 44).

In fact, the views of Suppes and van Fraassen sum up nicely a somewhat hasty acceptance of the semantic view. In the following decades, both philosophers of science relaxed their views on the term model. Both authors claimed that the term model used in scientific practice and the term of model as the structure of theories are separate. Nevertheless, they asserted that studying models in their different sense would be complementary in understanding science. For example, Suppes states that:

> There would seem to be no use in arguing about which use of the word model is primary or more appropriate in the empirical sense. My own contention is that the set-theoretical usage is the more fundamental. The highly physically minded or empirically minded scientists, who

> may disagree with this thesis and who believe that the notion of a physical model is the more important in a given branch of empirical science, may still agree with my systematic remarks (Suppes 2002, p.22).

Similarly, van Fraassen underlined that different approaches to models function complementary in characterizing science:

> Structural relations among models form a subject far removed from the intellectual processes that lead to those models in the actual course of scientific practice. So what is important depends on what is of one's interest. Accordingly, if one's interest is not in those structural relations but in the intellectual processes that lead to those models, then the semantic view of theories is nowhere near enough to pursue one's interests. Both interests are important [in] understanding science (van Fraassen 2008, p. 311).

On the face of it, at least, it has been accepted by both philosophers of science that the term model is used in two different senses. Nevertheless, examining the two different uses will contribute to the understanding of science in general. The issues about how theories can be formalized by means of models and how models function in science are apparently distinct. It is still a matter of open debate whether studying these two issues would be complementary in understanding science. I put this matter aside for now. Nevertheless, in the next chapter, I will briefly discuss this issue by focusing on the activity of modeling in scientific practice. For the present, let me move on to the outlines of the semantic or model theoretic formalization of science.

It would be appropriate to emphasize again that the semantic view is a philosophical position developed against the syntactic view. The main motivation for putting forward such a view is to remove some of the defects in the syntactic view. Suppe has provided the most refined list of what these defects are, and they are as follows:

> [First] theories are not linguistic entities; [second] correspondence rules were a heterogeneous confusion of meaning relationships, experimental design, measurement, and causal relationships some of which are not properly parts of theories; [and third] symbolic logic is an inappropriate formalism (Suppe 2000, p.103).

The first defect states that advocates of the syntactic view treat theories as linguistic entities. But why this is a deficiency? For one reason: as a syntactic linguistic system, theories always require interpretation. Hilary Putnam (1926-2016) asserts something similar when discussing the syntactic view of theory. According to him, this view accepts scientific theories as "partially interpreted calculi" (Putnam 1962,

p. 240). In general, the "notion of partial interpretation is intended to provide a reconstructive account of the *empirical* or *observational meaning* of theoretical terms" (Suppe, 1989, p.47). Scientific theories contain mainly mathematical statements, like differential equations. These equations describe the world around us, *i.e.* they have empirical meaning. To understand what these equations are about, however, additional interpretation is needed. So, a theory itself is nothing but a partially interpreted calculus. For example, take Gauss's Law concerning the magnetic field –in its differential equation form $\nabla . B = 0$. To understand the empirical piece of this equation, it is requisite to know that B denotes to the magnetic field. According to the semantic view, however, the framework of syntactic view do not allow to express 'the formal' at the level of 'the empirical' because, this view eventually would need to rely on the correspondence rules. As noted above, the correspondence rules have problematic status in connecting the two linguistic levels $\mathbf{L_T}$ and $\mathbf{L_O}$.

This takes us to the second defect of the syntactic view. Recall that correspondence rules purported to translate theoretical terms into observational terms. In this way, proponents of the syntactic view were having the opportunity to permit the interpretation of theory structure. If there were no such opportunity, physics, like mathematics, would remain at an un–interpreted level. However, as Carnap (1966) states "physics cannot have, as mathematical theories have, a splendid isolation from the world. Its axiomatic terms –'electron,' 'field,' and so on– must be interpreted by correspondence rules that connect the terms with observable phenomena" (p. 237). True, physics cannot be characterized as mathematics, because it aims to tell something about the world and thus must be empirically interpretable. Therefore, correspondence rules have a vital function for the 'Received View'.

According to the syntactic view, correspondence rules belong to the $\mathbf{L_T}$. These rules possess theoretical, logical and observational vocabulary at the same time. For this reason, as Suppe states, a correspondence rule possesses a hybrid or heterogeneous character. Since these rules are not acquired empirically, they are not the part of observation. Therefore, these rules are parts of the scientific theory. Viewed in this manner, when logical positivists speak of a theory, they actually mean theory (*T*) plus correspondence rules (*C*), *i.e.* (*TC)*. According to the semantic view, it is

misleading, as well as problematic, to treat correspondence rules as part of the scientific theories. It is because the list of these rules would expand continuously by the new developments of experimental methods. Let's recall the different solvents and the fictional SolDet device which I have mentioned in the preceding subsection. As we discover new solvents or develop different measuring devices such as SolDet, the correspondence rules will change in time, and so will the theory. In other words, with these new correspondence rules, the existing theory *TC* would be a new theory *TC'*. However, it is clear that *TC'* is not a new theory, it is still the same theory *TC*. What only changed is that we know "more about how to apply [TC] to phenomena" (Suppe 1977, p. 103). In this manner, the syntactic characterization of theories seems misleading.

The last defect of syntactic view is a general problem and is essentially responsible for generating the first two defects. As known, symbolic logic consists of two main branches: propositional logic and predicate logic. In propositional logic, we deal with the relation between basic propositions which are either true or false. Predicate logic, however, provides more tools than propositional logic by including predication and quantification. After all, both branches of symbolic logic operate on sentences. These operations inevitably led the adherents of syntactic view to take theories as sets of sentences. The source of the first defect lies in here.[6]

The reason of the second defect, namely the reason which lies behind the problematic status of correspondence rules was the insufficiency of symbolic logic. The defined rules of material implication led Carnap to develop correspondence rules as reduction sentences. Notwithstanding this development, reduction sentences weakly captured the nature of dispositional terms. Moreover, in due course, the set of reduction sentences would change with each new experimental development. This, in turn, would change the theory itself. In this case, however, the theory does not change, but in fact its assumptions or its auxiliary hypotheses do.

[6] It would not be appropriate to blame symbolic logic as the only reason for treating theories as linguistic entities. Undoubtedly, there were overlapping motivations that have also accelerated the treatment of theories as linguistic entities. For instance, the influence of Wittgenstein could be counted as a factor because he was the one who reduced all philosophical problems to issues of language and tried to develop a method which concerns the elimination of metaphysical sentences from philosophy.

The semantic view's criticisms of the syntactic characterization of science seem appropriate. Actually, as also its defenders admit, some defects do exist in syntactic view. Adherents of the semantic approach stressed that their characterization of scientific theories is devoid of these defects.

Primarily, semantic approach did not use formal logic to characterize scientific theories. Instead, they favored an emphasis on set theoretic or model theoretic approaches. Replacing symbolic logic with set/model theory had different implications than the syntactic view. The syntactic or axiomatic method was deriving theorems in a deductive way from the axioms (or laws) of the theory. On this view "the theory is identified with the set of axioms and its deductive consequences" (Suppe 2001, p. 9). According to the semantic view, generally, the Tarskian satisfaction relation should be favored instead of deduction. It would perhaps be useful to discuss idealized models in science to understand the satisfaction relation. For example, "no frictionless pendulum exists, nor does any body subject to no external forces whatsoever" (Giere 1988, p. 78). However, textbooks of science persistently refer to such things. How do we account for this situation? By claiming that, according to the semantic view, these models satisfy theories. For example, the idealized "simple harmonic oscillator […] satisfies the force law $F = -kx$" (Giere 1988, p. 78). In other words, the interpreted equation ($F = -kx$) is true (not in epistemological sense but in Tarskian sense) of the theoretical model, *i.e.* simple harmonic oscillator.

According to the semantic view "a theory is identified with a class of models. The models are 'non-linguistic entities' –that is, they do not contain propositions but are structures of elements" (Bailer-Jones 2009, p. 129). Identification is important here because the theory itself is not seen as a linguistic structure. The way to identify a theory is to view it as a family of models. According to the semantic view, the only function of theory is to describe abstract models, and in this sense, theory could easily be regarded as a purely "honorific label" (Giere 1988, p. 84).

Up to this point, the advantage of identifying theories as models is more or less clear. Recall that "one of the primary motivations for the model–theoretic approach has been to escape worries about how linguistic entities link up with the world"

(Chakravartty 2001, p. 327). For the syntactic view, the link was established by correspondence rules. Although, these rules are intended to connect linguistic entities with the empirical world, they eventually failed to do so. However, there seems no such problem in the semantic or model–theoretic view because theories are not linguistic entities; instead, they are extra linguistic abstract structures. In this manner, the question of what the logical connection between $\mathbf{L_T}$ and $\mathbf{L_O}$ does not apply.

A question remains, however: How do models relate to the empirical world if there are no correspondence rules at all? As stated, models are extra–linguistic entities, so on the semantic view, the relationship is meant to be between models and the world – it could not, as is in the syntactic view, be a relationship of truth between statements and the world. Therefore, there must be some other kind of link between models and the world. According to the semantic view, models link up with the world by a *representational* relationship. More specifically, with respect to models, the operative notion is not truth –so models are neither true nor false. Models, at best, either represent or not. As one of the proponents of the semantic view, Ronald Giere describes the structure of models as follows:

> For the purposes of understanding the relationship by which the model represents the real system, the concept of truth is of little value. A model, being an abstract object rather than something linguistic, cannot literally be true or false. We need another sort of relationship altogether (Giere 1999, p. 92).

The sort of relationship Giere offers is that of similarity. In more detail, he states that the representational relation between a model and world is constituted by means of similarity. Nevertheless, similarity is not the only game in town. According to van Fraassen, the representational relation between a model and world is isomorphism. I elaborate the issue of similarity and isomorphism in **§3.5**, but for now it is sufficient to note that, there seems no consensus among the adherents of the semantic approach about what that representational relationship specifically is.

So, even the semanticists disagree with each other on the issue of how models represent. It, thus, appears to be a problem. Nevertheless, this is not the only one. There is one other problematic thesis, namely the specific thesis of identifying theories with a class (or family) of models. For example, van Fraassen (1989) asserts that "if the theory as such, is to be identified with anything at all … [it] should be

identified with its class of models" (p. 222). This, in itself, is not a problematic thesis though, but is incompatible with the Tarskian semantics used for formalization. Margaret Morrison (2016) expresses this problematic aspect of the semantic view as follows:

> [I]f a theory is just a family of models, what does it mean to say that the model/structure is a realization of the theory? The model is not a realization of the theory because there is no theory, strictly speaking, for it to be a realization of (Morrison 2016, p.384).

This quote points out precisely what is wrong with semantic view. Recall that, according to Tarski, a model (M) of a theory (T) is a possible *realization* in which all valid sentences of a theory (T) are *satisfied*. To this description, (M) is about (T); however, proponents of the semantic view do not seem to be in line with the thought of Tarskian semantics. Accordingly, for them "models are not about the theory as they are for Tarski; for them the theory is simply defined or identified by its models" (Morrison 2016, p. 383). The semantic view has started off on a good foot for correcting or removing the problematic points in the syntactic view. However, it seemingly faces a different kind of stalemate. If speaking of a model is actually speaking of a theory, then how would it be possible for a model to satisfy the valid sentences of the theory, *i.e.* how would it be for a model to be a realization of a theory?

The problems of *the identification thesis* and *the representational relation* together constitute the weak points of the semantic view. For this reason, some suggest that a sort of deflationary attitude about semantic view would be a more fruitful approach than sticking to its classical version. Stephen M. Downes (1992), for example, states that "any reconstruction of a domain of scientific practice in terms of identity statements such as 'theories are families of models' can only give us limited purchase on making sense of science" (p. 151). On this view, the semantic approach should focus more on practical dimensions of science to handle such limitedness. For example, according to Downes, model construction could be a good starting point.

At this stage, I will not discuss the views on how the semantic view can be improved, because that is an issue that needs to be dealt with separately. What I want to underline, however, is that the semantic view is put forward as a better alternative to

the syntactic approach; nevertheless it suffers from some different critical problems as well.

1.6 Conclusion

Syntactic view has begun with many notable achievements and ended in a mess. Nevertheless, it provided a framework for philosophy of science, especially for the discussion of the status of scientific theories. Carnap and the other adherents of syntactic view promised various great things, and contributed significantly to the understanding of science but, as said, in the end they unavoidably failed. Alan Musgrave and Charles Pigden (2021) explain this failure in terms of Imre Lakatos' (1922-1974) degenerative problem shift:

> [To] solve his original problem, Carnap had to solve a series of sub-problems. Some were solved, others were not, generating sub–sub–problems of their own. Some of these were solved, others were not, generating sub–sub–sub–problems and sub–sub–sub–sub–problems etc. Since some of these sub–problems (or sub–sub–problems) were solved, the programme appeared to its proponents be busy and progressive. But it was drifting further and further away from achieving its original objectives (Musgrave and Pigden 2021).

It is a separate issue whether the enterprise of syntactic view constitute an instance for degenerative research program. Nevertheless, it would be quite appropriate to claim that it has moved away from its original objectives in due course. Especially, the employment of the reduction sentence seems metaphorically no different from approaching a bleeding patient with a painkiller. The main issue however, is not to stop the pain, but to stop the bleeding.

Semantic view of scientific theories began with the promise of stopping the bleed. In a sense, they succeeded. However, to continue metaphorically, this led to other complications in the body. Especially, two of them were vital, namely the complications of the identification thesis and the representational relation. Among semanticists, the representational relation between models and the world still remains an unsettled problem. This has inevitably led to many different positions on the issue of scientific representation (see Suarez 2010). The identification thesis on the other hand, was also subjected to criticisms. In particular, it has been argued that it would be misleading to identify the theory with a class of models, because a great deal of

models function as ‘autonomous agents’ that mediate between theory and phenomena (see Morrison 1999 and 2007).

The tension between the syntactic and semantic formalizations of theories can nowadays be considered as an outdated debate. Until recently, however, the debate has been revived by some philosophers of science who are sympathetic to the syntactic view. In particular, it is argued in the literature that some implications of the semantic view lead to absurdities (Halvorson 2012), and that a dose of syntactic view must be added to overcome these absurdities (Glymour 2013). It has also been argued from a more historical and technical point of view that the syntactic view is subject to a strawman argument (Lutz 2012). On this view, it has been suggested that correct characterization can be enlightening about theories and models (Lutz 2014; and 2015). The main trend in philosophy of science over the last fifty years is not driven by the formalization of theories. Thus, most likely, these proposed views will not revive the syntactic view. Nevertheless, they all help us to show that the semantic view, which has been an orthodox position for a long time, is not fully established, as might one likely suppose.

CHAPTER TWO

NEW PROBLEMS WITH NEW SUBJECT MATTERS

2.1 Introduction

The second half of the previous chapter focused on the formalization attempts of theories. I presented in detail both the weak and the strong points of the suggestions of formalization in **§1.5**. I have presented model–theoretic (semantic) version of formalization as an alternative to axiomatic (syntactic) formalization.

Model–theoretic version of formalization was featuring mathematical models. This idea was in line with the scientific discourse because scientists themselves frequently use and also talk about models (not merely mathematical models) when they present their ideas. Contrary to the theory–centric attitude of twentieth century scientists, they are inclined to speak of scientific models in general. To be sure, most scientists did not see models as a means for formalizing theories. Instead, their aim is to explain the phenomena under study by means of models. For example, American biologist James Watson and English physicist Francis Crick (1916–2004) proposed the double helix *model* of DNA in 1953. Watson and Crick mainly aimed at explaining the replication of DNA. They were not particularly interested in whether their model was a member of any family of models or whether it was associated or not with a particular theory.[7]

After the 1950s, models became more prominent in philosophical and scientific discourse. When this situation is taken into account, one may find a harmony between the two disciplines. Nevertheless, in philosophy of science, studies on models went into two different directions. As I tried to explain in **§1.5.2**, some philosophers of science considered models as a tool for formalizing theories. Moreover, they have tried to explain the representational relationship between models and the world. So, in a way, they focused on the semantics of the models. I call this approach '*model-theoretic* view of science'. On the other hand, a considerable group of philosophers of science addressed slightly different question,

[7] In both theoretical and practical aspects of science, models were treated as secondary with their heuristic role. Nevertheless, this inclination weakened by some scientific achievements which were obtained by the models. In other words, proposing a model became equally important as proposing a theory.

namely how models are used in science. These philosophers, so to speak, focused on the pragmatics of the models. I call this approach '*model-based* view of science'.

This chapter, briefly, discusses the model–based view of science, *i.e.*, the pragmatic aspects of scientific models. In the penultimate section of this chapter, I will show that approaching to models in pragmatic ways bring about a new problem. My aim is not to solve this problem; instead I argue that this problem arises primarily because of the model-based view of science itself. More specifically, an essential hallmark of science is inevitably compromised by the model–based view of science, namely the 'exactness'. Eventually this led to new problems in the philosophy of science.

2.2 Models in Scientific Practice

The concept of model is used in scientific practice in many different ways, which poses some difficulties. These difficulties do not arise from the issue itself. Rather, they are due to the ubiquity of entities that classified under the same term 'model'. Roman Frigg and Stephan Hartmann list the model types they came across in the philosophy of science literature as follows:

> *Probing models, phenomenological models, computational models, developmental models, explanatory models, impoverished models, testing models, idealized models, theoretical models, scale models, heuristic models, caricature models, exploratory models, didactic models, fantasy models, minimal models, toy models, imaginary models, mathematical models, mechanistic models, substitute models, iconic models, formal models, analogue models,* and *instrumental models* are but some of the notions that are used to categorize models (Frigg and Hartmann 2020).

As can be seen from this list, different types of scientific models have the potential to raise some confusion when considered merely under the umbrella term 'model'. Apparently, due to the diversity of models, the term 'model' is used in various ways. As Nelson Goodman (1976) rightly states: "Few terms are used in popular and scientific discourse more promiscuously than 'model'" (p. 171). Actually the promiscuous usage of the term is not arbitrary, many authors at best do not clearly state which model type they refer to when they speak of a model.[8] Nevertheless mathematical, computational and scale models are among the most studied models

[8] Actually one could further claim the following: Even though the promiscuous usage of the term is not arbitrary, it gets worse when authors do not clearly state which type of model they refer to exactly.

by the philosophers of science. Let me examine these models starting from the last one in reverse order.

2.2.1 Scale models

The studies of scale models in philosophy have always remained peripheral compared to the studies of mathematical and computational models. Perhaps this is because scale models "are often thought of by the layperson as being constructed as if the model were made by shrinking an object to a smaller size" (Sterret 2021, p. 397). Specific examples for these models include "the ship displayed in the showcase of a travel agency (a model of the Queen Mary), the airplane that emerges from a small boy's construction kit, the Stone Age village in the museum of natural history" (Black 1962, p. 219). Nevertheless, it would be misleading to reduce the function of scale models merely to a miniature object. A scale model is not merely a static model of an object. Rather, by means of a scale model, one can dynamically consider a particular object or a system at a faithful scale. Thus, in this way, one can draw certain conclusions about the target system by manipulating the scale model itself.

In general, scale models are physical models because they often scale a physical object and the relationships between its parts. However, not all physical models are scale models. To model a car accident, for example, one could represent the road by a table and the cars by pencils and books without being faithful to the scale size (see **§.3.6**). In order to build a successful model, it would be sufficient for the modeler, just at the outset, to specify which objects represent the road and the cars.

On the other hand, if one wishes to extract some information about the physical system, it would be appropriate for the available model to represent that physical system in a high fidelity. Undoubtedly, it is unrealistic to expect a scale model to fully represent every element of the physical system it models. For this reason, modeling only the relatively important parts of the system would be a more strategic move for the modeler. Christopher Pincock (2012) describes the use of this strategy as follows:

> When we model a physical system we often believe that certain features of the system are more important than others. One way to signal this importance is to speak of the relative scale of these features, as compared to other features that we deem unimportant. The scale of

> a feature can be understood in terms of its relationship to whatever parameters are used to approach the system (p. 88).

This quotation supports the idea that scale models have important and unimportant parts. The modeler draws information about the physical system by means of the representations of its important parts. Any modeler who might wish to model only the structure of a bridge has to represent all the beams of it proportionally, for instance with matchsticks. However, there seems no need to scale the air flow that passes through the beams of the bridge in actual way. With respect to the structure of the model, the proportions of the columns and beams are important while the air flow is not. However, "[t]he purpose of a scale model bridge may be to understand whether a *particular* bridge will stand up to strong wind" (Griesemer 1990, p. 8). In this case, the air flow becomes important for the model because the issue becomes relevant in the context of the beam resistance.

Scale models are not constructed as a result of arbitrary preferences. Mostly, it is inevitable for the modeler to construct a scale model. Especially, geology and its engineering applications rely heavily on scale models. The reason why geosciences favor scale models, according to Alisa Bokulich and Naomi Oreskes (2017) is that "the systems of interest are typically large-scale, complex, open systems [and they] are not amenable to experimental manipulation" (p. 893). This, of course, should not be confused with the field research of geosciences. In field research, the geological data is obtained by passive surveys and these activities do not provide manipulation techniques to the researcher. However, geoscientists can draw inferences about important phenomena such as mountain formation or earthquake mechanism from the scale models of the geological systems. In other words, scale models allow "a geoscientist to bring a version of the landscape into the laboratory, manipulate various variables in a controlled way, and explore hypothetical scenarios" (Bokulich and Oreskes 2017, p. 893).

2.2.2 Computational models

With the advancement of computer technology, computational models have become indispensable for many fields of science in the twentieth century. Computer simulations are central especially for the disciplines such as climate science,

astrophysics and economics. These models are even used in epidemiology to reduce the impact of the current pandemic we live in (see Currie *et al.* 2020). Except for some rare examples (see Schelling 1978), we do not encounter computer simulations in social sciences like sociology. Nevertheless, some sociologists who complain about this issue suggest that computer simulations (especially agent–based models) should be more dominant in the field (see Chattoe–Brown 2013).[9]

In general, it is "reasonable to say that computational models are a subclass of mathematical models" (Marraffa and Paternoster 2017, p. 929). What distinguishes the computer simulation from the mathematical model is that the former can be run through a computer. As Paul Humphreys (1990) states "[i]n order for something to be a computer simulation, the whole process between data input and output must be run on a computer" (p. 502). The reason to run a mathematical model through a computer is mostly because of methodological limitedness. Sometimes, particular models are highly resistant to a solution *via* any available analytic method. The analytic method generally provides the exact solution of the model. Broadly speaking, sometimes, it is faster and easier to calculate the solution manually, *i.e.* by hand. An often used example to illustrate this situation is as follows: Consider an equation like $x + 1 = 0$. As you may guess, a solution for this equation is analytically available. By adding both sides a minus one, one can simply find an exact solution for x:

$$x + 1 - 1 = 0 - 1$$

$$x = -1$$

Analytic approach is not the only one at our disposal to solve the equation. There also exists a numerical solution to the same equation. In this way, for example, based on a random initial value, one might iterate on (or estimate) certain values for x like $-½, -¼, 0, ¼$ and $½$. The values that estimated (or to put it roughly, guessed) for x in the equation $x + 1 = 0$ can be calculated as follows:

[9] There is a growing philosophy of science literature about agent–based modeling. Especially, in recent years, Daniel Frey and Dunja Šešelja have carefully examined the epistemic value of agent–based models of social aspects of scientific inquiry (see Frey and Šešelja 2018).

$$-½ + 1 = 0 \therefore ½ \neq 0$$

$$-¼ + 1 = 0 \therefore ¾ \neq 0$$

$$0 + 1 = 0 \therefore 1 \neq 0$$

$$¼ + 1 = 0 \therefore 1{,}25 \neq 0$$

$$½ + 1 = 0 \therefore 1{,}5 \neq 0$$

As it is seen, the iteration technique does not provide a solution to the equation for the positive fractional numbers because the values obtained by them are higher than zero. On the other hand, negative fractional numbers are likely to satisfy the equation. A closer look will reveal that when we estimate $-½$ as a value for x, we can have a more approximate solution than the negative fraction $-¼$. Therefore $x < -½$. Until we reach -1, we obtain the exact solution.

As it is understood, one can approach to the equation $x + 1 = 0$ with two different mathematical techniques. In this case, the advantage of the analytical technique over the numerical one seems apparent. However, not every mathematical equation (or model) has such an easy solution. More importantly, some equations do not have an exact solution, but only an approximate solution. In such cases, numerical methods would be faster to use for the particular calculation. Computer simulations do such calculations way faster than any other alternative.

Undoubtedly, it is almost obvious that computer simulations facilitate numerical methods. Nevertheless, identifying simulations with the numerical techniques would be somewhat misleading because "computational models are an essential component of simulations, whereas there need be no such model in numerical mathematics" (Humphreys, 2004, p. 112). In this sense, computer simulations use numerical techniques, but they are more than just that. Outputs of computer simulation (like a visual graphic, diagram or pattern) constitute a necessary part of the simulation itself. Think of an agent based model, like Schelling's model of segregation. This model "is constructed as a computer simulation, agents are randomly distributed through the city, and their utility functions are calculated according to the model's rules" (Elliot-

Graves and Weisberg 2014, p. 176). Schelling's model tries to explain how unintentional segregation occurs between individuals of different races living in the same city.

According to the model, individuals of same race come together by moving out of their own neighborhood to the next one. In a certain period of time, they form a little neighborhood of their own even though they never intended to do so. In other words, group of individuals unintentionally cluster together in a particular time when all of them are individually satisfied. At a certain time *t*, the simulation ends when there is no longer any room for any individual to move within the determined rules. The model is in a form of two–dimensional checkerboard, all neighborhoods in the city are represented by grids and each cell represents a location. The all–over picture of the grids shows us that individuals of the same race move out of their neighborhood to gather together with the individuals who are of the same race.

As one can see, there are too many things going on in the model. It "began with a mathematical model depicting the time-evolution of the system being studied in terms of equations, or rules-of-evolution, for the variables of the model" (Winsberg 2010, p. 4). However, the model ended up with a picture of grid and this picture lead us to understand what has happened within the simulation. In other words, "if we display the results of such models numerically or statically, we cannot 'see' the higher-order emergent patterns that result from the interactions between the agents" (Humphreys, 2004, p. 113). However, if we monitor the activity in the city by means of the outputs of the simulation (*i.e.*, with the patterns that the simulation displays), we can clearly determine how the clustering is formed at certain times *t* (see Figure 2.1).

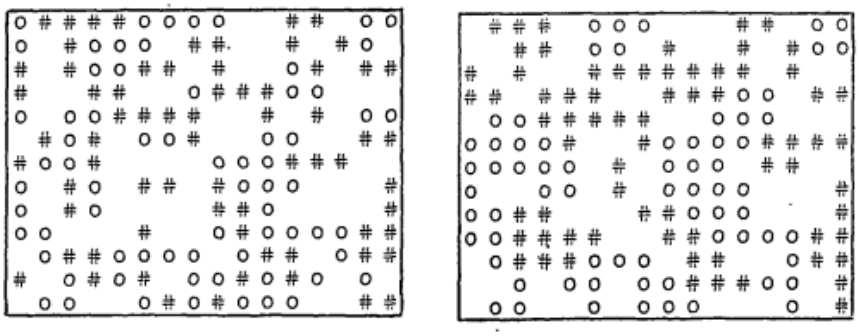

Figure 2.1 The pattern on the left-hand side shows the random distribution of the individuals in a neighborhood at time *t*. The other one illustrates how individuals bunch together unintentionally when the equilibrium is achieved at time *t'*. Reprinted from *Dynamic models of segregation* (p. 157) by Schelling (1971) Copyright © Tandfonline, Informa UK Limited.

In the figure, two separate groups of individuals are represented by the symbols ○ and #. On the left, the individuals are randomly distributed at time *t*. The state of satisfied individuals who moved according to the rules of simulation, at time *t'*, is shown on the right. In contrast with the figure on the left, the right figure displays the group of individuals who unintentionally come together. To better illustrate the segregation pattern, one might wish to see the boundaries-drawn version of the equilibrium state (see Figure 2.2).

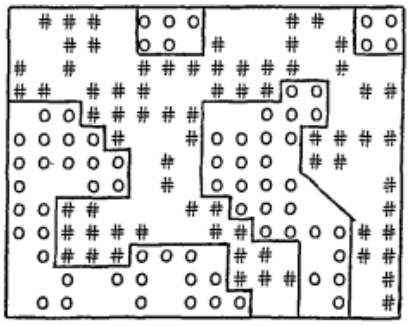

Figure 2.2 The boundaries-drawn version of the equilibrium state which is obtained at time *t'*. Reprinted from *Dynamic models of segregation* (p. 157) by Schelling (1971) Copyright © Tandfonline, Informa UK Limited.

Figure 2.2 displays how individuals of the same race bunch together unintentionally. The pattern is substantially different at time *t'* than at the initial time *t*. Agents moved over the grids according to the rules of the simulation and stopped their motion when they were satisfied.

Nonetheless, the rules of the simulation are not the necessary rules prevailing among the real individuals. In reality, a considerable amount of people is pleased to live in a neighborhood where individuals of their own race do not live. Moreover, unsatisfied individuals do not necessarily move to the next neighborhood. They might move to a remote neighborhood or might even decide to change the city they currently live in. Schelling's computational model, on the other hand, allows individuals to move only to the nearest location. Additionally, the locations of the city are represented as perfect grids. In truth, however, "a real city […] is not a perfect grid" (Elliot-Graves and Weisberg 2014, p. 176). Such factors are the parameters the model neglects. In this manner, the model does not represent a real city. In truth, the modeler does not aim at such a model that represents the real dynamics of citizen's behaviors. To be more precise, the modeler does not target any particular city; rather, his goal is to explain how certain micro-behaviors (being uncomfortable in a different neighborhood) lead to macro-behaviors (segregation of the city).

2.2.3 Mathematical models

Mathematical models are the ones that are widely used in science. With this feature, they have gained a special place in the philosophy of science literature. In the preceding sub-section, I have indicated that computational models could be seen as a version of mathematical models. Similar claims could be made for the scale models. Although many scale models are not mathematical structures *per se*, it can be argued that they have a mathematical part. After all, these models necessitate the calculation of the proportional reduction for a certain structure. In this context mathematics is essential.

The fact that mathematical models are so essential in scientific enterprise does not mean that science cannot be done without these models. Charles Darwin's (1809-1882) works are scientific but his "work on the foundations of evolutionary theory proceeded without the use of a single mathematical model" (Downes 2011, p. 758). Therefore, having mathematical models is not a criterion to decide whether an activity is scientific or not.[10]

[10] This claim is controversial, however, in part because Darwin himself says that the theory will be complete when it is mathematized. The modern synthesis starting in 1910s up to 1940s does exactly

A mathematical model is "complete and consistent set of mathematical equations which is thought to correspond to some other entity, its prototype" (Aris 1994, p. 1). Given this definition, the sciences such as physics, biology, chemistry, and economics are the prototypes of particular mathematical models. This definition, however, requires an additional assumption, namely the assumption that the model and its target system share a common structure. For example, different cars with their own characteristics correspond to a prototype car. They all share common mechanisms and elements (having tires, engine etc.) of their prototype; but also differ from their prototype, for instance, by having various body shell configurations. So, as a prototype the mathematical model should also have some commonalities with the empirical sciences.

Identifying mathematical models as prototypes for the empirical sciences might be seen as a useful analogy. The prototype of a car captures the essential features of many cars by leaving out the inessential elements. In a similar way, mathematical models of physical systems also capture some essential physical properties while neglecting inessential ones. As it is known, complexity is inherent in the physical systems. In this sense, it is not viable to deal with this overall complexity. Therefore, mathematical models essentially represent relevant factors and relations. In this respect, the models are simple versions of complex physical systems.

Apparently, the idea that explanations of the empirical world must be mathematical has been around since the Galilean dictum 'The great book of nature is written in mathematical language'. According to the general interpretation, the author of the book of nature is assumed to be God or an omnipotent power. However, regardless of its author, it is pretty obvious that we could not read this book as is because of the complexity of nature. So, we need to revise this dictum by saying: 'The great book of nature is written in mathematical language *to a certain extent*, or it is not *all over written* in mathematical language'.

It is somewhat clear that many mathematical models are successful with respect to their outputs (explanation and prediction). At the same time, however, they are

what Darwin hoped to be done in the future to complete the theory. This issue should be handled with utmost care for the history of science. Nevertheless, I would prefer to leave it aside because it is not central for this chapter.

smooth or simplified versions of the physical systems. This seems to raise a philosophical issue. Mathematical models are way simpler than the complex physical systems which they purportedly correspond to. Yet, it is often thought that the explanations and predictions of these models are eminently successful. But, how do these simple models successfully explain and predict complex phenomena? Adam Morton (1993) similarly formulated this question as follows:

> [M]athematical modeling … is marked by two features: the model is constructed against a background theory, and it embodies obviously false features. And from these false assumptions, carefully manipulated, we get predictions on matters of enormous public or practical importance. And we trust them. And very often we should [despite the fact that their] predictions are sometimes untrustworthy (p. 660).

Morton considers that untrustworthy predictions are inevitable because the model has false assumptions in the first place (see Chapter 4). He maintains that although these assumptions are false, the outputs of the model are trustworthy. This trustworthiness could be explained with the prototype analogy. If, as a prototype, a mathematical model describes the essential structure and the relations of their target systems, the outputs of it would be trustworthy. Therefore, one can state that "it is typical of the modeling process that details are left out, and yet it is thought that models can capture the essence of phenomena" (Bailer-Jones, 2002, p. 298). What is interesting here is that we are faced with a sort of trade-off during the process of mathematical modeling. The more precisely you represent a system with a mathematical model, the harder it would be to apply generally. On the other hand, if the model were constructed more generally, its precision would be decreased. Richard Levins (1966) likewise claims that "we have to simplify the models in a way that preserves the essential features of the problem", in this respect we either "sacrifice generality to precision… [or] sacrifice precision to generality" (p. 421-422).[11]

The use of mathematical models in scientific practice is an issue that needs to be addressed at length. For the moment, I aim to provide only a brief overview of mathematical models. The issue is discussed in more detail *via* an example in Chapter 5.

[11] A perfect example for tradeoffs in science is about the two models of Boyle's Law, *i.e.* the ideal gas model and the Van der Waals model. For details see §7.2

2.3 Exactness: An Essential Compromise of Model-Based View of Science

As I stated above, there are different types of models in science. Apparently, all these models are constructed by using distinct techniques of modeling. Distinct modeling techniques are related with how the modeler approaches to problems. Consider, for example, a geological model about mountain formation. If one wants to explain this formation process with a scale model, she needs to take into account the composition and dynamics of the earth's crust and represent all these factors as realistically as possible. On the other hand, if one wants to simulate the same process with the computational model, she also needs to consider the same factors. The important difference in simulation is that the modeler needs to describe the variables in a rule-based system which in turn would determine how the entire system evolves in temporal stages. Strictly speaking, it is not my intention to claim that computational models are superior to the scale models, or vice versa, in explaining a particular phenomenon. What I emphasize is that both models differ in the way they are constructed.

Although the techniques of modeling are not regulated by a set of strict rules, they include certain basic steps (see Gass 1983). At bottom, however, the issue is tied to the experience, attitudes, intuitions, and background knowledge of the modeler.[12] Mathematical scientist John A. Adam (2003) claims that "mathematical modeling is as much 'art' as 'science'" (p. xvi). I think this claim holds across many model building processes in science. The perspective of the modeler determines the structure of the model, such as which factors are need to be considered and which are not.[13]

[12] To be sure, there are many distinct ways to construct a scientific model. One of them is described in Appendix A by means of the case study that I discuss in Chapter 5.

[13] A similar analogy, albeit very *weakly*, might be drawn between artworks and models. Take for example the attitudes of artists and modelers. The artist decides which color or which note should or should not be included in the work. *Port de Bordeaux* is an iconic piece of modern artwork because its creator Édouard Manet decided to apply the particular combination of colors onto the canvas. Beyond this, however, he did not choose to use some specific colors, like magenta or dark blue in the painting. So, he deserves credit for avoiding any other different combination of colors. The success of a musical piece depends also on its melodic combination of the notes. However, tacet periods (whenever an instrument is not playing during a piece of music) are also important in making a musical piece an artwork. Viewed in this light, one can say that modelers also aim at explaining the intended target system as general as they can, but they also avoid to include many properties as well.

A model aims to explain its target system as general as possible, but while doing this it does not aim to describe the entire system. Conversely, it only aims to describe the core features of the target system. So, one of the implications of model–based view of science is to construct idealized/abstract 'successful' models in terms of explanations and predictions they provide (see Chapter 8).[14] Yet a closer look will reveal that this (model-based) view heads away from one of the ideals of syntactic/semantic view of science.

In the following sub–section, I present a hallmark of science, namely 'exactness'. However, there is a crucial point need to be noted before proceeding. Exactness is a property that thought to exhibit the success of science.[15] Proponents of the syntactic and semantic view of science think that mathematics made great progress from the middle of nineteenth century onwards just because the use of formalization. Thus, the formalization of scientific theories is crucial in this respect. If scientific theories are successfully formalized, for example, the content of a certain theory could be presented exactly, even though it has different formulations. In this manner, I will call this endeavor of syntactic/semantic view of science as 'ideal of exactness'.

Although the proponents of the syntactic/semantic view of science do not seem to have directly the ideal of exactness, their philosophical attitude clearly exhibits that they embrace such an ideal.[16] It is difficult to define the term 'exactness' in an exact manner. As Peter P. Kirschenmann (1981) ironically stated "it is hard and may be even undesirable to be precise about exactness itself" (p. 86).

Rather than giving a full-blown definition of exactness, it may be more appropriate to take a look at some of the formal disciplines considered as 'exact'. The first thing that comes to mind is logic and mathematics. These two fields exactly determine the meanings of the concepts they use and the relationships between them. To put it more specifically, "logic and mathematics are the fields in which certain types of

[14] Models are idealized and abstract *in nature*. Therefore, they do not describe their target system completely. In other words, they describe their target systems partially.

[15] Perhaps –for some– science without this ideal would not be a science but rather be a different enterprise.

[16] The ideal of exactness is not universally shared among the twentieth century scientific philosophers. For example, Otto Neurath (1938) states that "one can love exactness and nevertheless consciously tolerate a certain amount of vagueness" (p. 21). Nonetheless this comment apparently shows how the proponents of the syntactic view gave importance to the ideal of exactness. In this sense, Neurath's idea can be interpreted as a warning sign for the common attitude.

exactness are realized in a paradigmatic fashion: precise language, strictly defined concepts, unambiguous propositions, rigorous deduction, fully axiomatized theories" (Kirschenmann 1981, p. 91). It is clear that the particular types of exactness that Kirschenmann outlines reflect the most general aims of syntactic and semantic views toward science. As you may recall, for example, since its founding, the entire purpose of the Vienna Circle is to eliminate vagueness in language. For this reason, they have insisted on concept clarification and also emphasized the need to apply the formal rules of logic to ordinary language.

It seems important to underline, before proceeding further, that I use the term 'exactness' (and its synonym 'rigorousness') as antonym of the terms 'vagueness' and 'ambiguity'. Thus, for instance, when discussing exactness in this sense, there is no mention of an 'exact' measurement of objects (*e.g.* done with the help of experimental tools).

It is possible to trace the basis of exactness back to the days when modern science was established. Nevertheless, some of the Vienna Circle's members took the ideal of exactness to the extreme especially with the influence of Wittgenstein. I have mentioned earlier that two semesters were devoted to reading Wittgenstein's *Tractatus* in Vienna (see **§1.3**). In **§1.3**, I have also tried to show briefly the Wittgensteinian influence on the Vienna Circle. But, if I were to put it in a nutshell, the only fitting motto would be: "[W]hat can be said at all can be said clearly, and what we cannot talk about we must pass over in silence" (Wittgenstein 1922/2002, p. 3). This phrase basically sums up the project of eliminating the inherent vagueness of languages. According to Wittgenstein, once the ideal language is substituted for the ordinary language, there would be no room for linguistic unclearness and inexactness. In other words, ideal (or exact) language is free from ambiguities, so it is the only appropriate candidate for the philosophical analysis.[17] In this manner, as Julius R. Weinberg (1936) states "Wittgenstein's work may be regarded as an attempt to construct the rules of an exact language" (p. 25).

[17] Bertrand Russell (1872-1970) was among the names that intellectually influenced Wittgenstein. He believed that not only philosophical, but also physical problems "can probably be solved with as great a degree of certainty and as great an approach to exactness" (Russell 1949, p. 114).

The rules of this exact language are based on logic. According to Wittgenstein, logic essentially provides a methodology. Our linguistic expressions which represent our thoughts would be free from vagueness by means of logical method because "logic itself gives us the structure and limits of what can be said at all" and of what cannot be said as well (Biletzki and Matar 2018). So, logical language would prevent philosophers to get involved into meaningless debates and also would guide them in the right direction of addressing genuine philosophical problems. In other words, "the construction of an exact logical language is to serve in solving philosophical problems and in presenting a complete schematism for representing the structure of the world of science and experience" (Weinberg 1936, p. 33).

The last part of Weinberg's claim is noteworthy. Accordingly, the exact logical language does not only serve for solving philosophical problems. Additionally, it allows us to represent the structure of the world of science. At first glance, this idea seems to be in conflict with the claims of some logical positivists. For example, Schlick (1925/1979) argues that "the quantum theory … obviously represent … different aspects of what happens in nature, and … up to a certain point, with great perfection and exactitude" (p.62). Therefore, for Schlick, one cannot talk about the inexactness of the quantum theory. In some other place, Schlick (1934/1979) also argues for the exactness of scientific methodology:

> [T]he method of natural science… has… done most to promote the process of discovering the ultimate meaning of our assertions about reality. That no other method comes anywhere near even, to a similar refinement of concepts, can be outwardly seen from the fact that it is reserved to natural science alone to attain to the stage of *exactness*[emphasis in original] (p. 144).

The overall quotation suggests that the natural sciences actually have an exact method and an exact structure as well. So, a question needs to be asked at this point: How can one contribute to an exact science *via* an exact logical language? According to Schlick, the answer to this question lies in determining the role of philosophy.

Science undoubtedly produces reliable knowledge. However, some of its concepts are vague to a certain degree. In this manner, these scientific concepts need to be strictly defined in a reconstructed exact language. It is for this reason that, for Schlick "philosophy can be nothing else whatever but the activity whereby we clarify

all our concepts" (Schlick 1929/1979, p. 91). When the clarification of all concepts of science is completed, as Wittgenstein tried to do in philosophy, the linguistic aspect of the exactness ideal would be completed.

Logical positivists strive to bring exactness to the scientific concepts by the method of concept clarification. Nonetheless, it is just one of the goals they are aiming for. They also did additional studies to increase the exactness of scientific language. For example, they believed that recasting scientific theories as axiomatic systems is vital to achieving the ideal of exactness. In other words, if scientific theories are axiomatized, their exactness would be obtained in an absolute manner, *i.e.* they would not merely be evaluated as more exact than metaphysics or arts, but would be also exact like mathematics.

Axiomatization is a formal method for bringing systematization and rigor to the unorganized body of knowledge. By means of this method, for instance, one can, be able to show how scientific statements are justified. To be more specific "[t]he method of axiomatization … presents a theory by singling out certain primitive notions and defining others from them, and singling out certain propositions as axioms and deriving all other propositions of the theory by deduction" (Parsons 1974, p. 27).

Proponents of the syntactic view found such an axiomatization method in the works of David Hilbert (1862-1943). Actually, Hilbert was a mathematician and similar studies have been done before him. However, what makes Hilbert's approach unique is that the application of his axiomatic method is not limited to the fields of mathematics. To put it more specifically, he "used the axiomatic method more systematically and more broadly than ever before, applying it not only to the various fields of mathematics but also to the empirical sciences" (Stöltzner 2015, p. 12). Hilbert maintained the idea of considering any scientific subject in an exact manner. For him, the element which provides the exactness was the axiomatic method in logic. According to this line of thought, if the axiomatic approach is adopted properly, one could consider any scientific theory in terms of its axioms. Moreover, by means of this approach, the logical relations of the statements derived from these axioms can be expressed in an exact way.

> When we are engaged in investigating the foundations of a science, we must set up a system of axioms which contains an exact and complete description of the relations subsisting between the elementary ideas of that science. The axioms so set up are at the same time the definitions of those elementary ideas; and no statement within the realm of the science whose foundation we are testing is held to be correct unless it can be derived from those axioms by means of a finite number of logical steps (Hilbert 1902, p.447).

This axiomatic approach, which was later called as *Hilbert's Program*, created quite a stir among intellectuals. The mathematician Richard von Mises (1883-193), who was also inclined to support logical positivism, argued that mathematics as an axiomatic system is so exact that no one could doubt it. Accordingly, he suggested that axiomatized mathematics should be "the model which all the sciences must emulate if they are to attain the same or at least a similar certainty and exactness also for their own theorems" (von Mises 1938/1987, p. 174).

Another important figure who adopted Hilbert's approach was Carnap. Though it was not accepted by both the physics and philosophy departments at the University of Jena, his first dissertation "proposal was entitled '*Axiomatic Foundations of Kinematics*'" (Awodey and Carus 2001, p. 148). Carnap's expectations about the thesis were not met, but he did not abandon his studies on axiomatization. Hilbert axiomatized geometry in a more formal way than had ever been done before. In addition, the works of Richard Dedekind (1831-1916) and Giuseppe Peano (1858-1932) made some notable achievements, like the axiomatic formulation of natural numbers. Carnap closely followed the work of mathematicians at his time. According to Erich H. Reck (2007), these developments in mathematics were decisive for the development of Carnap's later work:

> Carnap's interest in axiomatics stemmed, directly or indirectly, from these mathematical sources. Indeed, later he often referred to Hilbert's *Grundlagen* in this connection, as well as to 'Axiomatisches Denken' (1918), an article in which Hilbert reflects programmatically on the development of the axiomatic method in mathematics, mentioning physics along the way. An axiomatic approach to both mathematics and physics, as championed by Hilbert, was thus a central goal for Carnap from early on (pp. 179-180).

Undoubtedly, axiomatization became the central goal for Carnap and for the other logical positivists. In **§1.5.1**, I tried to explain axiomatization of the scientific

theories, by specifically focusing on the works of Carnap. His works lies at the heart of the syntactic view of science.

The exactness ideal of the syntactic view of science can further be elaborated. However, I believe, this sketch would suffice to understand the point of view they have adopted. For them, characterizing scientific theories in an exact manner is not a mere logical exercise. Rather, they believe that, exactness will help to systematize the unorganized parts of the scientific knowledge. In this sense, one can contribute to science, for example, by determining whether a scientific conclusion is adequately supported by the initial premises. Therefore, moving away from the ideal of exactness was not a reasonable option for the advocates of syntactic view.

What is said for the syntactic view can also be said similarly for the semantic view. On this view, formalization is a desirable process for exactness. Suppes was one of the developers of the semantic view. According to him, for example, "unless [a scientific] discussion … is placed within a formal framework, there is little hope of an *exact* settlement of the issues" (Suppes 1968, p. 660, emphasis added).

Some new problems, however, surfaced when the syntactic/semantic view of science is substituted for the model-based science. To understand this, it would be sufficient to think of the three different model types that I have mentioned in the preceding subsections. Since these models contain idealizations *per se*, they cannot be the exact descriptions of the systems they represent. In other words, the aim of such models is not the exact truth (like the aim of theories), but rather the *approximate truth*.

Imprecise nature of the models is not a vice but a virtue for some philosophers of science. According to this line of thought, false models function as means to truer theories (see Wimsatt 2007, Ch. 6). Surely, false models might be means for truer theories as they claimed to be, but this does not remove the problem related to scientific models. How models explain, if they do contain false descriptions rather than exactly true ones? Apparently, syntactic and semantic views of science do not suffer from this problem. Since, to these views, scientific theories are (or can be) axiomatized *via* symbolic logic or set–theory. For example, *Hempelian* model of scientific explanation perfectly illustrates the syntactic view. Given this view, the explanandum is a logical consequence of the explanans of which its elements must

be true. On the other hand, models are in conflict with this view due to their idealized nature. In addition to true statements, models include false statements. Therefore, the question of how idealized scientific models can explain is a new one that arises within the model–based view of science. I discuss the proposed solutions to this problem in Chapter 4.

2.4 Conclusion

Though not shared by all defenders, *exactness* was one important ideal of the syntactic and semantic view.[18] This ideal was disappeared within the framework of model–based view of science. For one reason: models are different from theories in many respects. As I tried to show in this chapter, the models used in scientific practice contain false assumptions, *i.e.* idealizations. In this case, there were two options available for the advocates of the model–based view of science. Either they would correctly describe scientific practice or they would permit the previously held ideal of the syntactic and semantic views. They have chosen the former because imprecise or idealized models were ubiquitous in scientific practice. So, the ideal of exactness was in a sense abandoned.

However, the model–based understanding of science has created a new set of problems. These problems can be formulated in the form of one single question: How idealized models explain and represent? This question can be further refined as follows: If models are idealized (or inexact) descriptions of the actual system, then how do they provide explanation? And, accordingly, if models are idealized (or inexact), then what is the representational relationship between model and target system? This chapter is aimed at showing in which contexts these certain problems arise. The next two chapters explore the approaches to these certain problems.

[18] There are other ideals of the syntactic/semantic view of science. One of them is objective truth. Similar to the ideal of exactness, the ideal of objective truth is not explicitly spelled out by them. However, proponents of the syntactic/semantic view of science implicitly set the achievement of this ideal as their goal. For one important reason: science can be formalized by using the tools of mathematical logic. However, science is not mathematics; it has an empirical part. Nevertheless, like the ideal of exactness, the ideal of objective truth has been compromised by the proponents of the model–based view of science. The main reason for this is the ontological status of the models. I have discussed some of the model types in §2.2. These models are not true in the first place. More specifically, from the model-based science perspective, one cannot assign truth values to models. A model can be physical or abstract entity, as well as it can be a picture, diagram or graph –nevertheless a picture, for example, cannot be true or false. In effect, models cannot be true or false because truth values are merely reserved for linguistic entities.

CHAPTER THREE

SCIENTIFIC REPRESENTATION: QUESTIONS, DICHOTOMIES AND INTUITIVE STATEMENTS

3.1 Introduction

There is general agreement among philosophers that models represent phenomena. However, there seems to be little agreement on the issue of how this representational relation is constituted. So far, a wide range of views put forward to account for the issue. Nevertheless, there are diverse questions regarding representation. This diversity inevitably leads to confusion in identifying whether an author writing on representation is addressing the same or a different question. Thus, it appears to me, that to make some distinctions would be useful, and perhaps requisite to figure out the issue properly.

3.2 Questions

There are diverse questions regarding representation. I take three of them into consideration:

1. *The Constitution Question*: What is the relation R that constitutes representation between a model and the world (Suarez 2003)?
2. *The Demarcation Question*: What is the criterion C that distinguishes scientific representation from other types of representations (Hughes 1997)?
3. *The Practical Question*: How do scientists use models to represent the world (Giere 2004, Weisberg 2007)?

So, in the literature of scientific representation, one can encounter at least three main questions: (1) constitution question, (2) demarcation question and (3) practical question. The constitution question deals with the relation between a source (vehicle) and a target (system). Philosophers, who committed themselves to this question, mainly assume that there ought to be a constitutive relation capturing all types of representation. The demarcation question, on the other hand, deals with whether there are criteria to draw a distinction between scientific and other types of representations. Thus, if one deals with the question of demarcation, a reasonable way for one is to look at scientific practice, focusing particularly on distinctive

practices of scientists and revealing divergent properties of scientific representation from other ones. Finally, the practical question is about the use of models in scientific practice, especially about how models are built in specific contexts and to what extent they should be treated as representational vehicles that provide information about target systems. A possible solution to this question necessitates a strategy such that one should provide an answer not with armchair investigations, but with practical inquiries.

To be sure, these three questions are interrelated. It seems that to provide an answer to the question (1) would at the same time be an answer to the question (2) and (3) such that it's not only limited to scientific representation but also captures all types of representation including artistic, linguistic or cognitive ones. Conversely, an answer provided to the question (2), compared with the former, would be a modest one. Here, the intention is to determine the criteria that make scientific representation distinctive. From this point of view we ignore everything what is applicable to other types of representations as well. Conversely, ignoring the question (2) or answering it in a negative sense confronts us with the following question: Is it proper to draw on examples from art (or other types of representation) to support arguments about scientific representation? To some degree, one can use examples from non-scientific domains to make one's arguments clear in an analogical manner, but it does not seem appropriate to use these in scientific context. Lastly, as mentioned, an approach to the question (3) needs a similar strategy that suggested for question (2). Practical questions demand practical answers. So, one needs a practical investigation to capture the question of how scientists use models in certain ways.

3.3 Dichotomies

There are widely accepted distinctions regarding representation (see Chakravartty 2010; Suarez 2010). These distinctions can be summed up as follows:

3.3.1 Reductionism or primitivism

A reductionist would want to analyze representation in terms of other properties or relations. A primitivist, on the other hand, would say that representation is not further analyzable in terms of other properties or relations. The proper examples, for the comparison, would be DNA model and scale model of a bridge. For a reductionist,

DNA model and scale model of a bridge represent their targets in terms of a relation X shared by all types of representational vehicles. For a primitivist, on the other hand, DNA model might represent its target in terms of a relation Y, and scale model of a bridge might represent its target in terms of relation Z. To defend the view that a unique relation constitutes all types of representation seems not viable to me because there are different contexts in which scientists use models to stand for its purported target.

3.3.2 Substantive or deflationary accounts

Substantive (informational) account of representation takes representation as a relation that holds between model and the world. This relation is substantive in the sense that it explains representation in a naturalistic way. No contexts of use are essential to understand representation; rather representation is an objective relation between model and target which both includes shared properties. This is called a dyadic relationship.

On the other side, deflationary (functional) accounts of representation emphasize the use which is performed by agents. Agent, on this account, is the absent element of so-called naturalized substantive accounts. In order to understand representation, the intended use of model should be explicated prior to the objective relationship between vehicle and target pair. After all, a naturalized conception of representation seems untenable so much so that there would always be a usage of model by an agent to fulfill his specific purposes. “We must consider not only the object itself, but also the manner in which it is used” (Toon 2012, p. 245). So, the conclusion drawn from this is quite simple: representation is a triadic relation which holds between model, target and agent.

3.4 Intuitive Statements

There are a group of statements about representation that are accepted as true or at least plausible by philosophers of science. I also take these statements as true, but sort them into two groups, with three statements in each group. The reason to split these statements into two groups is to reflect the general inclination among philosophers of science towards the issue of representation. In other words, while the first group of statements seems intuitive or rests on rational insights, the second

group of statements is still open for discussion, for being susceptible to counterexamples.

[Ia]: Representation is non–reflexive

The statement above is about a feature relevant to all types of representational vehicles. By using a vehicle, we are intended to represent target phenomena, not particularly represent the vehicle itself. So, representational vehicles are essentially non-reflexive in character.

An Example: A scale model of a bridge constructed with toothpicks could represent a bridge which is made of cast iron or steel, but could not represent itself. Therefore, representation is a target–directed relation.

[Ib]: Representation is non–symmetric

With a broad look at representation, we expect to constitute a one-way relation between vehicle and its target. The statement above underlines this expectation. Then, our intuition about representation is to expect a vehicle to represent its target, but not vice versa.

An Example: Billiard ball model of gas molecules particularly represents the behavior of ideal gas molecules contained in a box. Nevertheless, even if gas molecules in that box share certain properties with the model, it does not represent billiard ball model of gas.

[Ic]: Representation is non–transitive

The transitive relation is an equivalence relation and does not hold for representation. In the case of representation, we could say, for instance, that A represents B and B represents C but this does not allow us to say that A represents C.

An Example: The political map of Turkey (A), for example, represents the borders of the country (B) which is drawn partly both in Europe and Asia. Beyond that, the land borders of Turkey (B) are also used for certain people to represent their nationalistic stance (C). However, the political map of Turkey (A) needs not necessarily be the representation of nationalist stance (or serve patriotic aims) for certain people.

These three intuitive statements are asserted by Nelson Goodman (1968, pp. 3-43) and commonly accepted as logical properties of representation. Now let me list the other group of statements about representation which could be seen as semi–intuitive:

[*SI(a)*]: The first semi–intuitive view about representation is based on the presumption that scientific representations differ from other types of representations in many ways. To put it differently, there are properties which are peculiar to scientific representation, but not to other types of representations such as linguistic, artistic or mental ones. So, in order to understand scientific representation, one needs to discover these properties and systematically combine them in accordance with certain scientific cases.

It is worth here to say that finding out common properties shared by all types of representation might help us to understand about the issues associated with different contexts. However, that would not help us to explain the nature of scientific representation in particular. Then, a useful strategy would be to reveal original aspects of scientific representation.

[*SI(b)*]: The second semi–intuitive view about representation emphasizes the independency relation between a reliable representation and its accuracy. In general, we tend to think that a representation is a reliable one, to the extent that it accurately maps its target. However, a representation could be successful apart from its accuracy. Aether models, for example, were suggested to represent a medium that allow light waves to propagate through. Currently, a number of well–conducted experiments (including famous Michelson–Morley experiment) show us that there is no such medium through which electromagnetic or light waves move. Although, the model is shown to be inaccurate, still no one denies that it was a representation of a medium –albeit a fictional one. So, the representative relation holds for a vehicle regardless of its accurate depiction of the world.

[*SI(c)*]: The third and the last semi–intuitive view about representation is its intentional character. That is to say, a representation is constituted insofar an agent intends a vehicle to stand for a purposed target. Hence, for instance, a mathematical model written on a paper or any scale model of a bridge represents, as long as it is

constituted by a user for specific purposes. Intention then seems the requisite character of the representation. Take for example the formula which represents the terminal velocity of objects falling in air. Symbols such as Vt or g and all the relations in the formula means nothing if a user did not specify beforehand that those symbols stand for terminal velocity and gravity respectively. So the upshot is that three–place relation between vehicle, target and user seems more appropriate in the case of representation, especially when compared with two–place relation between vehicle and target.

But, what does this list of statements tell us?

First of all, the list above is not exhaustive, but covers many of the statements that have been accepted or discussed, in one way or another, in the literature. Secondly, my aim, after all, is not to develop an exhaustive list. Instead, I aim to underline that any theory proposed (or will be proposed) to account for representation is generally expected to be consistent with these statements. In the following, I will proceed by giving two major substantive views on representation. Nevertheless, I will not elaborate on whether these major views are in conflict with one or more intuitive statements that I listed above. I briefly discuss this issue further in **§6.3**. My modest approach which is appropriate for my purposes in this chapter focuses on providing a broad overview of major accounts of representation. In the penultimate section of the chapter, I briefly discuss the view in which these two accounts are criticized.

3.5 Substantive Accounts

3.5.1 Isomorphism

Isomorphism is a term generally used in mathematics and specifically in abstract algebra. In one respect, the term has been fixed to define a mapping and equivalence relation between two sets. Given this definition, two sets are isomorphic if and only if their structural properties are same. So, set A is isomorphic to set B if and only if all relations and structural properties are preserved among the elements of set A and set B. To exemplify, let's say that set A contains all the odd numbers from 1 to 10 and set B contains all the even numbers from 11 to 20. These two sets are isomorphic in the sense that each member of set A maps a member of set B. Formally, it is expressed as,

$$f: \mathrm{A} \rightarrow \mathrm{B}$$

where f is a mapping function between finite sets A and B both including the mentioned odd and even numbers respectively. Here, we can express the f function as follows:

$$f\,(1) = 12$$

$$f\,(3) = 14$$

$$f\,(5) = 16$$

$$f\,(7) = 18$$

$$f\,(9) = 20$$

Notice that each member of set A is mapped into one and only one member of set B in an order. However, to construct an isomorphic structure a well–ordered mapping is not necessary between the members of two sets. So, a member of set A could be matched with another member of set B, say for example 9 with 16, 5 with 20 etc. The other important point to notice is that the members of our sets A and B perfectly correspond to each other. In mathematical terminology, this is called a bijective function. Bijective function is both one-to-one and onto function which is also called injection and surjection. Injective function means that each member of a domain (set A) is mapped into one member (not necessarily all the members) of its co–domain (set B). Surjective function, on the other hand, means that one (or more than one) member of domain is mapped into at least one member of its co–domain. In our example here, then, our function is both injective and surjective, so is bijective. In other words, the function f is both one-and-one and onto such that each member of domain is mapped into one and only one member of its co–domain.

Based on these definitions, to say there is an isomorphic relation between two sets is also to say that there is a bijective function between a domain and its co-domain. As our example shows, each odd number in set A is mapped into one and only one even number of set B. Moreover, mapping each member of a domain into one and only one member of its co-domain also allows us to create an inverse function. This is a distinctive feature of bijection which is both absent in injection and surjection. For

the injective function, there would be member(s) of co–domain that are not to be matched with a member of its domain. For example, if set B were composed of even numbers from 11 to 22, a member of co-domain (not necessarily 22) would not be matched with a member of its domain. Thus, if a function *f* is an injection, the inverse function does not exist. For the surjective function, in a similar vein, there would be at least two members of domain that are matched onto one member of its co-domain. For example, if set B were composed of even numbers from 11 to 22, two members of domain would necessarily be matched with one member, to cover all members, of its co–domain. Thus, again, if a function *f* is a surjection, the inverse function does not exist.

However, as seen in our example, bijective functions allow such an inverse mapping. To obtain an inverse function *f*' we reverse the mapping from co–domain to domain *i.e.* from set B to set A. In this case, we could express the function *f*' as follows:

$$f'(12) = 1$$

$$f'(14) = 3$$

$$f'(16) = 5$$

$$f'(18) = 7$$

$$f'(20) = 9$$

As it is seen, the inverse function *f*' (e) = o from the set of even numbers 11-20 to the set of odd numbers 1–10 is one–to–one and onto, so is a bijection. Recall the function *f* (o) = e from the set of odd numbers 1-10 to the set of even numbers 11-20. Essentially the same bijection exists, such that 12 is mapped with 1, 14 is mapped with 3 and so on. Notice again that what makes here bijection unique is its perfect matching character. So to say, each member of the co–domain can be the image of a member in the domain and vice versa as much as we apply the inverse function.

For our purposes here, perhaps, this general sketch will suffice. Apart from the algebraic considerations, what is important here is to keep in mind that an isomorphism is bijective morphism. That is, isomorphic objects possess a structure that may in turn allow us to construct a mapping relation among each other.

While isomorphism is a mathematical term with some specific meaning, it is used by philosophers of science in a manner analogous to account for the problem of representation. Especially, the mapping relation between isomorphic objects linked to the concept of representation. Since representation is a relation between the source and the target, these two need to share common structural properties. It is from this particular reason that isomorphism seems a good candidate to understand the mechanism which underlies the representational relation between two things. Therefore, various attempts have been made to broaden the term in contexts outside mathematics.[19]

Such an attempt was first made by van Fraassen (1980). We can relate van Fraassen's attempt to his general view of scientific theories. Let me briefly capture the relevant key points. According to van Fraassen, we cannot talk about true scientific theories, as realists suggest. As is known, scientific realists take the aim of science to be the discovery of the truth. So, for realists, "science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true" (van Fraassen 1980, p. 8). Van Fraassen disagrees. On his view, there is no rational basis to believe that a theory is true. A scientific theory, he maintains, is not true as realists suggest; rather it is at best empirically adequate. So, we cannot talk about scientific theories as vehicles that reflect us what the external world looks like. Alternatively, we should take them as vehicles which are empirically adequate or not such that whether or not it pictures the external world in an acceptable manner. Here, empirical adequacy comes in two ways. First, a theory is empirically adequate if it explains all empirical data we have, and second if it correctly predicts the world external to us. Notice that truth is not at issue here. Thus, the most important lesson we can draw from this overview perspective is simple: we can accept a theory without believing that it is true or with believing that it is empirically adequate. In other words, scientists can literally believe that what the theory says about observables is true but not what it says about unobservable ones.

[19] In this chapter, I merely introduce the main features of isomorphism. There are, however, different versions of isomorphism. For example, Andreas Bartels (2006) have defended that models are homomorphic representations of the world. The other version of isomorphism is called partial isomorphism which I take to be a more plausible option. I give my reasons why I do so in §6.5.

From this anti–realist perspective of science or sort of an epistemic attitude, it is easy to see that theories are not in a direct relation with phenomena. In order to understand this, we must additionally have an understanding of other relevant aspects of this view. For van Fraassen, a theory is a collection of models or structures which satisfy the axioms of that theory. So "any structure which satisfies the axioms of a theory in this way is called a *model* of that theory" (van Fraassen 1980, p. 43). Axioms of a theory are not true of phenomena; rather they if "suitably interpreted are true of the model" (van Fraassen 1980, p. 43). Notice that being true means a true description of a model in the sense that it satisfies the axioms, not in the sense of epistemological truth.[20] In this respect, models seem to be a mediator between theory and empirical data. The role of mediating is constituted by certain parts of models which are called empirical substructures. Hence, certain parts of models *i.e.* empirical substructures are identified with phenomena. Here, phenomena mean to be a structure in which a number of relations appear. Van Fraassen would rather call it appearances that "the structures which can be described in experimental and measurement reports" (van Fraassen 1980, p.64). Being structured in a certain way, thus, appearances can be embedded into other structures, namely empirical substructures or parts of a model.

Taking both these contexts into account, we can now consider how isomorphism is used to explain representation. As it is seen, for van Fraassen theories are not considered as candidates for direct representation of observable phenomena. This is because representation is a structural relation and theories are not themselves structures, at best they consist of structured models. So, not theories but their "models … and the *empirical substructures*" should be seen "as candidates for the direct representation of observable phenomena" (van Fraassen 1980, p. 64). In other words, the representing role is given to certain parts of models. However, parts of models are merely structures and in this respect require another structure to constitute a representation. Apparently, target system, phenomena or so-called appearances are also need to be structured in a certain way. To be sure, it is

[20] Notice that what distinguishes van Fraassen from other anti-realists, that we should take what the theories say literally as one would take scripture literally. What this means is that he does not deny that truth has a role to play. He is just claiming that no matter how successful the theory is, it is never rational to believe that things it says about unobservables to be true; although it is rational to believe the things the theory says about observables to be true.

necessary but not sufficient to represent a structure with any other structure. Insofar, any two things can be a structure but need not to be in a representational relationship with one another. Yet one other condition along with a structure is necessary: this is isomorphism. Thus, it is only after these conditions are met, then a target system is represented by empirical substructures of a model. In sum, according to van Fraassen, we are now led to the general conclusion that "the theory is empirically adequate if it has some model such that all appearances are isomorphic to empirical substructures of that model" (van Fraassen 1980, p. 64).

3.5.2 Similarity

Similarity[21] is the other prominent view championed by and originated with Giere (1988 and 1999). On this view, our sources such as theories or models represent phenomena or target systems by virtue of their shared similar properties. As it stands, this claim is far too quick. Yet, before proceeding, it would be better to review the basics of Giere's general views on theories and models, and their relation to phenomena.

Giere begins with a well-accepted assumption that theories are one of the primary means that are used by scientists to represent the world. To learn whether this assumption holds true for theories, he suggests that we need to understand theories at first. What is a scientific theory? For Giere, there is one proper way to answer this question. That is to examine science textbooks and describe "the character of a theory as it is understood by contemporary scientists" (Giere 1988, p. 63). To be sure, this may be seen as a weak suggestion because one can object many textbooks are inconsistent with each other and so cannot be regarded as an objective ground.

The objection is fair, but fails to come to grips with the issue of providing a better alternative. Is it "better to look at the work of philosophers of science and the few scientists concerned with the 'foundations' of their subject" (Giere 1988, p. 63)? According to Giere, this approach doesn't work any better. The reason is basically simple: philosophers start with the conception of a theory and try to reconstruct it in

[21] We must make a distinction between similarity and resemblance. Resemblance might differ from similarity in many ways. To put it simply, resemblance is usually understood as a relation of similar visual appearance between two objects; whereas similarity is a relation of similar subset properties shared by two objects (*cf.* Suárez 2003, p. 227). Thus, the concept of similarity is somewhat different from the concept of resemblance in the manner we use it in everyday life.

such a manner. They do not even look at scientific practice or examine textbooks to get a conception of what scientific theory means to scientists. Those with this attitude do not tend to speak about giving ear to what scientists really understand from a theory. Obviously, this tells little about the nature of scientific theory. Therefore, instead of seeking a perfect match between our understanding of a theory and actual theories, we need to concern how a theory is actually used in science.

An appropriate starting point for the examination of scientific theories is, then, case studies in the textbooks. Giere starts with focusing on classical mechanics which is usually treated as a typical example of a mature theory.[22] The two main conclusions he draws from his examination are as follows.

First, a theory is not an axiomatized collection of sentences with corresponding rules that allow attaching both theoretical terms to observational terms (see **§1.5.2**) . This is an objection to logical positivists who are also known as defenders of syntactic view of theories. According to Giere, a careful examination of scientific practice would show that theories consist of "informal references to the determination" of certain concepts such as "mass, force, position and momentum" (Giere 1988, p. 75). The reason to think that there should be such rules in the first place is a sort of fabrication. Still, if one wants to place correspondence rules to somewhere in the scientific theory, there is no logical way to refute this. However, Giere claims that this won't be an actual picture of a scientific theory; instead it will merely be a rational reconstruction.

Second, approximation and idealization are very common in scientific inquiry. Giere's examination on textbooks shows that scientists neglect particular factors which might be an obstacle to traceability and computability. So, a scientific law does not give a true description of phenomena. Even if it is intended to do so, a law for instance "would have to be incredibly more complex than any yet written down"

[22] The flagship example he used is simple pendulum. Typically, a simple pendulum consists of a mass suspended by a string in a vertical equilibrium position. When we displace the massive object with a particular angle, it would swing back and forth. The time that it completes one full oscillation we determine a period. This oscillation continues due to gravity. So, the object will continue to oscillate until the restoring force balances the object back to its equilibrium position. It seems proper to use this example (for many reasons that I will not get into now). One reason among others is that simple pendulum is closely related with Newton's laws, particularly the second one known as $f = ma$. To see what sort of a relation holds, it is enough to mention Hooke's law for springs which is formulated as $f = -kx$ where k and x stand for constant of stiffness and displacement, respectively.

(Giere 1988, p. 77).[23] Take for example the mass of a pendulum's bob. If oscillation occurs, we say that the Earth's mass is attracting, but we do not say that a tiny piece of dust on my shoulder is attracting as well. Actually, it does. However, it is a very small effect that is possible to be ignored. Besides, it is impossible to write down an equation involving all possible effects. Hence, for scientists "exact truth is not what matters. What matters is being close enough for the purpose at hand, whatever that might be" (Giere 1988, p.78).

Giere emphasizes these two main aspects of scientific theory. However the essential question still remains: What is a scientific theory? Some hints can be found, indeed. For example one may ask, if there are no correspondence rules, then how is it possible to connect theoretical sentences to observational sentences? A possible answer to this question would be that if there are no correspondence rules then there is no such thing as correspondence between theoretical statements and observational statements. This is acceptable, but leads to another question: how, then, it is possible for a theory to be a means that is used by scientists to represent phenomena? Here where models enter into the scene.

Giere suggests that not theories but "models are the means by which scientists represent the world" (Giere 1988, p. 80). Basically, the representational role that is given to scientific theories is changed or in fact it should never been expected from them to play that role at all. Alternatively, theories should be treated as a collection of models. This means that, elements of theories (like laws or equations) describes a wide range of models which constitute the theory itself. Here, theories gain a role describing true models. There is then mutually dependent relationship between a theory and its model. For example, "a theoretical model, the simple harmonic oscillator, perfectly satisfies its equation of motion" and the equation of motion "truly describes the model" (Giere 1988, p. 80).

With limiting the role of theories, in that sense, it seems apparent to many that the models gain a central position in science such as representing the phenomena or linking it with a theory. Therefore, the well-accepted assumption (the assumption that theories are one of the primary means to represent the world) that has been

[23] The issue is related to abstraction as well, see §8.4.

brought out at the outset is false. In other words, the assumption is false in that it does not reflect the actual scientific practice.

There is one other issue that needs to be addressed. So far, we have talked about theories, but we still have not managed to identify what a theory is. All we have is that a motto which states that theories are a collection of models. So, at first, one need to figure out this issue, either by identifying what a theory is or by saying what a model is. Giere chooses the latter. He goes on to suggest that if models are explained to an extent, then the scientific theory would be no more than an "honorific label" (Giere 1998, p. 84).

According to Giere, models are "nonlinguistic, abstract entities" (Giere 1988, p. 84). To put it differently, they are not statements which we can use to make claims directly about phenomena. However, this does not mean to say that we cannot "use language to characterize our models, and what we say of the model is true" (Giere 1999, p. 213). Of course, we can assert a statement about whether a representation relation holds or not. We can say, for example, it is a true statement that a model of a simple pendulum represents certain antique wall clock which has a pendulum bob. But we cannot say that the model is true of the clock. For one thing, a simple pendulum and an antique clock are both nonlinguistic entities that cannot be assigned truth values. Even, if we are to characterize them, we may talk about at best the concepts of accuracy, completeness or consistency; but not about the concept of truth. Nevertheless, we may speak of truth when the issue comes to the relationship between theories and models. However "the truth" we mean "here has no *epistemological* significance" (Giere 1988, p. 79). As distinct from its ordinary use, the truth here indicates that elements of a theory (*e.g.* axioms, equations) truly describe the model so much so that the model mutually satisfies the elements of that theory.[24]

According to Giere, the relationship between theories, models and phenomena is by and large explained thus far. What remains is now only to show how models

[24] According to Giere, laws of motion plus the universal gravitational law defines what it means for a system to be Newtonian and so these are just definitions *i.e.*, they are not laws of nature understood traditionally. However, the statement such 'our solar system is Newtonian' is either true or false and this is the empirical part. Hence, it does not seem exactly correct to say that truth plays no role in Giere's account.

represent the designated phenomena. To explain representation, in the preceding section, the candidate we considered was isomorphism. Given the isomorphism thesis, a model represents its target if all appearances are isomorphic to empirical substructures of that model. While there is no logical ground to reject this thesis, it is still suspicious for Giere, as to whether this view does reflect the actual science:

> Van Fraassen (1980) suggested that the desired relationship is one of isomorphism. Now, there is surely no logical reason why a real system might not actually be isomorphic to some model. Yet for none of the examples cited in standard mechanics texts, for example, is there any claim of isomorphism. Indeed, the texts often explicitly note respects in which the model fails to be isomorphic to the real system. In other words, the texts often explicitly rule out claims of isomorphism. If we are to do justice to the scientists' own presentations of theory, we must find a weaker interpretation of the relationship between model and real system (Giere 1988, pp. 80-81).

According to Giere, a weaker interpretation of the relationship between model and real system is needed. He suggests the relation of similarity. In other words, the representational relation between a model and a real system is constituted by virtue of similarity. So, he argues that a model represents a real system insofar the relation of similarity exists between them –but only with caveats. As we may guess, anything is similar to anything else in one way or another, but this does not mean necessarily that a representational relation would hold between any pair of particular thing. In this regard, we need to specify "relevant respects and degrees" (Giere 1988, p. 81). Thus, our hypothesis would be that if an "identifiable real system is similar to a designated model in indicated respects and degrees" then it is true that our model represents its target (Giere 1988, p. 81).

In order to explain what he means by the respects and degrees, Giere provides an example of scientific hypothesis and further restates it in a clear way. In the following example, both hypotheses are expressing the same thing which is about Newton's two-body system what we usually encounter in textbooks. The difference is that the second hypothesis, compared with the first, is a much more explicit version of the same expression. Oftentimes, scientists express their ideas in a compact way, like expressed in the first one.

> The earth and moon form, to a high degree of approximation, a two–particle Newtonian gravitational system.

> The positions and velocities of the earth and moon in the earth–moon system are very close to those of a two-particle Newtonian model with an inverse square central force (Giere 1988, p. 81).

According to Giere, this example shows in this case that similarity is coming in form of two respects and one degree. To put it specifically "here the respects are 'position' and 'velocity', while the degree is claimed to be 'very close'" (Giere 1988, p. 81). It is important to note that, the first claim also includes the same respects and degree, but in a sort of encapsulated form. As distinct from the first, in the second claim, the two respects and degree introduced together with the concept of 'approximation'. So, although it seems a difference, it is of no importance for Giere *i.e.,* similarity still stands.

Let me summarize what I have covered so far. Giere's first step is to argue against the view that scientific theories are sets of statements which allow us to represent directly the real world. Instead, he suggests a model-based account of scientific theory. On this account, theories are not in a direct relationship with the real world. On the contrary, "the relationship is indirect through the intermediary of a theoretical model" (Giere 1988, p. 82). So, there is no correspondence between statements and the real world, but there is correspondence between models and the real world. This correspondence is "not one of truth between a linguistic entity and a real object, but of similarity between two objects, one abstract and one real" (Giere 1988, p. 82). Thus, we need to explore whether similarity relation holds between model and target system. Under this kind of framework, the truth is not an issue in representational relationship.[25] "Similarity" is then "the relationship that does the heavy representational work" (Giere 1988, p. 82).

3.6 Much Ado about Nothing or Is There Really a Special Problem of Scientific Representation?

Is scientific representation a genuine and separate epistemic special problem at all? This question recently raised in a seminal paper by Craig Callender and Jonathan Cohen (2006). Although, they do not see the problem of scientific representation as

[25] To be sure, under a different framework, the statement 'this actual World system is similar to the theoretical model' is either true or false. Of course, a model is not a linguistic entity that takes truth values, nor is the real World system but the statement describing the relationship is. Between the two are linguistic entities since they are statements and hence can take the truth values.

entirely non–special, they specifically tackle the "constitution question" and treat it as a "non-issue" (Callender and Cohen 2006, pp. 67–68). On their view, the alleged constitution problem of scientific representation is reducible to the problem of mental representation –which already constitutes a great part of the philosophy of mind literature. In this way, including scientific one "we won't need separate theories to account for artistic, linguistic, representation, and culinary representation"; instead an all encompassing theory derived from the philosophy of mind would allow us to explain all types of representations, which is by all means "economical and natural" (Callender and Cohen 2006, p.70).

Callender and Cohen take their point of departure from the works of British philosopher Herbert Paul Grice (1913–1988). For them, Grice's works on mental representation provides a framework to understand scientific representation. This is called by them the 'General Gricean' account which is "straightforwardly applicable" to any type of representation (Callender and Cohen 2006, p. 77). On this view, representative entities such as equations, diagrams, models, toothpicks and the like, gain their representative status from a more fundamental level. In other words, *Griceanism* proposes that all types of representations "can be explained (in a unified way) as deriving from some more fundamental sorts of representations, which are typically taken to be mental states" (Callender and Cohen 2006, p. 70). In such a case, the representation user, driven by his needs, can in principle stipulate any vehicle to represent a thing or a system, provided that to the hearer beforehand what is or will be a vehicle to represent a target.

At first sight, one can find this view hard to swallow such that, if all vehicles essentially gain their representative status through its user's stipulation, then "virtually anything can be stipulated to be a representational vehicle for the representation of virtually anything" (Callender and Cohen 2006, p. 74). While Callender and Cohen embrace such an implication, they still remain cautious about the issue of selecting a representational vehicle. According to them, in principle, anything can be stipulated to represent anything else; however we still have pragmatic constraints in selecting or preferring our presentational vehicle for one rather than the other. To put it differently, pragmatic constraints play an important role for users to make a choice of their representational vehicle. A simple illustration

of this idea given by the authors is as follows: Suppose one wants to represent the geography of Michigan State with his hands. Which of his hand would be better to use? An intuitive answer would be that, his upturned right hand is more suitable to represent Michigan with respect to his upturned left hand. It is not because his upturned left hand, in principle, could not represent the same target; however his upturned right hand shares more *similar* geometric structure with Michigan's geography. A parallel situation can be seen in scientific contexts. Take for instance, the billiard ball model of an ideal gas. It is, in principle, possible for a user to represent an ideal gas behavior with a cup of instant coffee granules as long as it is provided for the hearer to think that each granule as a molecule. Nevertheless, compared with the billiard ball model, that would not be a suitable representation. The reason is because like molecules, billiard balls seems more identical to each other as well as would be more likely for the hearer to visualize the pressure on the walls, interaction of the molecules and the temperature.

In identifying similarity as a pragmatic aid for users' choices of representational vehicle, Callender and Cohen develop a sort of agent–based approach on representation. That is to say there is no representation without representers, and thus, for instance, similarity is "unnecessary for representation" (Callender and Cohen 2006, p. 76). Moreover, similarity is not the only candidate to be excluded in a constitution of representation. Other candidates such as isomorphism, partial isomorphism and homomorphism are also unnecessary for a user to constitute a representational activity.

In my opinion, this is the most original part of Gricean argument. As discussed in the previous sections, some views suggest that scientific representation is a two-place or dyadic relation which involves similarity, isomorphism, partial isomorphism or another type of relation between the vehicle and its target. This means that, when we say 'A (vehicle) represents B (target)' we say that 'A is similar; or isomorphic; or partially isomorphic to B', since a representation is a two-place relation between A and B. However, a Gricean would disagree. For him, user's stipulation is sufficient to represent B with A, irrespective of whether there is a similarity or an isomorphism in between. Therefore, such an encompassing theory might have one more advantage in preventing a possible problem concerning the explanation of dissimilar or non-

isomorphic representation. In such a case, we are allowed to say that the constitution of a representation depends not on the relation between A and B, but is on the stipulation of a user. In other words, if a user means B by uttering A in order to produce the intended belief in a hearer; he provides a representation that is all common to other types of representation.

To sum up, Callender and Cohen see similarity and isomorphism as "idle wheels in the representational machinery" and not "necessary features of representation" (Callender and Cohen 2006, p.78). Nevertheless, they still do not overlook the pragmatic role played by them, hence the choice of a *proper* vehicle. As I mentioned above, these relations only provide an explanation for a question why a user have chosen a vehicle (model X, equation X) rather than any other one (model Y, equation Y). But this process of choice has nothing to do with representing. In order to understand the activity of representing, one should commit himself to the issues in mental representation.

3.7 Conclusion

In this chapter, I considered descriptively the problem of scientific representation. This problem mainly associated with the representational relation between models and their target systems. In this regard, two major accounts of representation are presented, namely the views of isomorphism and similarity. I remained neutral between these two accounts deliberately because, as I will argue in **§6.4**, both views fail to account for some models and their representational relations of their targets. Especially, the case study that I present in Chapter 5 would be useful to see the failures of these two major accounts. I also argue against the Gricean argument in **§6.6**.

CHAPTER FOUR

THE EXPLANATORY STATUS OF IDEALIZED MODELS

4.1 Introduction

Idealizations are prominent characteristics of modeling. Each model, at least, involves one or more idealizations. We can briefly describe idealizations as false assumptions contained in a model. In other words, scientists distort physical reality in a particular way to specify a target system *via* idealization. This common attempt in science has some problematic implications, though not for science, but for philosophy. In this chapter, I aim to introduce and discuss these problems. In conclusion, I explore the strict relationship between idealization and representation by providing reasons why one depends on another and vice versa.

4.2 Idealized but Explanatory?

The issue concerning the nature of explanation has a long history in philosophy of science. A considerable part of the discussion has involved a debate about whether we can provide a model of explanation. Some philosophers of science asserted that they were able to provide an explanation model which is applicable to all scientific explanations in science (Hempel and Oppenheim 1948). In literature, their explanation model is known as deductive-nomological (D–N) model of explanation. This model has two parts: explanans and explanandum. Explanans, on the one hand, consist of true premises (composed of initial and background conditions) and at least a law of nature. On the other hand, explanandum is the phenomenon to be explained by means of the explanans. More specifically "the explanandum must be a logical consequence of the explanans; in other words, the explanandum must be logically deducible from the information contained in the explanans" (Hempel and Oppenheim 1948, p. 137).

However, D–N model of explanation cannot account for all scientific explanations because some explanations involve idealizations *i.e.,* false statements that constitute the explanans. If we consider false statements of the explanans as a means to do explanation, then it would be problematic to explain the explanandum as well. In other words, the truth (explanandum itself) is preserved if and only if the statements

of the explanans are true.[26] So, idealizations pose a serious problem for *Hempelian* explanation model. As far as I know, this point was firstly raised by Robert W. Batterman (2005). According to him, some explanations, like asymptotic ones, are not appropriate to fit D–N model of explanation. He states that "asymptotic explanation, in that it typically involves idealizations, fails (at least in some instances) to meet one of Hempel's conditions on an adequate explanation –namely, truth (Batterman 2005, p. 162, f.n.6).[27]

Two possible situations can be outlined briefly: we should either claim that there is a problem in D–N model of explanation or neglect idealizations in science. It seems reasonable to reject the former claim, because if we accept the latter; we might be faced with a situation in which we ignore a common scientific practice. In this case, we will be in need of another account of explanation. In other words, D–N model of explanation is apparently one model of explanation among its other possible alternatives.[28] Perhaps one might further ask whether we can provide a model of explanation that can capture all kinds of explanation or might urge to end this philosophical endeavor completely. However one also needs another argument for this sort of conclusion. So, for the present, the question still stands: How can models be explanatory while containing many idealizations?[29]

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[26] From the logical point of view, even false statements can lead to true conclusions in valid arguments; the explanandum can still be false. Thus the explanations that involve idealizations i.e. false statements are always subject to doubts in the sense whether they are genuine explanation or not.

[27] Subsequently, the point of Batterman was interpreted in a similar way by some other authors. For example, Daniele Molinini (2014) presents Batterman's observations about the inappropriateness of D–N model as follows: "His observation concerns the use of idealization in the explanation involved. According to Hempel and Oppenheim, an adequate D–N explanation must meet the 'empirical condition of adequacy' i.e. the sentences constituting the explanans must be true. Hence any explanation appealing to mathematical idealizations and idealized structures, which are false, is automatically excluded as good candidate for the D–N logical machinery" (pp. 229-230).

[28] Hempel also suggests inductive-statistical model of explanation (I-S). This model has many similarities with deductive-nomological model of explanation. It merely differs from I-S explanation by its logical entailment i.e., I-S explanations are not deductive arguments which entails the explanandum; instead they are inductive arguments which assign high probability on explanandum. Nevertheless, the I-S explanation involves true statements (like involved in the D-N model of explanation) to induce explanandum. It is beyond the scope of this chapter whether the similar problems about idealization might, at least in principle, hold for I-S model of explanation. For this reason, I confine myself to the deductive approach of explanation.

[29] Idealization is not the only problem for D-N model of explanation –there are many counterexamples against it. It would be sufficient to mention male pregnancy example of Wesley Salmon (1971). According to Salmon, the explanans might contain a scientific law (or better to put it a law-like statement) which is "is not required to explain the phenomenon" –like the one, for instance, 'Birth control pills prevent males to be pregnant' (p. 34). Despite such irrelevant law statement, the whole explanation fit the model of D-N explanation.

4.3 Approaches to Idealized Model Explanations

The standard philosophical position in the literature claims that idealized models can provide explanation (Bokulich 2011, Rice 2017). According to this view, idealizations can be employed because they do not play a fundamental role in explanation. The general framework of the philosophical discussion can be summarized in this way: Standard view of idealization claims that *despite* idealizations the models can explain or be explanatory. The other views, differently, say that models can explain or be explanatory not despite but *in virtue of* idealizations. In the following these two positions are briefly presented.

4.3.1 Standard views of idealization

4.3.1.1 Structural explanations of idealized models

According to Ernan McMullin (1978, 1985) structural explanations are the idealized type of explanations. A model has structural relationship with the system that intended to be explained. This relationship is not nomotetic as D–N model suggests because in nomotetic explanations explanans only manifest the explanandum deductively, and account for explanandum by means of true statements. For McMullin (1978), however, this is "neither necessary nor sufficient" for the explanations "the scientist ordinarily seeks" (p. 145). Rather, scientists put forward a different kind of explanations that can be called hypothetic-structural (H–S) explanations. By giving these explanations scientists argue "from cause to effect" not "from effect to cause" (McMullin 1978, p. 139). For example, the Sun's emission of radiant energy is caused by nuclear fusions; the seismographic movement over the Earth's surface is caused by motions in the mantle; or the hardness of the diamond is caused by covalent bonds. Here each explanation is causal because the structures that provide the explanations are also their own causes. We usually call these structures 'models' because different structures like different models can explain the same explanandum. For example, if an astronomer explains annual shadow variations on the Moon by means of mountains and peaks, he does not only explain the shadow variations –he also explains additional phenomenon related to the Moon *i.e.*, he explains that there exists mountains and peaks on the Moon.

> Explanations of this sort, when strongly supported, not only account for the explanandum by showing it to be deducible from the explanans (as does nomotetic explanation) but also warrant a hypothetical assertion about the elements and relations constituting the explanandum (McMullin 1978, p. 145).

Nonetheless "HS explanation is … always tentative" (1978, p. 145). In other words, H–S model of explanation does not disclose causes as nomotetic D–N explanation does. The factors that need to be included to the model would be added after during the course of the research program. McMullin calls this 'adding back' operation 'Galilean Idealization'. These kinds of idealizations are temporary because the model should be refined enough to be more and more accurate. The reason why in the beginning scientists lack such accurate model can be tied to preserve mathematical tractability or to give an easy explanation. These models "can be made more specific by eliminating simplifying assumptions and 'de-idealization', as it were, [then] the model serves as the basis for a continuing research program" (McMullin 1985, p. 261; see also **§7.2.1**). In brief, an idealized model, tentatively, can explain, or to put it more concisely an idealized model can provide approximate explanations. By de-idealization, scientist would make better approximations in due course.

It seems that McMullin is in a position to avoid the problems of *Hempelian* models of explanation. His H-S model of explanation makes room for idealized models (in his terms structures). In this manner, H–S explanations "warrant a hypothetical assertion about the elements and relations constituting the *explanandum*" (McMullin 1978, p. 145). The hypothetical assertion included in explanans is also called 'idealization'. So, it is always amenable to de-idealization. This is why, for McMullin, such explanation is always tentative.

4.3.1.2 Non-difference making idealizations

Non-difference making idealizations differ slightly from Galilean Idealizations. Given this view, the idealizations of some models are harmless because they have nothing to do with the components of the model that do the real explanatory work. Thus, these models, despite involving idealizations, are appropriate to use in scientific research.

a) Harmless idealizations. Mehmet Elgin and Elliot Sober (2002) claim that some models are explanatory in spite of containing idealizations. According to them, the

"optimality models [in evolutionary biology] are explanatory despite the fact that they contain idealizations" (Elgin and Sober 2002, p. 447). The basic idea behind this proposed view is the idealizations of this sort make little difference in the predicted outcome. "The idealizations in a causal model are harmless if correcting them wouldn't make much difference in the predicted value of the effect variable. Harmless idealizations can be explanatory" (Elgin and Sober 2002, p. 448).

The explanation by means of harmless idealizations differs from Galilean Idealizations in one respect. Galilean Idealizations are part of a scientific research program and commits to refine the model to be more accurate than its previous version by representing the intended target system more completely. Therefore, one might infer from this thought that the more accurate a model, the better explanation of the target explanandum. However, harmless idealizations need no such refinement because correcting the distortions of the model would make little difference in the predicted value of the effect.

b) The difference–making causal factors. According to Michael Strevens (2009) idealized models are explanatory too and these models can be described as follows:

> The content of an idealized model, then, can be divided into two parts. The first part contains the difference-makers for the explanatory target…The second part is all idealization… that do not make a difference to the explanatory target (Strevens 2009, p. 318).

According to Strevens, models can explain by the causal factors that make difference to the explanandum. So, if giving an explanation is to give a causal story about the occurrence of a phenomenon, then one needs to reveal the difference-making parts. The idealized parts of the model do not make difference in the occurrence of a phenomenon and thus they do not play a role in explanation. Therefore, for Strevens, the role of idealization is at best "to point to parts of the actual world that do not make a difference to the explanatory target" (Strevens 2009, p. 318). The view of Strevens is similar to the view about harmless idealizations. What makes his view distinctive is that idealized parts are 'explanatory irrelevancies'. However, they are still useful for us to identify that they are non-difference making parts in explanation.

c) Partial truth and idealizations. A similar view to that of Strevens has been proposed by Arnon Levy (2015). According to Levy, models consist of two parts,

one is the idealized part and the other is non-idealized part. However, these models are not approximately true just because they have idealized parts. Models, as he suggests, are partially true and partially untrue. For example, the ideal gas model to explain Boyle's law is not approximately true. Rather, it is partially untrue because it assumes that molecules do not collide (in real, however, they collide with each other). On the other hand, the same model is partially true because it explains energy distribution in terms of pressure and volume. Therefore, the ideal gas model "is partly true, *i.e.* with respect to the role of the motion of particles; and partly untrue, *i.e.* with respect to the role of collisions among particles" (Levy 2015, p. 793).

Here we see that there are partially true and partially untrue parts, and there is no more or less of them. The assumption of non–colliding particles is not less true, but untrue. Thus the idealizations involved in the model do not make a model approximately true. The true parts of the model subsume an explanandum and do the explanatory work. The remaining parts, such as particle collisions that mentioned in the example, do not make any difference.

4.3.2 Other views on idealization

There are some other views on how idealizations explain. It is possible to group them under different subsections. Contrary to the versions of standard view, these views broadly assert that idealizations occasionally play an important role for explanation.

4.3.2.1 Non–representational account

Standard views of idealization commonly accept that the statements that constitute explanans should truly represent. In parallel to this, Margaret Morrison asserts that "the explanatory role is a function of the representational features of the model" (Morrison 1999, p. 64). In this respect, the standard views agree with D–N model of explanation to a certain extent. However the distinctive suggestion of the standard views is that while the false statements of the explanans do not carry out explanatory work, the remaining part should represent reality in order to do so. At least, the standard views of idealization are categorized this way in the literature.

The philosophical positions that stand against the standard view claims that representation (generally observational statements) does not have an essential function in providing explanation. For example, according to Ashley Graham

Kennedy (2012) “scientific models explain via their parts that do not accurately or adequately represent the target system being modeled” and these inaccurately represented parts “often do the real explanatory work” (p. 327). She goes on to claim that idealized models function as an activity to allow scientist to make comparison with other idealized models (apart from their degree of idealization or their faithful representation). To support her view, she gives a case study about accretion disk models. These models are built to understand the disk–like flow of (generally) plasmatic particles around astronomical objects. To be sure, the modeler who applies the model inevitably makes some particular idealizations. The first thing what she does is to explain particular astronomic phenomena by means of 2–dimensional disk model. Intuitively, more refined or less idealized models, like a 3–dimensional disk model, would better explain the disks which locate around active galaxies and quasars. In this case, one might think that one should favor 3–dimensional disk model over 2–dimensional disk model. However this is not the case. According to Kennedy, the 2–dimensional disk model is essential part of explanation precisely because of its idealization. As she puts it, “[t]he simplified two–dimensional models are themselves required for explanation because they enable the scientists to identify which factors make a causal difference to the evolution of the disks” (Kennedy 2012, p. 331).

As this case study shows, it would not be possible to compare 3-dimensional model with 2–dimensional model if the latter model (with its false components) would not exist. However, for example, only by this comparison, scientists became able to show “magneto rotational instability exhibited by the evolving disks” (Kennedy 2012, p.331). So,“[t]his means that idealizations are not just as a way of identifying what does not make a causal difference in the modeled target, but are often a way of identifying what does make a causal difference” (Kennedy 2012, p.331).

In her next article, which is written with Julie Jebeile in 2015, Kennedy expanded this idea by stating that explanation should not be seen as a product. Instead, for them, explanation should be evaluated as an activity which specifically is the activity of comparison. They recapitulate their idea as follows: “[D]e–idealized models, in themselves, are often not enough for explanation of a target system. Instead, the

activity of comparison, which depends upon the use of idealization, in some cases plays an important explanatory role (Jebeile and Kennedy 2015, p. 389).

Andrew Wayne (2011), in a similar vein, rejects the idea that representation is a prerequisite for explanation. According to him, standard views are committed to the thesis that the statements of explanans should be, at least, approximately true in order to provide explanation. In other words, "statements figuring in successful explanations describe elements of an idealization that one is inclined to say 'successfully represent' or 'are about' relevant features of the physical system" (Wayne 2011, p. 834). The other tenet of the standard view is to accept de-idealization process as a step for a model to have better representation and thus to provide a better explanation.[30] Wayne challenges both these two aspects of the standard view.

According to Wayne, not all, but some explanations in physics are different from those that presented by the standard views. Many explanations do not successfully represent physical systems (or target systems). Additionally, many idealizations are not correctable and thus are not eliminable. In order to show this, Wayne uses an example about pendulums which have linear and quadratic damping behaviors. According to him, the model used for non-linear pendulum[31] is neither the correct version of linear models, nor its contained idealization is eliminable. However, "the idealization in [damping] case is non–Galilean, [but] underwrite a full explanation of the quadratic damping behavior" (Wayne 2011, p. 835).So, Wayne states that some idealizations cannot be de–idealized but can be explanatory. In other words, contrary to Galilean Idealizations, some idealizations are not correctable but are able to provide explanations. In this manner, Wayne (2011) suggests that "the role for explanatory idealizations should be expanded" because uncorrectable idealizations fail to fit with the standard understanding of scientific explanation (p. 840). The criticism of Wayne is reasonable to an extent. However, he does not provide an account of how uncorrectable idealizations explain. If one is able to present an

[30] However, as I indicated above, this is not a common assumption among *all* versions of standard views.
[31] As the author notes, non-linear pendulum is a pendulum with quadratic damping oscillation.

account of explanation that successfully cover the kind of idealizations Wayne introduces, then his critics would actually be more convincing, indeed.

4.3.2.2 Does the truth matter? The aim of science as understanding

Another view to open a space for idealization is based upon the concept of 'understanding'. Moti Mizrahi (2012) claims that idealizations mainly produce understanding. However, this understanding is quasi–factive. Namely, despite being idealized we can use models to develop a scientific understanding about facts. Therefore, the object of this scientific understanding is not limited to model, but also to the relationship between models and phenomena. In other words, scientific understanding is not merely an understanding of a model but an understanding of the inferences that drawn from the model to phenomenon and vice versa. Mizrahi bases his thesis on Jonathan L. Kvanvig's works, as he quotes him:

> One might understand the model or theory itself, as when one understands phlogiston theory. One does not thereby understand combustion, however. Understanding the world scientifically is not simply a matter of understanding the given model but involves, rather, some relationship between the model and reality. Scientific understanding of the sort in question consists in the possession of a model and a realization of the extent to which it is an idealization and what aspects of reality the model is intended to shed light on (Kvanvig 2009, p. 342).

According to Mizrahi, in many cases, having a scientific understanding does not mean to have a scientific understanding of a model. If this would be the case, that is if models would not "mirror the facts to some extent" then we couldn't attribute "a sort of cognitive success to scientists when they employ idealizations" (Mizrahi 2012, p. 246).

However, if one takes scientific understanding as quasi–factive (but not factive or non–factive), then one would be in a position to explain both idealizations and the model-reality relation. For one reason: scientific understanding is idealized in terms of involving false assumptions but this does not mean that the understanding could not be about facts. Therefore, idealized models can provide us quasi–factive understanding.

Angela Potochnik (2015), in her recent article, elaborates the assumption of Mizrahi. She does not specifically investigate whether scientific understanding is factive or

quasi–factive. Instead, her basic suggestion is to drop the idea of truth for the purpose of justification of idealizations in science. According to this line of thought, one can account for idealizations, if the aim of science is considered to provide 'understanding' (rather than to discover truth). Potochnik argues that scientific models are (and inevitably will) always be idealized. In this manner, "idealizations" would be "patently untrue" and "their continued presence in models cannot be justified by their contribution to the truth of those models" (Potochnik 2015, p.72).

However, if we replace the epistemic aim of science (*i.e.,* if we accept that the aim of science is to produce understanding), then idealizations would appropriately be tenable. Nevertheless, 'truth' still plays a vital role in producing understanding, so it would be unreasonable to put it aside. With this intention, Potochnik uses the 'true enough' idea of Catherine Z. Elgin (2004). This idea is about our explanations which are not completely true but consist of *enough* true propositions that are used for the explanation. For example,

> [S]nell's law [is a law that] governs light's angle of refraction when it passes from one medium to another. Snell's law is only true of optically isotropic media, but it is *true enough* of media that are nearly isotropic, which include a wide range of media in which physicists are interested (Potochnik 2015, p.73).

As shown in the example, Snell's law is not only applicable for a specific medium but also applicable to the mediums which are close to that specific medium. Therefore, the truth of the law (or scientific model) is enough to function in other mediums –even if it does not represent it truly.

This seems to be similar to de–idealization. For example, one can obtain a more 'accurate' model by de–idealization *i.e.,* bringing the model closer and closer to the target system. However, Potochnik disagrees. According to her, "there is no lower limit on when an idealization qualifies as true enough" (Potochnik 2015, p.74). In other words, a more accurate model need not improve understanding. She gives another example for this claim as follows:

> In many circumstances the idealized representation alone, without any corrections, is employed. For example, the ideal gas law remains central, even though there is a more accurate alternative –van der Waal's equation, which takes into account molecular size and

> intermolecular attraction, properties the ideal gas law simply idealizes away (Potochnik 2015, pp.73–74).

Apparently, for Potochnik, Galilean Idealization is not always an admissible strategy in science. In other words, the development of the accurate parts of the model does not warrant that the model produces more understanding.

4.3.2.3 Agent–based idealization

The other view concerning the tenability of idealizations is developed by Christopher Pincock (2007). Although he did not call his position by a particular name, we may call it 'agent–based idealization'. According to Pincock, some views on idealizations share the following idea: if a model is a good representation of its target system, then it is less idealized and thus quite mathematically intractable. Conversely, if a model is a bad representation then it is much idealized but mathematically tractable. Pincock claims that he has reduced the asymmetry between a model's representative status and its mathematical tractability.

According to Pincock, the modeler chooses a relevant model among the class of models. As such, the relevant model does not match directly with the physical structure that aimed to be modeled. For this reason, modeler needs to transform the relevant model to a matching model. Physical reality is smoothed out in matching models. In this way, a homomorphic (or isomorphic) relation between the matching model and the physical situation can be constructed. This procedure is driven by the interests of the modeler. For example, a model that constructed to explain heat transfer of iron bars should consider the changes of the position of the molecules with respect to time, whereas it need not take the color of the iron bars into account. What should be included and excluded from the model is specified by the intentions of modeler. In other words, there is not *only* a "structure preserving mapping" relation between physical reality and the matching model (Pincock 2007, p. 962). To understand whether a model is a good representation or not, one "must look at the goals that the scientists have" (Pincock 2007, p. 962). As he puts it,

> [A] model represents a physical situation when the scientists believe both that (i) there is an isomorphism between the physical situation and a matching model and (ii) there is an acceptable mathematical transformation between the equation model and the matching model (Pincock 2007, pp. 962-963).

At this point, however, it is still open for discussion to what extent one can objectively measure how much a transformation is acceptable, and how much is unacceptable. To avoid this problem, Pincock suggests that prediction of the idealized (matching) model is an objective criterion to understand its representational success:

> [A] series of successful predictions can convince us that the idealized scientific representation is a good one, despite our ignorance about exactly how the mathematical transformation between the equation model and the matching model should be specified (Pincock 2007, p. 964).

This view differs from the standard approaches to idealizations. Standard view includes the claim that idealizations are harmless as long as correcting them doesn't make much difference to the predicted value of the dependent effect variable. Pincock's account does not admit to this but claims that idealized scientific representation is good insofar its predictions are successful.[32]

4.4 Conclusion

In this chapter, I discussed the standard view of idealization with its alternatives. The standard view of idealization aims at accounting for how models satisfactorily explain *despite* idealizations. In this respect, there are generally two different types of justifications. According to the first type of justification, false assumptions (namely idealizations) are rationally acceptable, but only with a caveat. Given this, an idealization may be permissible under the commitment that it will further be de-idealized. So, although the idealizations we accept as provisional give an approximate explanation, they need to be corrected or de–idealized afterward to reach the desired explanation. In this manner, the idealizations are pragmatically justified. The other type of justification is somewhat different from the justification employed by the pragmatic view. According to this type of justification, there is no need to make commitment to correct the idealizations of the model. Given this view, although successful models contain idealizations, they also contain representations of true causal factors *i.e.,* the factors that give rise to the explanandum. To understand

[32] In some cases, the predicted phenomena are too long to be occurred for understanding whether a representation is good or not. Particular climate models, for instance, predict that there will be global warming about a hundred years later. Pincock's account of idealization does not provide any information on such situations.

this, one needs to contrast the values of the explanations and predictions with the actual data. The high match rate would give us strong reasons to believe that the model truly represents the relevant causal factors. The remaining idealized factors, then, could be treated as irrelevant by their 'harmless' or 'non–difference making' status. Therefore, de–idealizing such factors would not contribute crucially to the explanatory status of the model.

Other views developed as an alternative to the standard view of idealization are also diverse in themselves. However, it seems possible to gather these views into at least two main groups. The first group argues that some models provide explanations *in virtue of* idealizations. In this sense, de-idealization of some idealizations does not make a positive contribution to the explanation of the model; even worse, doing it could be an obstacle for the explanation itself because, according to this view, some models are built on indispensable idealizations (for some particular examples, see **§8.5**). The second group differs not only from the standard view, but also from those who support the '*in virtue of*' view of idealization. This view basically argues that the ideal of explanation in science is unattainable or unrealizable. Accordingly, science does not (and could not) aim at explanation in the first place because of the idealizations themselves. So, expecting an 'ideal explanation' from the model that contains false assumptions would be inconsistent. Nevertheless, many idealized models in science are deemed to be successful. So, the proponents of this view need to be clear on where their success lies. Given this view, the success of idealized models does not stem from the fact that they provide explanation, but from the fact that they provide understanding. In other words, science is successful in a sense that its models provide us understanding, but not explanation. If one goes beyond this idea, it seems unlikely –in principle– to present an answer to the question of how idealized models explain. Seen from this context, those who defend this view make room for idealization by sacrificing scientific explanation for understanding.

The essence of the dichotomy between the standard view and others boils down to the issue of how models should be evaluated.[33] Standard view suggests that models

[33] I should perhaps note that there are different aspects of the issue. For example, there is a growing literature on the two types of model explanations, namely how–actually and how–possibly explanations (see Resnik 1991; Bokulich 2014; Grüne–Yanoff and Verreault–Julien 2021). A how–actually explanation (hereafter HAE) is an actual explanation when a model both represents and

should capture their target systems to a certain degree. From this point of view, models are partially accurate, so they should truly represent the relevant aspects of the target system. Having a partially true representation of the system is crucial since it is this representation relationship that makes models capable of providing explanation. Thus, partially accurate representation is a prerequisite for model explanation. Some of the other views, however, are not inclined to defend such a view. Given these views, representation is indispensable for providing explanation, though it does not have to be accurate. In other words, some models in science provide *explanation by misrepresentation*.

In general, the disagreement between these two groups of views seems to be related to the nature of the models. A group of philosophers of science regard models as having a specific structure. According to this view, this structure accurately represents the intended target system. While this representation of the model is not a complete true representation of the target system, it will still be a sufficient representation of the factors that are explanatorily relevant to the explanandum. At the same time, the explanation or prediction to be obtained in this way could also be considered as sufficient. The other group, on the other hand, is neutral about whether models have specific structures or not. According to this view, models should be evaluated in terms of their function, not whether they could accurately represent or not. In order to evaluate models properly, it is sufficient to focus on their good explanations and predictions. In this sense, models become black boxes in which the ongoing internal process need not have to be known.

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explains the actual situation. A how–possibly explanation (hereafter HPE), on the other hand, is a possible explanation when a model provides explanation without representing the actual situation. The fundamental problem stems from the latter type of explanations. To put it more specifically, if a model explains a possible –but not an actual– situation, then does it essentially have any epistemic value at all (and if it does, how can we assess it properly)? The dichotomy that I seek to point out constitutes the core of the problem concerning HAEs and HPEs. Perhaps, bridging that dichotomy would be the key to resolving the current controversies regarding these two types of explanation. It may be proper to say that many –philosophical– distinctions are drawn for solution–oriented purposes. On the other hand, however, they are also troublesome. The distinction between HAEs and HPEs, for instance, is of this type. Perhaps the time has come for mandatory modifications with respect to the aim of science, *i.e.*, providing explanation. Some philosophers of science state that providing (the ideal) explanation is impossible due to idealizations in models. On this view, we should either weaken the concept of scientific explanation by embracing HPEs, or should identify understanding (but not explanation) as the aim of science (see also §4.3.2.2).

Throughout this chapter, I have briefly discussed the problem of explanatory status of idealized models within the framework of the standard view and its alternatives. I tried to achieve two purposes in doing so. The first is to demonstrate the intertwining relation of the issues like idealization, representation explanation and understanding. The second is to set a background for the issues of abstraction, idealization, and de–idealization, which I will discuss in more detail in Chapters 7 and 8. Although this chapter does not offer an answer to the question of how idealized models explain, it at least shows that the issue is in a dichotomized state in various aspects. In literature, models either are evaluated as structures which represent and thus explain phenomena; or are evaluated as functions which explain phenomena. Moreover, another tension appears between the views when the idea of understanding involved. On the face of it, one view asserts that some models provide explanation because of their idealizations; the other view states that models do not provide explanation but provide understanding because of their idealizations. I also discuss one implication of this tension in Chapter 9.

CHAPTER FIVE

MODELING IN THE SCIENCES: A CASE STUDY FROM GEOLOGY

5.1 Introduction

To make any epistemological or ontological claim about models, we need to understand how they are used in science. This chapter is based on this fundamental idea. Throughout, I discuss the most common type of model used in science, often categorized as a mathematical model. By using a sort of hybrid example which can be considered as both astronomical and geological, I aim to show how a mathematical model works in solving a scientific puzzle. Thus, the goal of this chapter is to consider broadly the process of modeling and its role in scientific practice. Moreover, I aim to provide a basis for other philosophical issues with respect to idealization and abstraction which will be dealt with more detail in the following chapters.

In what follows, I first explicate the problem to provide a basis for further progress toward the issue of modeling. This introduction will consist of a brief historical description of how the problem is framed. Secondly, I will review former approaches to the problem and will show how they failed to meet their objectives. Lastly, I will show how the Milankovitch model managed to capture all sufficient elements and succeeded in applying them to the problem. This section focuses on Milutin Milankovitch's genuine approach, including his accomplishment of tackling the problem mathematically.

5.2 The Problem

There seems no strict quantitative definition of ice ages (see Davis 2001; Barry and Yew Gan 2011, p. 299). However, we may easily say that ice ages are periods when severe temperature reduction occurs across the Earth. During such periods of time, surface of the Earth is largely covered with ice sheets and mountain glaciers. So far, scientists provide evidence that the Earth, in its history, has experienced many glacial periods. However, only few of these have established dominance over the whole climate system by lasting more than millions of years. The chart below shows these extensive ice ages in the past 2.4 billion years (see Figure 5.1).

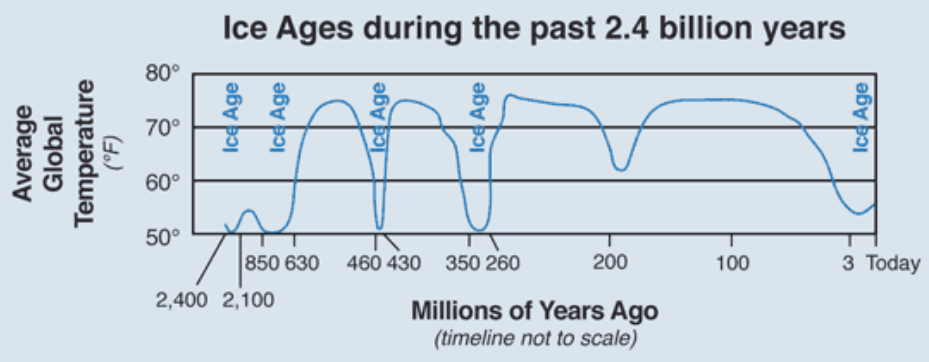

Figure 5.1 The chart shows five major ice ages during the past 2.4 billion years. Reprinted from Utah Geological Survey Notes, Eldredge S. and Biek B. (2010) [Adapted from Saltzman (2002)], Retrieved from https://geology.utah.gov/wp-content/uploads/ice_ages1. © Copyright – Utah Geological Survey–State of Utah. May, 15, 2017.

As seen from the chart, scientists have identified five significant ice age periods during the past 2.4 billion years. They are respectively called Huronian (2.4–2.1 billion years ago), Cryogenian (850–630 million years ago), Andean–Saharan (460–430 million years ago), Karoo (360–260 million years ago) and Quaternary (2.6 million years ago–present). Accordingly, we are living in the Quaternary period which began approximately 2.6 million years ago. Since we live in the Quaternary period, we are currently in the last major ice age period. So, the Earth should be in a notably colder climate compared to our current condition. At present, however, this is not the case. Neither climate is extremely cold, nor are our continents largely covered by ice sheets. On what grounds, then, scientists claim that we are in a major ice age period?

Geologists already have the answer to this question. It seems now the received view that the Earth currently is in an interglacial period that approximately started about 15 thousand years ago (Lisiecki and Raymo 2005). According to this view, our planet, sometime during an extensive ice age period, makes alterations toward warmer climatic conditions. After witnessing warmer climate conditions, it again goes into a deep freeze and this specific event recurs at intervals. Thus, in short, our climate is subject to certain periodical changes. With respect to time, these alterations are small steps for the Earth, but giant steps for humankind.

At first sight, the intuitive way to evaluate the alterations between glacial and interglacial periods is to assume a probable mechanism behind it. The scientists did

so as well. Whether periodically or not, the Earth has witnessed, and probably will continue to witness numerous glacial and interglacial periods. Thus, for scientists, the new task is to find out which factors are responsible for this. The common questions which they had dealt with are as follows: Why the Earth's climatic conditions change within major ice ages? What factors are involved in the separation of glacial and interglacial periods? Why series of glacial and interglacial intervals are either longer or shorter than their previous ones? The remainder of this chapter will be concerned with these questions and the answers given to them, but before proceeding let me say few words about the occurrence of major ice ages.

5.3 Major Ice Ages

There are many causes of major glaciation, including oceanic fluctuations, volcanic eruptions or surface albedo (see also Rohling *et al.* 2012). Among all these, however, the main cause responsible to initiate an ice age period is plate tectonics. Alfred Wegener (1880–1930), the German geologist and meteorologist was the first to come up with this hypothesis. He spent his time primarily in Greenland and his field research had mainly focused on continental drifts that later led Harry Hess (1906–1969) to develop the revolutionary theory of plate tectonics. More specifically, Wegener's theory is known as 'Continental Drift' a precursor to plate tectonics. The theory of plate tectonics was not peculiarly aimed to find out the mechanism of glacial periods, but it brought together a useful explanation for how these periods occur.

Given this theory, the separate continents today were once combined together. For instance, the well–known supercontinent Pangaea was one of them. It existed approximately between 350–250 million years ago and different from today's positioning, its continental mass was mostly located in southern hemisphere of the Earth. The east part of South America and the west part of Africa were bind together where India was beneath them. After a considerable time, each of these landmasses broke apart and drifted away until reaching their current positions (see Figure 5.2).

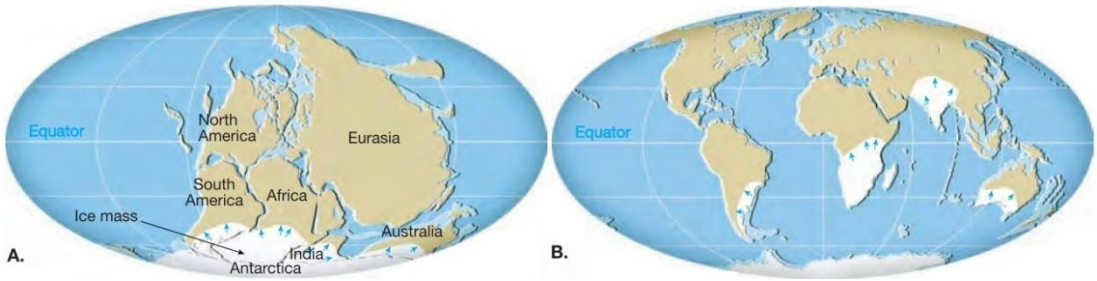

Figure 5.2 ***A.*** The supercontinent Pangaea existed approximately between 350–250 million years ago. Ice sheets are located close to the south pole of the Earth. ***B.*** Present position of the continents. Old ice sheet evidences are located on different land masses. Reprinted from Essentials of Geology (p. 282) by Lutgens, F.K., Tarbuck, E. J. and Tasa, D., 2012, U.S.A.: Prentice Hall. Copyright ©2012, 2009, 2006 by Pearson Education, Inc.

This theory has much to say about geological phenomena. With its principles, for example, we can deduce hypotheses about other natural events, such as volcanic eruptions, earthquakes or processes of mountain formation. Eventually, all these phenomena are results of the movements of plates which are segments of the continents and the oceans.

Similarly, the beginnings and the ends of major glacial periods are also related with the movements of continents. Many relevant textbooks give a clue about this relationship by using the term ‘continental glacier’ because glaciers (whether they are ice sheets or ice caps) can only form on ground. This information is crucial for the formation of glaciers, but it is also crucial for the explanation of the initial conditions of glacial periods. In order to understand this, it is sufficient to consider that the positions of continents have decisive influence over the global climate. To put it precisely, it happens as follows: When the continents are positioned mostly near the poles, low amount of sunlight falls on a big portion of total continental area. This means that snow and ice accumulate over large areas. Hence, the albedo increases and significant part of sunlight returns back to space. As a result, global temperature decreases and ineluctably produces a glacial period. That’s why “many scientists suggest that ice ages have occurred only when Earth’s shifting crustal plates have carried the continents from tropical latitudes to more poleward positions” (Lutgens *et al*., 2012, p. 281).

This natural process can be exemplified by reviewing the structure of the supercontinent Pangaea. As I mentioned above, large portion of Pangaea continent was located close to the south pole of the Earth. This means that suitable conditions were provided for a glacial period to start. Today, based on paleoclimatological evidence, we know that the Earth has experienced an extensive ice age about 350–250 million years ago, also the era when Pangaea existed. This can also be seen in Figure 5.1, where one of the lowest points of the curve denotes this period.

At this point, it is especially important to note that the theory of plate tectonics has no room or explanation for the questions raised at the end of the previous section. It is because this theory accounts for the long–term changes of global climate and falls short of establishing short–term temperature variations in major ice ages. According to the theory of plate tectonics, plates move very slowly (1–1.5 centimeters per year on average), and due to this fact, continents which are parts of plates move slowly too. This basic mechanism of the drift is extremely gradual such that continents shift their positions over millions of years. So, any model deduced from the theory of plate tectonics appears to be capable of explaining long term climatic changes. However, it would unavoidably fail to provide an explanation of what factors may be responsible for our climate to warm up and regress back to a cold climate within major ice age periods. “Therefore, we must look to some other triggering mechanism that may cause climate change on a scale of thousands rather than millions of years” (Lutgens *et al.*, 2012, p.282).

5.4 An Astronomical Solution for a Geological Puzzle

As described above, climatic changes within the ice age periods are not caused by movements or positioning of continents. For this reason, we need to seek the causes of these temperature changes, elsewhere, in the Earth system. Milutin Milankovitch (1879–1958), the Serbian mathematician, astronomer and engineer, thought so and came up with an idea to connect climatic changes with Earth’s orbital variations. According to him, temperature changes during the major ice age periods depend on the different amounts of solar irradiance falling on the surface of the Earth. By all means, the amount of solar irradiance is driven not by the Earth’s internal system but

by an external force, namely the Sun. Therefore, a possible solution lies not only in the field of Geology, but also in the field of Astronomy.

The idea to understand the dynamics of ice ages from the astronomical perspective is not new, and hence it does not belong entirely to Milankovitch. There are other details which make Milankovitch an original figure in science. These details and his other contributions will be spelled out in more detail after. But first, I will outline briefly the pioneers of the astronomical/geological theory who influenced Milankovitch's approach to the ice ages one way or another.

5.5 Pioneers of the Ice Age Theory

We know quite a lot about the glaciers and glaciation, such as how ice sheets move, how a glacier shrink and grow, or how glacial deposits are formed *etc*. The situation, however, was different over the past two centuries. In those days, few scientists were involved with the glaciation research. Moreover, many were not aware how crucial this natural phenomenon is in shaping the solid surface of the Earth.

The first sign of the ice ages was erratic boulders. These erratic boulders (large masses of rock) are found in places far from their bedrock source. Early to mid–18th century, the received view was that these giant rocks are somewhere distant from their original places because they are transported and deposited by a great flood –as almost told in the Bible. Although he changed his mind later on, Charles Lyell (1797–1875), the British geologist, confidently defended such a view and accepted it as the most likely explanation for the emplacement and movement of boulders (Imbrie and Imbrie 1979, pp. 21–22). While this view dominated the geological scene for over decades, it subsequently received criticisms from some researchers in the scientific community. The main objection urged against the flood theory was that such giant rocks are less likely to be carried from low to high elevations via floodwater. So, there must be another mechanism at work to transport massive boulders from one place to another. In addition, there was one other thing that made it hard for flood theorists to account for: scratched and grooved bedrock surfaces. These traces on the land surface were an indication of a serious problem because flood theorists were assuming a simple process of transportation of boulders from one place to another. However, in order to erode the underlying land surface such a

way, boulders must strongly scrape against the ground that they move over. Therefore, floodwater alone was not a possible candidate to be the cause of scratched and grooved surfaces.

The time when the level of the erratic boulder discussion manifested itself in a puzzling way, Louis Agassiz (1807–1873), the Swiss biologist and geologist, was working on fossil fishes. However, along with fossil research, his mind was busy with another issue: the possible causal link between erratic boulders and glaciation. According to Agassiz, the boulders which we find far from their bedrocks are in their current different places not because they are carried by the flowing water but because they are removed by an agent with much stronger force. His hypothesis was that the displacement of a boulder from its bedrock could most probably be produced by the effect of glaciers.

In 1837, Agassiz gave a talk about his glacial theory to the Swiss Society of Natural Sciences at Neuchatel. The expectations have failed, because members of the society were prepared to hear a talk about his new research results on fossil fishes. Instead, Agassiz took the opportunity and announced his new argument on the phenomenon on glaciation. Truly, from that moment onwards the dispute had begun. Within the time, Agassiz was exposed not only to the criticisms of opponent camps, but also showed resistance to the career–oriented advices from the authorities of geology, including William Buckland (1784–1856) and Alexander von Humboldt (1769–1859). For example, von Humboldt once wrote in a letter:

> I am afraid you work too much, and (shall I tell you frankly?) that you spread your intellect over too many subjects at once. I think that you should concentrate your moral and also your pecuniary strength upon this beautiful work on fossil fishes... In accepting considerable sums from England, you have, so to speak, contracted obligations to be met only by completing a work which will be at once a monument to your own glory and a landmark in the history of science... No more ice, not much of echinoderms, plenty of fish... (*Quoted* in Boylan 1998, pp. 146).

Despite such tough conditions, only about 30 years after, the theory suggesting the link between erratic boulders and glaciation was widely accepted in the scientific community, and this constituted a milestone in the advancement of glaciation research.

After being established as a physical process that occurred once or many times in the past, glaciation became a subject of geological investigations. Now the leading issue was no longer about the existence of past glacial periods. Instead, it was about the initial conditions of glaciations.

The French astronomer and mathematician Joseph Adhémar (1797–1862), was the first prominent scientist who drew attention to the onsets of glaciation process. The notable aspect of his contribution was his discovery of the relationship between global climate changes and Earth's orbital variation. According to Adhémar, the precession of the Earth's axis produces temporarily unequal seasons for each hemisphere. For this reason, one hemisphere enjoys longer summers, while the other experiences longer winters. The hemisphere which is in the latter case approaches to the process of glaciation. Therefore, he concluded that "whichever hemisphere had the longer winter would suffer an ice age" (Oldroyd, 1996, p.151).

Another remarkable attempt to understand the onsets of glacial periods was made by the Scottish scientist and geologist James Croll (1821–1890). Croll highlighted the importance of seasonal insolation and its role in glaciation. According to him, the amount of sunlight that the Earth receives during the winters and summers is related to the Earth's orbital parameters, namely the eccentricity and precession. These parameters play a crucial role in varying the amount of insolation. Due to these variations, the Earth's climate either warms up or cools down. According to Croll "a decrease in the amount of sunlight received during the winter favors the accumulation of snow, and that any small initial increase in the size of the area covered by snow would be amplified by the snowfields themselves" (Berger 2012, p. 113). Therefore, the Earth would gradually suffer an ice age.

Agassiz, Adhémar and Croll are three significant figures that influenced the course of the glaciation research. However, they are not the only ones who deserve all the respect. There are many other scientists who contributed substantially to the field of ice age studies, by mainly observing nature over the years and making hypothesis about the past, present and future of the environmental conditions. Particularly, Imbrie and Imbrie (1979), Bard (2004), Berger (2012) and Paillard (2015), in their seminal works, traced the history of great glaciologists, their naturalist approaches

and the breakthroughs in glaciation research, in detail. In the following section, however, I focus on Milankovitch whose contribution had a significant impact on the science of glaciation among all those researchers.

5.6 The Astronomical Model of Ice Ages

Like many scientists, Milankovitch stood on the shoulders of giants as well. The contribution he made to science, particularly to climatology was only possible with the past contributions of great scientists. In truth, Milankovitch often mentions the names of his predecessors in his papers, not only to proceed with their past research work, but also to give credit to their scientific legacy.

Nevertheless, as much as the past efforts should be recognized, we should also emphasize Milankovitch's genuine approach to the problems. His approach is different and genuine from most of his predecessors because it involves some sort of unifying framework. Until the era of Milankovitch, the mainstream methodology on particular issues in geology was descriptive. However, for Milankovitch exact methodology (or in other words, mathematical approach) should be integrated into this descriptive stance. For him, only if this condition is met, we could hope to discover the geological traits of the Earth and other planets as well.

The time when Milankovitch encountered the problem concerning the glacial and interglacial periods, he was already equipped with this line of thought. According to him, those who were dealing with the issue had the necessary information to proceed, but they were not able to achieve it. Of course, there have been reasons for that. Given Milankovitch's interpretation, some of the scientists had little idea what to do with empirical evidence and how to relate them with relevant theory; while the others were unable to use their theoretical knowledge to construct the model of the investigated phenomenon (see Imbrie and Imbrie 1979, pp. 97–99; Petrovic 2012). In all this, Milankovitch was defending a unified view that suggests a combination of both approaches. Although, scientists like Croll made similar attempts to unite these approaches, they inevitably failed due to lack of empirical evidence or their insufficient mathematical training (see Milankovitch 1941, p. 376).

As stated above, the way Milankovitch approached the problem was quite original. He argued that crucial climatic changes, such as series of glacial and interglacial

intervals, are causally connected with the distribution of solar radiation that reaches the Earth's surface. Thus, any change in the amount of insolation triggers a change in the global climate. So, in order to understand much about the periods of glacial and interglacial climates, we must determine the factors that vary the amounts of insolation.

According to Milankovitch, the distribution of incoming solar radiation happens in accordance with the Earth's orbital variations. Though these orbital variations "cause little or no variation in the total solar energy reaching the ground", they do play a role in seasonal changes (Lutgens *et al.*, 2012, p. 282). For example, they change the seasonal duration (long or short winters/summers) or the degree of difference between the seasons (hotter, milder or cooler winters/summers). Moreover, these orbital variations comprise three astronomical cycles: orbital eccentricity, axial tilt and precession. Most commonly they are called 'Milankovitch Cycles' in deference to the Serbian scientist.

5.7 Milankovitch Cycles

The first component of orbital variations is called orbital eccentricity. It can simply be described as the Earth's orbital path around the Sun. The gravitational tug of other large planets influences the Earth's orbit. This leads to a change in orbital shape and hence produces a cycle lasting about 90000–100000 years. Actually, our planet appears to orbit around the Sun elliptically. However, the shape of this orbit is not fixed. Sometimes it becomes more elliptical and sometimes it becomes more circular. The orbit becomes more elliptical when the difference between the furthest and closest distance of the Earth from the Sun increases. Alternatively, when the difference between the furthest and closest distance of the Earth from the Sun reduces, the orbit becomes more circular. The place where the Earth is nearest to the Sun is called perihelion, which occurs around January 3. On or around July 4, however, the Earth is at its greatest distance from the Sun, which is called aphelion (see Figure 5.3).

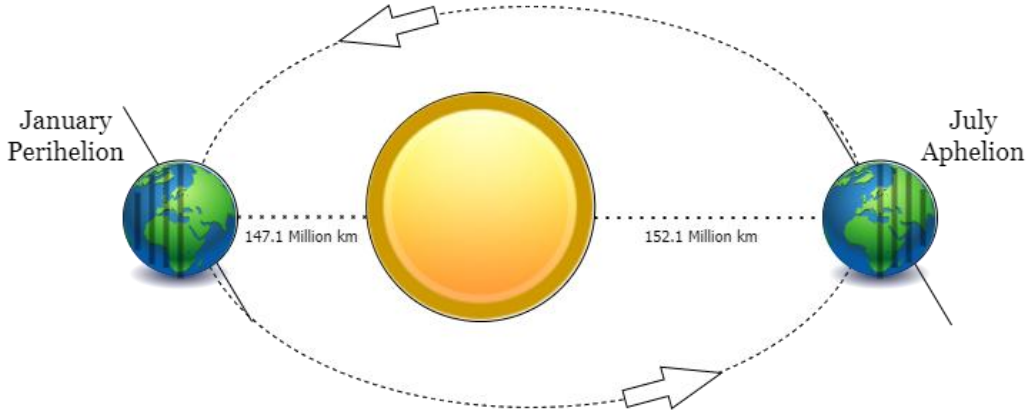

Figure 5.3 The distances from the Sun at perihelion and aphelion for the Earth.

Orbital eccentricity takes a value obtained by a simple formula using the variables aphelion (a) and perihelion (p) as follows:

$$e = (a - p)/(a + p) \quad (1)$$

If the obtained value for orbital eccentricity is equal to zero ($e = 0$), the shape of the orbit is a perfect circle. If it is greater than zero ($e > 0$), then the shape of the orbit is an ellipse. When the current estimated values are inserted into the above equation, we get

$$e = (152.1x10^6 - 147.1.1x10^6)/(299.2x10^6)$$

$$e = 0.0167112299$$

Orbital eccentricity has an influence on climate change. However, it is the least effective factor on glaciation, among other variations. The reason is that the eccentricity variations have small impact on total annual insolation, namely a difference of 0.03% (see Maslin and Ridgwell 2005, p. 21). Nevertheless, orbital eccentricity of the Earth may lead significant temperature contrast between seasons. For example, if the orbital path of the Earth around the Sun was circular, there would be no annual insolation difference between summer and winter for each hemisphere. But, due to gravitational effects of other planets, especially Jupiter's, this cannot happen. Today, our planet's path is elliptical in shape, with a value of 0.016. Therefore, the difference of seasonal insolation between summer and winter reaches to about 6%. So, this shows that the amount of solar radiation reaching to the Earth's surface is greater at perihelion. If the Earth's orbital eccentricity is at its maximum

value (0.07), namely the most elliptical shape, difference of seasonal insolation between summer and winter would reach to about 30%. As a result, seasons would be at their extremes in terms of temperature (*e.g.* very hot summers) for one hemisphere and would be moderate (*e.g.* milder summers) for the other hemisphere.

The second component of orbital variations is called axial tilt or obliquity. Our planet's rotational axis is tilted relative to its orbital plane. The angle of the tilt is determined by drawing a line perpendicular to the Earth's orbital plane. The angle of the tilt changes between 22° to 24.5° on a 41000 year cycle (see Figure 5.4).

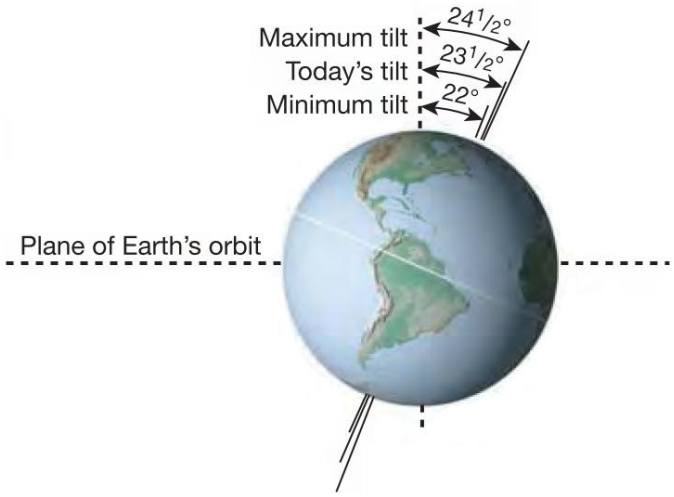

Figure 5.4 The figure shows the maximum, minimum and present tilt angles for the Earth. Reprinted from Essentials of Geology (p. 283) by Lutgens, F.K., Tarbuck, E. J. and Tasa, D., 2012, U.S.A.: Prentice Hall. Copyright ©2012, 2009, 2006 by Pearson Education, Inc.

Today, the obliquity of earth is measured as 23.5°. This tilt can be measured easily at solstices and equinoxes. In order to do that, it is sufficient to take the inverse tangent of the value which is found by dividing an object's height by its shadow length, at that particular time.

When the angle of tilt is about to increase its maximum value 24.5°, the temperature contrast grow sharper between the two seasons. In such a case, winters become colder and summers become warmer. Contrarily, when the angle of tilt decreases, the characteristics of seasons come closer. In this case, milder winters follow cooler summers.

The third and last component of orbital variations is called precession of the equinoxes. While orbiting the Sun, our planet also wobbles on its axis, like a spinning top. This wobbling of the Earth on its axis periodically repeats itself every 26.000 years. Today, the polar axis of the Earth points to Polaris, also known as the North Star. In fact, this won't last forever. Due to wobbling motion, the Earth's axis will gradually change and it will point to Vega. After 13000 years, Polaris will once again be the North Star (see Figure 5.5).

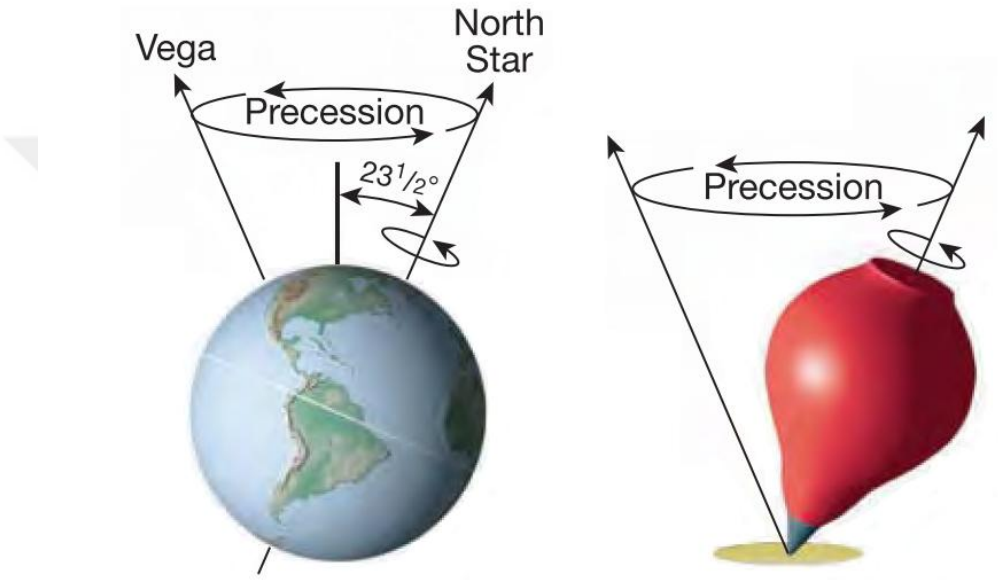

Figure 5.5 The figure shows the wobbling motion of the Earth. Reprinted from Essentials of Geology (p. 283) by Lutgens, F.K., Tarbuck, E. J. and Tasa, D., 2012, U.S.A.: Prentice Hall. Copyright © 2012, 2009, 2006 by Pearson Education, Inc.

The main effect produced by precession is an alternation of the seasons for each hemisphere. For example, today, the northern hemisphere is in winter, the southern hemisphere is in summer at perihelion. An opposite situation will be observed by the middle of the cyclic period. To put it differently, when the Earth's rotational axis points to Vega, the northern hemisphere would be in summer and the southern hemisphere would be in winter. In such a situation, the summers become warmer and the winters become colder. In other words, seasonal contrast in temperatures appears stark.

While the information of orbital cyclicity was not new at all for many astronomers, there was no available calculation of past orbital variations until Urbain Le Verrier (1811–1877), the French astronomer and mathematician. By applying Newtonian laws to the masses of planets, he calculated the past changes of orbital motions of the Earth. In this way, he created a sort of data table that displays the past variations of Earth's orbit in the last 100000 years. After decades, U.S. American astronomer John Nelson Stockwell (1832–1920) calculated the changes in obliquity and eccentricity of eight planets in the solar system. Furthermore, in 1904, German mathematician Ludwig Pilgrim (1844–1927) extended the calculations further and provided data on orbital changes of the Earth for the last million years.

Having these data is a big step forward for Milankovitch because they will be used in revealing the relationship, if there is any, between insolation amount and global climate change of the Earth. However, Milankovitch was extremely careful and meticulous in his endeavors. For this reason, despite having all the necessary information provided by Le Verrier, Stockwell and Pilgrim, he once more calculated the past variations of the Earth by inserting "more accurate values for the masses of the planets in the solar system" (Grubic 2006, p. 199). After settling these preliminary points, the remaining task was essentially to combine the orbital variations of our planet with a mathematical model.

Milankovitch was well aware of the past astronomical theories and their shortcomings in explaining the phenomenon of glaciation. For example, Adhémar correctly emphasized 26.000 years cycle of precession as one of the most responsible factor for glaciation but considered the orbital eccentricity as a constant. Hence, this incorrect thought, no doubt, led him to conclude that ice ages could occur only in one hemisphere. In other words, his theory suggested that one hemisphere would experience ice age condition while the other would be ice–free during a period of 13.000 years. Croll did not make the same mistake and paid much attention to the orbital eccentricity of Earth. Furthermore, he was the first who noticed the idea of albedo, or the reflectiveness of the Earth's surface. Although the effect of albedo is decisive to some extent, unfortunately that led him to think that colder winters would influence the expansion of glaciers. Thus, apart from neglecting the effect of obliquity, he insisted that ice ages are driven by very cold winters at aphelion.

According to Milankovitch, both earlier theories were incomplete because they do not contain all necessary parameters. In other words, either one or two orbital parameters are used in these early theories to give an explanation for glaciation. However, all parameters, one way or another, have an effect on the glaciation, and should all–together be taken into account. Besides, there was one other consideration which is in one sense related to the first. According to Milankovitch, both early approaches were mathematically incapable of linking the effects of orbital variations on insolation. He states in his book, *Kanon der Erdbestrahlung und seine Anwendung auf das Eiszeitenproblem*, as follows:

> All these theories have […] the same shortcoming: None of them have correctly grasped the variability of all the astronomical elements which affect the irradiation of the Earth. […] Besides, none of these theories was able to tackle mathematically the decisive influence of variations in the obliquity on the irradiation of the Earth (Milankovitch 1941, p. 376).

And then continues as follows:

> I was able to show that the astronomical problem of ice ages is far more complicated than had been assumed before, and that in order to arrive at a correct solution the whole problem had to be approached fundamentally and put on a broad basis (Milankovitch 1941, p. 376).

Milankovitch's fundamental approach was to base his work on a mathematical model. Before constructing the model, he set some preliminary ground for two concepts (see Grubic 2006, p. 199). First, he defined the unit of radiation. He took "the solar constant as unit of radiation and a hundred thousandth part of the year as the time unit" (Milankovitch 1941, p. 266). Compared with the langley unit (1 langley unit = 1 gram calory/cm^2), this unit is equivalent to .0576 langleys/day (see Lamb 2011, p.315). Second, he introduced the concept of caloric half–year. According to Milankovitch, the astronomical calendar was inappropriate to follow the secular march of insolation precisely because its length varies due to the variations of astronomical elements. To avoid this problem, he divided a year into two halves as caloric summer half–year and caloric winter half–year. Like in the astronomical calendar, each year's duration is 182 days, 14 hours and 54 minutes, but alternatively a caloric winter includes every day which is colder than the days in the summer half year (see Milankovitch 1941, pp. 271–275).

After having settled all these matters to his satisfaction, Milankovitch presented his mathematical model for insolation. The essential elements for the model are as follows: summer and winter caloric half–years for a certain year (ΔQs and ΔQw), changes of irradiation at a certain latitude for summer and winter (ΔWs and ΔWw), the change of the inclination of the ecliptic ($\Delta\varepsilon$), eccentricity of the ecliptic in the given year (e), longitude of the perihelion relative to equinox ($\prod\gamma$) and the coefficient (m) for individual latitudes.

So, the model comprises all the necessary components including three orbital parameters, the changes of irradiation amount for specific latitudes and the coefficient that expresses the insolation amount in caloric units. With all these, the mathematical model which allows us to calculate the secular march of insolation for the northern hemisphere is as follows:

$$\Delta Qs = \Delta Ws\,\Delta\varepsilon - m\Delta(e\,sin\prod\gamma) \quad (2)$$

$$\Delta Qw = \Delta Ww\Delta\varepsilon + m\Delta(e\,sin\prod\gamma) \quad (3)$$

For the southern hemisphere, it takes the following form:

$$\Delta Q^{-}s = \Delta Ws\,\Delta\varepsilon + m\Delta(e\,sin\prod\gamma) \quad (4)$$

$$\Delta Q^{-}w = \Delta Ww\Delta\varepsilon - m\Delta(e\,sin\prod\gamma) \quad (5)$$

The numerical values of the elements in the model can be taken from all the relevant tables in the *Kanon*. Based on the Le Verrier, Stockwell and Pilgrim calculations, Table IX displays the changes in orbital eccentricity, axial tilt and precession; Table XII displays the changes of the irradiation amount for a particular latitude in summer half–year and winter half–year; and Table XIV contains numerical values of coefficients. According to Milankovitch "tables IX, XII, and XIV contain all data necessary to compute […] the secular march of irradiation received by individual latitudes for the past six hundred thousand years" (Milankovitch 1941, p. 286).

In order to be a significant global change in the climate, all the cyclic variations of the orbital motions must be somewhat in a superposition. It is because all three

cycles operates independently of each other. “Sometimes their influence on the amount of heat received by certain parts of the Earth nullifies each other. Sometimes the changes increase or decrease the quantity of the heat” (Grubic 2006, p. 199). When the quantity of the heat decreases, a glacial period begins. In the opposite situation, when the quantity of heat increases, the global temperature significantly rises up and an interglacial period begins consequently.

With the Milankovitch model of insolation, it became possible to calculate how much solar radiation is received for any latitude in the past, present and future. Nevertheless, the model as such was not sufficient to understand the ice ages. To understand, completely, how the mechanism of ice age works, Milankovitch needed to know which seasons are crucial in triggering the glaciation process. The early theories were centered on very cold winters and accepted such severe winters as a decisive factor in accelerating glaciers (see Imbrie and Imbrie 1979, p. 104). Milankovitch was skeptical about this hypothesis. However, being untrained in geological dynamics, he was far from abandoning this idea and putting an alternative approach. Yet, he was aware that he “had to solve” this “preliminary question of principal importance: which of the meteorological elements and which season was to be selected in the Ice–Period” (Milankovitch 1941, p. 414).

Eventually, the expected help came from the Russian–German geographer, and climatologist Wladimir Peter Köppen (1846 – 1940). He was aware of the works of Milankovitch and had read the monograph which is about the model of insolation. Soon after, he contacted Milankovitch. At the time, Köppen was researching on climate classification and climates of geological past with his son–in–law Wegener. Both researchers were experienced in the field, and they were familiar with almost all the relevant geological record as well as the seminal studies on ice ages. On a weekly basis, Köppen and Milankovitch exchanged dozens of letters. Finally, the discussions ended up, when Köppen convinced him that the decisive factor on glaciation was milder summers, not colder winters as usually thought. That did seem reasonable to Milankovitch.

> After an exhaustive discussion off all the possibilities, Köppen answered the question by indicating that it is the diminution of heat during the summer half–year which is the decisive

> factor in glaciation… I therefore used Köppen's advice and directed my attention to the periods of cold summers (Milankovitch 1941, p.414).

Truly, the suggestion to focus on milder summers was genuine because the key factor in the development of glaciation was about whether or not glaciers are preserved. At the high latitudes of the Northern Hemisphere, large amount of snow were accumulating and the glaciers were being formed even in completely different past climatic conditions. However, when the summers were hotter in any hemisphere, the glaciers formed in the winter were starting to melt slightly. Therefore, while milder or relatively colder summers were preserving glaciers and hence culminating glaciation; hotter summers were setting the stage for interglacial periods (see also Oerlemans 1991).

Milankovitch had already constructed a model that makes it possible to calculate the insolation amount for a particular given latitude of the Earth. However, caloric half–years and canonic units were still not included. This model was constructed on the basis of astronomical half–years:

$$Ws = Ws^0 + \Delta Ws.\Delta\varepsilon \quad (6)$$

$$Ww = Ww^0 + \Delta Ww.\Delta\varepsilon \quad (7)$$

Here $\Delta\varepsilon = \varepsilon - \varepsilon_0$. While ε_0 denotes the present, ε refers to a particular past value of obliquity. The values of all other elements in the model (Ws^0 and $Ww^0, \Delta Ws$ and ΔWw, $\Delta\varepsilon$) can be found in the Tables XI, XII and IX, in the *Kanon*, respectively. With this model, the insolation amount of any given latitude (φ) can be calculated for both summer and winter half–years. In this early version of the model, the strategy was to determine the difference between present and past values of insolation. If any difference appears between the past and present values of astronomical elements, it could be represented with a graph (see Figure 5.6).

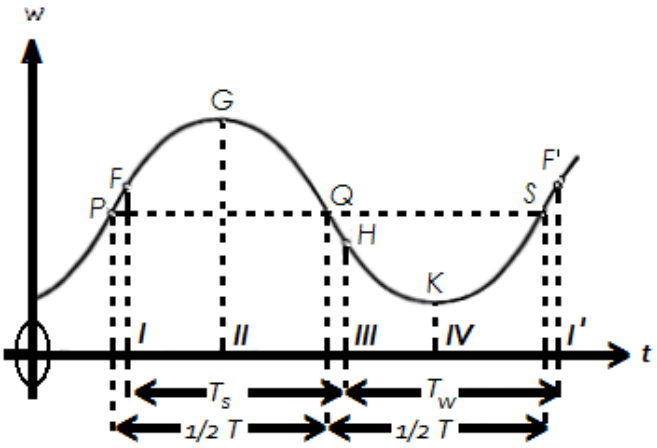

Figure 5.6 The annual march of the irradiation for any arbitrary latitude. Reprinted from *Kanon der Erdbestrahlung und seine Anwendung auf das Eiszeitenproblem* (p.272) by Milankovitch, M., 1941, Belgrade: Royal Serbian Academy Special Publications.

In the graph, we have the representation of the mean radiation w of a surface element at any arbitrary latitude (φ) as a function of time. The points on the curve PFGHKSF' represents the course of radiation at the latitude (φ). The points I,II, III, IV and I' on the timeline, respectively denotes the time of the vernal equinox, the summer solstice, the autumnal equinox, the winter solstice and the subsequent equinox. The caloric half–years, as stated above, have not been introduced here yet, hence the astronomical summer half–year (Ts) is represented by the segment I–III, and the astronomical winter half–year (Tw) is represented by the segment III–I' (see Milankovitch 1941, pp. 271–294).

The model was incomplete to some extent, but it was useful for calculating the insolation amount of any given latitude. For all that, Milankovitch took the opportunity to revise the model before his calculations. In this new version of the model, the strategy was nearly as the same as the first; comparing the values of the past astronomical parameters with the present ones. Additionally, this time, caloric half–years and canonic units were included in the model. As a result, the insolation amounts of any latitude were ready to be calculated with the following model:

$$Qs = Ws^0 + \Delta Ws \Delta\varepsilon \mp me \quad (8)$$

$$Qw = Ww^0 + \Delta Ww \Delta\varepsilon \pm me \quad (9)$$

Here, both the caloric half–years and canonic units are included in this revised version of the model. Additionally, the most important things that should be considered are upper and lower signs of both equations. According to Milankovitch, when the longitude of the perihelion is 90° the upper sign is valid and when it is 270° the below sign is valid. This is because extreme changes in the insolation amount of considered latitudes (which are thought by Köppen to be 55th, 60th and 65th parallels of the northern hemisphere) were happening when the degrees are at 90° and 270° with respect to the vernal equinox.

Using the latest version of his model, Milankovitch calculated past insolation amounts for the specific latitudes. He obtained the intended results over months and transformed these into fictitious latitudinal oscillations. The reason for that was to see a geometrical picture of the past summer insolation variations for the northern latitude of 65°. The created graph was an equivalent graph which showed the equivalent values of summer insolation amount for 65°N throughout the past 600.000 years (see Figure 5.7).

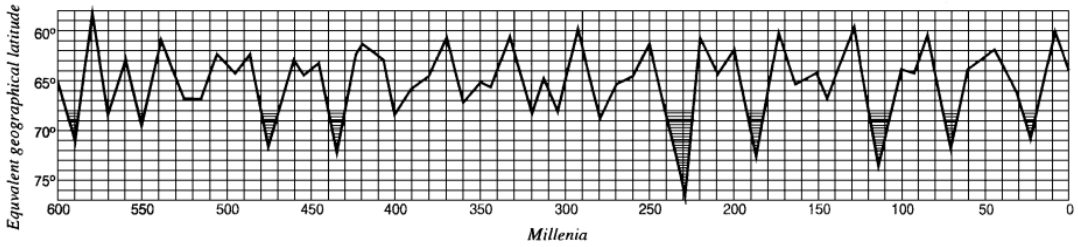

Figure 5.7 The equivalent graph which shows the variations of the summer insolation for 65° N. Reprinted from *Kanon der Erdbestrahlung und seine Anwendung auf das Eiszeitenproblem* (p.415) by Milankovitch, M., 1941, Belgrade: Royal Serbian Academy Special Publications.

As seen in the graph, the past equivalent insolation values of 65°N are shown by curves for comparison. For example, about 115.000 years ago, the summer insolation amount of 65°N was almost equal to today's summer insolation amount of 74°N. This could be evaluated as an indication of a relatively cold summer. In the opposite cases, the same latitude would of course have relatively warmer temperature averages in the summers.

Soon after completing his work, Milankovitch sent this radiation curve graph to Köppen. The graph must have been intriguing for Köppen, as it was in agreement

with the findings of two German geographers Albrecht Penck (1858–1945) and Eduard Brückner (1862–1927). Both scientists were already presented their results in a graph (see Figure 5.8).

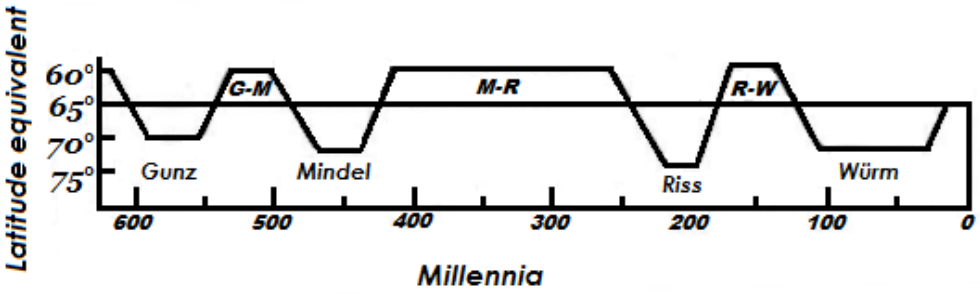

Figure 5.8 Penck and Brückner's scheme of glacial/interglacial periods in the Alpines. Reprinted from *Kanon der Erdbestrahlung und seine Anwendung auf das Eiszeitenproblem* (p.417) by Milankovitch, M., 1941, Belgrade: Royal Serbian Academy Special Publications.

Penck and Brückner were researching on Alpine glaciers and about 15 years before Milankovitch's work, they identified four great glacial periods in Earth's history by examining successive gravels, plant remains and moraines in the European Alps (see Anderson *et al.* 2013 p. 8). They displayed these different periods in a graph and named them chronologically as *Günz, Mindel, Riss*, and *Würm*. The close agreement of the two graphs was a sign of victory for Milankovitch. Moreover, with this new graph, Milankovitch provided a more precise graph compared to the old one. As seen in the Figure 5.7, the nine striped low points of the curve display colder summers which correspond to glacial periods occurred in the 589th, 548th, 475th, 434th, 231st, 187th, 116th, 72nd, and 22nd Millennia BP. Günz, Mindel and Riss, each includes two, and the last significant cold period Würm includes three low points. Clearly, Penck and Bruckner scheme does not have this preciseness.

On Milankovitch's part, the goal had been finally reached. His work "based upon exact science" passed into "the sphere of the descriptive natural sciences" and made the "link between celestial mechanics and geology" (Milankovitch 1941, p. xvi). In short, exact science and descriptive science met by means of geological research on the one side and astronomical computation on the other. Furthermore, by constructing a mathematical model based on the orbital variations of Earth, it became possible to trace past climatic variations and also to predict the future glacial or

interglacial periods. For example, the model predicts that, with or without human effects, interglacial period which we are still in may end about 50.000 years after (see Berger and Loutre 2002).

Although, in 1924, Köppen and Wegener confidently published radiation curves in their book *Die Klimate der geologischen Vorzeit*, some remained skeptical about the validity of Milankovitch's model until 1970's. In the following years, however, James D. Hays *et al.* (1976) published an article on the close link between climatic changes and the Earth's orbital variations. Based on the oxygen isotope records provided by deep ocean sediment cores, they came to the conclusion that orbital changes induced climatic change in the past 500.000 years. In their terms, orbital cycles of the Earth should be understood as pacemakers of the ice ages. Eventually, this could be counted as a clinching victory for Milankovitch's model. In other words, Milankovitch's work was not merely providing a basis for further studies on mathematical climatology; it was also reliable in estimating the relationship between orbital variations and climatic changes, in a robust manner.

5.8 Conclusion

In this chapter, I tried to show with an example from scientific practice how a mathematical model constructed and used in science. This chapter was both historical and descriptive. It was historical because my aim was to uncover past efforts in solving the problem of ice ages to set the stage for Milankovitch's remarkable breakthrough. It was descriptive because my aim was to show how the model is constructed and applied to the target phenomenon. In general, my approach was not yet directed at philosophical aspects of the issue. Nevertheless, the issues discussed so far present a transition towards these particular aspects. Therefore, this chapter can be summed up as an attempt to provide a basis for philosophical issues by directly investigating a mathematical model used in science.

To be sure, the example I have used is selected on purpose. If noticed carefully, one would see that the Milankovitch's model is interrelated with many different branches of science, including astronomy, geophysics and paleoclimatology, just to name a few. Moreover, the success of the model mostly rests on the successes of these other relevant science branches. From this point of view, such coherence increases the

robustness of Milankovitch's model. Thus, even though it is only one example among different types of models in science, it does provide a basis for philosophical issues, in its own classification.

As I tried to show in one section of this chapter, the success behind Milankovitch's model was based on a careful examination of the gaps in earlier models. In this way, the model was amended with the necessary modifications. Of course, underneath this scientific success, there lie many philosophical problems. In fact, I have tried to give hints about these matters throughout the sections –particularly, the issues like idealization, abstraction and representation. This confirms both ubiquitousness and importance of these problems. Doubtless to say, for a scientist, epistemological and ontological issues are one less thing to worry about. In other words, worries like to what extent an idealized model can provide a genuine knowledge of the empirical world or in what relation a model represents its target are questions mostly not included in modelers' agenda. In short, these are philosophical worries rather than scientific issues. In this manner, idealization and representation are both different in scope when it comes to evaluate them in the sciences and in the philosophy of science. This chapter focused on the former, while the remaining chapters of this dissertation will focus on the latter.

CHAPTER SIX

SCIENTIFIC MODELS AND REPRESENTATION

6.1 Introduction

In this chapter, I aim to discuss how a model represents a target system. To this end, I will refer to the case study, which is discussed in Chapter 5 and to the major accounts of representation (*i.e.* Isomorphism and Similarity) which are discussed in Chapter 3. More specifically, this chapter interprets the astronomical model of the ice ages within the framework of Isomorphism and Similarity. In **§6.4**, I try to show how both these major accounts fail to account for the astronomical model of the ice ages and for many other models in science as well. More specifically, the major accounts fail to capture how idealized and abstract models represent the corresponding natural phenomena. As a consequence, I resort to Partial Isomorphism as an alternative to these major accounts. Having provided the details of Partial Isomorphism, I attempt to justify the claim that this account captures far better the nature of idealized and abstract models than its alternatives. In this sense, I interpret the astronomical model of the ice ages once again within the framework of Partial Isomorphism. The last section of this chapter contains my rationale for why the path Partial Isomorphism takes is the proper and promising one with respect to the issue of representation.

6.2 The Isomorphism Conception of Scientific Representation Revisited

The details of the view in which representation is explained in terms of isomorphism is provided in **§3.5.1**. Given this view, the representational relation is a two–place or a dyadic relation. In other words, model (M) represents a target system (TS) *iff* M is isomorphic to TS.

As known, isomorphism holds between structures. For this reason, a structural commonality between M and TS is prerequisite. Nevertheless, M and TS sometimes have different structures. I have illustrated this case in Chapter 2 with different types of models. For example, computational models are mathematical structures with an algorithm that attempts to explain a physical phenomenon (for instance, non–mathematical individuals and their segregation). This shows that an assumption on the non–common structures must be made in order to provide an isomorphic representation.

The accepted view is that this assumption finds its basis in structuralization. In other words, "[t]o apply isomorphism to [target systems] or… objects, we need to appeal to the instantiation of structure" (Suárez 2010, p.96). In Schelling's model, for example, the individuals of the city are not mathematical entities as the computational model describes. Yet, individuals and the behavioral dynamics between them could still be thought as mathematical structures. Once the instantiation of structure is established, one can map the mathematical structure of computer simulation with the dynamics of individuals. That is why this mapping relation is often called *structural isomorphism*.

The approach of isomorphism includes a normative claim about scientific representation. If M represents TS, then there ought to exist an isomorphic relation. In this manner, if the astronomical model of the ice ages is purported to explain the ice ages, then there must be an isomorphic relationship between the model and the ice ages.[34]

Isomorphism, in a nutshell, could be given in the following way:[35] In terms of set–theoretic construction, target system is a non–empty set. In other words, TS is a domain (D) consisting of elements in its structure. These elements can be expressed as D_{TS}. As a structure TS, also, includes relations which describe the interactions between the elements of D_{TS}. The relations define different associations for each element which are symbolized with R. In this sense, there exist various relationships *[i]* between the elements of TS. Consequently, TS, as a structuralized system, can be expressed as $S_{TS} = \langle D_{TS}, R^{i}_{TS} \rangle$.

M or TS might not be mathematical, though their expression as a mathematical structure is vital to construct the relationship among them. There is no problem with regard to some types of models (like the mathematical, computational ones) because they are essentially mathematical structures. However, if the model is physical scale model constructed with the purpose of representing a physical target system, extra

[34] In this chapter, I treat the astronomical model as a representation of the dynamics of insolation. This plainly is the correct interpretation of the model. Nevertheless, the amount of insolation directly determines the ice age dynamics, so the astronomical model is actually a model that indirectly explains how ice age periods occur.

[35] The formal and more detailed versions of the isomorphic relationship between the model and the target system can be found in the literature (see Mundy 1986, Brading and Landry 2006).

work needs to be done. In this case, both the scale model and its target system should be structuralized. In other words, one needs to express the structure of the scale model as $S_M = \langle D_M, R^i_M \rangle$ for its elements and the relationships between them. If isomorphism holds between these two structures (*i.e.* between S_M and S_{TS}), then M represents TS.

At this point, the astronomical model of the ice ages can be explained with respect to the Isomorphism.[36] Given this view, the astronomical model is already a mathematical structure, thus it does not need to be structuralized or instantiated as a structure. On the other hand, this is not the case for the TS. The target system needs to be structuralized because the astronomical model aims to explain the amount of insolation that the Earth receives at certain time *t*. Therefore, explanandum is a state of a physical phenomenon and needs to be instantiated as structure. At first, the structural elements of D_{TS} need to be determined to explain the amount of insolation. According to Milankovitch's model, these elements are three orbital cycles: eccentricity, obliquity and precession. The relationship of these elements is decisive for the amount of received insolation, but their relationship with each other also determines the total amount of insolation. For this reason, the network of relations among those elements should be well specified. For example, a particular rate of decrease of the obliquity also decreases the insolation difference between the summers and winters of Northern Hemisphere. Clearly, this difference indicates that we are about to enter the ice age period. Nevertheless, this is only one factor about insolation. Other elements, like eccentricity and precession are likewise effective in governing the onset of ice ages. For instance, an increase in the value of eccentricity amplifies the insolation contrast between the seasons. This could, in turn, nullify (or most probably decrease) the overall insolation difference between the seasons. As a result, the amount of insolation required for the start of the ice age period might not occur. Therefore, the relationships of the elements contained in D_{TS} should be carefully stated. More specifically, both the relationship of each element to other elements and of the relationship of each element to the insolation should be taken

[36] Hereafter, I will abbreviate the isomorphism view of representation with 'Isomorphism' (with a capitalized I). The other use of the term isomorphism refers to the isomorphic relation.

into account; and all these relationships $\langle R^1, R^2, R^3, \ldots, R^n \rangle$ should be defined as belonging to R^i_M.

Two critical points need to be kept in mind with regard to Isomorphism. First, M represents TS *indirectly*. To put it clearer, M represents TS indirectly, but represents its structural instantiation (or extension) *directly*. Such an indirect representation of TS is inevitable because "isomorphism is well defined only as a mathematical relation between extensional structures" (Suárez 2003, p. 96). Second, isomorphism is a surjective function. More clearly, the structural extensions of M and TS hold if and only if each element of S_{TS} is completely mapped by the elements of S_M.

6.3 The Similarity Conception of Scientific Representation Revisited

Like isomorphism, similarity is also a dyadic relation between the M and TS (see **§3.5.2**). The similarity conception of scientific models[37] states that model (M) represents a target system (TS) *iff* M is similar to TS.[38]

One of the major advantages of the Similarity over Isomorphism is that it does not assume structuralization for representation. In other words, Similarity is neutral on the issue of whether representation is structural. In this sense, anything can represent anything as long as the similarity is specified among M and TS. As Roman Frigg (2006) states "the similarity view is not committed to a particular ontology of models. Unlike the isomorphism view, it enjoys complete freedom in choosing its models to be whatever it wants them to be" (p. 60).

Similarity is basically more liberal than the Isomorphism with respect to the model ontology. It does not invoke structural extensions to establish the representation relationship between M and TS. Nonetheless, this account of representation does not adopt a completely permissive relationship between the model and the target system because "representation and whatever is being represented can be similar in any

[37] Hereafter, I will abbreviate the similarity view of representation with 'Similarity' (with a capitalized S). The other use of the term similarity refers to the relation of similarity.

[38] As it seems, isomorphism is a form of similarity. It is because "isomorphic structures are *ipso facto* structurally similar" (Suàrez 2003, p. 228). From this point of view, isomorphism seems reducible to similarity. For this reason, the proponents of the similarity view argue that the similarity conception of scientific representation is more fundamental than the isomorphism conception of scientific representation, therefore it should be preferred over it. My aim here is not to argue for or against the claim that isomorphism can be reduced to similarity. Instead, I aim to interpret Milankovitch's mathematical model within the framework of similarity view.

number of properties" (Winther 2020, p. 124). For this reason, it needs to be determined in which respects and degrees the similarity holds between M and TS.

Like other scientific models, the astronomical model can be evaluated in terms of Similarity. The most general statement we can make at this point is that the mechanisms which determine the amount of insolation predicted by the astronomical model (M) are similar to the mechanisms which determine the amount of actual insolation received by the Earth (TS). The similarity, of course, comes with respects and degrees. The respects here are the relevant elements of TS, *i.e.* the three orbital cycles (eccentricity, obliquity and precession). The astronomical model aims to give a similar representation of the Earth by taking these three orbital motions into account. In other words, M is similar to these three aspects of TS.

It would be misleading to consider similarity view only in terms of *respects*. The reason is that each *respect* of M possesses a *degree* of similarity to the elements of its TS. For example, eccentricity is the least influential factor in producing annual insolation when compared to other orbital motions, like precession and obliquity (see **§5.7**). In other words, glacial periods are mainly driven by the latter two orbital cycles. True, the astronomical model is successful because it combines all three orbital factors of the insolation. For that reason it is a very close representation of its TS (*i.e.* it entails a high degree of approximation). Nevertheless, a more primitive version of the astronomical model (without taking eccentricity into account) could also represent the dynamics of insolation. In this case, this primitive model would not be *very close*, but *close enough* for the representation of insolation.

The similarity view, then, does not merely assert in what respects M is similar to TS. In addition to this, it asserts to what degree that particular respects are similar to the target system. In this way, alternative models of the same target system can be evaluated with regard to their representational successes. Two models might be similar in some respects to their same TS, but one might be a more approximate representation than the other. The reason for this is not because the less approximate

model is dissimilar to the TS in relevant respects; rather, it lacks specification of the *degrees* of relevant *respects*.[39]

The relation of similarity might be treated as relation of resemblance. However, the "notion of similarity begins from an everyday notion but rejects the idea that similarity is a strictly holistic relation of resemblance" (Weisberg 2012, p.794). The most central difference between similarity and resemblance is that the former relationship is established under the guidance of modelers' background knowledge. Thus, the concept of similarity in science is a much more systematic version of the concept of resemblance that has been widely used in everyday life. To put it differently, in scientific practice, the similarity relationship between M and TS is not established as loosely as in everyday life. Modeler carefully picks out relevant respects of the intended TS; she does not aim at a whole resemblance relation. This carefulness allows modelers to obtain information about TS by means of M (see also, Swoyer 1991). On the other hand, people often refer to resemblance relations in everyday life because they find them interesting; they rarely draw any information about the represented object or system.

6.4 Arguments against Isomorphism and Similarity

So far, I have tried to show how Milankovitch's astronomical model of the ice ages is interpreted in terms of the two major representation theories. Deliberately, I tried to remain neutral to both theories. Isomorphism states that the representational relation between the astronomical model (M) and insolation (TS) is based on the isomorphic relation between the structural extensions of M and TS. Similarity, on the other hand, rejects such an isomorphic relationship. Given this account, the representational relationship between any M and TS is based on similarity relation.

[39] An example would make my point clearer. Consider the scale model of a bridge that is actually planned to be made of steel. Suppose the purpose of constructing the scale model is to measure the durability of the real bridge. If we have two scale models, one of which is made of wooden matchsticks and the other of iron rods, it will be obvious that the latter model will be a better representation of the real bridge than the former one. For one reason, with regard to durability, an iron rod is more similar to steel material than a wooden matchstick. So, although both scale models represent the overall physical structure of the real bridge exactly the same (both models are similar in certain respects, for instance similar about the intersections of the columns and beams of the bridge), one used a more similar material than the other (iron rod model's *degree* of similarity is higher than wooden matchstick model with *respect* to its material).

When applied to the case study of ice ages presented in Chapter 5, both accounts, on their own, seem reasonable in capturing the nature of scientific representation. In this section, however, I claim that this is not strictly the case. To do so, I discuss briefly some problematic aspects of both accounts of scientific representation.[40]

The problems concerning Isomorphism and Similarity can be divided into two main parts. The first part is that both accounts of representation are in contradiction with the intuitive statements of representation. To recall, the three intuitive statements about scientific representation are as follows: (a) representation is *non–reflexive*, (b) representation is *non–symmetric*, and (c) representation is *non–transitive*.[41]

When Isomorphism is evaluated in terms of these three intuitive statements of representation, some serious inconsistencies become apparent. If M is isomorphic to particular TS, one can conclude that M represents TS. Nevertheless, M is also isomorphic to itself but it does not represent itself. Thus, this feature of isomorphism is at odds with statement (a). Moreover, if M is isomorphic to particular TS, one can conclude that that particular TS is also isomorphic to M. Nevertheless, TS does not represent M. Thus, this feature of isomorphism is at odds with statement (b). Finally, M can be isomorphic to TS, and TS can be isomorphic to any other system S. In this case, although M is isomorphic to S, it is not necessarily a representation of it. So, this feature of isomorphism is at odds with statement (c).

With respect to Isomorphism, Similarity does a better job of handling the three intuitive statements of representation by being compatible with statement (c). M might be similar to TS in certain respects and degrees (r, d), and TS might be similar to S in certain respects and degrees (r', d'). In this way, M is not similar to S, and thus does not represent it because (r, d) $\neq$ (r', d'). Therefore, the relation of similarity is non–transitive. However, this view fails to come to grips with statements of (a) and (b). If M is similar to particular TS, one can conclude that M represents TS. Nevertheless, M is perfectly similar to itself but it does not represent itself. Thus,

[40] Suàrez (2003) criticizes both of the accounts of isomorphism and similarity by using examples from different domains (e.g. artistic representations). In this section I only discuss representation in science.
[41] The intuitive statements of representation are firstly put forward by Nelson Goodman (1968). I prefer to call these statements as 'intuitive' (see §3.2). Nevertheless, these three properties of representation are also called 'logical properties of representation' (see Frigg and Nguyen 2020, p. 35).

similarity relation is not non–reflexive. If M is similar to TS, then TS is similar to M, but TS does not represent M. Thus, similarity relation is not non–symmetric.

To sum up, Isomorphism and Similarity each contradicts, one way or another, with the intuitive statements of representation. Similarity does not suffer from (c) so; it does a better job than Isomorphism. However, this view entails statements which are contradictory with statements (a) and (b).

Proponents of the Isomorphism and Similarity have revised their views, in due course, to tackle the first part of the problem. For example, as one of the developer of the Isomorphism, van Fraassen (2008) suggested that the *Hauptsatz* of representation (*i.e.* fundamental fact of representation) is the intention of the user.[42] Given this revised view "[*t*]*here is no representation except in the sense that some things are used, made, or taken, to represent things as thus and so*" (p. 23, emphasis in original). Likewise, Giere (2010), as a pioneer of the Similarity, argued that the user is a necessary component of representation. In his view, it would not be appropriate to treat representation as a two–place (dyadic) relation because if we shift "the focus to scientific practice" it will be realized that "we should begin with the activity of *representing*" (p. 743). Giere argues that the agents or more specifically the intentional scientists are the ones who do the representing. Therefore, the similarity between M and TS holds only if it is taken as more than two–place relation, *i.e.* the user is requisite for representation.

It is not hard to understand why both views (Isomorphism and Similarity) are equally committed to the significant role of the user. Both views aim at resolving first part of the problem that they confront in the same way. In other words, both views are purportedly revised to views that are compatible with the intuitive statements of representation. In this way, for example, Isomorphism accounts for non–reflexivity, *i.e.* the contradiction with (a) disappears. If the intentional agent (user) states that M is a representation of TS, but not a representation of itself, then the issue of reflexivity disappears altogether. Again, if the intentional agent states that M represents TS, but TS does not represent M, then the isomorphism view handle the issue of non–symmetry. In other words, the intention of the user breaks the symmetry

[42] For those who might wish to explore further how van Fraassen's understanding of scientific representation evolves may see (Ladyman *et al*. 2011).

between the isomorphic structures. So, Isomorphism becomes compatible with statement (b). Finally, the intentional agent might state that M is isomorphic to TS and TS is isomorphic to particular S. Nevertheless, if she specifies that M does not represent S (despite being isomorphic to it) then the required property of non–transitivity holds. In this sense, Isomorphism becomes compatible with statement (c). On the other hand, although Similarity is compatible with statement (c), the issues (a) and (b) still stand. Those who embrace Similarity also apply the same strategy. The intentional user could state that M merely represents its TS, despite being similar to itself. In like manner, the user could also state that TS might be similar to M, but it does not represent M. So, the specifications of the user settle the relation that M stands for TS. In this way, Similarity satisfies the logical requirements (or becomes compatible with the intuitive statements) of representation.

As it seems, integrating the user into representational relationship makes the two major theories compatible with the intuitive statements of representation. Once the user specifies the source (M) and the target (TS) of the representation, the isomorphism and similarity conceptions of representation become non–reflexive, non–symmetric, and non–transitive. Accordingly, the first part of the problem seems to be resolved for these two major accounts. However, these two views, in their agent–based version, still have to face the second part of the problem. The second part of the problem stems from the fact that most models are idealized and abstract.

It would be uncontroversial to claim that models are idealized and abstract by nature. The reason is that there are two truths about the representational status of models, *i.e.* analytical and synthetic. Models are *by definition* incomplete and inaccurate. So, this is an analytical truth about models. Thus, "if one has a complete and accurate description of the phenomenon, it is not clear … [t]hat one has a model at all" (Bokulich 2011, pp. 34–35). As Paul Teller (2001) similarly (but weirdly) stated "[t]he only PERFECT model of the world, perfect in every little detail, is, of course, the world itself" (p. 410). The other truth is synthetic and it is about us and the world. Our own cognitive capacity and the world around us prevent models from being complete representations of their target systems. In other words, we are "limited human beings and that the world we inhabit is incredibly complex" (Potochnik 2017, p. 2). Thus, from these perspectives, the truth about models seems to suggest that the

source in which a particular target is represented will always (and unavoidably) be inaccurate.

Inaccuracy comes in two flavors. A model might be deliberately constructed as inaccurate by the modeler. If this is the case, the model is idealized. In other words, some properties belonging to TS are misrepresented for a specific purpose in the model. On the other hand, a model might be constructed as inaccurate without any intentional omissions.[43] If this is the case, then the model is abstract. In this case, some properties of TS are not represented in the model.[44]

It turns out that representational relationships are problematic for isomorphism and similarity views, when it comes to misrepresented and non–represented properties. The reason for that is Isomorphism and Similarity both focus merely on *accurate* representation. Given the former conception of representation, a model represents its target system *iff* M is isomorphic to TS. In this context, for example, the isomorphic relationship (between the structural extensions of M and TS) constitutes the representation. In other words, each property needs to be accurately mapped between M and TS because this mapping relation is both necessary and sufficient condition for the representation.[45] Like Isomorphism, Similarity also grounded in necessary and sufficient conditions. Given this conception, a model represents its target system *iff* M is similar to TS.[46] The constituent of the representation is therefore the similarity relation between M and TS in which certain *respects* and *degrees* are specified. The represented *respects* might be similar to TS with a high or low *degree* of accuracy. If the former is the case, it can be stated that M is a good approximation of TS; if the latter holds, on the other hand, M would not be considered as a good approximation of TS.

[43] Sometimes, properties belonging to TS (which are not included in the model) might be deliberately not represented. However, this is a very rare case. In general, the properties we abstract are much more numerous than the ones we idealize. Moreover, properties that model misrepresents are included in the model in their misrepresented form, while abstract properties are not included in the model at all.

[44] A similar view was put forward by Thomson-Jones (2005). He pairs misrepresentation with idealization, and non-representation with abstraction. According to this view, "the term 'idealization' applies, first and foremost, to specific respects in which a given representation misrepresents, whereas the term 'abstraction' applies to mere omissions" (p. 174). Although, Thomson-Jones does not explicitly link abstraction to non-representation, it seems clear that the activity of omission corresponds precisely to the activity of *not-representing*.

[45] Of course, the sufficiency comes by the intention of the agent who aims at representing a target.

[46] The caveat about sufficiency in the previous footnote also applies here.

There is a critical point to be evaluated here: misrepresentation is a kind of representation. As Mauricio Suárez (2003) points out it, "is an ubiquitous phenomenon in ordinary–life representation" (p. 233). So, one should be careful not to confuse misrepresentation with non–representation. The act of misrepresentation differs from the act of representation only in that the former is constructed deliberately inaccurate, while the latter is not. In this sense, a theory of representation (like Isomorphism or Similarity) not only needs to account for representation, but also for misrepresentation. As we will see, both major approaches fail to offer an account of misrepresentation.

Isomorphism does not provide any room for misrepresentation. Given this conception, isomorphic relationship between M and TS does the real job in representing; non–isomorphic parts have no role in carrying out the representation. However, models are idealized and abstract by their nature. Therefore, they often deliberately misrepresent the target systems. Isomorphism has trouble explaining this aspect of the representation, or in general, the inaccuracy in models.

With regard to misrepresentation, Similarity shares the same problem. Given this view, similarity relationship between M and TS does the real job in representing. Thus, the other parts are non–functional in representation. Seemingly, the same problem arises with Similarity as with Isomorphism. For the reason that "a misrepresentation is one that portrays its target as having properties that are not similar in the relevant respects and to the relevant degrees to the true properties of the target" (Frigg and Nguyen 2020, p. 35). In this case, similarity view does not consider misrepresentation as a representation. Therefore, according to view of similarity, the inaccurate models in science become problematic.

It is apparent that misrepresentation creates problems for both Isomorphism and Similarity conceptions of representation. However, this is not the only one. Models are by nature not only idealized, but also abstract. Therefore, M does not represent many properties that TS actually possesses. As I emphasized above, this is *non*–representation, not *mis*representation. Yet Isomorphism and Similarity do not explain abstraction either. In other words, both accounts are insufficient to account for the issue of non–representation.

At first glance, one might doubt whether non–representation could have any relevance in regard to representation. After all the views about isomorphism and similarity are about representation (and misrepresentation as well), not about non–representation. Why, then, would this be a problem? Apparently, this is a topic that needs to be treated in more detail. So, I leave this issue for future research. Nevertheless, some views suggest that any representation theory should make room for abstraction (*i.e.* the non–representation of a property). The reason for this is because sometimes "intended abstractions have been discovered to be covert idealizations" (Potochnik 2017, p. 55).

6.5 Partial Isomorphism

The second part of the problem concerning inaccurate models was not properly treated by the two major views of representation. This has led to the emergence of new approaches to representation (see Hughes 1997, Suárez 2004, Contessa 2007, Frigg and Nguyen 2016). In this section, I discuss one of them, which to my mind is the most promising view, namely the partial isomorphism view.[47] As its name suggests, this view is a version of Isomorphism, albeit different from it in a number of important details.

The partial isomorphic relation between model and its target system can be expressed as follows: Model (M) represents the target system (TS) *iff* M is partially isomorphic to TS. As is seen, this view suggests a weaker criterion for the representation relationship between M and TS; it suggests partial isomorphism rather than a complete one. In this sense, the isomorphic relation constituting the representation is neither entirely abandoned (because some properties of TS are accurately mapped by M), nor accepted entirely (because some properties of TS are inaccurately or not at all mapped by M).

Partial Isomorphism is firstly proposed by Newton C. A. da Costa and Steven French (1990). This view was later supported by other philosophers of science (see Bueno 1997, French and Ladyman 1999). Formal versions of Partial Isomorphism can be found in the literature (see, da Costa and French 2003, Ch. 2, and also *cf.* Lutz 2015).

[47] Hereafter, I will abbreviate the partial isomorphism view of representation with 'Partial Isomorphism' (with capitalized P and I). The other use of the term partial isomorphism refers to the partial isomorphic relation.

Nonetheless, I will stick to the less formal version of it here, as I did in **§6.2**, because it would suffice for my purposes.

Partial structure is a set–theoretic construction. The target system is a non–empty set. In other words, TS is a set of elements that each of its members belongs to domain (D). The domain of TS is symbolized as D_{TS}. The structural extension of TS additionally involves a set of partial relations (R) which hold between the elements of D_{TS}. A partial relation R is considered as an ordered triple $\langle R_1, R_2, R_3\rangle$. These relations, (*i.e.* R_1, R_2 and R_3) do not make up a joint set. Instead, they are "mutually disjoint sets" (French 2003, p. 1480). R_1 is the sequence of relations that satisfy (or belong to) R; R_2 is the sequence of relations that do not satisfy R; and R_3 is the sequence of relations that is indeterminate whether or not a satisfaction relation stands for R (see also Bueno 1997, pp. 591–593). In this manner, there are three distinct relations $[i=i^1,i^2,i^3]$ among the elements of TS. The structuralized system, TS can be expressed as $S_{TS} = \langle D_{TS}, R^i_{TS}\rangle$ where $R^i_{TS} = \langle R^{i\boldsymbol{1}}_{TS,}\ R^{i\boldsymbol{2}}_{TS},\ R^{i\boldsymbol{3}}_{TS}\rangle$. If the constructed model M is expressed as $S_M = \langle D_M, R^i_M\rangle$ where $R^i_{M} = \langle R^{i\boldsymbol{1}}_{M,}\ R^{i\boldsymbol{2}}_{M},\ R^{i\boldsymbol{3}}_{M}\rangle$, then one can obtain two partial structures. As is seen, isomorphic relation holds between these two structures (*i.e.* between S_M and S_{TS}), so M represents TS.

It is not difficult to see that Partial Isomorphism has been developed to capture inaccurate models in an adequate way. The three distinct relations between the model and the target system, namely $R^{i\boldsymbol{1}}_{M,}\ R^{i\boldsymbol{2}}_{M}$, and $R^{i\boldsymbol{3}}_{M}$ each corresponds respectively to the properties represented, misrepresented and non–represented. In this sense, the inaccurate status of models ceases to be problematic for Partial Isomorphism, as it is for Isomorphism and Similarity. In other words, Partial Isomorphism takes idealized and abstracted properties of TS into account.

With this context in mind, we could try to interpret the astronomical model of the ice ages in terms of Partial Isomorphism. The three orbital cycles in the model, namely eccentricity, obliquity and precession, are accurately represented. In this sense, M is isomorphic to TS with respect to these three elements. So, the isomorphism relationship holds for the pair $\langle R^{i\boldsymbol{1}}_{M},\ R^{i\boldsymbol{1}}_{TS}\rangle$. However, there are also parts that the model misrepresents or does not represent, namely the relations which are non–isomorphic. For example, the albedo effect is a factor that is effective in determining

total insolation; however it is idealized from the model. Thus, with respect to the albedo effect, there is no isomorphic relationship between the M and the TS. In other words, the pair of relationship $\langle R^{i2}_{M}, R^{i2}_{TS} \rangle$ denotes that M and TS are, in one respect, structurally distinct. Finally, the model is in indeterminate relationship to some properties of its target system. For example, the influence of the atmospheric composition on the amount of insolation is unspecified. In other words, the model is completely silent about this factor. This means that the atmospheric composition of TS is not mapped by the model, *i.e*. one property of TS is non–isomorphic to M. This is expressed by the pair $\langle R^{i3}_{M}, R^{i3}_{TS} \rangle$.

Partial Isomorphism seems to be more inclusive than the two major views, *i.e*. Isomorphism and Similarity. The two major views are insufficient in explaining representation, especially when it comes to the inaccuracy in models. Recall that the representational problem concerning Isomorphism and Similarity was twofold. The second part of the problem was about the representational status of inaccurate models. With regard to the representational status of idealized and abstract models, Partial Isomorphism seems more explanatory than the two major views of representation. On the other hand, there remains the first part of the problem. Accordingly, a theory of representation is expected to be compatible with the (three) intuitive statements about representation. That is, Partial Isomorphism must be non–reflexive, non–symmetric[48], and non–transitive. Isomorphism and Similarity have satisfied this expectation with integrating the user into the representation. The same strategy can be used by Partial Isomorphism as well. In other words, agent based Partial Isomorphism does not violate the intuitive principles of representation.

6.6 Conclusion

In this chapter, I have recapitulated the two major representation theories which I have also discussed in detail in Chapter 3. I have also applied these two theories to the astronomical model of the ice ages. Both views of representation seemingly explain how the model represents. However, both explanations are limited in range to account for the inaccuracy in the models. Both Isomorphism and Similarity do not

[48] Many of the defenders of Partial Isomorphism underline the significance of the user. For example, Otávio Bueno and Steven French (2011) argue that "[the] use is crucial, for example, [to] break the symmetry" due to the fact that "partial isomorphism is an equivalence relation" (p. 885).

provide a substantial basis for understanding the representational status of idealized and abstract models.

Partial Isomorphism, as an alternative to these two major views, at least takes into account the inaccurateness of the models. Accounting for idealization is especially crucial for a theory of representation. The reason is because some models explain not *despite* but *in virtue of* their contained idealizations. From this perspective, misrepresented properties of a target system would be at least as important as the represented properties.

I also reviewed a *Gricean* critique of isomorphism and similarity in **§3.6**. According to this critique, structural similarities (isomorphism, homomorphism *etc.*) or similarity in general is not necessary for representation. This view suggests that for any kind of representation, the modeler's stipulation would suffice. If the *Gricean* view is accepted, then Partial Isomorphism, which is based on structural similarity, will also be subject to such criticism. To my mind, *Griceanism* appears to suffering from a serious problem. Although this view does not regard similarity (structural or not) as a necessary condition, it actually assumes it for representation. Recall the example of Michigan State. According to the *Griceanist* conception of representation, one can represent the geography of this state with both left and right hands. Given this view, however, it would be *preferable* to represent Michigan with an upturned right hand than with an upturned left hand. Why? The reason is that Michigan's geographical border is geometrically *more similar* to the structure of the upturned right hand than to the upturned left hand. In a similar perspective, Anjan Chakravartty (2010) rightly asks "how, one might wonder, could such practices (like interpretation and inference with respect to its target system) be facilitated successfully, were it not for some sort of similarity between the representation and the thing it represents—is it a miracle?" (p. 201). This question seems appropriate as well as meaningful because if there is a stipulative element in the representation relationship between M and TS, what would be the rational basis for preferring the right hand to the left one? In this sense, it is inevitable to assume a similarity (structural or not) between M and TS.

In this chapter, I have suggested that arguing for the similarity between partial structures provides a satisfactory framework for representation. Nevertheless, I tried to emphasize that the partial relationship between the extensional structures of M and TS is not sufficient for representation. It is because one needs to consider the user who performs the activity of representing. In this sense, representation is a three–place relation in which the intentional agent is present. To put it differently, *Agent Based Partial Isomorphism* seems to be the best candidate, among others to account for representation.

CHAPTER SEVEN

UNCOVERING THE EPISTEMIC FUNCTIONS OF DE–IDEALIZATION

7.1 Introduction

Complexity of the world seems to be an indisputable fact. True, the world we live in is complex, and to cope with this fact we often employ myriad of strategies. One of the strategies scientists use is to construct models. Models are structures that represent *particular* aspects of the complex world. Therefore, all models are idealized to some degree. Complexity of the world imposes challenges for scientists to build highly representative models of phenomena. For this reason, scientists deliberately exclude the causal factors that are considered to be less relevant for particular explanation, and many do so without any certain knowledge whether the excluded factors from the model are explanatorily relevant or not. In any case, scientists inevitably idealize. But this is not the whole story. There is another modeling strategy called de–idealization. The reverse of this idealization strategy enables scientists to add back the factors that are not represented by a model due to their status of explanatory relevance. In this way, explanatorily less relevant or unrecognized factors are included to the model afterwards. This, in turn, makes the model representatively more accurate.

At first glance, such a reverse modeling strategy seems hardly reasonable. The hallmark of scientific idealization is its facilitation of constructing sufficiently representative models of the complex world around us. Moreover, we cannot underestimate its role of providing successful explanations and predictions. If taking these benefits into account, it is hard to understand why scientists de–idealize or remove particular idealizations. What, then, are the roles and functions of de–idealization strategy in scientific modeling? In this chapter, I will try to answer this question by considering some prominent views in the scientific idealization literature. Although these views do not directly address the question that I pose, they implicitly contain assumptions regarding the issue of de-idealization. My broad aim is to outline the implications of these views on this reverse modeling strategy. After outlining the hidden assumptions, I reveal two major epistemic functions of de-idealization.

The first one of these epistemic functions is about scientific models themselves; the second is directly related to idealizations in a scientific model. The first epistemic function of de–idealization is to provide knowledge about whether a model is, as I call, a 'core model' *i.e.*, whether or not it has established true causal factors. The second epistemic function of de–idealization is to identify the degree of explanatory relevance that each corrected causal factor may have. Nevertheless, epistemic functions of de–idealization are not only limited to those two. As I will subsequently be able to show by the case study method, I reintroduce the example presented in Chapter 5 and attempt to demonstrate a third epistemic function of de-idealization. With this function, I argue, one may disclose new information about the corrected idealization itself. In other words, de-idealization sometimes functions as a modeling strategy to provide new knowledge about the idealized part of a model.

7.2 Five Roles of De-Idealization

In this section, I investigate the scientific strategy of de–idealization which has been omitted until now or indirectly touched upon in the scientific modeling literature. Firstly, I try to figure out what sort of roles de–idealizations play in scientific modeling. In doing so, I summarize the hypotheses of various approaches which indirectly addressed the roles of de–idealization strategy. Subsequently, I argue that these five roles essentially fulfill two major epistemic functions. Thus, I conclude that various roles of de–idealization might be reduced to two major epistemic functions.

7.2.1 Role #1: Argument for realism

Ernan McMullin (1985) was one of the first philosophers to define a role for de–idealization. Under the heading of 'Galilean Idealizations', McMullin brings together different kinds of idealizations (see Appendix B). Galilean idealizations fall into three primary categories: mathematical, causal and construct idealizations. Mathematical idealizations highlight the imposition of mathematical approaches over physical circumstances (see McMullin 1985, pp. 248–254). Causal idealizations are twofold and closely connected. This kind of idealization first emphasizes the step–by–step process of model building; including the representations of many causal factors and then idealizing some of them to see whether there are less relevant factors

for the phenomenon under study (see McMullin 1985, pp. 264-268). The last main kind of idealizations is construct idealizations where McMullin speaks about de–idealization strategy. Construct idealizations fall into two parts: 'formal' and 'material'. According to McMullin, the latter types of idealizations are the ones not to be *corrected* but to be *filled in* the conceptual framework. In his words, theories are constructs that are "unspecified, though open for question in different context" (McMullin 1985, p. 263). Therefore, a false assumption has not been corrected in the model. Instead, blanks are filled or a part that has not been expressly mentioned by a model is complemented. But, with regard to the correction, formal idealizations differentiate themselves from material idealizations. For this reason, we may directly address this former kind of idealizations in the context of de–idealizations.

Formal idealizations enable us to exclude the explanatorily relevant factors from the model to derive theoretical laws. Thus, in particular models, explanatorily relevant features are "simplified or omitted in order to obtain a result" (McMullin 1985, p. 258). To be sure, the results that we obtain from such models would be "approximate consequences" (McMullin 1985, p.258). When modeler wants to make the obtained results more accurate, he engages in the add-back process *i.e.* de–idealizes the model. In other words, "models can be made more specific by eliminating simplifying assumptions and 'de–idealizing', as it were" (McMullin 1985, p. 261). To illustrate his point McMullin gives an example of ideal gas model. As stated in many physics textbooks, the ideal gas model neglects and thus idealizes small molecular volumes and weak intermolecular forces to derive Boyle's Law. As most of us know by now, this model has been de–idealized by taking into account the space occupation and the intermolecular attractions of molecules. As we know it, the de–idealized version of the ideal gas model is *van der Waals*' gas model. Given this model, considering the volumes of molecules and the attractive or repulsive intermolecular forces provides us more proximate results of gas behavior in high pressures than compared to the ideal gas model.

Formal idealizations, as described by McMullin, have a wide usage across modeling and seemingly allow scientists to make corrections on idealizations which in turn yield more approximate results. Nevertheless, McMullin's main aim seems to be to devise a role to de–idealization as an argument for scientific realism. The reason is

that, he evaluates formal idealizations as an important component of continuing research program. Such an ongoing research program aims at showing that currently used models, though incompletely, represents, at least, the "existence of the [real] structures" (McMullin 1985, p. 262). According to this line of thought, if making some corrections on the structures postulated by the model makes possible to get more approximate results, then we can confidently state that there really exists such structures. In like manner, if the model misrepresented target phenomenon at the outset, we would expect to obtain completely different results from de–idealization.

7.2.2 Role #2: The means for pragmatic justification

The relationship between de–idealization strategy and the realism/anti-realism debate is not new (see Nowak 1979), but many controversies stemmed from McMullin's understanding of the role of de–idealization and its implications in favor of scientific realism (see Odenbaugh 2011; Saatsi 2016; Liu 2019). An important aspect of the controversies initiated an inspiration for Michael Weisberg. According to him, providing more details on how scientific idealizations are justified would solve out the problems related to scientific idealizations. I have to highlight that, Weisberg had not advanced the realism/anti-realism debate because for him, as I do not intend to go into detail now, "idealization cannot be used as ammunition for either side of the realism debate" (Elliott–Graves and Weisberg 2014, p. 183).

Weisberg (2007) believes that the philosophical problem of idealization is closely related to the goals that scientists are striving to reach. Accordingly, he attempts to articulate these goals that guide scientists' theoretical inquiry. Only with this understanding in hand, we can proceed to understand why idealization is needed and how it is justified as well. Weisberg specifies three distinct kinds of idealization each of which is characterized by diverse motivations. While all these kinds of idealizations are worth examining in detail, I will focus on Galilean idealizations which are relevant for the purposes the de–idealization strategy.

It would not be an overstatement to claim that Weisberg's approach to Galilean idealizations is hastily introduced. Actually, McMullin intends to characterize idealizations that each is unique in their own and distinguish them under a separate name as 'Galilean Idealizations'. However, what Weisberg understands from

Galilean idealizations are merely formal idealizations. To my mind, this point is not vital to the remainder of the discussion, especially to the discussions that I will mention in the following sections. However, I should note that Weisberg's misrepresentation of Galilean idealizations, as treating them merely as formal idealizations, do not do justice to the McMullin's original views and entails a risk of leading to conceptual confusions.

The merit of Weisberg's view lies in its goal-directed characteristic of modeling. Given this view, models are constructed with respect to the representational ideals. To put it bluntly, these ideals "guide the direction of theoretical inquiry" (Weisberg 2007, p. 648). If a modeler has an ideal of completeness, he would aim to represent the target phenomenon completely by applying the model. But approaching such a task is not always an easy matter. Oftentimes, modelers idealize in a Galilean way to accomplish their representational ideals. Yet only with such attitude, models can be "computationally tractable" (Weisberg 2007, p. 640). At the beginning, modeler starts her inquiry with a simple model. Her goal takes shape in the distortion of represented phenomenon to construct a mathematically tractable model. When the tractability condition is satisfied *i.e.* when the "advances in computational power and mathematical techniques" happen "the Galilean idealizer […] de–idealize(s), remove(s) distortion and add(s) back detail to her theories" (Weisberg 2013, p. 99). This process persists as the resident research program continues.

According to Weisberg, this persistent process of correction constitutes the fundamental feature of Galilean idealizations. The thing which distinguishes Galilean idealizations most from other kinds of idealizations (minimal and multiple idealizations) is its embracement of de–idealization. With future improvements in science, the modeler, in a sense, promises to make her model more accurate in order to represent target phenomenon completely. However, she recognizes that the issue should be settled one way or another for the moment. Therefore, the idealizations contained in the model are justified pragmatically. To be sure, the representational ideal of completeness does not rest on an assumption that there will be, sometime in the future, a model which would represent its target phenomenon completely. As a matter of fact, models are always imperfect representations of the world. Moreover, it would be misleading to call a fully representative model 'model'. Although

Weisberg did not put forward his own idea of completeness as such, the most charitable way to make sense of his view is to assume that models can hardly be *completely accurate* but at best be *more accurate*.

7.2.3 Role #3: Describing causal mechanisms

The other role that is played by de–idealization is to provide knowledge whether a model adequately represents causal relationships. This role of de–idealization is mainly proposed in the context of robustness analysis by Jay Odenbaugh and Anna Alexandrova (2011). As situated in the modeling literature, robustness analysis is an approach that allows the modeler to understand whether the results derived from a model hold under different idealizations *i.e.,* different false assumptions (see Levins 1966, Lisciandra 2017). Accordingly, if similar results are derived for a model in which different false assumptions are replaced, then we call that model 'robust'. In the contrary cases, where the results differ from the previous ones, we may conclude that our model is not robust just because its outcomes depend on the idealizations made.

Odenbaugh and Alexandrova mainly argue against the approach adopted by Jaakko Kuorikoski *et al.* (2010) which claims that robustness analysis is a method for evaluating whether a particular model represents causal mechanisms. According to this objection, method of robustness analysis would not be helpful in understanding that a model is *really* robust to its idealizations. There is an important reason for this. In order to speak of a robust model, they maintain, we should establish that our model describes the causal mechanisms; whereas this analysis merely tells us that our model is "robust to some idealizations" (Odenbaugh and Alexandrova 2011, p. 763). So, although robustness analysis method operates as a means of determining that a particular model does not depend on false assumptions, it does not imply a conclusion that the model describes causal relationships. To be sure, this does not show that robustness analysis method is useless. Odenbaugh and Alexandrova strictly emphasize that robustness analysis shows us that a model does not "depend on particular falsities" and at least "is a good tool of discovery" (Odenbaugh and Alexandrova 2011, p. 770).

Yet how is it possible to understand that the model specifies the actual causal relations and mechanisms of a phenomenon? According to Odenbaugh and Alexandrova, the key to understand this is to de–idealize the model. Frankly, this would be a quite shortcut interpretation of what authors are trying to say. The detailed argument is lengthier, but it comes down to one very simple idea: we cannot know that we really represented causal relations through the robustness analysis process, especially when it comes to the models that resist being de–idealized. In their words "unless we can 'de–idealize' our [false] assumptions, (and others if we cannot sharply distinguish between different types of idealized assumptions), we do not know that we have adequately represented a causal relationship" (Odenbaugh and Alexandrova 2011, p. 763). In this view, then, unless we can de–idealize the model, there is no way to grasp the relevance of idealizations in terms of its explanation. For that reason alone, robustness analysis approach and de–idealization are not interchangeable. So, merely being robust to different false assumptions does not amount to a good reason for a model to be a robust one. What we have to do is make sure that we corrected the idealizations. It is only after this correction process is completed, we will be able to know whether the model is robust *i.e.*, the results of the model again hold.

In this sense, we can assert that Odenbaugh and Alexandrova assign a vital role for de–idealization. For them, if we cannot de–idealize a model, we seem to have no way of saying that that model really represents causal relationships which explains targeted phenomenon. To say a model is robust to different idealizations, namely to get similar results by replacing false assumptions with other false assumptions does not allow us to conclude that the model we analyze is robust. Therefore, the genuine task of modeler is to check whether the results of the model hold by directly correcting the included idealizations.

7.2.4 Role #4: Identification of harmless idealizations

Mehmet Elgin and Elliott Sober (2002) present a different role of de–idealization. Even though their main attempt is not to focus on de–idealizations, some of the conclusions they have drawn seem to have implications for the model correction strategy.

Elgin and Sober argue against Nancy Cartwright's claim that fundamental laws of science are not applicable to the real world situations. Cartwright's (1980 and 1983) argument goes as follows: Any law that can be expressed in a conditional statement form, either makes false claims about the world or makes true claims that can be applied to idealized situations. For example, if one wants to make a true claim about the forces that two objects exert on each other, it would not be enough for him merely to consider each object's mass and the distance between them. Additionally, one need to attach a proviso to this claim, simply by assuming there are no other forces present except the gravitational ones. In this case, the law is stated in an idealized form and therefore its truth should be evaluated accordingly. It is because in real world situations, gravitational forces are not the only ones acting on the systems –for instance there are series of situations where electrical forces are present (see Cartwright 1980, pp. 77-79). Therefore the fundamental laws of science are false if certain provisos (ceteris paribus clauses) are not attached. Conversely, if these provisos are attached just this once, they are not applicable to the real world situations. There are further details on Cartwright's main argument but this is a nutshell version of her argument put forward in *How the Laws of Physics Lie*.

Nevertheless, Elgin and Sober assert that Cartwright's argument has a serious flaw. Their starting point is to apply the contrapositive rule of propositional logic to the law statement expressed in a conditional form. There is an important reason for this. It is because "if a conditional and its contrapositive apply to the same things, then either both apply to real objects or neither does" (Elgin and Sober 2002, p. 444). Following this logical principle of equivalence, they state that the contrapositive of the law statement is true about the real world objects. So, from the logical point of view the conditional should be of the same truth value. Thus, it follows that fundamental laws of science (and also idealized models which include idealized laws) are indeed applicable to the real world situations.

Contrary to the claim which is put forward by Cartwright, they maintain that, equipped with the knowledge of correct masses of objects and the distance between them, we can confidently apply the idealized model. Here is how they reason. For them, contrapositive of Cartwirght's original claim is as follows: If force does not equal to the multiplication of two masses divided by the square of the distance

between them, then there are non-gravitational forces acting on the system. This is so because Cartwright's original claim is this: If the gravity is the only force acting on the system then the force between two masses is equal to their multiplication divided by the square of the distance between them.

When the values predicted by the model and the actual values obtained agree with each other, this may be evidence that the model's idealizations are not harmless. However, one cannot conclude, as a matter of fact, that the right idealizations have been made. This is simply because of the problem of induction. However, when the values are too far off, one can conclude that something definitely is wrong with the idealization of the model. Therefore, a fundamental law of science, expressed as conditional statement form, makes a true claim. Furthermore, these laws of science are not only applicable to a real world situation, but also are explanatory. So, building on these ideas, the laws of science neither lies to us nor inapplicable to real world objects.

Elgin and Sober are able to show that idealized models can be explanatory without relying on the view that Cartwright adopts. Unquestionably, they are aware that the explanations provided by idealized models would not be "complete" because basically these models do not contain "*all* causally relevant factors" (Elgin and Sober 2002, p. 447). But even so, there is no need for a model to capture all causally relevant factors because it would suffice for it to merely describe causal factors that do the explanatory work. Besides, we can omit the factors that are causally relevant but explanatorily irrelevant to the outcome of the model.

Having said that, there is one more central point I should consider with respect to my purpose. According to Elgin and Sober, de–idealization is an important modeling strategy if one wants to know why causally relevant (but explanatorily irrelevant) factors do not really do explanatory work. Accordingly, they state that "the idealizations in a causal model are harmless if correcting them wouldn't make much difference in the predicted value of the effect variable" (Elgin and Sober 2002, p. 448). It means that, after de–idealization, if the false assumptions *i.e.* the idealizations make negligible difference to the outcome of a model, then they can be called harmless. Of course, after applying the model to physical situations, the little

difference between the predicted and observed values already provides evidence that the particular idealizations are harmless. But Elgin and Sober have something in mind that showing the negligible difference between the values (both obtained by prediction and observation) by correcting the idealizations of a causal model provides evidence the harmlessness of that particular idealizations.

7.2.5 Role #5: A basis for understanding non-difference makers

The last view, which implicitly designates a role for de–idealizations, is proposed by Michael Strevens (2004, 2009 and 2017). Strevens aims to show how idealized causal models can successfully explain. It seems a *bona fide* aim to account for this issue because if we idealize the causally efficacious factors from the model, then the explanation we get becomes hardly reliable. For Strevens, this is misleading because many scientific explanations provide explanations in this very way.

First, Strevens objects to the intuitive idea that veridical models would provide best explanations just because they contain the whole causal story in which the target phenomenon is produced. Among many other reasons, mostly the difficulty of modeling computationally complex systems, we cannot build such a realistic model that represents all causally relevant factors. Moreover, by representing all such causal factors as explanans for an explanandum, we treat each factor of our model as causally efficacious property which makes equal contribution to explanation. This would, in turn, lead to a removal of the distinction between difference-makers and non-difference-makers. For example, the ideal gas model that helps to explain Boyle's Law "deliberately misrepresents the causal process [by ignoring] the long range attractive forces between molecules [and assuming] molecules as not colliding" (Strevens 2004, p. 157). However, we know much about the attractive forces and collusions between the particles of a substance. And we also know that this leads to an alteration of the behavior of gases under specific pressures. But if so, how could we explain Boyle's Law with the false assumptions (*i.e.* idealizations) of the ideal gas model? According to Strevens the answer is simple. Idealizations of the ideal gas model are non-difference making features for explaining the Boylean behavior. To put it differently, the ideal gas model has successfully identified difference makers, namely it has been underpinned by explanatory kernel that

successfully explains Boyle's Law. But, there is more to the story. Strevens maintains that if we attempt to make the model more accurate by adding the volumes of molecules and the attractive intermolecular forces back in to it, there would be no real advancement in explanation over the previous one. In other words "if the distorted description were replaced by a true description of the details, there would be no improvement of the quality of the explanation [*i.e.*] the ideal gas explanation is in no way inferior to its counterpart" (Strevens 2009, p. 311).

Strevens' views on de–idealization are of interest for the subject matter of this work.[49] As summarized above, he asserts that if an idealized model describes truly the parts that make difference to the explanation, then it is explanatory -because the remaining part *i.e.* idealizations (you may call them causal distortions or non-difference makers) have no crucial role in explaining the phenomenon of interest. Ultimately, Strevens accounts for how models explain, but he also touches upon the issue of why non-difference makers do not make contribution to model explanations. He suggests that having an answer to this why-question, one needs to make corrections to the model by de–idealizing it. Of course, de–idealizing the model would be of no advantage to the explanation, but at least relaxing the idealizations would show why this is so. The intellectual benefit that ensued from the de–idealization, then, is to contribute to our understanding of why non-difference makers do not make difference.

If so, there are a couple of strategies that a modeler could employ. Firstly, the modeler "identif[ies] the difference–makers and (thus) the non-difference-makers for the phenomenon [and then] grasps the reasons why they are difference-makers and non-difference-makers" (Strevens 2017, p. 45). That is, in model building process; the first thing to do is to determine difference makers and non-difference makers for explanation. By this means, for example, we identify which factors are necessary to explain Boyle's Law and also which factors could be eliminated. Secondly, we become able to grasp why difference makers are required to derive explanation and

[49] To be fair, aspects of Streven's views about modeling are roughly discussed in this section. It is because much of the details of his arguments are out of the scope of this work. Nevertheless, I should note that, his assertions about the disadvantages of the unificationist theory of explanation over his *kairetic* account or his comparison of superiority/inferiority of explanations between canonical and idealized models are still open for discussion.

why absences of the remaining parts are really non-essential for particular explanation. Eventually, de–idealization or "creating more realistic models" are required in order to gain a good grasp of the situation (Strevens 2017, p. 45).

7.3 The Epistemic Functions of De–Idealization

In the preceding sections, I have outlined the roles that de–idealizations play in various contexts. Here is a synopsis of them:

a) *De-idealizations provide an argument for realism within the context of the debate about realist and anti-realist approach*. If predicted values of the present model can fit data more precisely than those before de–idealization, then the previous model has at least captured the structure of the target system. This, in turn, shows that there is a genuine representation between the model and its target system. Obtaining very similar outcomes from a model and its de-idealized version (or the less difference in predicted values between two versions of the models) supports the idea that there really exist such structures represented by the model.

b) *De-idealization justifies idealizations temporarily or pragmatically*. While constructing models, modelers inevitably make false assumptions. In this way, they rule out the possibility of dealing with complex cases by converting a mathematically intractable model into a tractable one. When mathematical techniques and computational power significantly advance, the modeler corrects idealizations *i.e.* increases the representative accuracy of the model by de–idealizing it. As it is understood, the modeler justifies the idealizations used in his model by promising to grant further correction. In other words, we are allowed to justify pragmatically only in case of positing de–idealization as a prior condition.

c) *De-idealizations provide an understanding of whether a model describes causal mechanisms that produce the phenomenon*. We could call a model robust, when we assume that it describes actual causal relations. Some methods can be used to find out about the robustness of a model. For example, if a model's new predicted values are close to those of the former ones after correcting the idealizations, we can validate the robustness of the model. In this respect, deciding on whether a model is robust requires directly de–idealization of the model's idealizations. Thus, replacing

a false statement of the model with another false alternative would not be a genuine test for robustness.

d) *De–idealizations identify harmless idealizations*. As its name implies, this kind of idealizations are harmless because models can be explanatory or predictively successful despite containing them. But, to say an idealized model is explanatory or predictively successful, we need to correct the idealized model and assess whether new outcomes and the observed values are significantly matched for the purposes. Subsequently, if both values are close to each other (namely, if the difference is acceptable), the idealizations of the models are harmless to explanation.

e) *De–idealizations show why some idealizations cannot play explanatory roles*. In many cases, modelers already explain a particular phenomenon by applying their models. This, however, merely shows that their model sufficiently represents the causal difference makers that produce the particular phenomenon. Doubtless, there is another part of the model which includes non–difference makers (*i.e.* idealizations). As we know, they do not play an important role for explanatory purposes. However, one might want to take the research one step further to analyze the explanatory inefficiency of these idealizations. To complete this research, the modeler might invoke de–idealization.

Thus far, I summarized the views in the literature that attribute certain roles for de-idealization. I believe, this in itself, shows the very importance of this reverse modeling strategy. However, there is more to all these five roles than meets the eye. I argue that these roles fulfill two major epistemic functions in the context of scientific modeling. In particular, I claim that de–idealizations provide an epistemic standpoint from which we gain an understanding about both models and idealizations.

In these terms, we come to the central question of this chapter: What can we learn from de–idealizations? At the outset, I raised a concern with respect to the significance of de–idealization: If we purposefully idealize particular factors at the outset of model building, on what rationale we de–idealize them after –while also having a successful explanation at hand. The five roles of de–idealization, which I have above listed on an item-by-item basis, indeed provide a response to my own concern. However, it would advance our goals more effectively if we were to reach

deep down epistemic functions of de–idealization. Hopefully, with uncovering these functions, we will be able to understand what can be learned from de–idealizations.

Before proceeding to further issues, it would be helpful to briefly elaborate my implicit assumption which implies a distinction between role and function. Perhaps an analogy would be useful here: Almost everybody knows that corkscrews are a must have in the kitchens of wine lovers. A corkscrew consists of a metal piece usually designed in a helix form. We first firmly screw it into the wine cork, then push down the levers and extract the cork from the bottle, and there you are –an open bottle of wine! According to their types, table wines accompany fish or red meat and frequently to the moments of joy as well as to the moments of despair. Apparently, corkscrews can play a positive role, for some of us, in boosting happiness or handling loneliness. But, as everybody knows, these are not the functions of corkscrews –rather, their specific function is to open wine bottles. This corkscrew analogy shows pretty much exactly what I have in mind when I talk about the terms 'role' and 'function'.

So far so good; now let me repeat the question: What can we learn from de–idealization? Broadly speaking, the first thing we learn from de–idealization is about the models themselves and the second thing is about the idealizations. Let me elaborate what I mean.

I argue that despite having many roles in science, de–idealizations have two major epistemic functions. Recall the roles that I have compactly presented at the outset of this section. I assert that the roles of (a), (b) and (c), all together, suggest that the de–idealization equips us with the knowledge that the model we use is reliable. Then, the first epistemic function of de–idealization is to provide reasons for trusting our particular models at hand. The roles and epistemic functions of de–idealization are the issues where respectively McMullin, Weisberg, and Odenbaugh and Alexandrova both differ and overlap. According to McMullin, we can confidently state that there exist real structures postulated by the model and thus commit ourselves to realism, inasmuch as the de–idealized model provides close outcomes with respect to its more idealized version (McMullin 1985, pp. 259–262). But this view is not without its critics. According to Weisberg, this point of view would not be able to resolve the

realism/anti-realism debate because all opposing views can equally generate arguments in favor of their positions (see Weisberg 2007, pp. 656-659). Nevertheless, with the expectation of further correction, de–idealization can play a role in justifying the idealizations in a pragmatic way. When advancements in computational techniques are available, for example, the modeler would be able to de–idealize her model. But, till that time, she keeps using the model in its less actualized form. From a different viewpoint, Odenbaugh and Alexandrova claim that de–idealization has a fundamental role in showing whether a model is robust *i.e.* the mechanisms that the model describes hold. In order to understand this, what needs to be done is not, as some insists, to replace particular idealizations with the alternative ones. Rather, the modeler needs to correct the idealizations contained in the model and needs to show the similar outcomes are obtained.

As seen, the difference between these views may not only be a matter of nuance. The first view suggests that de–idealizations play a role in providing argument for realism. The second view suggests that de-idealization plays a role in justifying idealizations temporarily or pragmatically. And, the third view suggests that de-idealizations play a role in providing understanding of whether a model describes causal mechanisms that produce the phenomenon.

Beyond these differences, however, all three views share a common point. Given these views, the de-idealization process indicates that we are on the right track to maintain our continuing research program, namely by showing that we are armed with reliable models of particular phenomena. Removing the idealizations of any model might either supports realism, or provides pragmatic justification, or gives insight about the model's causal mechanism. In addition to these, however, it gives hints about the model itself, in a sense that we have a core and reliable model. Put differently, de-idealizations serve as a function showing the model we use is a core model and is useful for prediction or explanation. In this manner, we have come to rely on a core model and its less idealized version equally well, in terms of its prediction and explanation. It is because obtaining more accurate results from de-idealization both increases confidence of the improved model and its more basic counterpart. Thus, one of the things we learn from de-idealization is that we have a robust core model. So, whatever the corrections to the robust core model may be;

they can all be classified as modifications. For example, as McMullin (1985) noted, the *van der Waals* gas model might seem to be a different model of gas behavior. However, it is not unique in itself. At the very most, its uniqueness lies in its modifications on some of the aspects of the ideal gas model –in such way that this modification consists of adding sizes and intermolecular forces of molecules back to the model. In other words, *van der Waals*' gas model is merely a modification and also a less idealized version of the robust core model (*i.e.* ideal gas model).

The remaining roles of de-idealization (d) and (e) fulfill a different epistemic function. This second major epistemic function is about the idealizations themselves, especially about their degree of explanatory relevance. To put it simply, correcting particular idealizations of the model would allow us to identify the degree of explanatory relevance of those particular idealizations. It is clear that with this statement, I assume that the idealizations contained in a particular model are causally relevant for that particular model's outcome. In other words, there are no irrelevant causal idealizations –yet if there are, they are at best abstractions. In fact, the explanatory relevance of all idealizations comes in degree. Yet this is the place where this second epistemic function comes into the picture. After all, we pre-theoretically conceive of particular idealizations as less relevant to the model's outcome at the outset of model construction. Nevertheless, we do not know to what degree exactly those particular idealizations are less relevant to the model's explanation. At this point, de-idealizing the model provides an advantage in identifying this degree of relevancy.

For example, Elgin and Sober claim that if correcting particular idealizations would not make much difference to the model's outcome then this shows the harmlessness degree of those particular idealizations, as it were. Given this view, for instance, if we identify similar values in both predicted values of an idealized model (such as an optimality model in evolutionary biology) and the observed values (such as trait values of a population), then we would provide evidence that to what extent a particular idealization is harmless (Elgin and Sober 2002, pp. 446-448). Strevens suggests a similar role for de-idealization as well. According to him, we should de-idealize the model to understand why non-difference makers are not necessary to derive particular outcomes (Strevens 2017, pp. 46-47). Seemingly, these two

accounts of de-idealization differ, though not dramatically, from each other. However, they both fulfill the same function with respect to their epistemic basis. Consider again the example of ideal gas model which explains Boyle's Law. In ideal gas model, molecular size and long range forces are set to zero. Nevertheless, modeler can de-idealize or can make the model more realistic by assigning small numbers to the parameters other than zero. Why? A significant or perhaps the only answer would be: 'Modeler wanted to show the effects of causal factors on explanation whose values were before valued as zero'. Accordingly, after de-idealization, we understand that the more realistic model has no crucial advantage over its idealized version. But more importantly, we figured out how the idealized model correctly described causal factors, as well as understood why particular causally relevant factors are properly not taken into consideration.[50]

Thus far, I have tried to show that de-idealization has two major epistemic functions. First, by means of de-idealization we provide reasons to trust our models. Sometimes, modelers correct or concretize particular idealizations to understand whether a model is epistemically reliable for explanatory purposes. If the correction ends with a model which has close results with respect to its more idealized version's (*i.e.* its core version), then we have at least good reason to think that the initial model has captured all causal factors that are explanatorily relevant. Secondly, by means of de-idealization we indicate the degree of explanatory relevance of each causal factor contained in particular model. To be sure, we idealize causally relevant but harmless (non difference making) factors at the outset of model building stage. But when these factors are added back to the model by de-idealization, we find an opportunity to understand to what degree that idealizations make difference to the provided explanation.

7.4 A Possible Objection: Are These Epistemic Functions Really Different?

When distinguishing de-idealizations with regard to their epistemic functions, I believe there is one thing that comes to mind –whether these two epistemic functions

[50] Whether a model explains a particular fact is context- dependent with respect to precision. Whether a general model of natural selection, for example, is explanatory or not depends on how many different kinds of problems it successfully addresses in its domain of applicability. Some of these problems will require perhaps more de-idealization strategy while others may require less. So how much de-idealizations actually required depends on a particular problem at hand.

are categorically distinct and whether or not one can be reduced to another. So, a possible objection to this distinction would be that among the two major epistemic functions, one of them implies the other, or vice versa. To put it more clearly: If we know that a particular model captures causal mechanisms that do the explanatory work, then we already have to know about the degree of explanatory relevance of its constituent idealizations. On the other hand, if we know that a particular idealization contained in the model is causally effective but has a very low degree of explanatory relevance, doesn't this imply that the model has captured relevant causal mechanisms that do the explanatory work?

This objection would be right if we were to look at the relevancy *status*, but not for the relevancy *degree* of the causal factors. But as a modeling strategy, de-idealization has nothing to do with the status of explanatory relevance. It is because the real issue about idealizations concerns to account for why some causal factors are not relevant for particular explanation. Therefore, the issue is not about to identify whether an idealization is relevant or not, because right from the beginnings of the model construction, its status is already approved as "explanatorily irrelevant" by the modelers. So, in this respect, an idealization is irrelevant to the explanation of phenomena but it is also causally relevant in terms of model's outcome. For example, Elgin and Sober (2002) mainly suggest that de-idealizations are made for identifying the degree but not the status of explanatory relevance of causal factors. Throughout their arguments, they never speak of harmful idealizations and I believe this silence is definitely a clue to start with. I think there's a charitable way to interpret this silence: If the authors claim that we can identify harmful idealizations by de-idealizing them, they would talk about the causal factors that do explanatory work. In other words, they would claim that such harmful idealizations are fully relevant for explaining a particular explanandum. And this would eventually change the explanatory status of those causal factors by rendering it explanatorily relevant to the particular explanandum. However, in such a case, the models were supposed to fail in the first place. It is because the model would contain an idealization which ignores the explanatorily relevant factors and also would predict values that are close to the values obtained from observations. For this reason, Elgin and Sober purposefully avoid using the term 'harmful'. Hence, they maintain that there are only harmless

idealizations which deliberately isolate causal but explanatorily irrelevant factors. If one wants to understand how harmless a particular idealization for model explanation is, then one needs to de-idealize. Only after this reverse modeling strategy has been employed and only after new predicted values have been obtained, will one be able to understand the degree of explanatory relevance (or the degree of harmlessness) of a particular idealization. Strevens (2009) is inclined to say similar things about the issue, except by calling harmless idealizations as non-difference makers. So, to sum up, harmless idealizations are means to isolate factors from the model which are causally less relevant but explanatorily irrelevant. The strategy of de-idealization serves the purpose to explain why particular causal factors fall at various points along the spectrum of explanatory relevance. A recently published paper of Tarja Knuuttila and Mary S. Morgan (2019) examines similar issues and includes a footnote that states "Elgin and Sober do not talk about harmful idealizations" (p. 645, f.n.6). As with the above reasons, I believe, this aspect of the issue became clarified.

A somewhat similar line of thought can be applied vice versa for the above objection. Just as describing causal mechanism does not necessitate for a model to explicate the degree of explanatory relevance of its own idealizations, identifying the degree of explanatory relevance of particular idealizations does not necessitate that the model has truly described the causal mechanism that produce the explanandum. First and foremost, a step-by-step process is developed here. To speak more clearly, in the model construction process, modeler first distinguishes explanatorily dispensable causal factors from the indispensable ones, by idealization. The function of de-idealization starts to appear only right after this distinction. Therefore, correcting the idealization of a model leads to a specific understanding (or quantification) of how much less relevant that a less explanatorily relevant causal factor is; it does not lead to an understanding of which causal factor is more relevant to the explanation and which is not. Accordingly, the first step of modeling is to identify explanatorily less relevant causal factors. Then comes the second step in which we de–idealize to understand to what degree such causal factors have less relevance to the explanation.

In brief, de-idealization establishes nothing more or less than the harmlessness of an idealization in the explanation. In other words, de-idealization explains why the determined status of an idealization essentially has that status. For example, the ideal

gas model states that pressure and volume are inversely proportional to each other, at a fix temperature. Nonetheless, it deliberately neglects the sizes of molecules and inter-molecular forces between them. As is seen, this successful model already presents the information, which causal factors are idealized and which not. The degree of explanatory relevance of the idealized causal factor(s) is not a prominent issue for the modeler in the model construction stage. At this stage, all she needs to do is to identify the explanatory status of the causal factors and to idealize the explanatory less relevant factors. Only after this step is it possible to show to what degree molecular size and inter-molecular forces together bear explanatory relevance to the explanandum (*i.e.* have less influence on the gas pressure). In sum, the epistemic function that is performed by de–idealization is somewhat different from the one before, namely it serves as a means of understanding the degree of explanatory relevance of the particular causal factors.

Let me summarize my point as I close this section. De-idealized models are the ones that are based on a more idealized core model. For example, *van der Waals*' model successfully explains the behavior of gases at very high pressures; nevertheless it is a modification of the ideal gas model and structured on the basis of it. So to speak, this less idealized model did a better job in explaining a different aspect of the phenomenon than did the more idealized model –but in the end it is based primarily upon this more idealized model. Therefore, at least within a particular paradigm, one could provide a good reason to infer that the initial model developed is a 'core model'. This is the first epistemic function of de–idealization. The second epistemic function of de-idealization is that it indicates the degree of explanatory relevance of the particular idealized causal factors by adding them back to the model. All scientific models including the core ones, by their very structure, isolate particular causal factors from themselves for specific purposes. This is a deliberate strategy on the modeler's part because empirically observed data closely fits the expected outcomes of the model even without these isolated causal factors. But, if one wishes to account for why there is a close agreement between the observed data and predicted outcomes of the model, one needs to de-idealize the model. Therefore, when one makes corrections to the particular idealizations of the model, the knowledge of both model and its idealizations would be obtained all at once, as it

were. It can be stated then that de-idealization is one reverse modeling strategy, but it fulfills two epistemic functions, each categorically distinct from the other.

7.5 More Than Two: A Less Common Epistemic Function of De-Idealization

Another important thing about de-idealization is that it serves a function to obtain new information about the idealized factor(s). To put it more precisely, de-idealization not only informs us about the model and its idealizations' degree of explanatory relevance, but also helps to reveal new information about the idealized factor itself. Strictly speaking, this epistemic function manifests itself less often; namely not all de-idealization strategies end with such a profit.

Let's recall the example from the previous section. Correcting the idealizations of the ideal gas model both showed that the model captured all explanatorily relevant causal factors (*i.e.* showed that the model is a core model) and accounted for why the idealized causal factors are explanatorily less relevant. Beyond this, however, no extra knowledge was gained from de-idealization, including why the size of the particles is small or why intermolecular forces of attraction are weak. As emphasized above, de-idealization process need not be epistemically effective every time. Generally, our knowledge about omitting such explanatorily less relevant causal factors was gained through the testable hypotheses of the fundamental scientific theories well before constructing the model. In other words, modeler easily idealizes such factors because she already has the knowledge about their status of explanatory relevance.

As I stressed, however, by way of de-idealizing we may sometimes hope to gain insight about the idealized causal factors. To support this claim, I will resort to the case study I presented in **§5.6**. Elaborating on it will allow me to better illustrate my argument. Generally speaking, I aim to present how this minor epistemic function is fulfilled (along with the two major functions) by de-idealization.

7.6 The Astronomical Model of Ice Ages

Within extremely long periods of time, the Earth sometimes enters an ice age. In such a condition, global temperatures drop considerably and ice sheets begin to surround the continents. However, at certain intervals, the Earth's temperature shifts from cold to relatively warm. As it is known today we, the inhabitants of the Earth,

sustain our lives under such moderate and warmer temperature conditions. In other words, we took a relatively short break from frosty conditions to enjoy a warm interval.

Thinking about these drastic temperature changes, one central question comes to mind: What are the responsible factors that influence these periodic changes of the climatic cycle (*i.e.* the cyclic patterns of glacials and interglacials)? In fact, as I discussed it from a different perspective in Chapter 5, this issue was not addressed adequately (in a scientific manner) till the first quarter of the nineteenth century. Nevertheless, the period from the nineteenth century and onwards to today, the problem is classified as a problem of geology and astronomy. Thanks to the efforts of past researchers, scientists have now a theoretical framework to explain why glaciations occur and what triggers the onset of the ice ages.

As far as we know today, the theory of plate tectonics successfully provides an explanation for the phenomenon of the ice ages. In extremely long time frames, the continents are rebuilt into many new configurations by tectonic movements. For instance, the continents that we know today were once together approximately 260 million years ago. Then, gradually they break apart from this single landmass which ultimately formed seven new continents. Plate tectonics is a successful descriptive theory in which we can derive plenty of testable predictions (or retrodictions). For example, formation of the mountains, earthquakes and volcanoes can all be explained in terms of this drift processes. Moreover, continental drift offers an insight into formation of the ice ages. Glaciers, for instance, can only be formed on landmasses. And if considerable portion of the landmass drift further to the North or South, namely to high latitudes, we would expect that an ice age will begin relatively soon. In other words, clustering of continents at high latitudes is prerequisite to the onsets of ice ages.

Take for example Africa, Australia and South America, the continents which now are located separately from each other. In the Paleozoic Era, these three continents underwent glaciation at the same time. However, in the same time span, there were no similar glacial deposits in North America and Eurasia. Why? According to the theory of plate tectonics, at that time continents were altogether in single landmass.

In particular, Africa, Australia and South America were the Earth's southernmost continents where North America and Eurasia are both located right above them. When the continents broke apart, each new landmass carried the deposits with them and this explains why Africa, Australia and South America have the similar glacial deposits or more generally have similar kinds of geologic structures.

However, the theory of plate tectonics states that slow movement of the continents can lead to long term ice ages. So, this theory cannot account for short term glacial and interglacial cycles. For this reason, we need another theory/model of global climate variations. Astronomical models of ice ages are proposed for this very purpose. Joseph Adhémar laid the foundations of such theories. According to Adhémar, precession of the Earth's axis was responsible for global climate variations. Given this view, minor changes in the rotational axis of the Earth affect the length of the seasons, either by causing long summers or long winters. To a certain extent, if the inequality of the periods increases in favor of long winters for any hemisphere, then earth's climate would change towards much colder temperatures. In other words "glaciation occurs when winters are anomalously long" (Raymo and Huybers 2008, p. 284). In his 1864 paper, James Croll objected to this theory because Adhémar has overlooked an important factor that plays role in climate change. According to Croll, orbital precession surely is a factor, but not the only one. As one of the orbital elements of the Earth, eccentricity is also significant in terms of its influence on glaciation. It is because the Earth's closeness to the sun increases (or decreases) the contrast between the seasons. As a result, an extremely cold winter would impact on the following summer by creating thick fogs or overcast sky which could prevent glaciers to melt. In such circumstance "the direct rays of the sun were prevented from penetrating to the earth, the heat of summer would not in such a case be sufficient to remove the snow and ice; and the formation of glaciers would be the inevitable result" (Croll 1864/2009, p.133).

However, these two astronomical models do not seem to sufficiently explain the glaciation phenomenon. It is because, besides eccentricity and precession, there is one more orbital element that is essential for explaining glaciation: obliquity. Milutin Milankovitch was the one who first recognized and added this orbital element to his model. According to Milankovitch, these three orbital cycles *all together* cause

changes in sunlight falling on the Earth through thousands of years of geological time. Previous models took into account only one or two orbital variations of the Earth and were not able to accurately predict glaciations and thereby ice ages. However, considering all orbital variations of the Earth would successfully work to calculate the amount of incoming solar radiation and hence provide insight into glaciation. Therefore, to explain the onset of glaciation, all that must be done is to calculate the changes of "irradiation of the Earth" by integrating "all astronomical elements" with the model (Milankovitch 1941, p.376). With all these, Milankovitch makes a prediction (precisely a retrodictive inference) about the march of insolation at specific latitudes for the past 600 thousand years. Subsequently, he transforms the obtained results into fictitious latitudinal oscillations to see a geometrical picture of the insolation. The secular variations of insolation for the critical latitude 65°N is shown in the Figure 5.7.

What this graph tells us is that past amounts of incoming solar radiation are different between the latitudes of 60°N and 75°N. For example, it can be seen from the graph that, 230 thousand years ago, 65° North was exposed to summer insolation values nearly equal to 77° North today. A quick glance would show that the summers were relatively cool at the time. Moreover, we also know that the penultimate ice age which is called the Riss spanned the interval between about 300 thousand to 130 thousand years ago. If we go back to the time of Mindel, which is the major ice age period (470 thousand years ago) before Riss, we would also see that 65° North was exposed to summer insolation values nearly equal to 72° North today.

By means of Milankovitch's astronomical model of ice ages, in which three orbital cycles are included, we are now able to shed light on the magnitude of past, present and as well as future glaciations. Nevertheless, at the time when this model was proposed, the existing scientific communities were not easily convinced of Milankovitch's model. As far as I can see, these doubts were centered upon two main concerns. The first one was about whether the retrodictive inferences of the model could be supported by empirical evidence. In other words, Milankovitch's astronomical model of ice ages merely identified the secular march of insolation through a mathematical perspective. But there still remained a portion of suspicion whether the insolation changes have taken place exactly the way Milankovitch puts.

Luckily, these doubts did not last long. As I already indicated in the last parts of **§5.7**, the researches on Alpine moraines, conducted by two geographers Albrecht Penck and Eduard Brückner, provide empirical support for the glacial periods in the distant past. This was supposed to be a real test for both sides. Remarkably, the timeline of the glacial and interglacial periods both presented in two different graphs were in close agreement with one another. Thus similar results "were obtained by quite different methods, one by geological research and the other by astronomical computation" (Milankovitch 1941, p. 416).

The second concern was about the phenomenon that is known since the time of Croll's early works. This phenomenon is called 'albedo effect'. Albedo effect is a mechanism that triggers the glaciation by reflecting the solar radiation back to space. As glaciers spread across the landmass, they cause the Earth to absorb less heat. In due course, the average temperature starts decreasing. This creates a positive feedback loop to promote further spread of glaciers till they reach their maximum level. The albedo effect was deemed to be a significant factor in causing glaciation. But, apparently, Milankovitch assumed that this effect to insolation was non-essential for the explanation and hence suitable for omission from his model. However, the scientific atmosphere of the time was not impressed by Milankovitch's astronomical model and seemingly prompted him to improve its specification (or modification) by including other significant variables (their greatest priority was definitely the albedo effect). Indeed, he was also aware of the criticisms, as puts it in a very concise way:

> Some scientists dubbed this [model and] concerned the question of whether the influence of the variability of the astronomical elements (which could now be calculated uniquely) on the march of insolation was sufficient to fully explain the largest climatic fluctuations of the Quaternary (Milankovitch 1941, p. xv).

Ultimately, Milankovitch has chosen his way of further research. He would embrace certain aspects of criticisms and would modify his core model by adding back *allegedly* an explanatorily relevant causal factor (*i.e.* albedo effect). However, there was an obstacle to that goal because he "did not have any reliable numerical data on what fraction of the solar heat was reflected into universal space from a surface element covered by snow or ice" (Milankovitch 1941, p. xvi). Soon afterward,

fortunately, Joseph Devaux (1902–1936) provided numerical aid for effective estimation of the diffuse reflection of solar radiation. The results about the reflective power of ice sheets, which he obtained in his time in the Alps and Greenland, were sufficient for Milankovitch's calculations. Accordingly, Milankovitch published a new volume containing a paper entitled *Neue Ergebnisse* (New Results) which takes "into consideration the varying reflective power of the Earth" (Milankovitch 1941, p. xvi). In this paper, Milankovitch introduces a new graph of the global climate variations in the Quaternary by taking the new results in to consideration (see Figure 7.1).

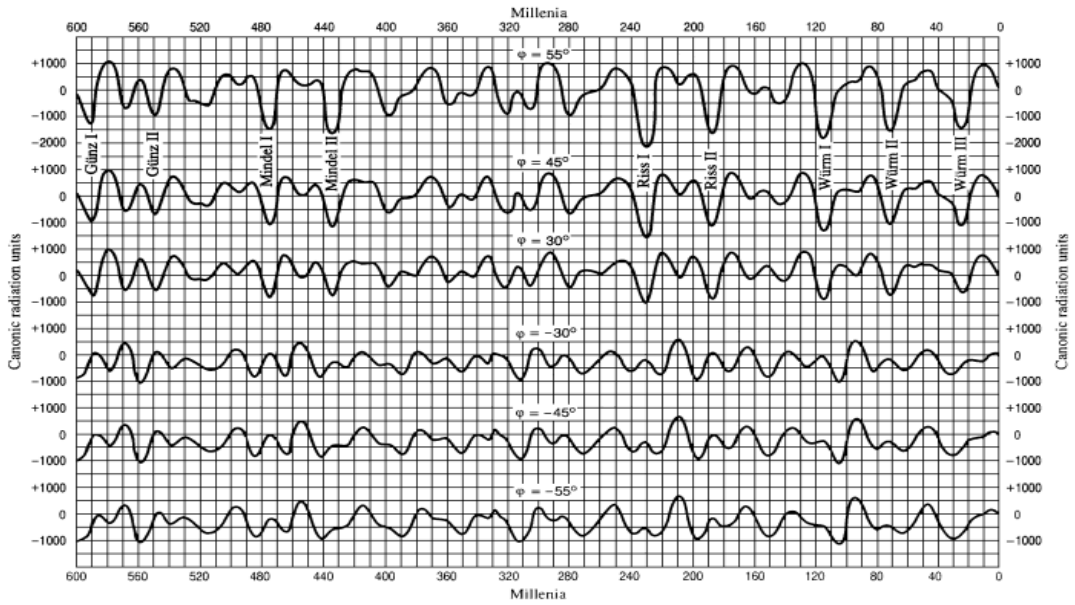

Figure 7.1 Earth's march of insolation taking into account the reflective power of the ice caps. Reprinted from Kanon der Erdbestrahlung und seine Anwendung auf das Eiszeitenproblem (p. 473) by Milankovitch, M., 1941, Belgrade: Royal Serbian Academy Special Publications.

Not surprisingly for us, "[t]he form of curves given in this volume did not differ greatly from the radiation curves published earlier" (Imbrie and Imbrie 1979, p. 108). In other words, the results of Milankovitch's new model (which modified in light of new data) was not fundamentally different from the results of its more basic counterpart. What was different in this new graph was that "the jagged lines of his earlier graphs took on the form of smoothly changing curves" (Imbrie and Imbrie 1979, p. 109).

Consequently, the second concern about Milankovitch's astronomical model of ice ages is adequately addressed and vanished, as it were. Through de-idealization, an initially idealized causal factor is added back to the model to show why its basic explanation is immune to that particular causal factor. In a related manner, the scientific community is convinced that the albedo effect can suitably be idealized, at least when considering the glacial and interglacial periods of the ice ages. This part of the story might perhaps be interesting for those who research on sociological and historical themes in science. But I would like to emphasize a somewhat different point. To my understanding, Milankovitch was inclined to feel that his work was incomplete despite having a successful mathematical model which is also supported by empirical research evidence. Otherwise, there seems no reasonable ground for de-idealizing his model. Actually, many geologists and recognized scholars of Milankovitch accept that de–idealizing the albedo effect was the last 'objective' of him (see Imbrie and Imbrie 1979; Grubic 2006). The remaining part of the chapter is devoted to show how fruitful such an objective can be.

7.7 A New Function for De-Idealization

Before proceeding further, let me start by emphasizing how two major epistemic functions of de-idealization manifest themselves in the above example. First of all, as we de-idealize the model by adding albedo effect back to it, we come to understand that there is no significant difference between the outcomes of model's more and less idealized versions. Conversely, we find a close agreement between the two graphs of past climate fluctuations, as one created by the initial and the other created by representatively more accurate (or de–idealized) model. Thus, de-idealization shows that Milankovitch's model has sorted out explanatorily relevant ones among all causal factors. And this finally confirms that Milankovitch has constructed a 'core model'. As I indicated, the epistemic contribution of de-idealization is not limited to provide understanding about the model itself; it also provides epistemic benefits regarding the idealized causal factors, in particular the degree of their explanatory relevance. So, by this reverse modeling strategy, we can also measure to what degree the albedo effect is less relevant for explaining past glacial and interglacial periods.

Actually, I have discussed about these two major epistemic functions at great length in this chapter, so it might not be interesting or insightful to repeat what I have already said on how de–idealization functions epistemically. What might be interesting, however, would be to show that there is a lot more to this example than meets the eye. By de-idealizing the model, Milankovitch provided us information about the albedo effect itself that was specifically not available to us before. In other words, by de-idealization we have been able to see that Milankovitch's model is essentially core and the idealizations included in it have less degree of explanatory relevance; but in addition to these two, we have also managed to learn about the nature of the idealized causal factor. What new information, indeed, we have learned from de-idealization? Milankovitch simply answers this question as follows:

> Results… in *Neue Ergebnisse* (New Results) indicated that the march of terrestrial insolation in remote time, calculated by taking into consideration the varying reflective power of the Earth, was quite sufficient to fully explain even the largest changes in the Quaternary. The cooling action of the temporarily enlarged ice caps of the Earth, *became a secondary effect of the secular march of terrestrial insolation, since it was produced by this march*. Therefore, the new radiation curves, determined by taking the effect into consideration have the same pattern as those computed earlier[…] My calendar of the Glacial Period is still valid (Milankovitch 1941, p. xvi) [emphasis added].

At first glance, Milankovitch's conclusion about the cooling effect of albedo might not seem so surprising. After all, when insolation is high enough, ice caps would not give way to such a strong global cooling effect or vice versa. But, in order to arrive at such a conclusion, one needs to de-idealize the model at the outset. In other words, mere hypothesis has morphed into a verified scientific truth only after de-idealization. Before constructing the insolation model, the albedo effect was most probably deemed by Milankovitch and also by others as an independent factor that has a huge influence on glaciations. However, the results of the model showed that this factor is dependent upon the incoming solar radiation. Having obtained the new results of the de-idealized model, it became apparent that the reflective power of the snow is produced by the insolation itself. It is because "geologists …had a graph from which they could obtain an estimate of the latitude of the ice-sheet margin for any time in the past 650,000 years" only after the de-idealization (Imbrie and Imbrie 1979, pp. 108-109). By making the core model more realistic (hence less idealized),

it became possible to introduce a new graph that depicts past climate variations. This new graph does not differ significantly from the previous one. Nevertheless, it provides scientists with an opportunity to make comparison possible, by interpreting particular phases of the climate changes that depicted by each model. In the new graph "[t]he cooling action of the polar caps" was added to model and it is observed that "the new radiation curves... show the same rhythm as the earlier ones" because the ice cap "enlargement itself was caused by this (insolation) march" (Milankovitch 1941, p. 477). Therefore, scientists were able to learn that the albedo is a byproduct (or secondary effect) of insolation mechanism. If they were not provided with the less idealized version of the core model (note that this is achieved by de-idealization), they would have nothing to compare the core model with. Accordingly, we would miss an opportunity to learn that albedo is produced by the insolation itself.

I believe some remarks may be useful to make my point clearer. First and foremost, I find it useful to draw a sort of distinction between two different statements here. As I wrote above, Milankovitch's de-idealization of the ice age model led him to infer that albedo is explanatorily less relevant in explaining ice ages because it has weak influence on triggering the glacial and interglacial periods. But, with regard to the explanation of ice ages, it is one thing to say that *albedo is a less relevant causal factor*; and it is quite another to say that *albedo is a less relevant causal factor due to insolation*. Besides indicating that idealizing albedo is harmless (or it is one of the non-difference-makers), the latter statement contains the additional information about the fact that albedo is driven by insolation. Thus, the information that we get through de-idealization includes but not limited to the knowledge about the model (about whether it offers a basis for less idealized model) and to the knowledge about its contained idealizations' degree of explanatory relevance. In addition to these, we also disclose new information about a particular idealized causal factor which was previously unavailable to us. Thus, the astronomical model of ice ages simply illustrates how de-idealization sometimes functions as a modeling strategy to provide new knowledge about the idealized part of a model.

The last crucial point I want to stress is that this minor epistemic function of de-idealization does not improve a model's explanatory power. Strictly speaking, I am

inclined to the view that particular causal factors in the model *indeed* do the explanatory work. So, as I tried to show through my example, there is no explanatory advantage between the more and less idealized version of the ice age models because both includes particular causal factors (all three orbital cycles) that are sufficiently relevant for the explanandum. Thus, comparing the new outcomes of a more realistic model of ice ages with its less idealized counterpart would play no positive role in the explanation itself. It is because the information we get from de-idealization does not belong to the explanatory part of the model. So, in the case study given, Milankovitch did not introduce a new parameter alongside the parameters of orbital cycles that produce insolation by which we determine the timeline of the glacial and interglacial periods. Rather, he provided an account of insolation by establishing that albedo is dependent upon the incoming solar radiation and also by rejecting the hypothesis that albedo is an independent factor that has a huge influence on glaciations.

In a recent paper, Julie Jebeile and Ashley Graham Kennedy (2015) defend that "in some cases… comparison between an idealized and a de-idealized model is pivotal in generating a successful model explanation" (p.390). The authors describe a case study of cosmological hydro–dynamical simulations, showing that the comparison between idealized and less idealized models of galaxy simulations allows scientists to confirm that equilibrium status is substantial in galactic temperature changes. Accordingly, scientists identify "the explanatory components within a model" only by means of such a comparison (Jebeile and Kennedy 2015, p. 383). I need to make clear that this view differs from the one that I have put forward, namely with regard to the information that derived from the comparison of the outcomes of the idealized model and its more realistic version. According to Jebeile and Kennedy, one could sometimes provide successful model explanations by the activity of comparing the idealized model of simulation with its de-idealized version. In my example, however, there is already a successful explanation (which is provided by the more idealized model *i.e.* the core model) of why glacial and interglacial periods happen. So, we can hardly get any explanatory benefit from de–idealizing the core model. In fact, Milankovitch had both persuaded himself and his scientific community to a greater extent, though he achieved it by showing that there is no significant difference in

explanation between his core model and its less idealized version. On the other hand, de-idealization came with epistemic benefits, namely it did provide us with a proper understanding of the model and its contained idealizations.

7.8 Conclusion

In this chapter, I have aimed to provide a plausible framework for de-idealization strategy and some reasons why deliberately idealized causal factors sometimes need to be corrected. By outlining hypotheses suggested by the relevant literature, I showed why this reverse modeling strategy is not futile. Subsequently, I have described five roles of de–idealization and proposed that they essentially fulfill two major epistemic functions. As I emphasized, the first one of these major epistemic functions is used to determine whether a model is a core model or not. The second function is also about the model, but specifically it is used to identify the degree of explanatory relevance of the model's constituent idealizations. Nevertheless, I argued that there is one more epistemic function of de–idealization. This epistemic function serves a purpose to provide new information about an idealized causal factor.

Why a modeler de-idealizes a model? Does she, for example, wish to learn whether that model is a core model? Or, does she aim at identifying the degree of explanatory relevance of that model's particular idealizations? To my mind, this frequently is not the case. Scientific practice usually shows us that modelers de-idealize models with diverse motivations. Just as it is in the ideal gas example that I used, the strategy of de-idealization is used for describing the behavior of the gases at high pressures –so, it is not used to learn whether the ideal gas model is core model or not. For this reason, the idealizations of the ideal gas model are corrected by adding new parameters to the model. These corrections did not invalidate the ideal gas model itself; rather it showed that it is a core model which further can be a basis for more complex models. The success of the *van der Waals* gas model does not depend on its originality; conversely its success rests more on its improvement of the ideal gas model. Thus, the less idealized model both owes its origin and success to its core counterpart. This is one of the crucial benefits that de-idealization provides.

That being all said, one more thing must be made clear as well. An assumption that runs through the entire chapter is that idealizations are eliminable and easily

reversible. However, according to some philosophers of science, certain idealizations are "explanatorily ineliminable" (Batterman 2009, p. 427), "uneliminated" (Odenbaugh 2011, p. 1177) and "irreversible processes" (Knuuttila and Morgan 2019). If we take granted that these views hold, my approach to the epistemic values of de–idealization will not work. But, it is quite obvious that not all idealized causal factors are uneliminated or explanatory ineliminable. In cases where one cannot add back particular causal factors to the model, one no doubt has a way of speaking of de-idealization and thus no way of learning any more about it. But if it is possible to de-idealize the model, there is a way to understand about whether it is a core model, or about its idealizations' degree of explanatory relevance, or sometimes about the nature of the idealized causal factor itself. By all means, limiting the epistemic functions of de–idealization with these three would be a hasty move. Science hitherto has led to a myriad of models in many domains. Considering this fact, at least, I can naturally suggest that there should be different epistemic functions of de-idealization waiting to be discovered.

CHAPTER EIGHT

IDEALIZATION AND ABSTRACTION

8.1 Introduction

There is now a substantial body of literature on scientific modeling and representation. Though varied in subject matter, what this literature shares in common is the emphasis on the distinction between idealization and abstraction (Wallis 1994, p.408; Earman 2004, p. 191; Pincock 2005, p. 1253; Portides 2007; Bailer-Jones 2009, pp. 146-150; Knox 2016, pp.51-52; Tee 2020; Nguyen 2020, Ch. 4, Sec. 3). In this chapter, I provide a detailed overview of the approaches that directly attempt to distinguish idealization and abstraction. This, in turn, will allow me to highlight the strengths and weaknesses of some existing views. My aim is not to present a sufficient criterion to distinguish idealization from abstraction. Instead, I argue that figuring out the reasons behind why modelers make idealization and abstraction provides a more constructive approach to separate the latter from the former.

Possibly, a question may arise to whether it is noteworthy distinguishing idealization from abstraction. In fact, the contours of the answer to this question have emerged in the preceding two chapters. As I claimed in Chapter 6, Partial Isomorphism better captures the representational status of idealized and abstract models than its alternatives. Actually, this is the fine line that separates the partial isomorphism view from its alternatives, especially from the isomorphism view. What partial isomorphism emphasizes is that these two practices, which are commonplace in science, cannot be neglected. For one reason: if these strategies used in science are not taken into account, the nature of scientific models cannot be properly explained from the philosophical point of view. Essentially, Chapter 7 is more relevant with regard to the issue of the relationship between idealization and abstraction. In general, I have tried to reveal the functions of de-idealization in that chapter and thus, by doing so, I have inevitably referred in some places to idealization. Nevertheless, the issue of abstraction also deserves of its own discussion. All scientific models –without exception– are more or less abstract. This is a true claim but comes with a cost. For example, one may ask whether concretizing abstractions

of a model contributes to the epistemic status of that model. To ask it differently, does an abstract model become more explanatory or predictive, if it becomes more concrete? Or, conversely, is it epistemically redundant to make an abstract model more concrete? In this chapter, I address these questions. By doing so, I assess some of the views put forward in the literature from a critical standpoint. In conclusion, I suggest that further research on idealizations will offer significant insights to distinguish them from abstraction.

8.2 Distinguishing Idealization from Abstraction

The distinction between idealization and abstraction was firstly and quite a bit systematically investigated by Nancy Cartwright (1994). Her work on the subject has played a pioneering role at least in the debate over how to distinguish these two concepts. Nevertheless, her views received considerable criticism which can be divided roughly into two types. One type of criticism suggests that abstraction can be useful if an idealization is present. The other type of criticism admits the reverse by claiming that idealization would be useless if abstraction were not possible.

In a chapter of her book, *Nature's Capacities and their Measurement* (1994) Cartwright offers a distinction between idealization and abstraction. According to Cartwright, it is crucial to make distinction between them because the former is "about the connection between models and applications in contemporary science" while the latter is "about connecting models with reality" (Cartwright 1994, pp. 186–187).

According to Cartwright, when we idealize "we start with a concrete object and we mentally rearrange some of its inconvenient features –some of its specific properties– before we try to write down a law for it" (Cartwright 1994, p. 187). Consider a simple model of two–body system, like the Sun and the Earth. To calculate the gravitational attraction between these two bodies, all we need to know is the mass of each body and the distance between them. Before doing the calculation, however, we treat each body as perfect spheres (or point masses). There is a certain advantage of specifying them in this way. It allows us to express the masses of the bodies, as if they were concentrated at their center locations. This subsequently eases the calculation of the gravitational attraction, in which we can apply Newton's law of

gravitation. Nevertheless, in actuality, neither the Sun nor the Earth is perfectly spherical: there is a certain level of irregularity present on their surfaces. According to Cartwright, this case strikingly illustrates the process of idealization. By means of idealization, we have changed some properties of the astronomical bodies in our thought-process and found opportunity to apply a simple law about gravitation. In other words, idealization enables the calculation of the gravitational effect by changing actual bodies which are imperfect in shape with perfect spheres.

On the other hand, when we abstract a property of a system, we follow a different route than used in idealization. According to Cartwright, the issue here is not "a matter of *changing* any particular features or properties, but rather of *subtracting*, not only the concrete circumstances but even the material in which the cause is embedded and all that follows from that" (Cartwright 1994, p. 187). At first glance, we might interpret this claim as adoption of an Aristotelian position. As a matter of fact, Cartwright underlines throughout her book that she sticks strictly to Aristotelian approach with respect to the issue of abstraction. Let me present Cartwright's example, to make a better understanding of her approach to abstraction. The following example is not given in its original form, but it follows Cartwright's wording. Suppose you draw a triangle on a white paper using a black pencil or pen. When we take a look at the paper, we would easily see a black shape drawn on a white paper. Moreover, we would easily realize that the shape of drawn geometric object has a property of being triangle. According to Cartwright, if we wish to employ an abstraction to that geometric object, we need to focus its "both accidental and essential" properties (Cartwright 1994, p. 213, see also Cartwright and Mendell 1984 and 2005, pp. 43-46). So, initially, we need to identify incidental properties, like the material of the paper and black ink, in our mind and then strip them away completely. After completing this process, we can treat the remainder –the triangle itself– as the substance itself. From one end to other, the whole process could be defined as abstraction.

As I pointed above, by distinguishing idealization from abstraction, Cartwright aims to argue that these two processes are about different modeling strategies. In idealization, we change actual parameters with the non-actual ones. By this way, we may apply a scientific law to a counterfactual situation. Yet, drawing actual

conclusions from the counterfactual situations is no big mystery. Because "the situation may be counterfactual; still it is realistic in one sense" and in this manner "actual effect can be calculated" (Cartwright 1994, pp. 191-192). In our example about the gravitational model of attraction, the Earth with its surface irregularities is replaced with the perfectly spherical Earth-like planet; but beside such an unrealistic situation, the model still truly describes some relevant factors –like the mass of each body and their distance between them. This correctly described part of the model is sufficient to calculate the intended effect –or to put it differently, it is sufficient to apply the idealized model to real world situations. On the other hand, the starting point of abstraction is the actual world itself because all properties that are suitable for abstraction are entirely within the realm of the actual world –that is why Cartwright calls abstraction as 'material abstraction' because the process of abstraction starts from the physical material itself (see Cartwright 1994, pp. 206–212). In other words, black ink, the paper on which it is drawn a triangle, and the triangle as geometric object itself all together exists in the actual world. Contrary to idealization, abstracted features are the features that have a correspondence to real world objects. For this reason, we don't replace actual properties with the fictional ones –like we do in idealization. In the process of abstraction, all that we are doing is subtracting away some particular real world properties. It then becomes unproblematic, as soon as we assume that abstraction is a process of subtracting away the real world properties because unlike idealization, we don't replace (or mainly distort) the properties of the actual world.

Cartwright's distinction between idealization and (Aristotelian) abstraction seems to hold intuitive appeal, but it has led to two *prima facie* extreme positions. On the one extreme, we find the view that abstraction essentially cannot be distinguished from idealization; in fact, it is useless without idealization. On the other extreme, however, we are left with the view that all idealizations presuppose abstraction.

The former view is defended by Paul Humphreys (1995) on the ground that Aristotelian abstraction could only be useful if an idealization is involved. According to Humphreys, the process of subtraction is nested within Cartwright's understanding of Aristotelian abstraction. By means of subtraction, we start from the reality and then eliminate certain pieces of that reality one by one. As exemplified in the triangle

case, we first take away the actual white paper, then remove the black ink and finally reach a real triangle. But is the triangle we obtain is a real triangle? Humphreys doesn't think so because no matter how carefully you draw, it would never be close to a perfect triangle in shape. Thus, the indication that we cannot obtain the real triangle will show that the process of subtraction (the merit of Cartwright's Aristotelian abstraction) would always end up with imperfect triangles.

In fact, Cartwright is also aware of this shortcoming. After all, she says "even when … all the other incidental features are subtracted, the shape that is left is never a real triangle. But let us pretend that it is" (Cartwright 1994, p. 197)... According to Humphreys, such a pretense would likely be contrary to Cartwright's argument, because in this case we would have to change imperfect triangle with the perfect one, just as we do in idealization. However, Cartwright's entire endeavor, as she puts it, was to state that abstraction does not involve such a process of replacement or distortion. If we change the property of the world in our thought-process, we would rely on idealization, not abstraction. But the example of triangle in fact shows the opposite, revealing that abstraction involves distortion. To be clear, what follows from this is that the distinction between idealization and abstraction disappears. As the example shows, there would be no difference "between the process of idealizing an imperfect real triangle and idealizing an imperfect real sphere" (Humphreys 1995, p. 158).

It seems that Humphreys successfully argue against Cartwright's Aristotelian abstraction as well as its differences from idealization. In his view, at least, Cartwrightian distinction between idealization and abstraction loses its validity. Nevertheless, he seeks to draw important conclusions from this denial. Especially, "in […] cases where we have a mathematical representation of a real system" he says, "the two –idealization and abstraction– cannot be kept apart" (Humphreys 1995, p. 159). Given this claim, we may subtract away to what extent we may wish to, but in order to represent a system in a mathematical fashion, we should employ idealization. In other words, (Aristotelian) abstractions would be useless if idealizations were not already possible. In our triangle example above, we might subtract away the incidental properties to any extent; but the process of abstraction would never end up with a perfect triangle. However, when we work with triangles,

circles, rectangles or any other geometrical object drawn on a paper, we treat them as if they were perfect in shape. By all means, it is for the sake of the mathematical operations. According to Humphreys, this is just a plain idealization. It is because we can obtain the perfect triangle in exactly the same way we did in obtaining perfectly spherical planet. In other words, we are able to carry out mathematical operations just by replacing (in Cartwright's terms idealizing away) any imperfect triangle with the perfect triangle.

Steffen Ducheyne (2007) provides a different approach to idealization and abstraction. With regard to these two concepts, Ducheyne's definitions are as follows:

> (abstraction)^{df} "We abstract from property $\mathbb{P}$ of a physical system…, iff in our corresponding scientific model property $\mathbb{P}$ is not included…"
>
> (idealization)^{df} "We idealize a property $\mathbb{P}$ of a physical system…, iff in our corresponding scientific model property $\mathbb{P}$ is not included and furthermore, is replaced by a different property $\mathbb{Q}$…" (Ducheyne 2007, pp. 9-10).

These definitions are based on Cartwright's distinction between idealization and abstraction. In contrast to Humphreys' view; he asserts that idealizations would be useless if abstraction were not possible. In fact, Ducheyne does not use the word 'useless', however he claims that "idealization always presupposes abstraction" (Ducheyne 2007, p.10).[51]

According to Ducheyne, his conceptual analysis of abstraction and idealization suggests that idealization always presupposes abstraction. Nevertheless, "the converse doesn't hold" (Ducheyne 2007, p.10). It is because subtracting away a property is possible without replacing it with another property, but without subtracting away a particular property of a system, we cannot change or replace it with any other property. Therefore, idealization is a "weaker notion than abstraction" (Ducheyne 2007, p.10). He maintains that pairing subtraction with abstraction and pairing distortion with idealization is also misleading. After all, the activity of

[51] Here, to my mind, the concept of 'presupposing' means simply presupposing the existence of something. If taken as such, to say that idealization presupposes abstraction is also to say that idealizations are useless without abstraction. In other words, if the existence of idealization is entailed by the existence of abstraction *per se*, then the usefulness of idealization would be due to the existence of abstraction (for another view claiming that the concept of '*presupposing'* entails the concept '*usefulness'* see Ladyman 2014, p. 408).

subtraction is both related to the processes of idealization and abstraction. Consider again our simple examples above. In idealization, we replace an imperfect triangle ($\mathbb{P}$) with the perfect one ($\mathbb{Q}$) or replace an irregularly shaped planet ($\mathbb{P}$) with the perfectly spherical one ($\mathbb{Q}$). In order to achieve this, we first need to subtract away the relevant property, change it with its idealized version and eventually add it back to the model. When abstraction is the case, it would be sufficient only to subtract the relevant property from our system and apply the model in this way. In other words, no thought activities such as changing or distorting are involved.

So far, no settlement has been reached with regard to these two views. On the one extreme, it is held that abstractions would be useless if idealizations were not already possible; while on the other extreme, it is claimed that idealization always presupposes abstraction. Apparently, these are not extreme views themselves. Rather, these views can only be regarded as extremes within the framework that is drawn up by Cartwright. Some possible approaches, outside the Cartwrightian framework, perhaps can account adequately for the distinction between idealization and abstraction. In the following section, I will address such possible approaches.

8.3 Distinguishing Idealization from Abstraction, *Again*

As stated in the previous section, identifying the activity of changing a property with idealization and of subtracting with abstraction, do not provide a fruitful ground for a clear distinction. For this reason, some philosophers of science have attempted in-depth analysis of these two concepts. In this section, I evaluate such attempts in a descriptive as well as a critical way.

Martin R. Thomson-Jones' (2005) approach to idealization and abstraction might be counted as a first attempt to overcome some difficulties of Cartwright's distinction. Based on this approach, he proposes that "the term 'idealization' applies, first and foremost, to specific respects in which a given representation misrepresents, whereas the term 'abstraction' applies to mere omissions" (Thomson-Jones 2005, p. 174). Considering misrepresentation as a hallmark of idealization resembles with the claims of Cartwright, indeed. Broadly speaking, when we change a particular property of a system and represent it in such a way, we simply make a misrepresentation of that system. When it comes to abstraction, however, Thomson-

Jones differs with Cartwright. That is because, Thomson-Jones understands from abstraction merely the omission of a property –more specifically, for him, abstraction is an omission without misrepresentation.

According to Thomson-Jones, we make "assertion of falsehood" in any proposed representation that includes idealization (Thomson-Jones 2005, p. 175). In other words, idealized representations can both, or either, contain statements about actual properties as if they are not present and statements about non-actual properties as if they are present. As Thomson-Jones symbolically asserts:

> If the model represents a system as having the properties ϕ_1, ϕ_2,…, ϕ_n,…, and lacking the properties ψ_1, ψ_2,…, ψ_n,…, then a given aspect of the picture of the system presented by the model is an idealization only when that aspect of the picture represents the system as having some ϕ which it does not in fact have, and/or as lacking some ψ which in fact it has (Thomson-Jones 2005, p. 183).

Unlike idealization, the abstraction process does not involve introducing distortion into the properties of a system. Yet, by means of abstraction, we truly but partially represent a particular property. By not mentioning some parts of the system, the representation would not distort a particular property but represent it in an incomplete manner. In this sense, abstraction is "a matter of complete silence" (Thomson-Jones 2005, p. 175).

According to Thomson-Jones, once the distinction between idealization and abstraction has been drawn, a very clear conclusion follows: These two concepts are mutually exclusive. Accordingly, "with respect to a particular feature of a certain real system, a given representation can contain an idealization, or an abstraction, or neither, but it cannot contain both" (Thomson–Jones 2005, pp. 175-176). At first glance, this claim might seem confusing. After all, most scientific representations can contain both idealization and abstraction. However, Thomson-Jones does not mention the whole representation here. Instead, he talks about a single property of a particular target system. To say more specifically, a model can represent a target system by idealizing and abstracting away certain properties; but if a certain property is represented in an idealized way, that particular representation can no more contain an abstraction or vice versa. I will discuss several problematic aspects of this claim

subsequently, but to make clearer what Thomson-Jones meant by this claim, it is worth reflecting on the example he gives.

Thomson–Jones asks us to consider a real cannonball fire. In order to explain or predict where the cannonball is going to land, we can use a simple projectile motion model. We first generate a two–dimensional graph, in which we determine the horizontal (x) axis and vertical (y) axis using the Cartesian coordinate system. After it is fired from the cannon, the cannonball will move along the x–axis till it reaches to its maximum distance. It will also reach its peak point and then decline gradually all along the y-axis. While building the model, we should take into account two things: gravitational acceleration (9,820 m/s^2) and the mass of the cannonball (*m*). This is because taking these two into account will allow us to calculate the force in the y–direction (note that, the force is zero for the x–direction). Applying Newton's second law of motion and by using simple integration techniques, it would be sufficient to compute the trajectory of the cannonball.

As a representation of projectile motion, the model contains both idealizations and abstractions (Thomson–Jones 2005, pp. 182-184). For example, the model assumes that the only force acting on cannonball is the gravitational force. However, the crews who fire the cannonball or perhaps each member of the curious audience are gravitationally attracted to the cannonball. Through its parabolic journey, the cannonball is also exposed to the air resistance at various points in the x–y axis. Moreover, the cannonball is not fired at a zero degree with respect to the x-axis, because the mouth of cannon is high above the ground level. According to Thomson–Jones, these actual factors are excluded from the model of projectile motion by means of idealization –and thus they are misrepresented. It is not so important which of these factors are more or less relevant in the explanation. For instance, taking the air resistance into account can lead to a better prediction of the cannonball's motion than considering gravitational attraction of the moon. But, again, this is not so important. Whatever degree of difference it makes to the explanation or prediction, all these idealized factors are falsely described in the model.

On the other hand, abstractions do not introduce any falsehoods into the representation. In abstraction, we merely avoid describing all the properties of the target system. For instance, the model is silent about the "color or temperature" of the cannonball, the "composition of the ground over which" it flies, or "of the country in which the firing takes place" (Thomson–Jones 2005, p. 184). However, all these properties are true of a given actual projectile motions. But despite they are true of a given actual system; we neither scientifically nor in principle can model such properties. Therefore, in abstraction, we do something different as we do in idealization *i.e.* we do not misrepresent but merely omit particular factors by being completely silent about them.

Peter Godfrey-Smith (2009) takes a closer approach to Thomson–Jones' view. While distinguishing these two concepts, he relies on a familiar distinction from ordinary life: *partial truth* and *lie*. For some, when a person tells the partial truth, he doesn't lie but rather expresses the truth incompletely. On the other hand, when someone distorts the truth, we might say that he lies. According to Godfrey-Smith, the former is similar to abstraction, while the latter is analogous to idealization. Given this view, therefore, "leaving things out, while still giving a literally true description" is an act of abstraction and "treating things as having features they clearly do not have" is an act of idealization (Godfrey-Smith 2009, p. 47). Yet another nearly identical idea of Godfrey-Smith, with respect to Thomson-Jones, is that when one attempts to describe something, he can make abstraction, idealization, or make both of them. In other words, a single property of the target system could be abstracted away, while another property of the same target system could be idealized away. However "it is not really possible for a single description to be both idealized and abstract at once, with respect to the same features of the object being described" (Godfrey-Smith 2009, p. 50).

Godfrey-Smith's example comes from the population genetics, specifically one locus model of natural selection. He states that in random mating "the description of the population in terms of gene frequencies at one locus is an abstraction … But the model makes an assumption of infinite population size; that is an idealization" (Godfrey-Smith 2009, p. 50). There are of course other idealizations of the model which need not be counted here, such as the population is not disrupted by mutation.

Scaling the population to infinite and assuming that there is no mutation in that population are each distinct idealizations. To put differently, these are not abstractions because they individually distort the truth about real populations and their actual dynamics. Therefore, this distinction shows that the abstract description of a system is tied to the truth, whereas "an idealized description of a system is a description that *fictionalizes in the service of simplification*" (Godfrey-Smith 2009, p. 48). When we give an abstract description of a system, our intention is not to introduce falsehoods. Instead, the intention here is only to focus on difference makers that bear the model's explanatory relations. However, when we give an idealized description of a system, our aim is to make false assumptions or invent purposeful fictions. These descriptions need not to be tied to the truth.

8.4 Idealization and Abstraction: Mutually Exclusive or Not?

Let me summarize, briefly, the points where Thomson–Jones and Godfrey–Smith agree upon.[52] To begin with, we may note that both views hold that omitting particular details from the truth is a key to abstraction. To put it more precisely, abstraction is a mental operation, in which we neglect a great deal of parts of the actual target system and also truly describe the remaining part of that system. We neither represent, nor misrepresent the parts that we neglected. Thomson-Jones and Godfrey-Smith also hold that, when we idealize, we fall away from the truth. The idealized property of a system actually exists, but we do not omit it. Rather, we misrepresent that single actual property in a different way. Contrary to the properties that are abstracted away, we cannot maintain our silence about the idealized properties. These properties should be included in the model in its idealized form. Additionally, their proposed distinction between idealization and abstraction leads to a significant conclusion: while describing a property of a system, we can either make abstraction or idealization, but never both.

The distinction proposed by Thomson-Jones and Godfrey-Smith, accordingly, shares two claims:

[52] An earlier and condensed version of this section can be found in Ateş (2021).

a) when we make abstraction, we are in complete silence about the properties that we abstracted away from the system, while in idealization we distort particular properties that we idealized away from the system;

b) while representing a certain target system, we either make abstraction or idealization of its single property, so that these two thought processes cannot be applied to the same property.

One further point that came out of these two is that the claim **a** leads to the claim **b**. To put it more specifically, if we omit a single actual property without misrepresenting it, we are simply making an abstraction; however, we are making idealization if we are not omitting a single actual property, but including it in our representation in a distorted way. According to Thomson–Jones and Godfrey–Smith, such different characteristics of idealization and abstraction make them mutually exclusive.

As it is seen, both authors claim that the hallmark of abstraction is omission. If one makes an abstraction, he does not speak of particular properties. According to my interpretation, this claim should be understood in this way: abstraction has no significant role specific to the scientific context. Here, I am not claiming that abstraction is not significant with respect to scientific modeling and representation. Needless to say, for many reasons, the explanatorily irrelevant parts should be abstracted away from the system in order to model or represent that specific system. However, I argue that taking abstraction in this way shows that there is no significant distinction between scientific abstraction and ordinary abstraction. Consider again the example of Thomson–Jones' projectile motion model. Given this model, we are in complete silence about some features of the actual system –as Thomson-Jones illustrates, for instance, we omit (by means of abstraction) the country in which cannonball fire takes place and the color of the cannonball or its overall composition. This process is similar to that used by people in everyday life for every communicational situation. Assume that I bought a brand new car. While sharing this happy news with my friends, I omit to say that my new car has four tires, two in the front and the two at the rear. In other words, when I talk about my new car, I would not mention its tires. In the same manner, when we model the course of the cannonball's trajectory, we know that the cannonball has a color or is fired in a

country. However, we neglect such properties by being silent about them. Therefore, this view shows that there is nothing special about scientific abstraction because it is as same as the act of common or ordinary abstraction.

Holding such a view about abstraction, I think, is not problematic. After all, it shows that abstraction is essential to ordinary communication as much as it is essential to scientific activity. What seems problematic to me is the claim about the distinctive characteristic of abstraction. According to Thomson-Jones and Godfrey-Smith, abstraction involves an omission without misrepresentation. When we abstract away, we are in complete silence with respect to the properties of the target system. I think, however, some idealizations share this similar characteristic of abstraction.

First and foremost, I would like to explain what I understand from the proposed characteristics of abstraction. As I mentioned before, abstraction is a matter of complete silence, such that it is neither representation, nor misrepresentation of the particular property of a system. In case of scientific representation, abstracted parts of the actual system are not included in the models. Subsequently, we maintain our silence about so many properties (color of the cannonball, the country where firing takes place and so on) and apply the abstract model. The same process even goes for some idealizations. For valid reasons, we neglect a particular physical parameter by excluding it from the models (*e.g.* assuming zero air resistance in the path of the cannonball) and then apply it to a certain actual situation. In terms of being silent about properties, I hardly see any difference between these two scientific activities. Neglecting the color of the cannonball or neglecting the air resistance both amount to the same thing with respect to our model. Therefore, some idealizations also neglect to mention about particular properties as abstractions do. This means that being silent about a system's particular property is not unique to abstraction –so to speak, it does not seem a distinctive feature of abstraction as it were.

In standard textbooks of science, authors usually list their assumptions or highlight what they have excluded from the model at the outset. There are various assumptions for instance, from zero air resistance to frictionless planes or perfectly rational agents to zero volume gas molecules, just to mention a few. However, almost no textbook can cover (or even attempt to cover) the properties that are abstracted away from the

model. It is nearly impossible to find descriptions about the concerned model, like 'the country in which the cannonball fire takes place is omitted' or 'the material which the cannonball is made of is neglected'. This is not a coincidence, nor is it some kind of arbitrary practice. The reason, however, is simple. Idealizations can be put in an order because they are intentionally determined and set in the beginning. Nevertheless, the properties that are abstracted away from the model could be numerous. When it comes to employing the model, however, abstraction and idealization sometimes function indistinguishably from one another. So, contrary to the claim **a**, we sometimes neglect to mention about particular properties in idealization. If my claim is not convincing then perhaps we can put it differently as follows: if abstractions are a matter of complete silence, then (some) idealizations are, at best, processes that allow this silence to be heard.

For more clarification, I will make use of a distinction proposed by Frederick Suppe (1989). According to Suppe "physical parameters are nothing other than *kinds of attributes* which physical particulars may possess, and physical quantities are attributes of the requisite kinds which the particulars may possess" (Suppe 1989, p. 93). Given this distinction, physical parameters refer to the properties of a target system, while physical quantities are units which allow us to quantify the physical parameters. For example, a mole of gas has a volume of 24,000 cm^3 at room temperature. The volume is a physical parameter of one mole of gas; however, its expression in units is a physical quantity of that measurable physical parameter (for a similar interpretation, see Jones 2008, and also Podlaskowski and Jones 2012, pp.170-171). Yet, some parameters cannot be determined in a numerical sense. Although, the color (blue, black, grey etc.) is a physical property of the cannonball, it is not a measurable property of the system with respect to the properties that affect projectile motion (like mass or gravitational force). For this reason, specifying physical parameters in the model, especially in the mathematical model, is possible when that parameter is a quantifiable variable whose value contributes information to the state of a phenomenon of interest. I think what Thomson-Jones and Godfrey-Smith understand from idealization and abstraction becomes clearer by relying on Suppe's distinction. In this sense, abstraction is the omission of the parameters that do not have any relevant value to assess the represented system; whereas idealization

is the omission of the parameters that have the values (physical quantities) relevant to the represented system.[53]

I may now turn to the claim **b**. First of all, I would like to object to the assessment of a situation expressed by Thomson-Jones (Godfrey-Smith does not explicitly adopt the following idea). According to Thomson-Jones, a representation of a system's particular property either contains idealization or abstraction, or neither, but it cannot contain both (see Thomson-Jones 2005, pp. 175-176). It seems a strong claim because every representation (scientific, artistic etc.) is at least a product of these two thought processes.[54] By definition, a representation is a representation of something – irrespective of whether that is a *representation of* an actual thing or not. Thus, thinking of a representation as not abstract or as not idealized would be *contradictio in adjecto* because if a representation is completely an accurate representation of its target system, then it is not a representation of its target system, but rather it is the target system itself. Although, this aspect of Thomson-Jones' view is not a direct implication of and neither it is in connection with the claim **b**, it invites some unintended consequences. Therefore, this separate claim needs to be evaluated for conceptual purposes.

At present, however, my discussion is limited to the claim **b**, which depends on the idea that a certain representation can contain either abstraction or idealization. In a recent paper, Arnon Levy (2018) takes an opposing view to this claim. According to Levy, there might well be a counterexample to the claim **b**. The example he considers is a universal physical constant: the speed of light. As symbolized with the letter '*c*', speed of light is approximately $3x10^8$ m/s –or to put it more specifically light travels in vacuo with a speed of 299.792.458 m/s. Regardless of the well-known information about *c*, Levy (2018, p.4) asks us to consider these two sentences "the speed of light in a vacuum is several hundred meters per second" and "the speed of light in a vacuum is 300 m/s". He maintains that both representations about the

[53] For example, omitting the temperature of the cannonball seems to be a process of abstraction because the temperature is an irrelevant factor in explaining projectile motions. However, exceedingly high temperatures can convert the cannonball to plasma in which case we could not fire any cannonball at all. In such cases, we either have to treat the omission of the temperature as idealization or accept that Suppe's distinction fails to account for some cases.

[54] If you distinguish approximation from idealization, then this claim would certainly not be a strong claim (see, Portides 2007; Norton 2012).

speed of light are false, because *c* is not several hundred meters but several millions of meters per second. Thus, because both representations introduce falsehoods, they contain idealizations. Furthermore, when we compare these two representations of the speed of light, we see one representation is more abstract than the other. Strictly speaking, '*c* is 300 m/s' is a less abstract representation than '*c* is several hundred m/s'. In this way, at least, we can come to the conclusion that the latter representation contains abstraction. Though the statement '*c* in a vacuum is several hundred meters per second' is a representation of a single property (the speed of light), it both contains idealization and abstraction. Therefore, contrary to the view that Thomson-Jones and Godfrey-Smith shares, Levy concludes that "a given representation can be both abstract and idealized with respect to one and the same (feature of one and the same) thing" (Levy 2018, p.4). A similar example is further illustrated in the same example as follows:

> [B]oth Jones and Godfrey-Smith presented abstractness as the omission of *true* information. They also took idealization to consist in the introduction of falsehood. One consequence of this is that a model cannot be both abstract and idealized, at least not with respect to one and the same fact or feature of its subject matter (Jones explicitly accepts this). By my lights this is not so… To return to our simple example, saying that the speed of light in a vacuum is somewhere in the range of 200,000–250,000 m/s is more abstract than putting it at 220,510 m/s. But both statements are false (Levy 2018, p.12).

Again, the speed of light is falsely described by means of idealization.[55] Also, these two idealized representations contain abstractions. At least, the statement that describes value of *c* somewhere in the range of 200,000–250,000 m/s is more abstract than the statement that sets value of *c* specifically to 220,510 m/s. It is because the former representation is less detailed than the latter with respect to the same subject matter. According to Levy, this counterexample shows that idealization and abstraction are not mutually exclusive, such that a representation of the single property can contain both of them.

At this point, it seems crucial to discuss the assumption of Levy's counterexample for a better understanding. According to Levy (2018, p.5) "abstractness is essentially a comparative notion" and it is "not about fidelity to reality". That is why Levy uses

[55] Levy is not clear about the rationale for such an idealization but suppose for the sake of argument that some modelers hold such a strategy.

not one, but two representations. In his example, the two representations (‘*c* in a vacuum is several hundred meters per second’ and ‘*c* in a vacuum is 300 m/s’) are both false descriptions of one and the same subject matter (*i.e.* speed of light). Nevertheless, they differ in their level of abstraction. Levy suggests that in order to understand whether one representation is more (or less) abstract then the other, we need to compare them in the first place. After the comparison, we can state that one of these two representations encompasses the other with respect to the level of detail. The ‘encompassed’ representation would be more detailed and thus would be less abstract. In Levy’s example, the representation that takes light’s speed as 300 m/s is encompassed by the other representation that takes light’s speed as several hundred meters per second. For this reason, the representation that specifies the exact speed of light is less abstract than the other.

The core of Levy’s objection is the standard interpretation of abstraction. Given such interpretation, abstractness is a two-place relation, such that we determine the abstractness of a representation with relation to its targeted system. For example, a street map of a city is an abstract representation in which the streets of the city are depicted. The map accurately represents the streets of the city. If a particular main street is actually 3 kilometers and the side street that intersects it is 1 kilometer, then we can, for instance, represent these streets respectively with 6 and 2 centimeters in high fidelity. This constitutes the true part of the model. Nevertheless, we can still say that the map is inaccurate with respect to not containing the parking facilities, playgrounds and shopping malls of the city. However, despite having such incomplete information, the streets are drawn in proportion by scaling down real street distances. Thus the street map is still an abstract representation of the given city streets.

Yet, Levy’s understanding of abstractness is not two-place but three-place relation. In other words, abstractness is not a relation between one description and some portion of the world but “between two descriptions and some portion of the world they are (both) about” (Levy 2018, p. 5). What is crucial for this relative abstractness is that both descriptions should represent or be about one single thing. Consider again the example of the city maps. If a map depicts the streets, parking facilities, playgrounds and shopping malls of the city and the other map depicts only the streets

of that same city, then it could be said that the former one is a more abstract representation than the latter. Notice that both maps depict the same city so that they can be compared with one another. Things would be different, for instance, if we have to make comparison between the city map and the earthquake hazard map of that same city. For one reason, these two maps have difference in their subject matter –though not in the right context, but we may say that both maps are incommensurable. One depicts the locations of both settlements and streets from bird's eye view, whereas the other depicts the level of ground motions in the city that probably will be exceeded in a particular time span. Therefore, in a nutshell, for Levy (2018, p.5) "abstractness pertains to two (or more) representations of one and the same subject matter; or (speaking loosely) of one and the same thing".

Levy also maintains that his view on abstraction is compatible with the view that Michael Strevens defends. According to Strevens "a model *M* is an abstraction of another model *M** just in case *M* generates *M**" (Strevens 2004, p. 167). Due to this understanding, Strevens introduces the concept of generation as an inverse operation of abstraction. To put it differently, if a model could generate another model, it is also more abstract than the other. He provides an example as follows:

> Suppose that I wish to explain the breaking of a window. As it happens, I threw a rather heavy brick at the window. Question: did the mass of the brick make a difference to the fact of the window's breaking? Answer: if you mean, did the exact mass of the brick make a difference, then no. The brick weighed 2 kg, but had it weighed 1 kg or 3 kg, the window would have broken just the same. However, if you mean did the approximate mass of the brick make a difference, then yes, it did. It is our explanatory practice, then, to say (a) the exact mass of the brick did not make a difference, but (b) the fact that the brick weighed between (say) 1 and 5 kg did make a difference (Strevens 2004, p. 167).

In this example, Strevens asserts that weighing 2 kilograms for a brick does not make difference to the breaking of the window. It is because the brick could weigh more or less than 2 kilograms. No doubt, there should be an upper, as well as a lower limit on the amount of weight for the brick; otherwise in the former case the brick would not cover the distance and in the latter case we could not carry it. Therefore, we need to specify a range from minimum to maximum. According to Strevens, the proper model that provides the exact range of weight values would be the most abstract model of the window's breaking. Since, by means of this model, we can further

explain why 1 kilogram, or 3, 4 or 5 kilograms of bricks break the windows. Another way to say is that the abstract M model (1–5 kg brick model) can generate less abstract models, like the M* model (2 kg brick model) or the M** model (3 kg brick model).

Generally speaking, Streven's main aim is to give an account of specification of difference makers in an explanation. In this manner, he offers the process abstraction. He does, because the proper abstract model contains "only causal factors and laws, or more exactly, only properties of causal factors and laws –such as the brick's mass's property of being between 1 kg and 5 kg – that made a difference to the occurrence of *E*" (Strevens 2004, p. 168). In Levy's account, however, a slightly different conclusion is drawn. Accordingly, to speak of abstractness of a representation we need to compare it with its alternative(s). In this way, it would be possible to make a claim against Thomson-Jones and Godfrey-Smith who both argue that idealization and abstraction are mutually exclusive. For my part, I see no inconvenience in asserting that abstractness could be addressed by comparing a representation with its more (or less) abstract counterpart. Without doubt, a representation of a system can be more or less abstract than the other representation of the same system. Nevertheless, this does not mean that abstractness is necessarily a three-place relation. As I stated above, a representation is by definition involves abstraction, and thus abstract by its very nature. Therefore, we can specify representations as abstract without assuming a comparison process. In fact, Levy engages this line of thought as follows:

> While I have suggested that abstractness is the more basic notion, I do not mean to imply by this that judgments about abstractness are made against a background of two *pre-specified* representations, one of which we judge to be more abstract than the other. That can but need not be the case… In other words, *we often rightly speak of a representation as abstract even though we are not comparing it to some specific, more concrete alternative* [emphasis added]… [But making] a judgment about abstractness will, naturally, be more interesting and informative if it specifies, or at least suggests, what a less abstract representation would look like (Levy 2018, p. 6).

As this quotation suggests, Levy explicitly accepts the possibility of abstractness as a two-place relation. In other words, for him, to specify a representation as abstract, we need not to compare it with its more abstract or concrete version. Despite this,

however, Levy argues that comparing two alternative representations with each other will be more *interesting* and *informative*. True, comparing a representation with its alternative will be informative, but it would only be informative to know which representation is more (or less) abstract compared to the other (or, at best, it would be informative to know which representation can generate the other) –but it would not be informative about the nature of abstraction. On the other hand, it seems hard to me to see what is interesting about comparing two alternative representations. I believe, Levy has special reasons for that, but it is difficult for me to see how being interesting could be an objective part of an argument.

Overall, then, we have come to the conclusion that assuming a three–place relation between two representations and the target system is not necessary to claim that idealization and abstraction are not mutually exclusive. The statement 'inclined plane has a friction coefficient of .5' is less abstract than the statement 'inclined plane has a friction coefficient between .1 and .9', but they are also separately abstract with regard to the omission of the color of both inclined planes. That is to say, when we represent an inclined plane with a friction coefficient of .5 or any other value, we have an abstract representation because of the omission of that plane's property *e.g.* its color. The same goes for the frictionless inclined plane as well. In this case, the inclined plane does not have any value of friction because it is frictionless, but it does not have any color either. Strictly speaking, we are not in a position to know exactly whether frictionless planes have colors or perfectly spherical planets have any inhabitants. For one reason, these objects or systems do not belong to the actual world. Nevertheless, I have no hesitation in saying that if frictionless planes have a particular color; it would be omitted from the representations by the modelers.

Before closing this section, there is one more point that remains to be noticed about Levy's argument. In order to claim that idealization and abstraction are not mutually exclusive, Levy distinguishes abstraction from abstractness (see Levy 2018, p.3-4). Accordingly, abstraction is a process, whereas abstractness is a product. Without any exceptions, all abstraction processes end up with abstract products. But, on the other hand, not all abstract products are produced by abstraction processes. We can identify an abstract representation without knowing whether it is an end-product of a process of abstraction or not. Process/product distinction is crucial for Levy because

it allows him to reject the standard view that the category of abstraction and the category of idealization are mutually exclusive. In this manner, Levy defines these concepts as follows: abstraction designates "the process—moving to a detail-poor representation" and abstractness designates "a property of the representation in question—its level of detail" (Levy 2018, p.4). To be more explicit about Levy's argument, let's recall the example of the city map that I used above. It may perhaps seem inconvenient to use this example, but it is not. This is because, Levy states that his theory of representation "is not restricted to causal representations" but may be applied to many types of representations (Levy 2018, p.6, f.n.5). So, as spatial and scale representation, it seems suitable to continue from the city map example.

Consider we have two maps of the same city, one depicts the streets, parking facilities, playgrounds and shopping malls, and the other depicts only the streets. According to Levy, there are two ways to produce these maps. One way is to generate the latter from the former. That is to say, one might generate the less abstract map from its more abstract version. The second way to produce these maps is to design each one independently from the other. This means that the less abstract map does not derive its depictions from its more abstract counterpart. In my opinion, it would be appropriate to examine these claims in detail.

As far as I can see, generating a representation from its more abstract version supposedly means two different things. Firstly, with respect to the same subject matter (the city), we find our way by *extracting* particular elements (only the streets of the city) from the more abstract map (the city's streets, parking facilities, playgrounds and shopping malls). Secondly, again with respect to the same subject matter (the city), we find our way by *subtracting* particular elements (by only focusing on the streets of the city) from the more abstract map (the city's streets, parking facilities, playgrounds and shopping malls). The two processes can be called as omission-as-extraction and omission-as-subtraction (on this usage see also Portides 2018). In the first process, we create a new representation by focusing on particular features of the more abstract representation; while in the second, we do not create but rather omit particular features of the more abstract representation and operate within the scope of it. No matter which process we choose, they are all based on an omission process. Nevertheless according to Levy, these two versions of

omission are not abstraction but generation. That is why we can proceed from more abstract to less abstract representation. But, when we take into account Strevens' (2004, p. 167) claim that "abstraction I define as the inverse of generation" and consider Levy's commitment to this view, could we inversely proceed from less abstract representation to more abstract representation? Though Levy did not explicitly state it, I think the answer is positive. If we were to make abstraction, we could end up with a more abstract representation in which the other settlements of the city (streets, parking facilities, playgrounds and shopping malls) are depicted. But this contradicts with Levy's definition of abstraction because for Levy abstraction designates "the process –moving to a detail-poor representation" (Levy 2018, p. 4). However, at the end of this process of abstraction, we came up with not a detail-poor, but rather a detail-rich representation. In other words, after the abstraction process, the representation we have is more detailed and involves not only the streets, but also the parking facilities, playgrounds and shopping malls of the city.

That contradictory conclusion aside, let us refer to the case in which the representation is not generated by its more abstract version. As two different representations, both maps could be created independently without using abstraction. According to Levy, this could be because of the asymmetrical relationship between the process of abstraction and the abstract product:

> By definition, the process of abstraction results in an abstract product. So the process can be understood in terms of the product -what makes it a process of abstraction is the end product, a detail poor representation. But not the other way around: an abstract representation need not be the product of a process of abstraction (Levy 2018, p. 4).

Accordingly, not all representations are produced by means of the abstraction process. A detailed representation (or map) of a city need not be created by adding relevant elements (Strevens calls this abstraction) to its less abstract version. Or, as is shown in the brick-window model, the representation that holds brick's mass as approximate need not to be the product of the abstraction process (*i.e.* it need not be obtained by the less abstract representation that provide the exact mass of the brick). According to Levy, then, a detailed representation of the city map or the brick-window model that takes brick's mass as approximate both could be produced independent of their less abstract representations or models. Levy argues –and I

agree with him– that the abstractness of a representation could be a matter of degree. As he states, relative abstractness of two representations can only be indicated by the comparison of two abstract products. So, we should understand abstractness via abstract products -not in terms of the process of abstraction. But does this argument adequately address or really undermine the mutual exclusivity account of idealization and abstraction. Based on Thomson-Jones' wording, it does not seem so:

> [T]he term 'idealization' and 'abstraction' on which idealizations and abstractions are tied to particular features of systems, the particular features they misrepresent or omit to mention, respectively. But models themselves are sometimes said to be idealizations or (more commonly to be idealized... [W]hen we say that a model M is an idealization, or is idealized, we mean just that it contains one or more idealizations in the sense discussed above and talk of idealization as a *process* or *activity*… simply denote the process or activity of constructing an idealized model in this simple sense [emphasis added]… The same holds, *mutatis mutandis*, for the terms 'abstraction' and 'abstract' (Thomson-Jones 2005, p.184).

I believe, what Thomson-Jones has in mind here includes the process/product approach and its implication for the distinction between the abstract product and the process of abstraction (and also for the distinction between idealized product and the process of idealization). Accordingly, let me illustrate Thomson-Jones' argument charitably, as best as I can. Consider a version of inclined plane models. We might, purposefully, misrepresent the plane's friction by setting its value to zero, at the level of model construction. Furthermore, we might omit plane's color by not mentioning it. Given Thomson-Jones' and Godfrey-Smith's views, the former denotes the process of idealization; while the latter denotes to the process of abstraction. As it is seen, their subject matters are different –one idealizes away the friction, and the other abstracts away the color of the plane. When all the processes are done, we obtain a model including a large number of statements that each represents a single property. The model is abstract and idealized. It might also be less or more abstract with regard to its counterpart. But the message is very clear: mutual exclusivity of idealization and abstraction is based on the process of abstraction (or idealization), not on the abstract (or idealized) product.

One final note: from what I have said so far, we cannot draw a conclusion about whether idealization and abstraction are mutually exclusive or not. A novel view can account for it, so I remain neutral between two positions. What I want to underline is

that the idea of both philosopher's argument under discussion was not properly addressed by Levy.

8.5 Clarifying the Distinction: Reasons and Motives

In the last section, I have not tried to resolve the issue about whether idealization and abstraction are mutually exclusive. The one thing I have attempted is to uncover the weak points of the arguments which conclude that idealizations and abstractions are distinguishable. The other issue I have discussed was whether idealization and abstraction are mutually exclusive or not. In this section, I will claim that these two thought processes can be distinguished from each other with respect to their background reasons or motives for the modelers. In doing so, my aim is not to provide a criterion to strictly distinguish between them. It is not because, as I examined above, there are common characteristics that are shared between them. Rather, my aim is to argue for the claim that some of the reasons for idealization are unique to that process, so that they are not common to abstraction.

First and foremost, the main thing that distinguishes idealization from abstraction is that the former can be made for many reasons, whereas the latter is made for one main reason. Let me start with the latter *i.e.* abstraction. As pointed out earlier, scientific abstraction seems very akin to the ordinary abstraction. When I try to explain the address of my apartment to a delivery man, I do not share with him the information who lives on the second floor of the apartment next to me, or in what year the apartment I live in was built. This is not because nobody lives on the second floor of the next apartment, nor is it because my apartment was not constructed in a particular year. The only reason I give my address in this way is that to receive my package as quick as possible. Therefore, it does not seem like a good idea to bother the delivery man with such twaddle. There are numerous details that could have been expanded on, like the information about the third and fourth floor of the next apartment. However, all of these details are abstracted away for being out of context because they are not relevant for the delivery issue. The same is true in science. When we model the projectile motion of a cannonball, we do not mention whether the firing took place in a democratic country or not, neither we mention that country cooperates peacefully with its neighbor countries, nor speak of that country's

agricultural production or geographical regions. Nevertheless, this is not essentially because such details are numerous and complicated. More essentially, these details are not contextually salient with respect to the explanation we seek.

The situation is different when it comes to idealization. First of all, most idealizations are made intentionally. Thus, compared to abstraction, we are in a more well-informed position to know about the properties we have idealized away (see, Wimsatt 2007, Ch.6). This does not mean that all idealizations contain intentional elements. In principle, we can discover afterwards that the property we have unintentionally neglected was in fact turns out to be an idealization. But such examples are errors, and rather rare. Yet, by and large, modelers know what they have removed –by means of idealization– from the model, and they do so much for multiple reasons.

The multiple reasons behind the idealization have been discussed by several philosophers of science in the literature. For example, Michael Weisberg (2007) speaks of three kinds of idealization: Galilean idealization, minimalist idealization, and multiple-models idealization. These three idealizations are built on different goals or representational ideals (Weisberg 2007, pp. 648–655). For example, Galilean idealizations are the ones in which we idealize away particular properties of the target system with the caveat of further correction. When particular conditions are met (*e.g.* computational techniques are developed efficiently) the idealized property is added back to the model. In this case, the ultimate goal of such idealization is completeness, such that the modeler aims at complete representation of the interested phenomenon. In minimal idealizations, the modeler does not have such a representational ideal. Rather, his/her aim is to figure out the core causal factors that produce the investigated phenomenon. In this way, the representational ideal behind minimal idealization is 1–Causal because the modeler aims at describing primary causal factors in the model. Lastly, in multiple-models idealization, modeler has a representational ideal of both 'completeness' and '1–Causal'. The reason in doing so is because a model could not be appropriate in explaining a phenomenon, so that in such cases the modeler uses another alternative model. Therefore, the representational goal here varies from case to case. Sometimes the modeler searches for a sufficient number of minimal models and sometimes aims at explaining the

phenomenon completely with these models. In a two-author paper, Yasha Rohwer and Collin Rice (2013) argue that there is one more kind of idealization (they call it hypothetical pattern idealization) and it is not captured by the representational ideals that Weisberg puts forward. According to them, hypothetical pattern idealizations are governed by a different representational goal which is to "produce understanding relevant to patterns across extremely complex and heterogeneous systems" (Rohwer and Rice 2013, p. 353).

In a recent book *Idealization and the Aims of Science* (2017), Angela Potochnik overviews the reasons behind idealization. Based on this overview, Weisberg's approach to idealizations is not appropriate. Potochnik holds that we rarely find "discrete kinds of idealization motivated by distinct representational aims", because "there are many *intertwined reasons to idealize*" (Potochnik 2017, p.47). I will not touch upon these intertwined reasons separately, yet for Potochnik as well, such sort of a list is open–ended. In this manner, merely mentioning some of the common reasons for idealization in science would suffice to show how idealization differs from abstraction.

It is a widely held belief that one of the main reasons to idealize includes an assumption that the idealized factor has little effect on the explanation or prediction of the model (see McMullin 1985; Strevens 2009; Contessa 2011). That is, we idealize away some of the properties from the system to have a compact model containing only essential variables. Subsequently, the idealized factors could be added back to the model by means of de-idealization. Nevertheless, such addition does not make crucial contribution to the outcomes of the model. The albedo effect is one example, that de-idealizing it indeed provided benefits with respect to the different issues, but doing so did not substantially contribute to the overall explanation and prediction (see **§7.7**) A typical example can be given of the ideal gas model. The *van der Waals* model takes intermolecular force of attraction into account to better describe the behavior of the real gases at low and high pressures. But such a modification was not sufficient to overthrow the explanation and prediction of the ideal gas model on the whole. To sum up briefly, the modelers usually idealize away particular factors from the models because they know that these factors have little effect on the proposed explanation (and prediction).

Correcting or de-idealizing the model is not always an easy task. On certain occasions, the model either does not work without idealizations or becomes nearly impossible to apply due to its mathematical intractability. There are several examples in the literature that show such cases. In hydrodynamics (Batterman 2009) and population genetics (Plutynski 2006; Morrison 2015, Ch.1) we come across some models that turn out to be non–explanatory when the idealizations are removed. For example, in population genetics, the populations under study are assumed as infinitely large. This is because the modelers aim to see how the dynamics of natural selection work when evolutionary forces (*e.g.* genetic drift, migration) are not acting on the population. Actually, no such infinite populations exist, but here the "point is that if populations are large (though finite), one can be 'almost certain' that the predictions of the model are correct" (Sober 2000, pp. 67–68). Apparently assuming infinite populations allows us to eliminate sampling error. Thus, in short, the outcomes we obtain will not be reliable, if we prefer non-idealized model over its idealized version.

We can find several examples in population dynamics (Weisberg 2007; Colyvan 2013) and economics (Hindriks 2011) of how models become mathematically intractable to derive particular results without using idealization. The Lotka–Volterra model is one example of population dynamics. The model simply represents predator–prey interaction of fish species and is designed to explain the relationship of each species at the level of population abundance. The model contains idealization by assuming actual discrete populations as continuum. Whether it is predator or prey, we know that growth process of fish is discrete in time, but colonies in a certain time *t* will be at different growth levels, so it would be appropriate for modelers to take this process as continuum. It is because assuming populations as continuum would make it relatively easier to find a possible solution to the system we mathematically represent (for more detailed analysis, see Lisciandra 2017). If we remove this idealization (*i.e.* if we treat the growth process of the population as discrete) we end up with a mathematically intractable model because "discrete difference equations, where time is treated discretely... are notoriously hard to deal with" (Ginzburg and Colyvan 2004, p. 72).

No doubt, many other reasons could be listed as to why idealizations are made. But this merely indicates that, the more reasons we provide, the less mutuality we find in the relationship between idealization and abstraction. This does not show that abstractions are less important. Indeed, the most (and perhaps the only) pertaining reason of abstraction is to figure out on what properties should our representation focus and on what properties should be eliminated from it as well. This process is almost the same as we do in ordinary abstraction and is entirely indispensable for scientific representation. It is because there are myriad of properties to be abstracted. Besides the impossibility of adding such properties to our representations, these properties are not contextually salient with respect to the explanation we are about to introduce. On the other hand, the number of reasons to idealize is more limited with regard to the reasons for abstraction. Thus, this allows us to know the *whats* and *whys* of idealization. In this section, I have only touched upon three types of reasons for idealization. Let me sum them up very briefly. De–idealization or removing the idealizations makes difference in the outcomes of the model. But this difference at least comes in two flavors. Some idealizations are not essential, so de–idealizing them makes a positive difference; whereas some idealizations are essential, so correcting them makes a negative difference. For example, adding explanatorily less relevant parameters back to the model can make a positive contribution to the model's outcome. In this way, the model could represent its target more precisely and this would in turn provide more accurate predictions. But sometimes things don't work out right. De-idealizing essential idealizations could lead to a model being mathematically intractable. In such circumstances, leaving idealizations as they are could have more positive consequences in understanding the considered systems.

8.6 Conclusion

Idealization and abstraction are both indispensable for scientific representation and modeling. Accordingly, characterizing these processes is crucial to understanding one of the widespread modeling strategies. It would at least be a useful philosophical exercise to highlight the typical features of each process. However, it is not an easy task to strictly distinguish one from the other. Especially, uncovering the basic differences among idealizations would show which type of idealization supports the model's explanation and which does not.

There still exists a conceptual confusion in the literature surrounding the terms idealization and abstraction. For example, we see a different usage of these terms in Steven Rappaport's (1996) work. On his view, abstractions are "partial representations" and "idealizations" are "quite distinct from partial representations" (Rappaport 1996, p. 219). His definition of the concepts is as follows: "a partial representation neglects to mention something that is present. But an idealization… assumes the nonexistence of something that is present" (Rappaport 1996, p. 219). As I pointed out in the previous sections, there seems no significant difference between neglecting the presence and assuming the absence of a particular factor, at the level of model application. In other words, with respect to the outcomes of the model, assigning a value of zero to a particular physical parameter is same as neglecting to mention that a particular physical parameter exists. Therefore, the issue is not about whether neglecting the presence or assuming the absence of a particular factor. Rather, the issue here is to make sense of both idealization and abstraction, by uncovering the reasons why they are employed in the first place. My approach to the issue is to think of the difference between them in terms of their background motives. In idealization, we neglect to mention a particular property because its effect is too small to be of theoretical interest; whereas, in abstraction we neglect to mention the particular property because it has no effect on the explanation and prediction at all.

There are different approaches to the difference between idealization and abstraction. Anjan Chakravartty (2001), for example, states that "models are generally, at best, abstracted and idealized versions of reality" and accordingly such versions are generated by distinct processes (p. 328). As he puts it:

> The assumption in classical mechanics of masses concentrated at extensionless points is an idealization. We do not actually think that masses of bodies are concentrated at extensionless points. Contrast this situation with one of the aspects of our model of the simple pendulum. Masses swinging in terrestrial pendulums usually encounter air resistance. In ignoring this parameter, our model of the simple pendulum is an abstraction (Chakravartty 2001, p. 328).

According to my viewpoint, the assumption of masses as extensionless points is idealization; but ignoring the air resistance, I think, is not an abstraction. For one reason, the air resistance, one way or another, has an effect on the explanandum-phenomenon *i.e.* the swing of the pendulum. But compared with the dominant forces

(like the force of the gravity and the force of the tension), the drag force of the air resistance is pretty weak. Thus, it can be idealized away not because it has no effect but because it has less effect on the explanation of the pendulum oscillation.

A very different interpretation of the concepts of idealization and abstraction is made by Margaret Morrison (2015) as follows:

> [A]bstraction is a process whereby we describe phenomena in ways that cannot possibly be realised in the physical world (for example, infinite populations). Idealisation on the other hand typically involves a process of approximation whereby the system can become less idealised by adding correction factors (such as friction to a model pendulum) (Morrison 2015, p.20).

As noticed from this quote, one of the two processes that I consider as idealization is apparently considered as abstraction. Yet Morrison does this on purpose because she wants "to focus on the kind of mathematical abstraction" which is different from the "abstraction that is not amenable to approximation techniques" (Morrison 2015, p.21). To be sure, fixing the terminology at the outset is definitely important, but such arbitrary usage seems to endanger the possible advance of the current debate.

We may perhaps come across varieties of understandings of the terms 'idealization' and 'abstraction'. If such is the case, then the attempts to distinguish these terms would be significant in clarifying several points of confusion in the existing literature. In this chapter, my aim was to contribute to how such clarification might be accomplished. Additionally, I tried to show the weak and strong points of the existing views on the subject. Distinguishing sharply between idealization and abstraction is not an easy matter, so I have in particular suggested focusing on the background reasons why idealizations are made. If we know more about the reasons for idealization in scientific modeling, we can better asses to what extent it differs from abstraction.

CHAPTER NINE

TAKING STOCK

In this concluding chapter, I will provide a systematic overview of my dissertation study. To exhibit such systematicity, I will briefly recapitulate the main points and themes of each chapter. As the reader can notice, I have already provided short conclusions for each chapter above. In this context, this last chapter can be considered as synopsis of the present dissertation. On the other hand, the aim of this chapter is not only to summarize all the philosophical issues I dealt with. Additionally, I aim to express some main ideas of my arguments in crystallized form. My purpose in doing so is to illustrate how I addressed particular questions about the nature of modeling. The last aim of this chapter is to identify some fruitful problems relevant to scientific models. Accordingly, I offer my thoughts on promising directions for future research.

In Chapter 1, I provided a historical background on how philosophy of science shifted its focus away from theories and onto models. I explored the reasons why, in its infancy, scientific theories occupied a central position in philosophy of science. I suggested that the most important reason for insisting on the centrality of the theories was the domination of grand theories in science, namely the relativity and quantum theories (see **§1.2**). At the beginning of the twentieth century, it was nearly impossible to remain indifferent to the significant achievements of these two scientific theories. In this manner, explanation of scientific success was no more than giving a clear insight into the scientific theories. Nevertheless, science itself has not much to say about its theories; it merely *uses* its own theories to provide explanation and prediction. This is where philosophy of science comes in, in its attempt to understand the structure of scientific theories.

Given the syntactic view of science, philosophy has tools that are useful for presenting scientific theories (which are considered as linguistic entities) in a formalized manner (see **§1.5.1**). In this way, scientific theories can be rationally reconstructed in terms of logical language. Rational reconstruction yields beneficial aspects. The most important one is about the concepts and statements of science. If scientific concepts and statements are defined *explicitly* enough and shown to be

empirically *verifiable* respectively, then science would leave no room for metaphysical disagreements ever. In other words, unlike various disciplines of philosophy, science would progress in a more systematic and reliable fashion without descending into abysmal metaphysical arguments. Rational reconstruction provides a basis for constructing a logical and axiomatic structure. According to the advocates of the syntactic view, if the language of science is considered as a logical system in which complex statements are deduced from the basic ones, then it becomes possible to identify, for example, where the possible error might lie. For some of them the never–ending philosophical problems were not about the world, but about the language. Nonetheless, they thought that it was not sufficient to resolve these problems in ordinary languages. For this purpose, they found it necessary to recast the problems in an explicit meta–language where all the logical steps can be followed.

Nevertheless, this splendid project has faced some significant challenges soon afterwards. To put it briefly, rational reconstruction of scientific theories was not an easy task as they had hoped. The idea of formalization, for example, has led to a cluster of problems that contained dead–end situations. At this juncture, some philosophers of science proposed a new approach to evaluate theories in a more reasonable way. This approach is called semantic view of theories which takes models (instead of theories) as central to their concerns. Though the semantic conception of scientific theories agrees upon the centrality of models, proponents of this view fall into two subgroups. On one side, some advocates of the semantic view stated that model–centric approach to science needs to sustain the goal of formalization. Given this view, syntactic approach failed not because their ideal of formalization was untenable, but because they used wrong tools for formalizing theories (*i.e.* specifically the tools of symbolic logic). Accordingly, new formalization tools (*e.g.* set theoretical predicates, state spaces or phase spaces) need to be adapted to properly evaluate theories. On the other side, some defenders of the semantic view (not to confuse with model–centric approach, I call it model–based approach) suggested that the idea of formalization is an undesired legacy from the received view that needs to be rejected as a whole. On this view, scientific theories are best understood in terms of their usages for specific purposes. In other words, the

guiding question should be asked is how scientists use models to study target systems. I have provided details of the debate between semantic and syntactic views of scientific theories in **§1.5.1** and **§1.5.2**.

It would, however, be imprudent to claim that the semantic view of theories was the only game in town when it comes to criticisms of syntactic view. Before the semantic conception was articulated, the ongoing program had been already weakened by some other challenges. The method of falsification against verification, the disputed distinction between analytic and synthetic truths and the widespread thesis that states all observations are theory–laden are some but not all the problems that they had to deal with. Under such circumstances, actually, proponents of the semantic view of theories have delivered merely the *coup de grâce* to the syntacticists. Nevertheless, as the focus of my dissertation is scientific models, I limited my considerations to the semantic view of theories. The whole aim of Chapter 1 was to reveal the roots of the debates surrounding scientific models –which are in turn discussed in the following chapters. The historical perspective provided in Chapter 1 is significant in understanding the current philosophical problems in model–based view of science. The idea that models –on their own– raise some difficulties for philosophy of science is plainly misleading. It is because the history of philosophy of science shows that this is an 'escape the bear and fall to the lion' situation. To put it more specifically, replacing the syntactic view with the semantic view (particularly model–based view) brings along a new set of problems that are equally difficult to solve.

The aim of Chapter 2 of this dissertation was to describe one of these new problems. As I emphasized in **§1.4**, some of the proponents of the syntactic view were inherently Wittgensteinian. Following this line of thought, they inclined to defend the idea that philosophical problems arise from misunderstanding; and misunderstanding is induced by the lack of an exact language. They also believed that the exact language of logic would provide instruments in resolving such philosophical problems, or help us to identify them as pseudo–problems, if so. According to them, the language of science is indisputably exact *relative* to metaphysics and arts. However, it should also be demonstrated that the language of science is exact *in itself* by reconstructing it in a formalized or axiomatized way. This is why logical positivists recognized the role of philosophy as a way to provide an exact language

for science. So, the ideal of exactness is closely connected with the formalization project.

Nevertheless, there is no easy, straightforward way to accommodate this ideal in model–based view of science. First and foremost, models are idealized and abstract descriptions of factual reality. Therefore, models do not map onto facts in a one to one manner. The inexact relation between models and facts essentially hampers the objective representation. An idealized and abstract model does not *completely* correspond to facts. Therefore, the representational relation is not a truth relation between the basic statements and facts. A particular model M represents some aspects of reality, while ignoring the others. A different model M*, on the other hand, might represent the aspects of reality which are ignored by M, while excluding the others.

Chapters 3 and 4 introduce the new set of problems the model–based view of science faces. Chapter 3 focuses on the problem of scientific representation. This specific problem is related with the abandoned ideal of exactness. Some models introduce fictional objects or systems, like perfectly rational agents or infinite populations. These models do not correspond *exactly* to reality. This is why in the jargon of model–based view of science, the term 'target system' is preferably used over the term 'reality'. Furthermore, some models, for example, are scale models (see **§2.2.1**). Models of this kind do not have truth values, like the statements generally do. In this manner, models are different from statements; being either representation (or misrepresentation) of their target system. This inevitably leads to a new problem (*i.e.* the constitution question): What is the relation R that constitutes representation between a model and the world (see **§3.2**)? One view suggests that models successfully represents by being similar to their target system; another view, on the other hand, asserts that models successfully represents by being isomorphic to their target system. In **§3.5**, the arguments of both views are summarized. In sections **§6.2** and **§6.3** I evaluate these views by means of a case study which is presented in **§5**.

Chapter 4 deals with the problem of idealization and explanation. I also associate this problem with the abandoned ideal of exactness. If models are not *exact* representations of reality, then how do they provide successful explanations? I

classify the approaches to this problem under two main groups. One view argues that models can explain or be explanatory *despite* idealizations (**§4.3.1**). The other views, differently, suggests that models can explain or be explanatory not despite but *in virtue of* idealizations (see **§4.3.2**). Nevertheless, there is another philosophical position which holds that models cannot provide explanation. On this view, framing the problem in such a way is misleading because explanation requires a non-idealized representation of the reality which seems *prima facie* impossible. Therefore, idealized models should provide us something different from explanation. Within this context, some philosophers of science suggest that scientific models provide understanding of target systems. Understanding, according to this view, is not only a cognitive achievement. Additionally, it is an epistemic achievement by contributing to explanations with its nearly true (or *true enough*) status.

An essential conclusion I draw in **§4.3** is the main dichotomy of models that exists between explanation and representation. According to one interpretation, models should be addressed in terms of their functions. Therefore, it would be appropriate to focus on the successes of model outcomes, namely its explanations and predictions. If the outcomes of the model are not far-fetched compared to the actual data, then further considerations with regard to the model's representational relation becomes futile. On the other interpretation, models should be evaluated in terms of their representational relation. On this view, models provide explanations or predictions by being (similar or isomorphic) representations of their particular target system. Accordingly, models primarily represent and then explain the intended target system.

The first interpretation seems promising but its particular implications with regard to the explanatory status of idealized models are open for discussion. This is why some philosophers of science favored understanding over explanation as the aim of science. On this view, idealized models cannot explain just because the false assumption(s) they contain. So, one should take this vexed issue into account and commit to a different view. The second interpretation is put forward to avoid the so-called dichotomy. Nevertheless, it does not seem particularly helpful in providing a solution either. The reason is somewhat straightforward: one can figure out whether a model is similar or isomorphic to its target system with respect to its outcomes. In other words, a model is a good representation insofar its predictions are successful.

This, however, takes us back to the first interpretation because a particular representational relation is not a requirement for a model to be successful in terms of its prediction.

Chapter 5 is intended to function as a hinge joint, allowing me to treat the introduced problems (representation, abstraction, idealization and de–idealization) on the basis of a historical case study. In this chapter, I discuss the Milankovitch model of ice ages that was proposed at the beginnings of the twentieth century to explain glacial and interglacial periods within the major ice ages. In **§5.5** some previous versions of ice age models were briefly discussed. This, I believe, helps to provide a bird's eye view of the scientific collaboration in developing a model; and of the reasons behind the success of the Milankovitch model as well. Moreover, I describe from also a non–historical perspective some challenges (especially some criticisms of the scientific community at the time) that Milankovitch faced *while* building and *after* applying his model. The remainder of this chapter draws insights from **§5.7** that turn out to be a very useful approach to tackle the problems presented in Chapters 6 and 7.

Chapter 6 begins with a critical recapitulation of the major accounts of representation that are successively dealt within **§3.5.1** and **§3.5.2**. I present the internal inconsistencies of these major accounts by relying on the existing literature concerning representation. To put it specifically, these inconsistencies arise due to the logical properties (so–called 'intuitive statements') of representation. Nevertheless, the issues these major views suffer are not simply confined to the inconsistencies they have. Beyond this, these accounts do not seem to be able to come to grip with the problem of idealized and abstract models and their representational status. For this particular reason, I introduced the partial isomorphism view (which seems to me the most promising one in the literature) to account for the problem of representation. Yet I should emphasize that my purpose was not to fully advocate for Partial Isomorphism. Accordingly, I tried only to show that Partial Isomorphism is heading in the right direction in solving the problem of representation. What backs up my opinion about Partial Isomorphism is its comprehensiveness in capturing idealization and abstraction. The historical case study I presented in Chapter 5 also supports my contention. Furthermore, I have

argued against the Gricean idea in **§6.6** by giving some reasons as to why representation is not a mere mental stipulation.

Chapter 7 looks at a less discussed issue in the modeling literature, namely de–idealization. Although the issue of de–idealization is relatively a neglected topic compared to the issue of idealization, it is actually a very common strategy in scientific practice. The essence of the issue about de–idealization hinges on whether such a reverse modeling strategy epistemically contributes to the outcomes of the model. Though not openly discussed in the relevant literature, I point out to some particular views which have implications for de–idealization. To the best of my knowledge, there is no philosophical standpoint that posits the idea that de–idealization is epistemologically a fruitless endeavor. Nevertheless, there is also no straightforward philosophical argument in the modeling literature as to why de-idealization is not a futile strategy as well. **§7.5** and **§7.6** both sought to fill this specific gap in the literature. By examining, once again, the case study in Chapter 5 I propose a new function for de–idealization in **§7.7**.

Discussing the epistemic functions of de–idealization inevitably brings a related question to mind: Does the process of concretization (the reverse strategy of abstraction) fulfill any epistemic function as well. With this question in mind, Chapter 8 mainly aims to distinguish idealization from abstraction. It is perhaps needless to emphasize the fact that models contain numerous abstractions compared to their idealizations. In this sense, it is nearly impossible to deal with all the abstractions that a model contains. However, if it were possible to concretize some of them, could this process serve any epistemic function for a model, as de–idealization usually can? The answer I give to this question is negative because abstractions are by and large explanatory redundant factors for model explanation. By concretizing abstractions, one can obtain a less abstract model in which the target system is described in a more detailed (but not in a *more correct*) fashion. However, a model would not necessarily be more explanatory than its less abstract and thus more detailed alternative. The consequences I draw from the reverse processes of idealization and abstraction also help me to distinguish between these two scientific strategies. In **§8.5**, I try to develop an approach that focuses the background reasons for applying for both strategies.

How *idealized*/*abstract* models *represent* and *explain*? This question endures as a fruitful philosophical question among the current philosophers of science. In addition, it is also a question with many dimensions. So far, this dissertation has covered various dimensions of this multi-faceted question. Nevertheless, some related issues have not been sufficiently addressed. As stated in the last footnote in **§4.4**, for example, the core of the problem concerning HAEs and HPEs lies in the dichotomy between the standard view and its alternatives.

German philosopher Wilhelm Dilthey (1833-1991) once argued that humanities and natural sciences are distinct by having different aims. Given this view, the aim of humanities is to understand cultural phenomena, while the aim of natural sciences is to explain physical phenomena. At the present time, however, Dilthey's views seem to have been turned upside down. Some views see understanding as the aim of natural science (**§4.3.2.2**). This idea inevitably leads to a series of problems. It appears, however, the core of the problem is tied to the very nature of understanding. To put it more specifically, the issue, in particular, boils down to the fact that understanding is a psychological process. This, in turn, makes questionable whether understanding is an epistemic value compared to explanation. In our current global culture, it would not be incorrect to claim that the authority of science rests on its successful explanations. When this taken into consideration, the psychology of the explainer (*i.e.*, the understanding process) seems epistemically irrelevant.

Can *understanding* be the aim of science as well? If it can, then what sort of an understanding should be sought? Do researchers receive financial support for explaining or merely understanding a particular phenomenon? For example, do we expect from scientists to explain or merely understand the impact of future global climate change? Can understanding be evaluated as a unit of explanation? Although the growing literature on model explanation has clarified the concept of understanding somewhat, these problems also need further studies.

The issue of de–idealization discussed in Chapter 7 is mostly neglected in the relevant philosophical literature. Careful analyses of historical cases from science would make it easier to characterize the nature of this commonplace practice. On the other hand, there are idealizations that cannot be de–idealized. This is surely a topic

in itself and needs to be further elaborated to facilitate the assessment of the different aspects of models. For example, some idealizations contained in models are explanatorily indispensable or ineliminable, while some are not. Why? What makes this difference? Moreover, revealing new functions of de–idealization (apart from the ones that I have mentioned in **§7.3** and **§7.7**) would make a significant contribution to our understanding of this reverse modeling strategy.

I focused on the details of the distinction between idealization and abstraction in Chapter 8. Nevertheless, there are also different strategies that scientists employ, like approximation and simplification. Are they each separate modeling strategies or can they be classified into idealizations and abstractions. The answers to these questions are also of eminent importance for a better characterization of the scientific practice.

Coda: Models are ubiquitous in the sciences. Watson–Crick model of DNA structure, billiard ball model of gases and the ΛCDM model of the universe are some (but not all) of the major examples. Scientists use models for specific purposes. By using models, they give explanations or make predictions of a particular phenomenon. Thus, it is almost unnecessary to emphasize the importance of the role of models in sciences. Nevertheless, in terms of philosophy, the issue concerning models received a considerable attention after a period of negligence. One important reason for that was a sort of attitude which favored the investigation of scientific theories over any other issues.

However, the overall situation, in philosophy of science, changed after the nineteen fifties. Philosophers of science changed their direction towards a more viable arrangement by focusing on actual scientific practice. This, in turn, led philosophers to realize the importance of models and their relation to theories. Thereafter, the amount of publications dedicated to the topic profoundly increased and the issues concerning models have gradually started to occupy more space in philosophical discussions. Valuable debates have flourished about the science itself, including its aim and methodology. There are many important issues to be considered within these debates. In this dissertation, I have not only aimed to cover some of the fundamental issues concerning scientific models, but also attempted to fill several gaps in the

growing literature by focusing on different aspects of scientific modeling. By all means, further research on the issues raised in this dissertation could help to develop a more nuanced view of the practice of scientific modeling.

APPENDIX A: WHAT DID MILANKOVITCH DO?

In this appendix, I discuss the modeling process in a step-by-step fashion to see how the solution is working for the particular problem by using a scientific model. In order to do this, I will reveal the steps taken by Milankovitch in particular order. To put it differently, I will present the stages of model construction. Doubtless, these stages are neither unique to the stages of Milankovitch's model of ice ages, nor encompass all types of model construction steps. Thus, I refrain from proposing a general account of model building steps. In this sense, while these stages may be followed by some modelers, they may well not be followed by the others.

Identifying Main Causal Factor. The primary step a modeler takes in explaining a natural phenomenon is to identify the main causal factor (MCF) that underlies the phenomenon in question. This is because such a factor could produce or dominate the investigated effect. The causal factor is generally considered as 'main' because there might be many other factors which can play role in the occurrence of the effect. However, these secondary factors, if there are any, are generally omitted by the modelers due to preferences. Here my aim is not to review the causality literature from a philosophical perspective. For this reason, it would be useless for my purposes to discuss the issue further. However, it would be sufficient to mention a common strategy in identifying MCF. For example, in order to understand whether a factor should be omitted or not, modelers generally use counterfactual reasoning. This reasoning can be explained by means of the following capsule form of conditional statement: 'If X had not occurred, Y would not have occurred'. If a modeler has strong belief in the fact that Y would not have been possible without X, he has already identified MCF. Engaged with such a thought, modeler could have and still can omit the other factors for his purposes.

In Milankovitch's case, the phenomenon waiting for an explanation was the shifts between glacial and interglacial periods. To put it in a question form: why the Earth leads completely two opposite climates at global level? According to Milankovitch, the MCF responsible for these successive climate periods was the amount of annual insolation of the Earth. In other words, for him, if the changes in the amount of insolation had not occurred, changes in the Earth's global climate would not have

occurred either. Hence, calculating the distribution of annual insolation is at the same time calculating the changes in the Earth's global climate system. Eventually, there is a causal dependence relation (albeit asymmetrically) between the changes in the insolation amount and in the global climate system. Therefore, the changes in the insolation amount would trigger the changes in the global climate system (but certainly not vice versa).

At the same time, MCF may bring an empirical aspect to any model that had not been thought before. For example, Milankovitch's mathematical model, in its isolated form, merely calculates the past, current and future values of insolation amount for a particular latitude. Under these circumstances, the model would be useless in the context of any other natural phenomena; it would merely provide limited information about how much solar radiation the planet receives in a given year. In brief, the model in this sense is a model of insolation. When the variation of the amount of insolation is identified as a MCF for the global climate changes, then the model produces additional empirical relations.

From this point of view, it could be said that certain models are about phenomenon that carry a potential to relate themselves with other phenomena. Milankovitch's model is one of an example. Thus, it is a model of insolation as much as a model of climate changes. After all, if one limits oneself to the claim that the global changes of the Earth are dominated by astronomical factors (that's what Milankovitch did), one can also conclude easily that these changes could be explained and predicted merely by appealing to the model.

Associating Relevant Elements. The other step of modeling is to associate related elements with MCF. At this point, the relationship between the relevant elements and MCF is established on the basis of modulation. In other words, the relevant elements have an effect on MCF by modulating it in some way. The relevant elements as modulators have not a direct effect on the phenomena, but their association with MCF makes them related indirectly to phenomena.

In the example, the amount of insolation is identified as MCF, and as a factor the level of insolation decreases or increases with respect to the three cyclic motions. In other words, Milankovitch cycles are responsible for variation in the level of solar

irradiation exposure. Nevertheless, the cyclic motions of the Earth, each alone, does not produce a considerable effect on global climate change. For example, low level of obliquity balances the insolation amount in a seasonal manner and diminishes the seasonal temperature contrast. Thus, the winters become milder, while the summers become colder. Therefore, one may conclude that, this climatic scenario paces the glaciation process because, as we recall, colder summers are not enough to melt the ice cover which are formed during the winter. However, the low obliquity that is effective in accelerating the glaciation would be conversely ineffective under the conditions of high eccentricity and high longitude of perihelion (measured according to the precession). This is because these latter motions diminish the effect of obliquity and in this way, hinder the development of glaciation. This can be shown by looking at past and present values of orbital parameters. The table below shows three cyclic motions of the Earth with three different periods (see Table Appx.1) which computed according to the formulas of Berger (1978) in J.F. Royer et al (1984, p. 738).

	125 kyr	115 kyr	Present
Obliquity	23°.798	22°.405	23°.447
Eccentricity	0.04001	0.04142	0.01672
Longitude of the perihelion	307°.14	110°.9	102°.0

Table Appx.1 Orbital parameters for the computation of solar insolation. Reprinted from "A sensitivity experiment to astronomical forcing with a spectral GCM: Simulation of the annual cycle at 125000 BP and 115000 BP" by J.F. Royer et al, 1984, Milankovitch and Climate, Part 2, NATO ASI Series, p.738. © 1984 by Springer Science+Business Media Dordrecht.

The above table gives the calculated values of solar irradiation amount for the present, 115 thousand years BP and 125 thousand years BP. Especially, the values calculated for 115 kyr BP are of great importance, because this period corresponds to the glacial period which was named as *Würm I*. Moreover, the period 125 kyr BP comes across as a warm period of interglacial, right before the *Würm I*. When compared with each other, there seems no big eccentricity difference between the two past periods, albeit the values are on average with respect to maximum eccentricity (0.7). However, it seems a considerable difference on the values of

obliquity and longitude of the perihelion. 115 kyr ago, low obliquity and low longitude of the perihelion due to precession both had been decisive in determining the amount of insolation. This actually furthered the process of glaciation on the land surface. Dissimilarly, 125 kyr ago, higher latitudes were exposed sunlight equivalent to the current exposure levels of lower latitudes. Thus, the Earth, at that period of time, experienced an interglacial period.

These cycles are neither singly nor jointly the direct cause of glaciation; instead they merely determine the crucial levels of insolation. Thus, glaciation (as well as inter-glaciation) process causally connected with the amount of solar insolation, not with the orbital cycles of the Earth. However, these cycles, incorporate as modulating elements of main causal factor that eventually influence global climate change.

Establishing the context and scope. Milankovitch's astronomical model of glaciation is an idealized mathematical model of the Earth's climate system, in certain respects. If Milankovitch had not restricted his endeavors to Quaternary and had made his calculations for the far past, he would have to include the positions of the continents as a parameter in the model. Recall that glaciers can only form on ground. Also recall that the continental grounds were in different places as we know them today. Therefore, if, sometime in the past, lesser continents had been located near the equator or continental mass of the Earth had been smaller, glaciation would be unlikely to occur even in the case of a superposition of the three orbital cycles.

Actually, Milankovitch's model limited itself to explaining glacial and interglacial periods in the Quaternary. In other words, Milankovitch did not intend to calculate longer term global climate changes (like changes in the *Huronian* or *Cryogenian* glaciation). However, there still exist idealizations in the model. For example, the model does not contain parameters such as the chemical composition of atmosphere, the ocean circulations or the reflectivity of Earth's surface (see Lutgens *et al.*, 2012, p.283). However, each of these, one way or another, has an influence on glaciation. The Earth's atmosphere, for example, reflects a small amount of sunlight back into space. Moreover, it traps the remaining absorbed heat and keeps our temperature at a particular degree. Subsequently, CO_2 and methane concentration in the atmosphere rises up to the critical level and glaciation process delays. Oceanic circulations also

have an effect on climate change. For instance, warm currents increase and cold currents decrease the temperature of certain regions by the activity of sea breeze. More or less, this factor also plays a role in climate variability. Finally, reflectivity effect of the Earth's surface triggers glaciation. This effect, which is known as albedo, promotes glaciation by reflecting sunlight back into space. In course of time, too much ice accumulates on the ground and too much sunlight goes off into space which positively feedbacks the colder global climate.

In general, modelers are intended to measure the main factor that produces the dominant effect. It is because the natural world is complex, and yet all the relevant parameters could not be included in the model. Perhaps, oppositely to this situation, the relevant parameters could be included in the model but it would turn out to be mathematically intractable. In other words, the proposed model would represent the target phenomenon more realistically while limiting its tractability. Thus, it seems that, if the modeler intends to obtain the values of the inessential or secondary factors alongside the MCF, he needs to include each in the model. However, such an approach might not be useful every time. It is because adding other inessential factors to the model, though, makes it more realistic; it would presumably makes it less tractable or completely intractable than its initial form. Also, pragmatic purposes of modelers take place here. Since inserting any detail into the model makes it intractable, the explanatory character of the model would be lost for the sake of realistic approach. So, adding more details perhaps makes the model more complete while doing this does not mean that the model would be more explanatory. In short, a modeler picks his poison, so to speak, pays the price for his needs.

Construction. Recall the stages that I have explained up to this point. MCF is identified; relevant elements are associated with MCF; and sufficient parameters of the model are inserted. Combining all three stages constitutes a further stage which could be named the stage of construction.

During construction, a modeler may prefer to use strategies to facilitate understanding of the elements used in the model. These are generally visual tools such as diagrams, charts or graphs. For example, Milankovitch cycles that modulate insolation are all together shown in a compact diagram (see Figure Appx. 1)

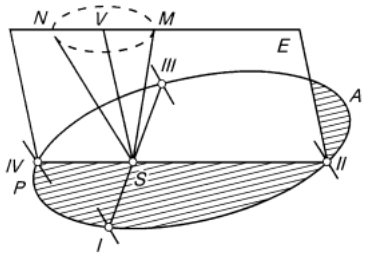

Figure Appx. 1 The figure shows all astronomical elements that have an influence in modulating the amount of insolation. Reprinted from *Kanon der Erdbestrahlung und seine Anwendung auf das Eiszeitenproblem* (p. 247) by Milankovitch, M., 1941, Belgrade: Royal Serbian Academy Special Publications.

In this diagram, all orbital motions are combined, that is shown separately before. In the figure "S represents the centre of the [Sun], and the ellipse P**I**A**III**P [represents] the annual terrestrial orbit round the Sun" (Milankovitch 1941, p.246). The line SV drawn through S is normal to the orbital plane and "the angle VSN represents the inclination of the rotational axis of the Earth or the obliquity ε of the ecliptic" (Milankovitch 1941, p. 246). P and A respectively denote the perihelion and aphelion. Finally "the angle **III**SP, which may be called ∏γ, represents the longitude of the perihelion to the vernal point" (Milankovitch 1941, p. 246).

Of course, this single diagram does not capture the whole story, nor does it provide an extra explanatory power for the model. Such diagrams are proposed to represent visually the phenomena under study. Although, the diagram has no strict relation with the calculations of the Earth's orbital parameters, it illustrates relevant astronomical elements of the model in a compact manner.

Likewise, three graphs regarding the cycles of the Earth, one under the other, would display us how insolation changes are driven over time. Moreover, as Milankovitch did in the radiation curve case, the results obtained by the mathematical model can be transformed into whatever form a modeler needs. Thus, the resulting graph, just as had done with the Penck–Brückner scheme, can be compared with various graphs which are thought to be interrelated in certain respects. Accordingly, a modeler can check the results of others and decide whether he is on the right road or not. Having thought in this way, the stage of construction would be instructive for a modeler who works with visual tools.

Consulting to the World. The last stage of modeling is to consult the empirical world. This is a stage when a modeler checks whether his model is a model of target phenomenon or not. In other words, a modeler asks whether his model represents the target phenomenon, at this stage. If the model correctly represents the phenomenon, then the model is a model of that phenomenon.

Milankovitch's model represents two–body motion in celestial mechanics. It contains a body, in the center, with a very large mass and energy, like the Sun and contains another body which revolves round it, like the Earth. In such a two–body system, the model calculates the annual amount of radiation that a planet receives from a focal heat source. In this way, our planet's backward and forward climate variations are respectively explained and predicted. Nevertheless, the merits of the model are not merely limited to the Sun–Earth duo. To put it differently, the model is not only the model of the Sun–Earth duo, but also the model of the Sun and each planet in our galaxy, and thus functions as a model for them too. In fact, Milankovitch, in his late works, showed how his model can be applied to other planets, especially to Mars and Venus. Therefore, the model is not merely the model of our solar system, but also the model of every system consisting of a body like the Sun and a planet revolving about it.

Actually, most mathematical models have similar features in this respect. These models are the models of possible worlds as much as our actual world. Therefore, mathematical models can represent possible worlds as well as our actual world.

I believe it is important to be clearer on what is meant by representation. Generally speaking, representation is explained by mapping relation. Accordingly, a model represents the target phenomenon if each member of a domain (the model) is mapped into the members of its co–domain (target phenomenon). If there is such a mapping relation, then the model represents its target; if there is not, the model does not represent its target, namely does not reflect the features of target phenomenon and thus is a misrepresentation.

Scientific models almost never constructed with the aim of complete representation. In other words, models are mostly about specific phenomena and involve idealizations. So, after all, they do not represent the target phenomena completely, in

the first place. However, that does not mean that a model is always a misrepresentation of the empirical phenomenon. Since construction process of any model goes hand in hand with pragmatic purposes of a modeler, a model can correctly represent even without a fully mapping. Here, a correct representation is correct with respect to the purposes of a modeler. Not, for instance, strictly correct as a complete representation. Thus, while it is in one sense a type of representation, it is also a type of misrepresentation. Therefore, the mark of whether a model is a model of a target phenomenon is dependent upon correct representation.

APPENDIX B: MCMULLIN'S TAXONOMY OF IDEALIZATIONS

The first comprehensive taxonomy of idealizations is presented by Ernan McMullin (1985). At bottom, two reasons lie behind this taxonomy. The first reason is historical. In this context, he reveals how idealization techniques were used in modern science. The second reason is epistemological (and it also has a metaphysical importance). In this sense, he gives an argument (by means of idealizations) against the claims of anti-realists. In other words, he attempts to justify the claim that idealizations strengthen the position of the defenders of realism.

McMullin groups different types of idealization under the heading of *Galilean Idealization*. The fact that idealizations are Galilean here does not mean that Galileo invented idealizations (McMullin 1985, p. 248). In a different sense, McMullin gives credit to Galileo, who often and perfectly used idealizations in his era.

Galilean Idealizations fall into three basic categories: Mathematical, Constructive, and Causal idealizations. Construct idealization, subsequently, is divided into two categories: Formal and Material idealizations. In addition to these three basic idealizations, there is also Subjunctive idealization. This type of idealization has causal features as well as having formal ones. Let me illustrate this taxonomy with a chart and then explain, one-by-one, what features each idealization has (see Figure Appx. 2).

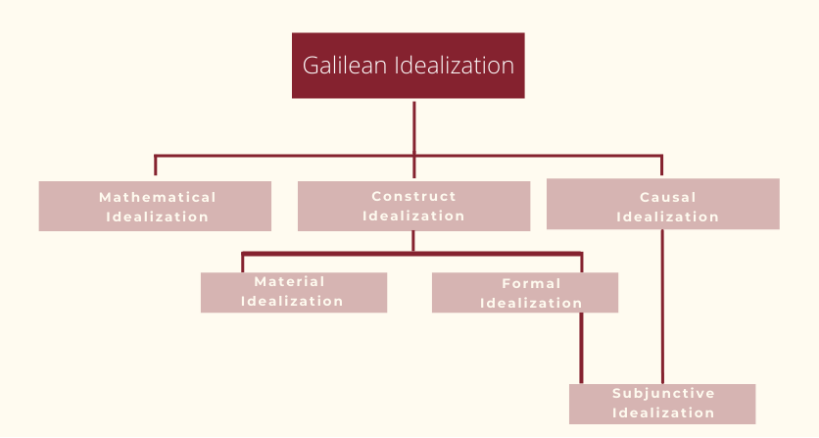

Figure Appx. 2 The chart shows the taxonomy of scientific idealizations put forward by Ernan McMullin (1985).

Mathematical Idealization. This idealization is about having a mathematical way of representing nature. Until modern science, it was thought that there was no such a way (or at least it was thought that it could not be the only way). Empirical method, especially the version emphasized by Aristotelian physics, ignored the mathematical representation of nature, in one sense. Mathematical idealization, in fact, triggered the beginning of modern science. Therefore, this approach not only points to a kind of shift in science, but also constitutes the foundation on which modern science is built.

For McMullin, Galileo's role in this case cannot be underestimated because his perspective underlines the imposition of a mathematical approach on physical situations. However, those who oppose this new approach have raised some challenges. Especially the Aristotelian tradition contends that nature cannot be represented mathematically in an entire fashion. As a matter of fact, this seems to be a fair criticism due to the difficulty of representing the complex nature of natural systems mathematically. But, this is where idealization comes into play. The main characteristic of the Galilean approach is not to represent physical states in purely mathematical form. Rather, this approach "is a matter of imposing a mathematical formalism on a physical situation, in the hope that the essentials of that situation will lend themselves to mathematical representation" (McMullin 1985, p. 254). From this point of view, it seems clear that Galileo's approach to science was wrongly criticized. Although the anti-Galileans do not aim at representing the world completely, they argue that the Galileans do so. However, if the Anti-Galileans insist on the possibility that their empirical methods may one day yield successful results in grasping reality, they overlook the possibility that the Galileans may claim in the same manner.

It is not clear whether McMullin takes mathematical idealization as a common feature of Galilean idealization or as categorically distinct kind of idealization, one that has fundamental properties that excludes others. In general terms, it can be said that mathematical idealization is one of the main feature of Galilean idealization. It can be asserted that one of the main purposes of this kind of idealization has to do with making calculations easier. In a special sense, it can also be argued that some Galilean idealizations are made to reveal causal relationships. A fully mathematical

representation of the physical situation is not specifically intended here. Instead, the essential intention is to highlight the situations in where explanatory irrelevant causal factors do not take place. Whatever McMullin intends to say, the crucial point is that we cannot characterize science, including today, free from mathematical idealization.

Construct Idealization. Construct idealization is concerned with the conceptual representation of the objects. According to McMullin, Galileo was aware that the external reality cannot be directly represented. The earlier Aristotelian understanding, on the other hand, followed the opposite path. Given this tradition, false assumptions, even if they are approximations, should be excluded from scientific enterprise (McMullin 1985, pp. 255-256). According to this view, reality should be represented as it is. However, Galileo's approach is not so strict. Given this view, the correct way to represent reality is not about being completely faithful to reality; but about knowing how far to *get away from reality*. In other words, one can correctly represent a real system by having an idea of how the real situation is idealized or how far we have drifted from reality (nevertheless, according to McMullin, Galileo did not explicitly tell us how one can come up with such an idea). In this manner, reality is not represented as it is, but constructed as models, which can be grasped properly by the mind. Models cannot be constructed as accurate representations of nature, nor can they be constructed by completely ignoring the relevant properties of it. Instead, models should include some parameters and ignore the others.

Construct idealization has two important advantages in this sense. The first is that it simplifies calculations. By means of construct idealization, explanatorily (and perhaps causally) irrelevant factors are not represented by the models (this in turn facilitates mathematical tractability). The second is that it provides explanation by neglecting explanatorily (and perhaps causally) irrelevant factors. Sometimes these irrelevant properties are not contained in the models to easily explain the laws (McMullin 1985, 256-258). Ernan McMullin asserts that construct idealizations have two aspects: formal and material. These two idealizations differ from each other as follows: formal idealizations are the ones to be *corrected*, while material idealizations are the ones that need to be *completed*. Let me briefly discuss these two different aspects.

Formal idealization excludes factors from the model that are known to be relevant but are not necessary for deriving theoretical laws. For this reason, relevant features in models are often "simplified or omitted in order to obtain a result" (McMullin 1985, p. 258). Undoubtedly, the results to be obtained from these models will be "approximate consequences" (McMullin 1985, p. 258).

When it is aimed to achieve more approximate values than the values given by the present model, the add-back process takes place. McMullin illustrates this process by giving an example of de-idealization of the ideal gas model. The ideal gas model does not contain the volume of molecules and the long-range intermolecular forces between them to derive Boyle's Law. In other words, the model idealizes these factors. These factors need to be included in the model to obtain more approximate results. Indeed, the *van der Waals* gas model has been proposed for this. According to this model, the behavior of high-pressure gases is explained in a more approximate way compared to the ideal gas model, when the volume of molecules and the attractions between them are taken into account.

According to McMullin, another important result of formal idealization is that it "support[s] the moderate version of scientific realism" (McMullin 1985, p. 258). An ongoing research program is closely related to formal idealization. It is because the goal of any continuing research program aims at de-idealization to show that the structure represented by the model is real, if not complete. For McMullin, there is a strong reason to believe that such structures really exist, if we obtain more approximate results by de-idealizing the model that represents them. In other words, if the modeled target phenomenon was not a representation of the actual structure, that model would yield a completely different result when it was de-idealized.

Material idealization, another aspect of construct idealization, is the feature that is expected to be filled, not to be corrected, within the conceptual framework of the given theories. According to McMullin, theories are constructs that are "unspecified, though open for question in different context" (McMullin 1985, p. 263). These theories have been left incomplete for certain reasons.

McMullin explains material idealization by exemplifying the *Rutherford atomic model*. For example, the nucleus is left unspecified in this model. McMullin indicates

"that this blank serves as the 'material' out of which an explanation of the scattering experiments can be constructed" (McMullin 1985, p. 263). Later, the research program was expanded to include electrons and protons, specifying the unspecified nucleus within the conceptual framework set by Ernest Rutherford (1871-1937).

Notice that, in this case, the issue is not about a false (or an incorrect) assumption that needs to be corrected. On the contrary, it is an issue of filling a blank left by a particular model. Of course, this 'filling' process shows that the previous model, which can be considered incomplete, has become a better model. But on the other hand, the incomplete model "indirectly validates" itself because the development of the new model took place according to the blanks left by the incomplete model (McMullin 1985, p. 263). Like formal idealization, material idealization also supports the view of scientific realism. As a matter of fact, the blanks of the model, which is a part of a successful scientific research program, were filled properly that led scientists to obtain fruitful results.

Causal Idealization. In general, theories and models are constructions put forward to represent the causes that give rise to the phenomena. While constructing a model, the main focus is likely to centre around the reasons that give rise to the phenomena. Like the way adopted in construct idealization, the aim of the modeler is, in general, to correct the model or make it more specific by adding relevant causes back to the model. McMullin defines these stages as de-idealization processes. However, not every model is made more de-idealized over time than before. This specific type of idealization is called causal idealization by McMullin.

The main difference of causal idealization is that it idealizes the causal relations represented by the model. This means that this idealization can be made only after representation. In other words, the factors that are represented during the construction of the model are idealized step-by-step right after the model is built. This process is about making changes on the causal parts of the model after the model represents its target system.

According to McMullin, this type of idealization distinguishes the Galilean understanding of science from the Aristotelian one. It is also an attitude that takes Galileo to an original level. This attitude is about a perspective towards to the natural

phenomena. It is impossible to treat natural phenomena as they are because there are factors that prevent us from understanding their true nature. That is why Galileo focused on causes one by one. “His insight was that complex causal situations can only be understood by first taking the causal lines separately and then combining them” (McMullin 1985, p. 265). For example, as much as its weight, the factor that affects the motion of an object is air resistance, namely a type of friction which objects exposed to. The main issue here is to see what would happen when this cause is not at work. In this way, Galileo, as the story goes, showed that weight really is insignificant when air resistance is not taken into account.

To be sure, for Anti-Galileans, Galileo went too far with his daring attitude. According to them, it would be doubtful to define nature without considering this factor (*i.e.*, air resistance). At least, naturally, Anti-Galileans demand an empirical element or request a testimony of experience in order to escape from their doubts. This is where Galileo's “experimental idealization” comes into play (McMullin 1985, p. 267). Galileo simply compares the motion of a round object on a polished inclined plane with the motion of a rounder object on a more nicely polished plane. In the more de-idealized state, *i.e.*, where the friction is extremely low, the weight does not have a significant effect. In the perfect state of this situation (*i.e.*, when the causes are totally removed), it can easily be deduced that he is right.

In brief, causal idealization is a process in which the modeler excludes the factor(s) that give rise to the observed phenomenon. The main reason for invoking causal idealization is that to see, for example, “what the body *would* do on its own, apart from the effects of causes (like the resistance of air) external to it” (McMullin 1985, p. 266). When the barrier (like the resistance of air) is removed, one can access such information about phenomena.

Subjunctive Idealization. The last type of idealization is called subjunctive idealization. Although this idealization has a hybrid character, it can basically be associated with thought experiment(s). In other words, this type of idealization is “conceptual instead of experimental” and allows one to isolate causes in thought (McMullin 1985, p. 266).

McMullin argues that Galileo used this type of idealization to explain the nature of projectile motion. To provide a definition, Galileo discusses two idealized situation with regard to the motion of objects. The first situation describes the infinite motion of the object on the vertical plane (without the influence of any factor). The second situation describes the downward motion of the object according to its own weight (without any hindrance). According to Galileo, projectile motion consists of the combination of these two motions. What is new (and ingenious) here is that an observable projectile motion is explained (or at least tried to be proven) by two separate movements that cannot be demonstrated in a physical experiment. In other words, Galileo shows or intends to prove that projectile motion consist of these "two conceptually idealized separate motions" (McMullin 1985, p. 270).

TÜRKÇE GENİŞ ÖZET

1. Bölümde bilim felsefesinin modeller meselesini konu edinişini ve bu konunun zamanla nasıl evrildiğini anlatmak için tarihsel bir arkaplan sundum. Bilim felsefesinin henüz bir disiplin olarak emekleme yıllarında, teorileri nasıl ve neden merkezi bir yere konumlandırdığını araştırdım. Bana göre teori merkezli bilim felsefesinin yapılmasındaki en önemli nedenlerden biri, o dönemde genel görelilik ve kuantum mekaniği gibi büyük teorilerin baskınlığı idi (bkz. §1.2) Yirminci yüzyıl başlarında bilimsel teorilerin, diğer tüm disiplinlerdeki teoriler karşısındaki sarsıcı gücüne kayıtsız kalmak olası gözükmemektedir. Bu yüzden bilimi açıklamak demek aslında teorileri açıklamaktır. Bilim yalnızca teorilerini açıklama verme ya da öndeyi yapma gibi belirli amaçlar için kullanır; bir teorinin doğasının ne olduğuna yönelik incelemelerde bulunmaz. İşte burası felsefenin devreye girdiği yerdir.

Felsefe birer dilsel entite olan bilimsel teorileri biçimsel halde sunabilmenin araçlarına sahiptir (bkz. §1.5.1). Bu yolla bilimsel teoriler mantıksal dilde rasyonel olarak yeninden inşa edilebilir. Rasyonel olarak yeniden inşa etmenin bilim için önemli iki avantajı vardır. İlki bilimin önermelerine ilişkindir. Bir bilimsel önermenin yeterince belirgin ifade edilmesi ve empirik olarak doğrulanabilir olduğu gösterilmesi, onun metafizik bir önerme olmadığını gösterecektir. Bu yolla bilim sonu gelmeyen –kimi felsefi tartışmalara benzer– tartışmaların içinde boğulma riskini asgari düzeye indirecektir. Rasyonel olarak yeniden inşa etmenin ikinci önemli avantajı ise mantıksal bir yapı kurabilme olanağı yaratmasıdır. Bilimsel teorilerin dili sentaktik bir biçimde ifade edilirse çoğu bilimsel problemin özünde belki problem olmadığı ya da sahte–problem olduğu görülebilecektir. Nitekim kimi mantıksal pozitivistlerin genel eğilimi felsefi ya da bilimsel tüm problemlerin *dünya* ile ilgili değil, *dil* ile ilgili olduğu yönündedir. Bu mantıksal meta–dil içinde hangi önermenin hangi önermeden türetildiği açıkça görülebilir. Bu da gündelik dilde tespit edemediğimiz problemleri, mantıksal dilde avantajlı biçimde tespit edebileceğimizi göstermektedir.

Ne var ki, bu oldukça görkemli sayılabilecek proje bir süre sonra kimi zorluklarla karşılaşmıştır. Kısaca söylemek gerekirse, bilimsel teorileri rasyonel olarak yeninden inşa etmek pek de görüldüğü kadar kolay bir iş değildir. Biçimlendirme (diğer bir

ifadeyle *formalizasyon*) çabası teorilere kesinlik kazandırmaktaki yaşadığı sıkıntılar bir yana, çok daha yeni problemleri karşımıza çıkarmıştır. Bu noktada bilim felsefesinde semantik görüş olarak adlandırılan bir görüş ortaya çıkmıştır. Ancak bu görüş kendi içinde ikiye ayrılmaktadır. Bir taraftaki semantikçiler teori biçimlendirmesi için kullanılan araçların (özellikle sembolik mantığın araçlarının) uygun olmadığı yönünde fikir geliştirir. Bu anlamda, bu görüş farklı (örneğin, durum uzayı yaklaşımı gibi) biçimsel araçlara başvurulması gerektiğini vurgular. Diğer taraftakiler ise bilim felsefesinin biçimlendirme idealinden vazgeçmesi yönünde konum alır. Bu görüşe göre meselenin özü bilimsel teorilerin ve modellerin pratikte nasıl kullanıldığına bakmakta yatar. Semantik görüşün ne olduğunu ve bilimin sentaktik nitelendirmesini nasıl eleştirdiğini §1.5.2'de detaylı olarak takdim ettim.

Diğer taraftan sentaktik görüşün *yalnızca* semantik görüşü savunanların eleştirilerine maruz kaldığını söylemek yanlış olacaktır. Bu görüş ortaya çıkmazdan önce kendilerinin bilime ilişkin kimi görüşleri birçok bakımdan zaten zayıflatılmıştır. Daha detaylı değinmek gerekirse, doğrulama ölçütünün karşısında yanlışlama ölçütünün koyulması; analitik ve sentetik ayrımının problemli statüsü; tüm gözlemlerin kuram–yüklü olması ve buna benzer eleştiriler zaten sentaktik görüşü yeterince yıpratmıştır. Semantik görüş aslında bu yıpranmış olan görüşe sadece son darbeyi vurmuştur. Ne var ki, bu tez modellere ilişkin problemlere odaklandığı için yalnızca semantik görüşün eleştirileri bağlamında konuyu ele almıştır. 1. Bölümün tüm amacı sonraki bölümlerde tartışılacak ve modellere ilişkin problemlerin kökünün nerede olduğunu gösterecek zemini hazırlamaktan ibarettir çünkü güncel bilim felsefesi tartışmalarında bu tarihsel kısım kimi zaman gözden kaçmaktadır. Başka deyişle, kimi yerlerde, modellerin sanki başlı başına bilim felsefesine konu olacak bazı zorluklar barındırdığı varsayımı ile hareket edilmektedir. Hâlbuki mesele aslında yağmurdan kaçarken doluya tutulmaktır. Yani spesifik olarak söylemek gerekirse, sentaktik görüşten sakınıp, semantik görüşe yaklaşmak beraberinde çözülmesi güç bambaşka problemler getirmektedir.

2. Bölüm tam da bu problemlerden birini göstermeyi hedeflemektedir. §1.4'te anlattığım üzere sentaktik görüşü savunan kimi bilim felsefecileri, felsefi problemlerin kökünü kesin bir dilden yoksun olmamıza bağlayan *Wittgensteincı* görüşü benimsemekteydi. Bu nedenle mantığın kesin dilinin bu problemleri çözmede

ya da onların sahte problem olduğunu göstermede bir araç olduğuna inanıyorlardı. Kimi pozitivistlere göre bilimin dili şüphesiz metafiziğe göre kesindi ama kendi içinde de kesin olması gerekiyordu. Felsefenin rolü bu kesin dili bilime temin etmek idi. Bunun için bilimsel teorileri aksiyomatize etme projesi devreye sokuldu.

Model merkezli bilim anlayışının ise bu ideali ikame etmesi hiç de kolay değildir. En başta modeller gerçekliğin idealize ve soyut temsilleridir. Dolayısıyla, modeller ile gerçeklik arasında kesin bir eşlem bulunmamaktadır. Modellerin gerçeklik ile kesinlik ilişkisi olmayışı aslında nesnel bir temsilin de önünü tıkamaktadır. İdealize bir model bütünüyle olgulara tekabül edemez. Böylece temsil ilişkisi mantıksal pozitivistlerin savunduğu gibi temel önermeler ile olgular arasında bir hakikat ilişkisi değildir. Bir model olguların bir yönünü temsil ederken, diğer yönlerini göz ardı eder. Diğer bir model ise göz ardı edilen yönleri temsil ederken başka olgulara ait başka yönleri içermez.

3. ve 4. Bölümler kesinlik idealinden vazgeçildiğinde ne tür yeni problemlerle karşılaştığımızı göstermeyi hedeflemektedir. Bölüm 3 bilimsel temsil meselesine odaklanmaktadır. Bu noktada meselenin kesinlik idealine bağlı olduğunu söylemek uygun olacaktır. Modeller hedef sistemine benzer ya da izomorfik olabilir. 3. Bölümün temel amacı bu temsil ilişkisinin nasıl kurulduğuna ilişkin görüşleri takdim etmektir. 4. Bölüm ise kesinlik ile idealize modeller ilişkisini açığa çıkarmaktadır. Gerçekliğin kesin olmayan idealize temsilleri nasıl olur da bir açıklama temin eder? Bu bölümde birbirleri ile uyumsuz olan görüşlerin nasıl konumlandığı gösterilmek istenmiştir.

5. Bölüm tezin sonraki bölümlerinde sırasıyla değineceğim temsil, soyutlama, idealizasyon ve de–idealizasyon meselelerine yaklaşım getireceğim bir vaka çalışması içermektedir. Bu bölümde majör buzul çağlarının içinde yaşanan ara–buzul dönemlerinin açıklamasını veren Milankovitch modeline değinmekteyim. Günümüzde en geçerli buzul modellerinden biri olarak görülen bu modele değinmeden önce daha önceki modellerin kısa bir tarihçesini sunmaktayım. Bu tarihçe Milankovitch modelinin, önceki diğer modellere kıyasla, neyi başardığını göstermektedir. Bununla birlikte Milankovitch'in buzul çağlarına ilişkin astronomik modelini inşa ederken ve ettikten sonra yaşadığı kimi entelektüel ve sosyolojik

zorlukları da tarihsel olmayan bir perspektiften anlatıyorum. Buradaki amacım da daha sonraki bölümlerde bu vaka çalışmasından çıkaracağım felsefi sonuçlara zemin hazırlamaktır.

6. Bölümde §3.5.1'de ve §3.5.2.'de incelemiş olduğum temsilin majör görüşlerini tekrar eleştirel bir gözle ele almaktayım. Bu majör görüşler zaten kendi içlerinde kimi eksikliklere sahiptir. Özellikle belirtmek gerekirse, bu eksiklikler temsilin mantıksal özellikleri ile uyumsuzluğundan kaynaklanmaktadır. Bununla birlikte bu majör görüşler idealize ve soyut modelleri açıklamakta da yetersiz kalmaktadırlar. Bu nedenle ben literatürde çözüme en yaklaşan görüş olan kısmi izomorfizmi bu bölümde takdim ettim. Bu görüşün avantajını idealizasyonu ve soyutlamayı diğer iki majör görüşe göre daha iyi açıklaması olarak saptadım. 5. Bölümde takdim ettiğim vaka çalışması üzerinden kısmi izomorfizmin nasıl iş gördüğünü anlattım. Buna ek olarak 3. Bölümde bilimsel temsilin özel bir problem olmadığını savunan görüşe de karşı çıkarak, en azından temsil ilişkisinin salt keyfi bir zihinsel edimden ibaret olmadığını savundum.

7. Bölüm literatürde henüz kendisine çok da fazla yer bulamamış bir meseleyi içermektedir. Bu mesele idealizasyon stratejisinin tersi olarak ifade edebileceğimiz de–idealizasyon meselesidir. De–idealizasyon her ne kadar idealizasyon meselesi kadar bilim felsefesi literatüründe ilgi görmese de, aslında bilim pratiğinde çok yaygın bir şekilde tatbik edilen bir stratejidir. Buradaki asıl mesele, söz konusu stratejinin epistemik anlamda ne tür katkılarının olduğunu belirlemektedir. Literatürde doğrudan değinilmeyen görüşleri ele aldığımızda, de–idealizasyonun iki temel işlevi ortaya çıkmaktadır. Bununla birlikte 5. Bölümde takdim ettiğim vaka çalışması aracılığıyla bu ikisine ek olarak yeni bir işlev daha gösteriyorum. Bildiğim kadarıyla de–idealizasyonun beyhude bir strateji olduğunu söyleyen yok ancak neden beyhude olmadığını temellendiren de yok. Bu bölüm en azından de–idealizasyon meselesindeki bu açığı kapatmayı hedeflemektedir.

De–idealize edilen idealizasyonların model açısından epistemik bir işlevi olduğunu göstermek akla hemen bir başka soruyu getirmekte: soyutlamaları somutlaştırmak modelin çıktılarına herhangi bir epistemik katkı sağlar mı? 8. Bölüm doğrudan idealizasyon ve soyutlama arasındaki farkların ne olduğunu göstermeyi

hedeflemektedir. Tahmin edileceği üzere modellerde idealizasyona nazaran çok daha fazla soyutlama içerilmektedir. Bunları eleyip modeli daha somut hale getirmek bir modeli de–idealize etmek kadar faydalı olabilir mi? Bu soruya yanıtım olumsuz çünkü soyutlamaları elemek ve modeli daha somut durumu getirmek daha detaylı bir temsil temin etse de epistemik anlamda herhangi bir katkı vermemektedir. Buna dair en önemli neden soyutlamaların çoğu zaman açıklayıcılık bakımından gereksiz faktörler olmasıdır. Bu açıdan bakıldığında soyutlama ve idealizasyonların birbirlerinden birçok bakımdan ayrıldığını öne sürüyorum.

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