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MODEL THEORY AND EXACTNESS OF SCIENTIFIC REPRESENTATION

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ТЕОРИЈА МОДЕЛА И ЕГЗАКТНОСТ НАУЧНОГ ПРЕДСТАВЉАЊА

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Gratitude

I am endlessly grateful to my mentor whose wise advices and incessant tolerance have enabled me to tackle this challenging philosophical discipline.

I dedicate this dissertation to my girlfriend Jovana, without whose cunning intelligence I would never recognize the dubious advantages of academic education.

Abstract

Models play a central role in many scientific contexts, which makes model theory one of the focal points of the contemporary philosophy of science. The topic of this doctoral thesis is the gradual constitution of model theory during the last half-century, starting with the abandonment of the logical positivism program and the pioneering works of Suppes and others. The succeeding semantic approach, which resulted in emphasis of scientific models, grew to the dominant contemporary position in understanding science. However, reconstruction of scientific constructs and scientific reasoning by mathematics, the common tool of the semantic approach, spawns many difficulties, specifically the ones related to the scientific practice and the application of scientific theories. I will argue that, in spite of the significant advantages gained with the semantic model-revolution, reconstructing science by formal tools is futile, most notably on the grounds that practice-analysis reveals that, even in the most exact sciences, mathematics is used in limited and approximating ways, suitably adapted to the specific scientific needs at hand. We’ll build on the analyses revealing the improvising character of models and science in general, and try to strengthen the conception of models as the main cognitive and representational instruments of modern science.

Our main goal will be to unify diverse critiques of the semantic orthodoxy, in order to give a unitary, practice-oriented account of modeling in science. We'll try to accomplish this by evolving similarity-based approach in such a way as to preserve model related insights of the sematic approach and by employing a functionalistic analysis of models. This way, we hope, we'll distance ourselves from the restrictive formalisms and develop a framework able to accommodate both scientific cognitive reality, notably its human aspect, and her methodological openness we so much cherish.

Key words: models, theories, the semantic approach, mathematical reconstruction, idealization and approximation, the practice turn, scientific representation, methodological openness, unified model theory, naturalistic epistemology.

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Резиме

Модели играју централну улогу у многим начним контекстима, што чини теорију модела једном од фокалних тачака савремене филозофије науке. Тема ове докторске дисертације је постепено конституисање теорије модела током последњих пола века, почевши са напуштањем програма логичког позитивизма и пионирским радовима Супеса и других аутора. Семантички приступ научним конструктима, који је наследио доминантну позицију у савременом разумевању науке, резултовао је у оштром истицању модела присутних у науци. Ипак, реконструкција научних конструката и научног резоновања помоћу математике, која је уобичајена алатка семантичког приступа, производи многе потешкоће, посебно оне везане за нучну праксу и примену научних теорија. Ја ћу тврдити да, упркос значајним предностима добивеним семантичком моделском ревоулицијом, реконструисање науке формалним средствима је у крајњој линији узалудно, првенствено зато што анализа научне праксе открива да се, чак и у најегзактнијим наукама, математика употребљава на ограничен и апроксимативан начин, погодно прилагођен специфичним научним потребама датог случаја. Ослањајући се на анализе које откривају импровизирајући карактер модела и науке уопште, покушаћемо заправо да ојачамо схватање модела као главних сазнајних и представљачких инструмената модерне науке.

Главни циљ ове дисертације биће да обједини разнолике критике семантичке ортодоксије, у циљу да пружи јединствени приступ научног моделовања усмерен на праксу. Ово ћемо покушати да постигнемо путем развијања приступа заснованог на појму сличности, истовремено чувајући с моделима повезане увиде семантичког приступа и примењујући функционалистичку анализу модела. Овим методом, надамо се, дистанцираћемо се од ограничавајућих формализама и развити оквир способан да прихвати и научну когнитивну реалност, наиме њен "људски аспект", и њену методолошку отворност коју толико ценимо.

Кључне речи: модели, семантички приступ, математичка реконструкција, идеализација и апроксимација, заокрет ка пракси, научно представљање, методолошка отвореност, обједињена теорија модела, натуралистичка епистемологија.

Научна област: Филозофија

Ужа научна област: Филозофија науке

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Part I

Models in Science and Philosophy

Introduction. Towards a unified approach.

Reconsidering the place of models in science. The search for a unified theory of scientific models.

Introduction

Models play a central role in the scientific discourse. Their essential significance manifests itself in the first place in organizing and acquiring scientific knowledge. A great deal of scientific work is dedicated to building, application, testing and interpretation of models, and models such as the liquid drop model of the atomic nucleus, the standard model of particle physics, expanding model of the universe, irreversible processes models in statistical mechanics, the double helix model of the DNA, gene frequency dynamics models in population genetics, the Solow–Swan model of economic growth, and many others, each play a dominating role in their respective fields.

Diverse modeling techniques and model usages, together with the diverse usage of the term 'model' by the different scientists, sometimes even within the same field, make a general and informative definition of modeling a vague possibility. Furthermore, philosophers have just recently started to pay significant attention to the scientific models and their roles in the scientific practice. From these analyses, especially the early ones, identification of certain kinds of models resulted. In a short time it became a widespread understanding that models are the main cognitive and representational instruments of modern science and differentiating model-types through different model-usages soon followed. Scale models, such as a cardboard model of the Thames's bridge, which model their target systems as a simplified and scaled down physical objects; mathematical models, such as the ideal gas model in statistical mechanics, which employs descriptions in mathematical language; iconic model, such as a graph or picture of the mentioned bridge, which are emphasizing certain features of interest; analogical models, such as the model of the atom as the solar system, which uses representation of one system and, on the basis of similarity, apply it to another; computer simulations which digitally model their target systems and their evolution; phenomenological models which ensembles the data in a unified form; theoretical models which instantiate theories; toy models; impoverished models; idealized models; experimental models; developmental models, and many others fall into descriptions used by philosophers to classify models.

These many characterizations of models present in the philosophical literature are neither mutually exclusive nor posited from the same philosophical standards. Often mixing epistemological problems with ontological ones, or representational functions of models with their representational means, these overlapping categories fail to depict the common function and the steering conceptual processes of all different kinds of models. Finding a unified perspective on this plethora of analyses is necessary if we are to gain a better understanding of the scientific models and thus it became the main objective of general model theory. The diversity of scientific constructs and usages subsumed under the term ‘modeling’, we argue, is not an excuse for the heterogenous, unorganized philosophical approach to this diversity. In short, if we are to represent the messy scientific diversity, we believe, a unifying philosophical framework must be acquired.

Of course, we must have in our mind that the idea of general model theory can be criticized from the perspective of the specific usage of models in different sciences. Modeling might simply be case specific, developed in a number of ways, each corresponding to the peculiar circumstances of, say, a scientific field or even subdiscipline. The resulting family resemblance, strengthen with the sheer number of previously identified model-types, might not be enough for a general framework to be possible. Precisely for this reason, together with our belief in inappropriateness of prescriptive epistemological definitions, we will not opt for a search of necessary and sufficient conditions of modeling. Trying to bring into harmony our main methodological drive of constructing model theory on the basis of the real cases of scientific modeling and, at the same time, providing a rational reconstruction of it, we will search only for minimalistic, descriptive features of models and modeling in the sciences. In this aim, we will examine one after another, in our mind, exemplary cases of scientific models and try to case-by-case augment our philosophical conception. Parallelly we will analyze, by our judgment, the best philosophical attempts of a general approach and try to broaden them with the problematic cases into a unified and precise framework. Whether this will prove to be possible is a question which we will yet need to answer.

Towards a unified approach

Opposite to some contemporary views [e.g. Frig & Hartman 2012], it seems to us that numerous divisions and classifications of models, by resulting in mutually non-exclusive categories, are hinting towards a unified concept. We often find in the literature same cases illustrating different characterizations of models. For example, is the billiard ball model of a gas iconic [Suppe 1977] or analogical model [Hesse 1966]? Is the Bohr model of the atom analogical [Black 1962] or theoretical model [Achinstein 1968]? Well in both cases models are arguably both; yet these examples are repeatedly used to illustrate specific classifications. For us, these practices by, arguably, describing only aspects of the same models are only contributing to the possibility of a unified approach. Since most models contain an element of abstraction or idealization, different 'kinds' of models might be only different generic ways of application of abstraction-idealization procedures. Also, since any model represents something else, whether physical system or ideal (theoretical) construct and whether by physical or linguistical means, connection with the notion of representation emerges as crucial. Deeper investigation into these processes (or characteristics of models) is needed for the basis of a general approach to model theory, and we will devote to it a significant part of our dissertation. We will focus specifically on the problems of representation, idealization, abstraction and approximation in models, and to the independence of models in both construction and in performing their epistemic functions.

In addition, models which are most usually the objects of the philosophy of science are mathematical models. Representation by this kind of models is philosophically most intriguing because, due to the mathematical character of natural laws, these models are closely connected with scientific, specifically physical theories. As physicist themselves mainly focus on mathematical models, philosophers long considered other kinds of models as analogical, metaphor-like devices, playing important heuristic roles but not being the vehicles of the scientific knowledge [Bailer-Jones 2009]. However, since many of these auxiliary models are also mathematical constructs, we now can observe that clear-cut division between representative and explanatory models on the one side and auxiliary, heuristic models on the other cannot be maintained. Take for example cartoon models of protein interaction in cell biology [see Edelstein-Keshet 2004]! Although these models do not accurately describe the systems studied and require more precise mathematical models for simulating and predicting protein behavior, they are easy to visualize and provide important psychological! insights into the cellular dynamics. This cognitive perspective is not specifically peculiar at all, since modeling is usually done in science by reducing the number of dimensions, fields or variables, or restricting them to a particular symmetric form. Some models, even started as oversimplified toys, grow to hold important positions in the growth of knowledge or serve as pivots in their respective fields. Take for example the Ising model for ferromagnetism which although originated as a two-dimensional toy problem given to Ising by his mentor, became an invaluable insight into ferromagnetism and the basis for further development of statistical mechanics. Even clearer case is Crick and Watson's tin and cardboard DNA model whose unifying simplicity prompted revolution in modern biology. There were previous results such as the X-ray diffraction images of the DNA, specifically crystallograph patterns obtained by Rosalind Franklin (so called "B form") and other detailed information about the DNA molecule which, however, failed to impact the field the model did. Cognitive appeal of models here seems strikingly apt for disseminating the concepts and methodologies throughout the scientific community. Perhaps the most striking case of this impact of models which began as 'toys' are Feynman's diagrams whose shocking simplicity extruded and replaced extremely difficult to solve Dirac equations, and more importantly laborious and time consuming, and impose themselves as the rule for doing contemporary quantum electro dynamics. Here we see that even deliberately oversimplified modeling has a potential for improving science, especially since almost all models, including toy or pedagogical, contain some degree of mathematics.

But models in general are far from being oversimplified imprecise toys. To distinguish among these more exact models we will try to draw on the physicists' interests, and their corresponding usage of the term 'model', which seems keen to distinguish models by the methods of their construction [Portides 2008]. In this manner, models are usually divided on theory-driven and phenomenological models. Importantly, physicists, unlike philosophers, do not relate the distinction between theoretical and phenomenological with the distinction between observable and unobservable. Since Cartwright [1983] diverted attention to this discrepancy, philosophers gradually began to adapt their usage of these terms to the one used by physicists. It is widespread today to understand theoretical model as a direct (deductive or structural) product of a theory, supplemented by locally operative hypothesis, usually a specification of initial conditions. In short, they are constructed in way systematically guided by theory. Phenomenological models are constructed in heterogeneous, less organized manner, by the use of ad hoc hypotheses, semi-empirical results, or by a conceptual and mathematical apparatus not directly derivable, or even connected with a theory. There are analogous distinctions among scientific laws or theories, with the difference that focus here is not on the derivability from theory (nor in observability), but on *explanation* of appearances, as opposed to their description. There are phenomenological theories of superconductors or phenomenological models of meson-nucleon collisions and, also, fundamental theories or theoretical models of 'more directly' observable systems, such as planetary motions or the physical pendulum. Grouping models in the theoretical/phenomenological categories, together with their common nature as idealized constructs and vehicles of representation, both commonly achieved through mathematical means, provide the grounds on which we will explore the possibility of a unified account of models and modeling in science.

Reconsidering the place of models in science

This brings us to the problem of determining, on the one side, the relation of models to scientific theories and, on the other side, the relation of models to the physical world. Here we have a mass of unknowns and even more questions. What is a scientific theory and how does it relate to the world? What are models? How they produce scientific knowledge? How do they differ from theories? How models represent while containing abstract and idealized elements? What is the relation of their construction to their function? Are there exact models? Do we know precisely how our picture of the world is distorted? Do we compensate it by being aware of it? Leaving these and similar question open for now, we can say, firstly, that models are at the heart of scientific experimentation, observation, instrumentation and experimental design, organizing and interpreting data in scientifically meaningful ways. Secondly, abstract theories are often impossible to relate straightforwardly to the messy physical circumstances and, in many cases, models are necessary for theory application, to instantiate them in empirically testable forms. In this sense, we can say that modeling lies at the center between scientific theories and experimental data, relating the two in many cases, or standing alone where a theory is not available.

By putting models in the center stage of the scientific enterprise, model theory challenges much conventional philosophical wisdom. Traditionally, models were completely neglected by philosophers; until the early 20th century, where they played only a subsidiary role in the logical positivism, determined more by their philosophical agendas than investigations into the actual scientific practices [Suppe 1989; for a detail and comprehensive analysis of early history of models see Bailer-Jones 2009]. First changes in the philosophical approach to science were propelled by the works of Hesse [1953] and Hutten [1954], who demonstrated and pushed for analysis of *actual examples* from the scientific practice before building our picture of science [Hutten 1954, 283]. By promoting the importance of the case studies, they served as avant-garde of the 1980s revolution in the studies of science known as the practice turn [Hacking 1983; Cartwright 1983; Galison 1987]. Step by step, the understanding gained by focusing on scientific constructs, their mutual relations, and how they serve as vehicles of scientific knowledge resulted with enhanced understanding of nature of theories and experimentation, and their epistemic roles – and, most importantly, place of models in science.

The ensuing several decades brought the bewildering abundance of model analysis we mentioned at the beginning. Many epistemic roles emerged, various representational means were detected, and unexpecting constructional methods obtruded. Growing concern to understand scientific modeling resulted in proliferation of model-types and a looming conviction that a general and unitary approach to models is not possible. Black and Hesse were especially significant for setting the stage for a unified model theory by exploring the idea that all models are, abstractly speaking, icons [Black 1962] or analogies [Hesse 1963] in the sense that they embody actual features of target systems and include some irrelevant (yet necessary for representation) attributes.

However, these early attempts of unification were overwhelmed by the growing abundance of case-by-case analyses which, together with the looming internal failure of logico-positivistic program [Achinstein 1968], formed the basis for the view that the nature and usage of models in science is too perplex to comprehend by unitary, specifically logical analysis. Ensuing acknowledgment of the complexities and diverse natures of models prompted reconsideration of the position they occupy between theories and experimentation, and the appreciation of their unique epistemic roles.

At the same time with the above pioneering philosophical case studies of the scientific models, a wider approach to the philosophy of science, emerging from the set-theoretical model theory was forming. Instigated by von Neumann, Beth and, specifically, by Tarski's development of set-theory semantics, the works of Suppes, Suppe, van Fraassen and others formulated a unitary perspective on science, centered around models. The crucial advancement was that previous problems are bypassed, or so it seems, by describing models in mathematical, instead of meta-mathematical terms. However, accordance of this approach with the above mentioned model-diversity standpoint based in the pragmatic turn is questionable, and a great deal of our work will be devoted investigating it. We will specifically focus on the development of the Achinstein-Hesse line on complexity of the actual modeling practice by Cartwright, Redhead, Morgan and Morrison, Suarez, Teller, and some others. Confronting this later line with the mathematical approach, notably to its most developed partial structures formulation [French and da Costa], will be the main task of the middle part of this dissertation. In a sense, we will use the former to test the later.

The search for a unified theory of scientific models

From the methodological point of view, by challenging the formalistic approaches, our prime interest will not be to deconstruct their certain instantiations – concrete encompassing positions on models and science they spawn. We will focus primarily on the 'snapshots' into the nature and function of scientific models these confrontations illuminate, and try to extricate them as the building blocks for a possible unification. In other words, we will search for the middle ground which incorporates both the results of the semantic and of the practice model revolutions. On that basis we will try in the last part of this dissertation to formulate a framework for the new unifying approach to scientific modeling. Apart from the incorporation of the results scattered through the literature, often in the form of critique of the unification attempts and the semantic approach specifically, we will build on the unification works of several contemporary authors including Suarez, Frigg, Teller, Contessa, and, perhaps most notably, Giere. We'll try to include, specifically, Teller's unifying work on exactness of models [2003], Contessa's on non-formal framework [2006], and Giere's agent based approach [2010].

Our main drive will be to broaden the narrow view on modeling through mathematical and metamathematical philosophical tools, and delineate a picture which perceives the improvising way scientist use artificial languages, notably mathematics [Feynman 1967]. Our thesis regarding the dominating views on models will be that, by focusing exclusively on the structure of models, they have neglected or even obscured the wider picture of modeling, specifically its practice and nonformal aspects. We will try to liberate the impressive philosophical advances of these views out of infelicitous formalizations and their artifacts. In a sense, this is a modern continuation of the old Humean empiristic struggle against the mathematics and deduction as the models of the scientific cognition. To stay in the Achinstein-Hesse-Cartwright-Morrison descriptive approach to scientific modeling to which we incline, we will methodologically be committed to a functionalistic approach, where the search for a unified model theory will be conceived in the first place as a generalization of the examined cases of scientific practice, without any normative positions regarding modeling. This informal, pragmatic framework, of course, remains under construction and we will try to delineate only its basic features necessary for an integrative approach. The focus will be on preserving the diversity and pluralism of models, emphasizing at the same time the constructional and functional autonomy of models and human aspects in modeling, notably its fit-to-the-purpose character and representation only as a means of knowledge acquisition, subordinated to the ends of scientific enquiry. In a wider sense, we'll try to defend the unique place of models in science by trying to sketch a coherent and unified position, engulfing diverse multitude of critiques of the semantic orthodoxy.

Part II

The Semantic Approach to Models and its Problems

Relevant Prehistory – the Received View.

The Semantic Approach. Challenging the Mt's dominance _

Idealization and approximation in scientific practice.

Structural Reconstruction Reenvisioned.

Feynmanian Perspective.

Introduction

Part II aims at presenting the dominant unifying views on scientific models in modern philosophy of science and its key problems. I will try to show that although the semantic approach attributes to models the central role in science, it manages to explain only the part of philosophically interesting characteristics of models. The problems of the semantic approach on which I will focus are: 1) its inability to explain the constructional and, thereby, representational and explanatory autonomy of models, 2) evolution of models, and 3) approximation and idealization as mechanisms of model construction.

Looking from a broader perspective, in the last hundred years or so the philosophy of science has been mainly focused on the search for an integrated description of theories and models. A mature form of logical positivism, i.e. the received view of it, has long dominated the understanding of these scientific constructs. It represents the relevant prehistory of modern views on theories and models which are the object of this dissertation. Although the received view (RV) is abandoned when Carl Hempel, its main advocate, gave up on its further revision [Hempel 1974], some 'received' views remained widespread among philosophers of science. Reason being that it is abandoned first and foremost out of technical problems regarding its vehicle for displaying the structure of theories and models – namely predicate logic, and not due to its failed attempts to reconcile the RV with the history of science [Suppe 2000]. Relevant prehistory for modern views on scientific modeling, and with that on scientific theories, concerns mainly the later point – incapability of the RV to properly individuate important aspects of scientific practice, due to its focus on philosophical agendas instead on studying science, and its inability to cope with bewildering array of important insights into the history of science supplied in the last few decades [Bailer-Jones 2009].

The transition from the logic-driven unification was provided by the discovery that scientific theories can be represented directly – by informal set-theory [McKinsey, Sugar, and Suppes 1953; Suppes 1957]. Since this discovery, proliferation of insights in scientific models emerged, and the search for a new unifying approach began. The bulk of the most important attempts to this end can be classified in the same group – the so called semantic approach [Beth, Suppe, Suppes, van Fraassen, Sneed, Giere, etc.]. In this part of dissertation we will examine whether this approach actually provides a unification of the most important known aspects of the scientific modeling and theorizing, or is it also entangled in technical problems of its science-reconstructing formalism, similarly to what happened to the received view with its metamathematical means. Our thesis will be that it is and that, in spite of tremendous advances, axiomatic reconstructions haven't managed to perform the function they were purported to do. Consequently, in order to properly grasp the scientific constructs we opt for abandoning rigorous reconstructions of models and theories, including mathematical reconstruction, because scientific methodology arguably does not follow nor fit in a such imagined mathematical strictness. The emerging perspective which we share is that science is only a refinement of everyday thinking and, in the course of this analysis, we hope we will show why the partially amended natural language is the best means for her representation and understanding.

Chapter 1

Relevant Prehistory – the Received View

Theories. Where the models fit the picture? Achinstein and the disconfirming scientific practice. Formal problems. Two tracks of philosophy of science.

This chapter will consider some premises of logico-positivistic heirloom which, it seems to us, are still present in contemporary philosophy of science, and are specifically relevant for understanding the semantic approach. The epistemic heart of Logical Positivism was the Received View on theories. Its view on models hinges on its view on theories, which forms the early framework of philosophical interpretations of science and, at the same time, establishes a broader picture of how we came to investigate sciences in the philosophy.

Theories

The Received View, also known as 'syntactic' view, gets its name from the way it represents the structure of theories syntactically in terms of first order logic. Basic elements of technical apparatus for its deductive logico-linguistic expression date back up to Rudolf Carnap [1939; 1949; 1956; 1958]. According to him, a theory is a collection of statements and can be formally reconstructed as axiomatic calculus, in which theoretical terms are given with partial observation interpretation (and with it the empirical content). This is done by correspondence rules, which function as some kind of dictionary which relates terms from theoretical vocabulary (language in which theory is expressed) to terms from observational vocabulary. Observational statements are considered as being true of the real systems in the world, and they give support to the theoretical statements by connecting them to the world.

On this view, theory is, strictly speaking, a logico-linguistic entity. Accordingly, we can discern the following components:

(i) an abstract formalism F;

(ii) a set of theoretical postulates (axioms) T; and,

(iii) a set of correspondence rules C.

F consists out of a language L in which the theory is formulated, and a deductive calculus defined. L contains logical and nonlogical terms, and the latter can be divided into a set of theoretical terms and a set of observational terms. The correspondence rules relate the former to the latter, thereby (partially) interpreting theoretical vocabulary in descriptive terms [see Carnap 56, 46-7]. In a case where the language is already given, the scientific theory is the conjunction TC of the theoretical postulates T and the correspondence rules C.

A model for a theory is gained by substitution of certain predicate symbols by interpreted predicates, certain individual constants by interpreted constants, etc., in the underlying formal calculus [for e.g. Nagel 1961; or Braithwaite 1962]. This way the model partially determines meaning to the symbols of formal calculus [Hutten 1954]. Carnap, for that matter, uses the phrases "constructing models" and "giving interpretations" synonymously [Carnap 1942]. This blurs the distinction between theories and models up to the point of indistinguishability – it turns out that a model for a theory T is just another theory M which corresponds to T regarding its deductive structure.[1] Hence, a model is only a different interpretation of the theory's formal calculus and can be regarded as "little theory" [French 2008], with appropriate formal structure.

Where the models fit the picture?

This opens up a question of the role and importance of models in the scientific enterprise. If a model is structurally identical interpretation of the theory, what is its epistemic significance, if there is any? This question is commonly tackled in the RV by pointing out that models will, either conceptually or visually typically refer to objects and processes more familiar than those referred by theory [e.g. Nagel 1961, 90]. For example, the Bohr model of the atom utilizes the conceptual apparatus of Newtonian cosmology by using properties of the Solar system to investigate the physics of the atom [e.g. Hesse 1966]. On this ground, two different stances are set forth in the RV camp.

[1] Braithwaite states: "A model for a theory T is another theory M which corresponds to the theory T in respect of deductive structure. *By correspondence in deductive structure between M and T is meant that there is a one-one correlation between the concepts of T and those of M which gives rise to a one-one correlation between the propositions of T and those of M"* [1962, 225] (italics added).

Carnap forcefully claimed that the above role of models is only pedagogical or heuristic at best – essentially they have aesthetic or didactic value [Carnap 1939; Braithwaite 1962]. Part of motivation for this sidetracking of models was the insight that a model, and understanding that goes with it, simply doesn't exist for (standardly interpreted) quantum mechanics, which ultimately took over the place of Bohr's explanation of atom. Theoretical meaning and understanding, it is claimed, are gained contextually by relations among theoretical terms, and by relations among them and observational terms claimed by correspondence rules. The model here is practically unnecessary, and is effectively discarded.

The above approach on epistemic significance of models was weakened by some logical-positivists by acknowledging that a model, as interpretation in more familiar terms, can contribute to our understanding of the theory [Hutten 1954; Nagel 1961; Hesse 1963]. In a sense, this was just a concession of the RV to the widespread usage of the term. Hutten points out that the bare fact that the term 'model' is used by scientist as epistemically relevant is itself enough to steer us to adapt our philosophical notion of it [1954, 285]. Regarding this he finds models misleading [ibid., 296], in the sense of having some openly false characteristics. Hesse explored this line of argument further and claimed that interpretation in familiar terms enables models to function as *analogies* [1963, 8], which include some disagreement of properties and relations in the form of negative analogy.[2] These steps introduced models into the philosophical analysis of science and were one of the first to make a turn to historical case studies of real scientific models "instead of philosophical illustrations of old-fashioned and very simplified examples" [Hutten 54, 284]. Although Hutten and Hesse stayed within the boundaries of the hypothetico-deductive approach to theories and models, they, together with others, started an anti-formalistic approach to theories and models, and thereby undermined the possibility of a unified philosophical approach to them. Most significantly, their work will later on lead to many new, detailed case studies of science and especially modeling – which forms basic pillars of our current view on science, which we will explore later on in this dissertation, during the analysis of conceptually

[2] But also some veridically unknown statements or features which form the crucial neutral analogy. Neutral analogy refers to those aspects for which commonality or difference is yet to be established. Some of these features are those which we eventually transfer and thereby import new content to our knowledge, and hence they make models epistemically significant (e.g., knowledge of the mechanics of billiard balls can be used to make predictions about the expected behavior of gases).

unified frameworks. All this shaken the RV fundamentally, and forced it to further and further revisions; but that became clear only in the following decades.

Returning to the RV, both of its approaches essentially declared the role of models as subsidiary – in their rational reconstruction of scientific knowledge, theories play a central epistemic role, not models. Models can play a role in the context of discovery or in (personal) clarification, but not in the contexts of *explanation* or *justification*. Also, models were seen in the RV as preliminary steps to theories ("it is only a model"). In short, mature theories were thought to render models redundant. Most importantly, models were not seen as vehicles of *scientific representation* of real systems [see Bailer-Jones 2009]. These vehicles in the RV are sentences constituting the theory, and models are only seen as secondary form of theorizing that facilitates the understanding of the formal calculus.

Achinstein and the disconfirming scientific practice

This was at the time maybe appropriate for quantum mechanics since the 'no hidden variables' hypothesis suggested that the theory could not be supplied with a model in terms of more familiar properties and hence understanding shouldn't be tied to the provision of a model [French & da Costa 2003, 44-5], but when we consider broader scientific practice certain issues comes forth.[3] Difficulties were noted in detail already by Peter Achinstein [1968], of which the most important are the following three. He begins with that, contrary to the RV, certain representational models, such as scale models, cannot be viewed as sets of statements, since they are objects [Achinstein 1968, 230-31]. Obvious example is groundbreaking Crick and Watson's wire and tinplate model of DNA. It is certainly not a set of sentences, and even if we describe it that way, we would fail to capture nonformal similarities that may be essential for the use of the model. It is not just that RV misses to identify models ontologically, it is rather that if we reconstruct this scientific model axiomatically, we would lose its direct representational value and weaken its explanatory power.

[3] We should notice however that early logical positivists, specifically Carnap, never tried to describe a general structure of scientific theories or models, but only of our 'best' physical theories, and then apply it prescriptively to other disciplines. Even if he managed to accurately describe the former, it would turn its head on him simply because other theories (statistical physics, population genetics, etc.) are not of the same structure (otherwise why would we need prescriptions?) and there is no grounds to impose on them the structure of quantum mechanics or of some other physical theory. In short, 'the structure of theories' based on theoretical physics is, strictly speaking, eating its own tail.

Secondly, since both theoretical models and theories are regarded as partial interpretations of the same uninterpreted calculus, the RV fails to capture the essential differences between the two [ibid., 233-235]. It is impossible to claim, for example, that Bohr's model of the atom or Maxwell's model of electromagnetic field are interpretations of some calculi – simply there isn't any (well formed) theory behind theirs construction [ibid., 239]. Rather they are both consisted of a set of simplifying assumptions combined into the description of the structure representing the target system – the model. Combining this with the previous objection Achinstein notes that the requirement of structural identity between theories and models is way too strong. Also, the cross-fertilization in science doesn't hinge on formal relations. Respective formal structures - mathematical expressions describing the heat conduction and electrostatic attraction for example, are quite different. Their analogy entails only similarity, hence the laws governing one phenomenon cannot be treated as mere interpreted formulas that the other laws are supposed to interpret [ibid., 236].

Finally, Achinstein challenges even the views of Hutten and Hesse (regarding models as analogies). Although in Bohr's model of the atom the analogy with the solar system was important for the model construction, the model itself is not an analogy since the Newtonian laws of the later do not entail construction of the atomic model. The same is true for the laws of heat conduction and electrostatics, where heat-conduction laws cannot be constructed as a theoretical model of the later [ibid., 248].

Achinstein's work was the benchmark for further analysis of models in science outside the framework of the RV. Floating underneath of all its objections to the RV is the observation that we simply do not find[4] axiomatizations in scientific practice.[5] In it he combines both giving up on syntax, and case-by-case focusing to models and theories – two attributes that will grow characteristic of modern approaches. The second one will take form of an investigation into the methodology of model construction and the role of models in scientific representation; we will explore these in the next chapter. Regarding the abandonment of syntax, some further insights were required.

[4] And cannot place!

[5] This divergence between the scientific practice and that what a theory includes syntactically most directly (negatively) influences the possibility of understanding and clarifying the theory.

Formal problems

Real, substantive problems of modeling, theorizing, and scientific representation were overshadowed by rational reconstruction of these phenomena in formal logic. Entanglement in formalism, and philosophical delight by it, obscured the fact that the RV views on scientific practice were inadequate, as Achinstein and others have noted. Accordingly (and paradoxically), the RV collapsed not primarily due to its incompatibility with this practice – to this new revisions/epicycles were always an option[6], but due to mounting formal problems that drove this view next to absurdity. More importantly, these technical problems were an important factor (together with mentioned discoveries regarding models) which provoked the semantic approach to theories and models, and shaped the course they have taken.

There are many, minor and major, formal difficulties which pledged the RV. For example, C rules supposed exhaustive explicit specification of allowable experimental procedures, what implied that a change or addition of experimental procedure produces a change of the C rules [cf. Suppe 2000]. Also, a change in the language in which a theory is presented entails that we are dealing with a new theory. Although it is maybe trivial to say that Bohr's theory of atom is the same whether we formulate it in Serbian or English, it is not trivial to say that it is the same theory whether we logico-linguistically axiomatize it in one way or the other. If we take the C rules, which connect theoretical and observational terms, they thereby individuate certain parts of the theory. Since the focus is on the linguistic characterization, even minute change in these rules must, strictly speaking, produce a new theory. This way the evolution of theories becomes extremely difficult to represent within the RV framework, because logic seems to be inapt for exposing the identity in change [e.g. Laudan 1977.]. Furthermore, it remained cloudy how first order logic can depict mathematized sciences in detail, since for grounding the mathematics either the higher order logic or the set-theory is necessary [Quine 1986].

Undoubtedly the greatest formal problem for the RV was the Lowenheim-Skolem theorem which implied that no first-order theory (with an infinite model) can have a unique model up to isomorphism. Problem is for the RV that theoretical assertions are supposed to refer

[6] To name just a few: Craig reductions, Ramsey sentences, projectable predicates, disposition terms, etc.

to observable reality in the TC form. According to the Lowenheim-Skolem theorem, TC models must include both intended and wildly unintended models, which provide potential counterexamples [e.g. Suppe 2000]. Looking form a perspective of a half of a century later, it seems that the logical formalism, after astounding initial success in explaining, most notably, the testability of science, aggravated the analysis and diverted its proponents from investigating the real scientific modeling. Instead, much of the time was spent on blocking these unintended models, consequences of logico-syntactical approach. It is not just as Achinstein noted that we don't find axiomatizations in science and the RV is presenting us with a distorted picture. It's a rather bigger problem, that the formal logic, meticulously applied, is inadequate tool for representing science since it struggles to individuates scientific models![7] Reason for this is that the RV defines the class of models indirectly (over syntax), and in practice scientists separate only intended models, without recourse to syntactic axiomatizations. Gentle point is that our ordinary linguistic resources suffice and even has the advantage in uniquely isolating the desired class.

Two tracks of philosophy of science

Up to now, we saw that formal problems ultimately grinded to a halt the RV project. Out of many reasons for abandoning the RV, most prominent for model theory are repudiating models as independent and necessary tools of science and denying their representational role. Importantly, divisions of terms, vocabularies and languages the RV postulated remained difficult to apply to the real cases, hence the logical reconstruction seems to be just too crude tool to grasp modeling practices of science. Although the syntactic perspective still has several able defenders today [Friedman 2001, Demopoulos 2003], or even some who are developing it further by cutting-edge metamathematical tools such as category theory [Awodey 2006], we must say that these approaches has more to do with structure of theories than with the representational means of science and scientific practice in general. Eventually, from the perspective of model theory, it was the metamathematics itself (in the form of the Lowenheim-Skolem theorem and the irreducibility of mathematics to logic thesis) that brought the RV's demise. Problems of model-individuation by formal means mounted and, even putting these problems aside, the substantive

[7] Another important part of this inadequacy are correspondence rules, which are a confounding mess of experimental design, meaning relationships, measurement, and causal relationships some of which are not even proper parts of theories [Suppe 2000, S103].

problem of rendering models non-representational and non-explanatory proved simply false. Actually, mentioned formal problems were striking the same point as Achinstein's critique earlier did – the RV has problems with correctly individuating scientific models, i.e. when we apply the logically reconstructed scientific theory we do not get precisely the models science provides us with.

These technical problems mislead the RV analysis away from substantive ideas they began with – notably, regarding the theory/world relation and the mechanics of scientific confirmation. Following Achinstein's directions, and work of Hutten, Hesse, Black and others, models came into the central picture of contemporary philosophy of science. These pioneering works identified a plurality of meanings and uses of models, and generated diversity of model accounts we mentioned in the part I. In the last several decades we were witnesses of two tracks of development. On the one side the works concerning the concrete analysis – case-by-case studies of scientific practice, and on the other track the attempts of a creation of a unified picture of science – universally considered to be dominated by models (a complete twist from the RV's epistemic focus on theories). We will try to follow these two lines in the rest of this dissertation, all in the search of a model theory that unifies them.

Part II

Chapter 2

The Semantic Approach

Patrick Suppes – axiomatization Tarski style. Hierarchy of models. Representation of theories and theoretical representation of the world. The common core _ Mt/Md morphism . Theory application _ the simple pendulum case. The SA's impact.

As problems of application of logically reconstructed scientific theories to phenomena mounted, the studies of models came to the central stage. Combining these two features, the semantic approach (SA) emerged as the orthodoxy in contemporary views on nature and function of scientific theories and models, and for a good reason. Its unitary approach to these constructs significantly contributed to our philosophical understanding of science, mainly by disentangling our ideas of scientific representation and explanation from logical formalism and diverting them to models.

According to the SA, theories stopped to be viewed as axiomatic sets of sentences (and respective deductive closure) and are instead identified with model-types, which are (or can be represented as) classes of mathematical structures. These are specified either within set-theory by defining a set-theoretical predicate [Suppes 1957; and 2002; French &da Costa 1990; and 2003] or by means of the mathematical language in which particular scientific theory is formulated [van Fraassen 1980; and 1989; Suppe 1977; and 1989]. In the first case the theory structure is exactly the family of models that satisfy the set-theoretical predicate, in the second it is the class of state space types.

In this chapter I will present the basic setting of the SA and its different variants, using as the basis its prevalent, more general formulation in the set theory [following Suppes]. We'll see in what manner structural reconstruction plays its role in understanding scientific models, and why this approach ties representational function to models of theories; two features of the SA we will try to undermine in subsequent chapters. In this chapter, the focus will be on the common framework the SA versions share and on explicating the main philosophical gains of model revolution, both in theory and in the application of mathematical reconstruction to the real cases of scientific modeling and theorizing.

Patrick Suppes – axiomatization Tarski style

Before Suppes, the structure of scientific theories was presented in terms of logical axiomatization, which was generally understood in the same sense as was in logic since Frege, supplemented with steps specific for scientific theories. Precisely the new discoveries in logic, concretely development of Tarski's model theory in 1930's [Tarski 1931; and 1944], inspired Suppes to publish with collaborators in 1953 [McKinsey, Sugar, and Suppes 1953] a revolutionary axiomatization of classical particle mechanics Newtonian style.[8] When he axiomatized classical mechanics, Suppes didn't follow logical axiomatization steps of logical positivists: he didn't create the formal language, nor he explicitly described deductive calculus, nor he established formal-deductive system (i.e. defined formalization of T). And finally (and crucially), he didn't divide vocabulary into theoretical and observational predicates. Instead, he adopted the informal language of axiomatic set-theory (e.g. Zermelo's theory of 1908 with the axiom of choice), and, in order to be able to reason rigorously about it, elementary predicate logic as the background deductive apparatus.[9] He then rigorously reconstructed classical mechanics (CM) by following steps [Suppes 1957][10]:

[8] von Neumann's formalization of quantum mechanics a few years later, shared similar assumptions with this approach [von Neumann 1955], which also influenced Beth, which then, together with Suppes influenced Suppe and van Fraassen.

[9] These are the same in any reconstruction of scientific theory, hence are simply omitted because they are not directly concerned with representing the scientific content of the theory.

[10] For lucid and detailed reconstruction of Suppes steps see [Muller 2011]

a) (1) typify the fundamental concepts in CM set-theoretically;[11]

(2) express the postulates, principles and laws that characterize CM set-theoretically; and

(3) obtain a set-theoretical predicate which defines the CM — frequently called a Suppes-predicate; its set-extension T consists of exactly the sets that are structures meeting the Suppes-predicate—also referred to as models.

b) let Dt (CM) be the set of data structures that are obtained until historical time t from the measurement – results of experiments or observations relevant for CM. Call CM observationally adequate at time t iff for every data structure $\Delta \in Dt$ (CM), there is some structure (model) $S \in T$ such that Δ is embeddable in S, where 'embeddability' is broadly construed as some morphism from Δ into (some part of) S.

Step (a) is common both for reconstruction of scientific, and mathematical theories. What is specific only for scientific theories, and what differentiates them from the later, is (b) – it determines observational relation of a theory and the world, and inasmuch it express empirical essence of science – its connection to the phenomena that the theory is supposed to describe, to explain or to predict. For the sake of this 'saving' of phenomena, it is necessary to embed data structures Δ into the theoretical models of T. As long as we manage to embed the emerging data structures, the T will remain observationally adequate and will become better confirmed. Otherwise, the data structures will falsify it

Hierarchy of models

In many contemporary retrospectives often is overlooked that above relationship between a scientific theory and the world is deliberately simplified picture. Exact analysis of theory-data relation calls for a hierarchy of models of different logical type, each one more concrete and closer to the actual situation [Suppe 1962]. Hierarchy, however, is not straightforward. Although some model-types incorporate the others, direct comparison is not always possible, specifically when comparing theory and measurements. Experimental and observational data models (Md) are *different kind* of models than theoretical models (Mt) because:

I) theoretical notions do not have direct observable analogues in the raw experimental data (e.g. no actual experiment can include an infinite number of discrete trials); and,

[11] Whether they are properties, relations, functions, operations, etc. and what their domains and codomains are.

II) theoretical models often contain continuous functions or infinite sequences although the confirming data are highly discrete and finitistic(!) in character (e.g. theoretical parameters with real number value are not directly observable and are not part of the recorded data) [ibid, 253].

These differences notwithstanding, Suppes defines Md, i.e. possible realizations of data, in the usual way – set-theoretically, in the same manner as possible realizations of theory.[12] Condition of Md's precise definition for the given experiment, aside a measurement results, is a theory of the data in the sense of: a) a theory of the experiment, background knowledge and auxiliary theories (i.e. a model of experiment), and b) a description of experimental mechanism and procedure – structure of apparatus and ways of its usage (i.e. a model of experimental design) [Suppes 1962, 253 and 259].[13] Above condition practically means that auxiliary theories, theories of experimentation, and experimental design are included in the construction of the Md. Together they constitutes complex picture, where theory/phenomena relation is represented by hierarchy of models, not always strictly commensurable. Theory itself (that is, theoretical models) and description of apparatus (model of experimental design) are the endpoints of this hierarchy, and models of experiment (auxiliary theories and background knowledge included) and data models lie in between.

Although every model in the hierarchy is important for understanding scientific practice (latter on we will examine exactly how this prompted our previous conceptions), we will focus here on theoretical and data models, and on their relation. They are especially significant, because the SA in general captures the relation of theoretical scientific knowledge to the relevant phenomena by rendering Mt's representational of Md's, and the later are thought to subsume the other classes of model, namely models of experimental design and of experiment. Representational capacity is transferred from theories to Mt, and Md is generated in order to incorporate all experimental information which can be used in (statistical) tests of the theory's (i.e. Mt's) empirical adequacy. Either scientists try to structure the data in adequate Md so we

[12] There is a slight discrepancy here between his claim that the situation in science diverges from that of mathematics, where the comparison is between the models of the same logical type [Suppe 1962], and the presupposition that the concept of model is the same in mathematics and the empirical sciences [1960]. However, Tarski's notion of model, due to its abstractness, can indeed accommodate both of these cases.

[13] A nice illustration of how the hierarchy of models is not straightforward is that problems of experimental design (e.g. randomization of subjects to different experimental groups) often far exceed the limits of particular theory being tested. We might say suggestively from the fifty years distance that hierarchy is rather dappled.

can compare it to the given Mt, or, more common, they search for a Mt that overreaches the given Md's and maximizes the probability of detected data represented in the Md's. Background knowledge, auxiliary theories, experimental design, and other influencing factors that are known to influence the data, all are included in the conversion of raw data to the appropriate Md [Suppes 1962; Suppe 1977]. As Suppe will later state, the data collected from the target physical system is converted by means of theories of experimental design and auxiliary theories into an Md, wich represents what the data would have been had the target system been the isolated ideal system that the corresponding MT (and hence the theory) dictates [Suppe 1989, 103-4]

Since obtaining a data structure is a complex issue and is best left to the recently emerging philosophy of experimentation [e.g. Galison 1987], the SA focuses on the construction and role of theoretical models. We already sad that the theory came to be viewed in the SA as a family of (theoretical) models. To fully understand their conception of Mt, the question that needs to be answered precisely is what characterizes this family, that is "What is a theory *proper* and how it is related to Mt?" Since, obviously, the best answer is to look at an example out of scientific practice, the question for philosophy of science becomes "What is the most appropriate representation of a theory and its relation to Mt in order to prompt philosophical understanding and clarification?"

Representation of theories and theoretical representation of the world

If we represent a theory by collection of models, as Suppes argues, a question arises regarding the logical benefits. Both Freidman [1982] and Worrall [1984] have argued that there isn't any important logical difference between defining the class of models directly (in set-theory), as opposed to meta-mathematically (in predicate calculus). Actually, if the class of Mt's is an *elementary* class, then it contains precisely the models of the theory formalized in first-order logic, what yields the equivalence of the SA and the RV [Friedman 1982, 276]. However, already Suppes pointed out that the scientific theories, as quantum mechanics, classical thermodynamics, etc., usually use many results containing the real numbers [1967, 58]. Since the real number continuum is infinite, then the already mentioned Lowenheim-Skolem theorem yields existence of many models of logical axiomatization, not isomorphic to any member of Mt class [van Fraassen 1985, 301-2]. This result, that the Lowenheim-Skolem theorem crashes first-order logic at the real number continuum, implies that many interesting scientific models cannot be represented by an indirect, syntactic axiomatization, which proves to be too rough to individualize them.

In order to avoid above and similar technical problems, van Fraassen stresses the Suppes's hint that syntax should be cast off even in the set-theoretical form in order to understand important notions concerning the Mt [van Fraassen 1989, 222, ff.4; Suppes 1957, 260] – notably, how we define Mt's and what is their relation to Md's. Van Fraassen proposes that we should understand Mt as constitutional of the theory, not just as representing it [1989] – that's why he favors understanding of the theory structure as a class of state space types in terms of mathematical language in which it's formulated [1980]. This way Suppes's revolution comes at its full swing – we are free to concentrate on mathematical models in science, and explore their nature and function, without thinking on problems of their reconstruction – in a sense, the mathematical reconstruction seems simply superfluous.

On van Fraassen's trail, Suppe ditches reconstruction altogether and closes in on the real scientific theories by asking the question "How the class of Mt is defined?" [1989]. Notably, this question together with the question "What the Mt-Md relation is?" marks the pivot from the problems of philosophical representation of scientific theory to the problems of scientific representation of the world – from "How should we represent scientific theory?" to "How scientific theory represents the world?" Suppe remarks that the class of Mt is defined by a small number of parameters abstracted from the phenomena and created as the idealized system describing them [1989]. He thinks of models as 'iconic', since they function as abstract and idealized replica of what they are modeling [1977, 97]. In this characterization, formal set-theoretic characteristics are neglected for the sake of philosophically significant mechanisms involved in the scientific modeling – specifically, it is approached from the perspective of idealization.

Nevertheless, both van Fraassen's and Suppe's version of the SA requires that theory needs to be somehow represented, even when we equate it with the relevant class of models. All main versions of the SA have the same function regarding the representation of theory – set-theoretic (Suppes, Sneed, French & da Costa), state-space (Beth, van Fraassen), and relational systems (Suppe) mathematical characterizations all aims to properly (and directly) individualize models and thereby specify the admissible behavior of physical systems. Since logical differences regarding characterizations are not decisive (and having in mind that some important insights into the nature of theories diverge – such as above Suppe's iconic perspective on models), we will try in the next section to delineate the core of the SA, in order to evaluate the philosophical revolution that it brougth.

The common core - Mt/Md morphism

A unified framework by which all of the SA versions conceive scientific models is based on the views that theories are (or can be represented by) families of (theoretical) models and that empirical evidence is expressed through a collection of (data) models. Thus the SA framework can be delineated in the following way [following Portides 2005]:

1) Mt class constitutes the theory structure;

2) Md is constructed using the raw empirical data, theories of experimental design, and auxiliary theories;

3) Mt and Md stand in a mapping relation of one structure into the other.

Summarized this way the SA stands in the stark contrast to the previous conceptions of the theory/world relation. The RV account where the theory, together with auxiliaries, materially implies raw empirical data [e.g. Carnap 1950; or Hempel 1945] is replaced by the relation between theoretical and data models. Instead of a deduction of the phenomena, definition of a theory structure (i.e. scientific laws) entails a class of models (Mt's) which represents the target systems. Furthermore, the SA abandons the idea that theories are directly compared with raw empirical data, and replaces it with data models as "possible realization of the data" (constructed by the interplay of the data with our background knowledge) [Suppe 1962, 253]. By this step they eschew the use of deeply problematic correspondence rules which specify how the laws manifest themselves in observable phenomena, and transfers background knowledge – auxiliaries, experimental design, and so on (which were previously lumped in C rules), into the construction kit for Md's, and leave its exploration to the philosophy of experimentation [see Suppe 1989, spec. pp. 64 and 69-71]. A further consequence of this is that, since background knowledge is detached from the theory proper and included in the Md's, the construction of Mt's now hinges solely on pure ingredients of the theory.[14] This sharp distinction between theoretical

[14] We'll examine this key SA feature more closely in the next chapter.

and data models proves crucial for theory and model individuation, and thereby marks the core of the SA.

Finally, the theory/world relation is expressed by comparing models that realize the theory and models that realize the data. Since both kinds of models are considered to be mathematical structures (following Suppe [1960]), the relation that holds is some sort of structure preserving mapping (i.e. a morphism) between the elements and relations of one structure and the other. We see that the entire representational capacity of science, formerly attributed to theories, is now transferred to theoretical models; that's why the SA is also called the model-theoretic view. This is how the SA puts the models into the central picture – they are the locus of representation. Function expressed in the SA exclusively in terms of mathematical mapping.

The most proponents of the SA would concur to the general theses outlined above. However, some differences exist on the question how *precisely* this representational function of theoretical models is cashed out. It mainly concerns which part of mathematics is supposed to do the representation (the question of appropriate morphism), which carries with it the scientific realism debate [van Fraassen 1980; Suppe 1989; Suppes 2002]. Nevertheless, all these versions of the SA instigated new illuminating analysis of the real cases of modeling in science and, as we understand, that is precisely the greatest import of the SA to the philosophy of science – diverting attention to the real scientific issues, away from formal reconstructions. As Suppe noted, problems that plagued philosophy of science up to the SA emergence were that it addressed all too frequently philosophical impositions on the science, rather than problems arising out of it [Suppe 1989, 20]. To this end, numerous specific analyses of the actual scientific theories and models were made which, seemingly, demonstrated the correctness of the SA's general framework.

Theory application - the simple pendulum case

How the SA reconstruction of theory application works we can see on the case of the harmonic oscillator. The harmonic oscillator model is very important in physics, because any mass subject to a force in stable equilibrium acts as a harmonic oscillator for small vibrations. The specification of the force function in classical mechanics, by means of the position and momentum vectors, establishes an Mt. If that force function is specified as $\vec{F} = -k\vec{x}$, where the displacement x is proportional to the restoring force F, and k is a positive constant, we get a simple harmonic oscillator (SHO) (for physical details see [Nelson & Olsson 1986]). Another example where the Newton's second law defines an Mt is when the friction is included in the balance of forces, that is when the force function is specified as $F = -kx - c\dot{x}$, where c represents the damping coefficient, we get a model of the damped harmonic oscillator (DHO).

The second law of motion enables in this manner an indefinite number of Mt's to model the theory's domain, each corresponding to a differently defined force function. This seems perfectly aligned with the SA conception of a theory structure and Mt's construction – Newton's theory is his laws and a family of models they define. Now, how this represents physical systems? Take for an example a physical system of a bob suspended on the cord - the real pendulum. The mathematics of a pendulum is generally quite complicated. Furthermore, there is simply too many factors that need to be accounted for – pendulum-support damping, support motion, air friction, buoyancy in the air, changing gravity field (i.e. different value in different bob positions), elasticity/stretching of the cord, impact of the cord and the supporting ring on mass distribution, dispersed mass (i.e. not concentrated in a point, requiring multiplication of the force function), etc.

Adding correction factors and simplifying assumption makes the mathematics tractable. That way, by assuming that the cord is rigid, the mass is concentrated in a point, that there is no friction, and so on, we are able to apply CM to the case and construct the model of simple gravity pendulum. For the sake of further tractability, infinitesimal displacements are introduced and the model is reduced to the SHO discussed above Theoretical model accounts for a point mass bob supported by a massless, inextensible cord in a system without damping, uniform gravity field, etc. By this model, if the cord length and the period are known, we can solve the equations for the acceleration of gravity.[15] Since the latter can be determined by independent methods, the SHO model (and Newton second law) can thereby be tested. The fundamental experimental problem is then to precisely measure the cord length and the period of oscillation. However, once the length and time are measured, a large number of corrections must be applied,

[15] For mathematical details and derivations see [Nelson & Olsson 1986].

to include the factors mentioned in the previous passage, since any actual pendulum deviates from the idealized assumptions underlying the LHO [ibid., 113]. This physical analysis seems to corroborate the assumptions of the SA that auxiliary theories, experimental design, and other correction factors enters not into the Mt amendments, but into the construction of Md, which in the given case serves as the input for the calculation of gravitational constant. Accordingly, mass distribution corrections due to the finite size of the bob and the mass of the cord, accounting for the cord connections to the pivot and the bob, flexibility of the cord, motion of the support (including its elasticity)[16], the system's buoyancy in the air, damping (air resistance, friction at the point of support), finite amplitude correction, addition of the mass of the displaced air-fluid (and due to its viscosity)[17]. Air corrections are specifically subjected to approximations, since the auxiliaries' equations are not analytic for this case (hence the physically relevant solutions are approximated and adapted to this case) [ibid., 116].

All the factors contributing to the measured period (or at least, for the aimed precision of the experiment, non-negligible ones) are summarized, and experimental period and cord length are amended in the sense that those factors are precluded. Thereby we gain the values for period and cord length for which SHO model outputs correct gravitational acceleration value (up to 10^{-3} agreement with independently measured value; if one wishes to push the experiment to six-figure accuracy, further measures must be taken: variations in the room temperature, clocks errors, and atmospheric pressure must be accounted).

The Newtonian theory application in the case of the real pendulum seems to corroborate the SA's reconstruction of scientific theories. The model in question (SHO) is constructed as a force function specification of Newton's second law, and is pure derivative of the theory proper. The theory is not compared with the raw data (measurements of the cord length and pendulum period), but with a model of data (corrected values), where real measurements are amended by auxiliary theories (Archimedes principle, Fourier's analysis, Hooke's law, etc.) and theories of experimental design (accounting for: rigidity of the support, pivot stress capability, time

[16] For the illustration of the factor's impact, if four-figure accuracy is required, the support should be at least 10^4 times more massive than the pendulum bob (or the natural support frequency should exceed the pendulum frequency by a factor of 100 [p. 119].

[17] A significant amendment since if the pendulum were a clock (with 3m cord, 856g bob, and 3.5s period), it would lose 8.6s in one day on account of added mass and another 7.3s due to buoyancy, as opposed to the similar pendulum swinging in vacuum [ibid., 117].

measurement error by taking average value, etc.). Finally, the theory is 'tested' by comparison between the Mt and the Md, and representational function is completely transferred to a theory-driven model which represents the data through the Md. In this sense, a Mt represent the data 'in an abstract and idealized way' [Suppe 1989] by relating not to the data itself (here the measurements of the cord length and the period), but to its amendment version represented by the Md.

In this manner, following pioneering Suppes's reconstructions of classical mechanics [McKinsey, Sugar, and Suppes 1953; Suppes 1957], the SA superiority was demonstrated in a number of studies in physics and biology. Suppe demonstrated the framework in evolutionary biology on Kimura's multiple-locus, multiple allele generalization of Fisher's genetic theory of natural selection [1977]. John Beatty applied the SA to evolutionary biology optimal design models [Beatty 1980; and 1981]. Paul Thompson stressed its importance for foundational research in evolutionary biology, and claimed that the SA naturally corresponds to "the way in which biologists expand, employ, and explore the theory" [Thompson 1983, 227]. His [1985] addresses the testability of sociobiological theories and [1986] the laws of interaction in evolutionary theory, where he finds in both cases the advantages of the SA's theory/phenomena conception (via MT/Md relation). Van Fraassen has examined the application of the SA to the quantum mechanics [1970; and later 1991], and he [1985a] and Michael Friedman [1983] have discussed advantages of the SA perspectives in the foundations of space-time theories. Newton da Costa [1987; da Costa & Doria 1992] demonstrated how Maxwell's electromagnetic theory can also be reconstructed in this manner. Elisabeth Lloyd [1988] showed how the SA state-space models conform to the population genetics ecological and species selection models.

The SA's impact

In spite of the mentioned studies, it is not clear that in all these SA applications to the real scientific models and theories mathematical reconstruction played a crucial role, or it could be that the SA's overall framework contributed to our informal philosophical understanding, as in the above case of the SHO. Steven French and Newton da Costa, two of today's most prominent SA proponents, claim that set-theoretic axiomatization might have important consequences, both philosophical and scientific, and that this is exemplified by the result that including or deleting certain set-theoretical axioms produces definitive physical results – for example, inclusion of continuum hypotheses or Martin's axiom in axiomatization of thermodynamics entails a positive entropy shift instead of a zero-entropy shift [French & da Costa 2003, 26]. Although this may shed some significant light on the foundations of the theory, Suppes point, reiterated by van Fraassen and Suppe, remains puzzling: if Mt's constitute, not just represent a theory, why do we need to reconstruct the theory when, on the one side, all of the mathematics is already there, and on the other, it is not clear that mathematics is necessary for understanding of all of the important notions regarding scientific theory application (notably: Mt's construction and theoretical representation of phenomena)?

Leaving this question open, the SA didn't remain completely unchallenged. Some early criticisms concerned specifically its applications to biology – Beatty's, Lloyd's, and Thompson's work, on the grounds that the SA only instantiate scientific theory/model specifications (in the sense that the mathematically reconstruction only reiterates, not explain science) [Sloep & van der Steen 1987; 1991]. Nevertheless these few criticisms stayed in the shadow as the SA gained plausibility primarily since reconstruction of theory application diverted philosophical focus to and propagated the concrete, seemingly successful, case studies. The key contributions in this respect are diverting attention to models and exclusion of the raw data from the theory-testing. The later, however, remained a delicate issue since the inclusion of auxiliaries as amendments to raw data, not to the theory, solved the problem of correspondence rules, but left us with unresolved empirical basis of science. Not to suggest that transferring this question to the philosophy of experimentation is a setback - quite the opposite. However, from the model theory standpoint status of Md's, particularly the flexibility of their relation to raw data, remained in need of a further clarification.

What the structural reconstruction most forcefully brings us is philosophical focus on the scientific models in two related ways: firstly, it stresses the pervasiveness of models in theory application (theories are generally applied not by testing their empirical consequences, but by constructing a model) and, secondly, it transfers the representational capacity from theories to their corresponding models. How precisely the SA cashes out these positions on scientific representation and theory testing is generally characterized by a division of scientific models into the theory-driven class on the one side (Mt's) and the raw data-background knowledge-driven class (Md's) constituting the rest. Key impact is that the relation to the world is detached from theories and its main bearers became the models that realize the theory (Mt's). Along with encompassing the different explanatory, predictive, representational and other roles of models in the scientific practice, a deeper (although not complete) understanding was gained regarding the complex relationship between theories, data, and phenomena. In this sense the SA presents the orthodoxy, and with a good reason, of today's philosophy of science – it's not the sentences, but the complex mathematical structures that do the representing, predicting, explaining, etc. (There is an obvious echo of holism in this perspective.)

Crucial differences among its variants are mainly regarding the specifics of scientific theory application – notably, concrete mechanics of scientific model construction and theoretical representation of phenomena. Concretely, they concern the type of Mt/Md mapping that best express the theory/experiment relation (usually some sort of morphism, which concerns primarily which part of the data is "saved"). Since these differences are not of the importance for our case here, we will not investigate them in full detail. Key point for us will be the SA's understanding of theoretical models as vehicles of theoretical representation and explanation of phenomena.

These two main questions – "How scientific models are constructed (how the class of Mt's is defined)?" and "How they represent the world (what is the theory/evidence relation)?", marks the SA's approach to scientific practice. By facing the philosophical analysis with real scientific problems represented by case-by-case analysis of scientific practice (the famous historical turn), instead of dealing with philosophical impositions which plagued previous reflections, philosophy of science gained firmer testing grounds. Sticking to the model-centered conception of science, in the next two chapters we will pay closer attention to these questions and discuss the challenges to the SA's supremacy.

Part II

Chapter 3

Challenging the Mt's dominance

Idealization and approximation in scientific practice

A new revolution. Challenge one: representational capacity of Mt's – their abstractness and ideality. Implications for the model theory _ Mt's don't truly represent. Challenge two: the construction of scientific models – theory-unrelated features. A: lacking Mt's on intersections of theories. B: approximations that improve on the accuracy - Mt's get corrected empirically. Autonomy of scientific models _ construction and representation. Functional autonomy.

In spite of the significant reversal in understanding of scientific theories, the SA has, in a way, kept through the relation of Mt/Md morphism the positivistic paradigm that theories (that is Mt's) are candidates for representation of physical systems. This problem of theoretical representational capacities and whether Mt's at all correspond to the actual scientific models are the focal points around which, by and large, the critics of the SA have centered. Namely, it looks that the SA ties the construction and role of the scientific models too closely to the theories and the question is "Whether that is really the case in scientific practice?" If this thesis of the SA is true, then the real scientific models must be either identical with theoretical models, or in some way reducible to them. Since this is the hypothesis regarding the character of scientific modeling and the role of actual scientific models, it looks like in this point the SA can be almost empirically tested. With an eye on the limitations of 'empirical tests' of philosophical theories, in this chapter we will examine some case-studies of actual scientific modeling, which apparently challenge the above SA thesis, and evaluate its consequences regarding the theory of models.

A new revolution

During the first few decades of the SA development, starting with the revolutionary axiomatization of Newtonian classical particle mechanics [McKinsey, Sugar & Suppes 1957] and culminating in the 1980s, it looked like the representative-theory-driven-Mt's hypothesis was confirmed. Examples of the SA application, at least those less controversial, were mainly concerning the classical, and in lesser extent quantum mechanics and their Mt's (like the harmonic oscillator models of the vibrating string, or of the physical pendulum)[18]. Exactly on their basis the SA was enthroned (even institutionalized), instead of the RV, as the dominant explanation of the scientific theories and models.

New reversal began with the revolutionary book of Nancy Cartwright [1983] *How the Laws of Physics Lie* [Teller 2001, 395; 2008, 91; Hoefer 2008, 12]. This book, in significant contrast with the semantic tradition, pointed out the problems and mistakes in the descriptions of physics by philosophers of science. Its main thesis is that physics functions by carefully prepared descriptions or models, which are rarely derivable from theory, and which often sacrifices precision in the name of explanatory purposes. The key difference between the actual scientific models and theories is that the models struck a delicate balance of generality and representational accuracy, while the theories are significantly less limited in their use of idealizations and consequently are deprived of representational capabilities. Cartwright's book contains a staggering number of physical examples, and the focus is on surprisingly pervasive use of approximations and idealizations in both scientific modeling and theorizing. We'll understand an idealization in the most rudimentary, minimalistic way – as a characterization of a system where some of the properties are deliberately distorted in a way that makes them inaccurate descriptions of some aspects of the target system (say, a description of a particle as a point mass); an abstraction as a representation that doesn't include all of the system's properties, leaving out features that the system has in its concrete form (e.g. the omission of the intermolecular forces

[18] I am not including here many applications of the SA to the evolutionary biology because it is an open question which principles should be regarded as belonging to the fundamental theory and which to phenomenological or operational theories [see Suppe 1989, 20]. Hence already within the SA was acknowledge that the problem of whether representational model is theory-driven or not cannon be unambiguously determined. Also, many of these applications were both early [Sloep & van der Steen 1987; 1991], and more recently challenged [Gildenhuys 2013].

from the ideal gas law); and approximation as an inexact (propositional) description of a target system [Norton 2012].[19]

We well take a selection of relevant examples and analyze them from the perspective, and in contrast with, the SA conception developed in the previous chapter. Also, we will see how these examples started a philosophical turn which, till this day, is still mounting case studies of specifically scientific constructs, not conflating them in analysis with mathematics or logic. Crucially for our purposes, Cartwright challenged the SA's conception of how theories are applied – how the scientific models are constructed and how the models represent the world. First, regarding the claim that the Mt's represent actual physical systems, we will examine Cartwright's suggestion that Mt's are very limited in this respect since notions used in them are abstract and idealized [Cartwright 1983; and 1999]. Secondly, regarding the construction of scientific models, we will investigate examples to which Cartwright indicates, mainly from the perspective of scientific representation. Specifically, we will investigate whether limited representational capacity of Mt's entails that the scientific models that actually do the representing are not derived from theory – or in today's language, that representational (or phenomenological) models, contrary to the SA, are autonomously constructed.

Challenge one: representational capacity of Mt's – their abstractness and ideality

Let's take quantum mechanics (QM) for example. Schrödinger's equation tells us how the quantum system evolves in time when the Hamiltonian of the system is known. The Hamiltonian is a mathematical representation of the kinetic and potential energies for the system. In typical formalization of QM [e.g. Messiah 1969], principles like the conservation of energy, momentum, or parity, may also appear, although they are sometimes derived from other basic principles. Along with these, there are principles which connect the mathematical language of the theory with potential target systems: operators represent observable quantities; vectors represent states of the system; and vector-operator products represent the average value of a quantity in a given state [Messiah 69, Ch. 5]. Although these principles look like they provide the theory with representational capacity, to apply QM to some real system, one has to know how to pick the Hamiltonian.

[19] For a detailed discussion of these terms see Frigg, Imbert and Hartmann [2009], or specifically concerning idealizations McMullin [1985] and Weisberg [2007]. We'll return to some of these in the finishing chapters.

To learn how to pick the Hamiltonian and give content to the theory, a physicist is introduced with the sequence of exemplary Hamiltonians. By studying some of these theoretical models, she acquires the tools for dealing with the real systems. Examples include Hamiltonians for: free particle motion (in several settings); the (quantum) LHO; piecewise constant potentials (also in several forms); the hydrogen atom; diatomic molecules; and central potential scattering [Messiah, ibid.].

Problem to which Cartwright points our attention is that these content giving Hamiltonians are all highly idealized models, in a sense that they fit only highly fictionalized objects [1983, 136]. For example, the LHO Hamiltonian is used repeatedly in QM (as it was in CM) to model various sorts of real systems: the hydrogen atom is pictured as an oscillating electron; the electromagnetic field as a collection of quantized oscillators; the laser as a van der Pol oscillator; and so forth. As Cartwright notice, LHO is used "even when it is difficult to figure out exactly what is supposed to be oscillating" [ibid., 145] – a striking example would be Dirac's description (quantization) of the electromagnetic field as an ensemble of harmonic oscillators, or a (weakly coupled) quantum field theory where the quantum field is sometimes understood as an infinite set of harmonic oscillators, one at each space-time point. The same holds for examples of other Hamiltonians, say for free particle motion, where the particle is represented, dictated by the Schrödinger's equation, as a plane wave stretching to infinity in both directions. However, no particle is ever absolutely free and there is inevitably some confinement, such as the walls in the accelerator tube. The way to get this effect in the model is to set the potential at the walls to infinity [ibid, 142] – an obvious distortion of the truth, first by considering the particle as 'free', then by attributing 'infinite potential' to the enclosure. Consequently, the only real thing mentioned in the above list of QM's exemplary Hamiltonians seems to be 'the hydrogen atom'.

However, not even this Hamiltonian is of any real hydrogen atom, but of a hypothetically isolated atom where the effects of an environment are not reflected in the Hamiltonian [ibid, 137]. This accounts only for a very limited set of experiments – the proposed Hamiltonian only provides a solution accounting for the energy spectrum of hydrogen. Even worse, it does it crudely – although it correctly accounts for the position of the spectral lines, it does not for their fine structure. Discovery of the splitting of the spectral lines of hydrogen – its fine structure, was a significant event in QM (Willis Lamb got the Nobel prize for its discovery in 1947, and it was the harbinger of quantum electrodynamics). The fine structure thought physicists important lessons regarding electron spin and the requirement for relativistic corrections. The Schrödinger theory essential shortcoming to represent an atom in its real situation (effects of the environment precluded) comes from being a non-relativistic theory, and, also, that it doesn't take the electron spin into account [Messiah 1969, 419].

In order to include these two features, the hydrogen atom Hamiltonian can be derived using the relativistic theory of the Dirac electron [Messiah 1969, 932]. The theory's predictions generally agree with the experimental results regarding the hydrogen fine structure, although not perfectly. The largest discrepancy is observed in the fine structure of the n=2 level of the hydrogen atom – in the Dirac theory the $2s_{1/2}$ and $2p_{1/2}$ orbitals should have the same energy, while the $2p_{3/2}$ level is slightly lover (the separation is of the order of 10^{-4} eV) [ibid.]. Contrary to it, observed difference in the energy levels $2s_{1/2}$ and $2p_{1/2}$ is known as the Lamb shift – it is the result of the interaction between the electron, the proton, and the quantized electromagnetic field. Since the Dirac theory retain only the main term in that interaction (the Coulomb potential), the Lamb shift represents its 'radiative correction' – the change produced in the charge of a particle as the result of the particle's interactions with electromagnetic field [ibid, 933]

The real problem which emerges here for the SA is that neither Schrödinger's nor Dirac's theory Mt's do not use the Hamiltonian for the real hydrogen atom (even abstracted from the environment)[20]. Instead, the Hamiltonians express *only the Coulomb's potential* between an electron and the proton (relativistically and non-relativistically, respectively). In each case, the Mt called "the hydrogen atom" is "merely the simplest system of two bodies with the Coulomb interaction" [Messiah 1969, 412]. Even if the system stood alone in the universe, we could not eliminate the electromagnetic field, for it gives rise to the Lamb shift. To account for the Lamb shift we need to incorporate elements neither provided nor derivable from the theory, contrary to what the SA framework suggests. To include the phenomenon the theory alone is not enough. This case illustrate that a theory represents only abstract and idealized circumstances (not even representing the electromagnetic filed, responsible for distorting the fine structure of hydrogen)

[20] In spite that these atoms always appear in a certain setting, say on a benzene molecule or in a cold tank, which influences the energy of the system.

and that we must complement it for the sake of representation of the real systems. We will return to this shortly in the analysis of Weisskopf-Wigner method.

Implications for the model theory _ Mt's don't truly represent

So how does such highly abstract and highly idealized Hamiltonians represent some real system? And why do we have such a small number of Hamiltonians in QM for representing endless real cases? According to the SA, every theory has potentially infinite number of Mt's (members of the class entailed by the theory), by which, in principle, the specific scientific laws can be derived from fundamental laws (remnant of the deductive-nomological model of explanation). Firstly, from this perspective, there is no obvious reason for a smaller number of Hamiltonians compared to the types of application since the theory should be capable to provide us with as many as we need. Constraining to a small number of Hamiltonians seems awkward if others can be easily provided by the theory. Far greater problem is that the Hamiltonians, as derivatives of the theory, should be representative of the specific mechanisms in their target systems. They are not always so. On the contrary, morphism from highly abstract and idealized Mt's, as the one discussed above, can only represent (and explain) general features of phenomena, not its specific mechanisms. Cartwright explains that the explanatory power of quantum theory comes precisely from its ability to deploy a small number of well-understood Hamiltonians to cover a broad range of cases, and not from its ability to match each situation one-to-one with a new mathematical representation – "it is no theory that needs a new Hamiltonian for each new physical circumstance" [1983, 139]. The problem is that the price of this explanatory power is the exact representation. Not only that models do not describe physical systems completely accurately, since in many cases they embody an element of idealization and abstraction, but frequently this is purposely done for the sake of making mathematics simpler, intentionally sacrificing precise representation of the real physical objects, e.g. representing the electron as an infinite wave and dealing with its localization afterwards, or representing a physical object as a point (a point particle) consciously omitting its spatial extension. Limiting the number of Hamiltonians will produce limited number of highly abstract and highly idealized Mt's – the same description/model will be deployed again and again in physically very different situations. So, for example, that's why we have the harmonic oscillator used repeatedly in QM (as it was in CM). Returning to the question how they represent physical systems, Cartwright argues they simply don't. That is, they represent them at best in an abstract and general way, or they are only idealized explanations of their behavior. We'll return to the problem of explanation later on. For now, if Mt's fails to provide us with exact representations, than the SA central thesis depicting them as standing in this relation to the theory stands challenged.

We saw that the QM consists of small number of highly abstract and highly idealized Mt's. These models can represent things realistically only in very specific circumstances, namely when only Coulomb's interaction is happening. In reality, this represents very limited set of events – some isolated phenomena are indeed described by it but, as Lamb experiments show, many others stay in a need of a more detailed model; which cannot always be constructed on the basis of the fundamental theory. In most cases, other forces are at work and we need more complicated, more specific models to stand for them. Let us illustrate this point more clearly on the case from CM. We'll take the physics Nobelist Richard Feynman's example of the law of universal gravitation [Feynman 1967]. Since the main claim of the SA is that models entailed by the theory (Mt's) are representing representations of data (Md's), the crucial question in this case becomes: can a Mt of the universal gravitation theory truly describe how bodies behave (stand in some kind of morphism with the relevant Md's)? The law of gravitation states that two bodies exert a force between each other which varies inversely as the square of the distance between them, and varies directly as the product of their masses: $F = Gmm'/r^2$; this law together with F=ma entails everything else in an Mt as a consequence [Feynman 1967, ch.1]. But electricity also exerts forces inversely as the square of the distance, this time between the charges – by Coulombs law. The problem is that, for bodies that are both massive and charged, their behavior cannot be modeled only by using the law of gravitation, since both the law of gravitation and Coulomb's law interact to determine the final force.[21] In most interesting cases, the resultant force will be significantly different than the force the law of universal gravitation, or its Mt's, predicts.

Looked from this perspective, the law of gravitation is simply false in most of the cases – it doesn't (even remotely accurately) describe how bodies with mass generally behave; hence

[21] These laws cannot even be regarded as approximately true since, for example, in the above QM example of the interaction between the electrons and the protons of an atom, the Coulomb effect swaps the gravitational one, and the force that actually occurs is very different from that described by the law of gravity [see Cartwright 1983, 57].

there is no Mt/Md morphism. If on the other hand we understand it not as a universal law, but with implicit *ceteris paribus* modifier, in the sense that it pertains only to the cases when *only* force of gravity is at work, then the law can explain only very simple, generally ideal, circumstances - why the force is as it is when there are no forces except gravity at work.[22] Problem is that the law is now irrelevant to the more complex and interesting situations. In short, there are many cases where an Mt derived from it would be either ideal (which generally hinders its representational capabilities – at least when the Mt stands on its own) or simply false. In both cases, an Mt we derive from the law would be in *some* (in fact in many) cases irrelevant for representation – more concrete descriptions would be necessary.[23]

Gähde observes that there isn't an exactly clear line to be drawn between fundamental principles of physical theories and phenomenological laws (as used in representative models) [Gähde 2008, 62]. The gravitational law, for example, has numerous modifications and is far from clear whether they should be interpreted as fundamental principles or as phenomenological laws [ibid.]. However, no exact line is necessary for the above thesis. It is enough to know that any significant amendment of a theory is in a need of a different confirmation.[24] Existence of these cases, where we need to significantly amend our Mt's for the concrete exact representations, endangers the SA thesis that it is the Mt's that do the job. More problematic for Cartwright's argument is the insistence on a claim that the hydrogen atoms behave the same within the laboratory and outside of it [Hoefer 2008], hence if the Mt's are representational in the first context they are also in the second. However, if we understand it literally this seems as an untenable claim since hydrogen atoms are subjected to different influences in different places – a law governing things in one setting might not do so in some another. Usually only in very special circumstances predictively successful models can be constructed in a principled way from the resources of a single theory [Steed at all. 2011, 98]. As the case where both gravity and electricity are at work demonstrates, a theory application has significant constraints for representing the actual cases.

Challenge two: the construction of scientific models – theory-unrelated features

[22] For problems concerning representative limitations of idealizations see also Norton [1995; 2012; and 2014].

[23] We'll return to the place of laws in the recently emerging picture of science in the chapter 3 of the last part of this dissertation.

[24] F=Gmm'/ r2+ a/r4 , for example, is an unconfirmed hypothetical principle whose confirmation does not follow from the confirmation of the unamended gravitational law.

In previous two sections we examined the challenge to representational capacities of fundamental theories by showing that concepts used in them – such as Hamiltonians or force functions – are highly abstract and highly idealized. But already Suppes [1962] pointed that theoretical notions do not have observable analogues and that Mt's contain idealized elements such as continuous functions or infinite sequences [Suppes 1962, 253] (see previous chapter, section "Hierarchy of models"). Nevertheless, the SA proponents conceived the Mt's as representative of physical systems, through their relation with the relevant Md's.[25] What Cartwright's work illuminated is that in most cases, because of their abstractness and ideality, the Mt's can't be used successfully to represent the real systems – in most cases they simply fail to represent and explain relevant *specific mechanisms* at work in physical systems. Ordinary classical phenomena like Neurath's bill – a thousand dollars bill swept by the wind and pulled by the gravity in Saint Stephen square [Steed at all. 2011], or forces acting on two children tobogganing down the hill [Contessa 2011] stay out of reach of the theoretical models entailed by the classical mechanics, quantum and relativistic considerations aside. Now the crucial question is posed: since science undoubtedly represents the world, how models that actually do the representing are constructed?

A: lacking Mt's on intersections of theories

Before tackling this arguably the central question for model theory, we want to stress in what way it challenges the SA. Firstly, if there are cases where the Mt's cannot represent the real systems, then these contradict the main theses of the SA that Mt's are the locus of scientific activity. In fact, we know many cases where our theories cannot spawn theoretical models. Feynman would say we know this for all fundamental theories [1967, 37] – we know precise cases where they fail and specific and detail ways in which they are deficient.[26] Concretely, although both QM and relativity are highly developed, detailed and sophisticated, we have little theory about what happens in the intersection of domains, where both quantum and gravitational phenomena are occurring. Presumably the best candidate for such a theory would be our most

[25] Though nobody in the SA camp explained how *precisely* these abstract and idealized elements share the same structure with discrete and finitistic data/concrete Md's. When this was tried, as in Suppe [1989], the crucial question "Is that sharing *enough* for the claim of theoretical representation?" remained floating.

[26] This is connected with Popper's [1959] "every theory is born refuted"; which was later developed by Kuhn [1962] and Lakatos [1978] in a manner quite different that the one present in model theory, in their works regarding the *grand dynamics* of theories, theoretical frameworks, and science in general.

accurate micro-theory – relativistic quantum field theory and its realization in the quantum chromodynamics (QCD). QCD works by quantizing normal modes [cf. Teller 2004, 432-3]. The quarks and gluons of QCD (i.e. the quanta) are understood as 'excitations' of these normal modes which are called the positive and negative solutions of a wave equation. "But in any model that characterizes the space-time as irregularly curved, as do our most accurate models of space-time, there are no positive and negative frequency solutions of the field equations." [ibid, 433] Hence we see that even in this best-case scenario there are no theoretical models which are in accordance with both of the intersecting theories and the ones that we do have are idealizations – we know precisely where they are false.

In practice, especially as we move towards the treatment of specific effects, many treatments piece together laws from different theories [Cartwright 1983, 51]. As Feynman points out:

> "Today our theories of physics, the laws of physics, are a multitude of different parts and pieces that do not fit together very well. We don't have one structure from which all is deduced; we have several pieces that do not quite fit exactly yet." [1967, 30]

We saw this in detail on the intersection of electricity and gravity. Since the fundamental laws are severely limited in scope, in representation and explanation of phenomena the law of gravitation and the Coulomb's law are going to describe only influences which gravitation and electricity produce, *not the total effect* (for this, as we saw, they will be simply false). Work of modeling what these influences do – body behavior that results – will be done in many cases not by fundamental laws, but by a variety of complex and ill-organized phenomenological laws [Cartwright 1999; but also 1983, especially essay no. 3]. This is especially the case where theories intersect (but as we will see shortly, not only there); here would practically be no Mt's. Some philosophers tend to think that this is practical difficulty and that there is *in principle* a law to cover every (intersection) case [Smith 2001]. Although we might give in to the claim that in many problematic cases construction of an Mt is principally possible, the problem for the SA remains: irregardless of a principal possibility, in practice it is impossible to model the questioned phenomena with Mt's (and Smith acknowledges this). As in many other important theory-intersections, Mt's are ultimately not used for representation and hence the SA's theory/world relation stays wanting of a confirmation – principled possibility proves nothing. In the same sense that our estimates of the future development cannot be the content of our history books, philosophers' visions of 'future science' are not the proper objects for the philosophy of science since unknown cowering laws, although possible, cannot be our grounds for understanding explanations in actual science.

The key feature of science illuminated by Cartwright's work (I would say – the reason why her [1983] sparked a revolution in model theory) is that science is broken into various distinct domains: hydrodynamics, genetics, laser theory, etc., and (although we have many detailed and sophisticated theories about what happens within the various domains) general laws that describe what happens in the intersection of domains are not always available [Cartwright 1983, 50-1; also 1999]. The vast array of phenomenological models science presents us in these intersection cases are tailored to specific situations, not derived and governed in an orderly way dictated by the theoretical principles. We saw this starkly in the analysis of the hydrogen atom and the Lamb shift, where spectrum levels supposed to be produced by Coulomb's potential are levels that, due to interactions with electromagnetic field, do not actually occur. Now we'll see an example where the independence of the model from the fundamental theory happens, not when some other fundamental forces intervene, but within the scope of the single theory and the corresponding law which properly covers the theory's domain.

B: approximations that improve on the accuracy - Mt's get corrected empirically.

To take a detail example of phenomenologically-driven model (an opposition of the theory-driven models of the SA) we do not need to go to the intersections of theories (although many, if not most, interesting science happens there). Models are constructed phenomenologically even within the scope of our best fundamental theories. In these cases, instead of a strict deduction (mathematical entailment) that takes scientist from fundamental equations to the representational model, a variety of different approximations are required. In some cases approximations are, of course, due to calculational difficulties. The SA generally interprets approximations in this way [see French & da Costa 2003, 57]. This case favors the SA, because a theory structure (basically its laws) has an indefinite number of Mt's available for modeling a physical situation. When the conditions of Mt/Md morphism are not met in the concrete case of modeling, the SA can always resort to problems of mathematical tractability (on the grounds that the Mt/Md fit would be more perfect *if* theoretical equations could be exactly solved).[27] Practical inability to exactly solve theoretical equations, the SA attributes mainly to theoretical concepts present in Mt's, which are hard to interpret into discrete data and hence produce calculational difficulties – remember [Suppes 1962]. Since morphism condition can be regarded as obtainable in the limiting case, where "all the mathematics is done right", this argument enables the SA to accommodate itself with the fact that in any field of physics rigorous solutions are rarely available

Contrary to the case of approximations due to the calculation difficulties, many approximations spawn problems for the SA in the following two related ways. Firstly, there are approximations that are not mathematical simplifications of fundamental laws, but which improve their accuracy by producing results that are far more exact than theory-driven solutions [see Cartwright 1983, 107]. Secondly, there are choices (different approximation procedures or other model features) in model building which are, although constrained, not dictated by facts, and yet which are not theory-driven [ibid, 119]. Idea is that the former concerns the improvements of accuracy and the later concerns conceptual choices, and that both are non-theory-driven solutions. In both cases, accuracy-improving approximations and not-theory-driven-not-dictated-by-facts choices, different choices give rise to different, incompatible results. It's not that the theory does not have any role in constructing the resulting model, it is that some extremely significant model-building choices are made not on the basis of theory, and that the model, consequently, has theory-independent representational capabilities. For the sake of the depth and unity of analysis, we'll demonstrate both points (although the example will concern primarily the second) on an example from QM, with which we started the discussion at the beginning of this chapter – on the case of the Lamb shift and the application and evolution of the Weisskopf-Wigner method.

The Dirac theory set the starting quantum electrodynamics (QED) framework, and, under it, the theory of the spontaneous emission was first calculated by Weisskopf and Wigner in 1930. It is well known that this result is not a rigorous consequence of quantum mechanics but the result of somewhat delicate approximations; and nevertheless has a wealth of empirical support in radioactive processes [see e.g. Merzbacher 1970, 484-5]. It makes several important

[27] For a demonstration how these difficulties are solved on the case of a harmonic oscillator see [Portides 2005, 1290].

approximations, of which the two crucial ones regarding the exponential decay of a two level atom are: 1) the replacement of a sum by an integral over the modes of the electromagnetic field and the factoring out of terms that vary slowly in the frequency; and 2) factoring out a slowly-varying term from the time integral and extending the limit on the integral to infinity [Cartwright 1983, 119]. We will concentrate on how these approximations affect the theory's capability to predict the Lamb shift in the excited state of an atom. Each approximation is separately justified by the physical characteristics of the atom-field pair: (1) because the models of the field are supposed to form a near continuum – this allow us to replace the sum by an integral because constant *w* can be factored outside the integral with little loss of accuracy; and (2) is justified because the term from the time integral is slowly varying comparing to the rapid oscillations from the exponential, and the extension of the upper limit of the time integral to infinity enable us treat the change in the system as dependent only on the facts about the system at that time, not on its entire past history [for physical details see e.g. Barnett & Radmore 2002, Ch. 5.3].

In spite separate justifications, there is a difference in what order approximations are applied. If we start with the first approximation and continue with the second, what Weisskopf and Wigner originally did, the Lamb shift is not predicted. If we take the integrals in the reverse order – first perform the t integral, then evaluate the sum over the modes (*t*-then-*w* order), the theory predicts a Lamb shift in the excited state. Problem is: the physical characteristics of the atom-field pair on which the approximations are based do not determine (or even indicate) in what order they should be applied [Cartwright 1983, Essay 6]. The *w*-then-*t* order was conventional treatment for almost twenty years; and this is a long time in modern physics. When the Lamb shift was discovered the original Weisskopf-Wigner method had to be amended. Now physicists do *t*-then-*w*, but that decision was not made on the facts known about the atom, the electromagnetic field, and their interaction; nor it couldn't be decided on the fundamental QED theory (not from Dirac's, nor even from Enrico Fermi's formulation). Observed Lamb shift, although compatible with QED, is not entailed by the theory.

The point here is not that these two procedures give the same result; they don't, obviously the Lamb shift is predicted by the *t*-then-*w* order. The point concerns how approximations work in practice. Weisskopf-Wigner method presents us not with approximate solutions to the exact equations of the QED theory, but rather it searches for exact solution to approximate equations by simplifying our equation before solving it.[28] Since, in the case of models the question of physical interpretation arises, approximations cannot be treated only as in mathematical context [Redhead ibid; see also Morrison 1999]. Hence the approximating procedure concerns the accurate solutions to a simplified theory, rather than approximate solutions to an exact theory.

However, the issue is not just about picking the more accurate procedure. In many cases this is extremely difficult to decide in advance. To assess the original method (*w*-then-*t* order), physicists had to calculate the value of the term discarded by approximation (a slowly-varying term from the time integral), which meant to tackle the notorious QED divergences that arise from the Dirac theory. Before Lamb experiments, these problems were just pushed aside, and only after them the order of approximations was changed and original Weisskopf-Wigner method was amended. Although a possible explanation of these divergences was offered in terms of the interaction of the electron with the radiation field, the resulting shift of energy levels comes out infinite in all existing theories, and has therefore always been ignored [Bethe 1947, 339]. Only with Bethe treatment physics community found out that there was physical content behind QED divergences, which previous to Lamb's experiments "were just mathematical debris which represented nothing of physical significance and were, correctly, omitted." [Cartwright 1983, 123].

This is not to say that amended Weisskopf-Wigner method, by producing the accurate model for the spontaneous emission in the excited state of the atom, resolves the QCD application to the Lamb shift phenomenon. Case-by-case nature of modeling can be seen in the fact that, even with amendments, Weisskopf-Wigner method does not predict the Lamb shift in the ground state of the atom. But the point that concerns us here is that even in the excited state the reasoning that approved the two approximating procedures stayed silent about in which order we should apply them; although the original (*w*-then-*t* order) and the amended one (*t*-then-*w*) yield two incompatible predictions.

Autonomy of scientific models _ construction and representation

[28] For the difference between the two and the pervasiveness of exactness improving approximations see [Redhead 1980].

We began this chapter by pointing out that the SA has kept through the relation of Mt/Md morphism the positivistic paradigm that theories (now via Mt's) are candidates for representation of physical systems. Actually, we can observe that the entire SA presents only a model-theoretic variation of the deductive-nomological (D-N) account[29], where Mt's instead of theories are vehicles of scientific representation and explanation.[30] According to the SA's D-N framework, theoretical derivations describe through Mt's what happens on the phenomenological level, and any deviation from an Mt is only due to mathematical tractability problems. In the challenge to the construction of scientific models (challenge two) we just saw cases of approximations which *improve the fit* of the theory (B), connecting her more accurately with reality, and thereby undermining the D-N model. Also, in the previously discussed examples of theory intersection (A) the models are derived and governed in theory-independent ways, which shows empty the claim of an overreaching Mt. In both cases we see that the problem for the SA is fundamental – these fairly typical examples of scientific modeling cannot be regarded as cases where representative models are entailed by the theory and thereby they falsify the central SA claim on the character and role of scientific models.

The possible SA response can be that in the case of a theory-intersection (A) physics still doesn't have a general theory for, say relativistic QM phenomena – a theory is still evolving, of course, and we shouldn't judge on this stage of development. Therefore the lack of a governing Mt and (temporal) need for phenomenological solutions should not strike the SA specifically. Also, in the case of the fit-improving approximations (B), the SA can here again evocate the argument of tractability problems and claim that the physicist's opting for a phenomenological (non-theory-driven) solution doesn't undermine *the possibility* that an exact Mt can be derived in any of these cases, if the right equations are found and mathematics is done rigorously (and there are no principal problems for doing that in QCD, at least not for the cases at stake here; although they are practically extremely difficult to solve).

However, these potential SA arguments would be strikingly missing the point for the following reason: the SA's main trust in constructing the new picture of science with models in

[29] It's no difference whether the D-N account is in its weaker, probabilistic instead of a deductive version. We are interested only in its main framework where fundamental theory entails the phenomenological solutions, both in the sense that it explains them, and in the sense that it produces the exact representations of phenomena.

[30] We must add that not all SA proponents would agree that their framework fall under the D-N model [e.g. Bueno, French & Ladyman 2012b]

its central stage was the idea to look what happens in the scientific practice [e.g. van Fraassen 1980; 1989; Suppe 1989; but especially Suppes 1960]. The entire current chapter is presenting cases from this practice – and they do not confirm the Mt's-dominance thesis. These examples can indeed be made compatible with the SA thesis, but these examples of scientific practice *do not argue for Mt's-dominance thesis.* To draw again on Feynman's lectures, "This [non exact laws and Mt's] may or may not be a property of Nature, but is certainly common to all the laws as we know them today." [1967, 37] If we are to believe in the SA's reconstruction of scientific model-construction, it's not the scientific practice that provides the reason for it.[31] Representational models may be derived from theory in principle, but that has little to do with *current* autonomy of scientific modeling both regarding representation and explanation; hence the possibility of stricter mathematics does not change the severity of falsification of the SA main thesis. In short, principled theoretical derivability doesn't mean much when the practice itself is autonomous.

Functional autonomy

Up to now we were talking about autonomy of models only in the sense of independence from theory in their construction. This is the crucial point in the critic of the SA, but that's not all there is. In fact, models are also autonomous in the sense that they, although constrained by theory, function in theory-independent ways as agents of knowledge production [Morrison 1999]. It's not just that models enable us to fit a theory to concrete situations as the SA suggests, it's rather that they allow us to see how the laws apply and connect various parts of model structure and thereby explain specific mechanisms behind the phenomena [see also Redhead 1980]. One aspect of this functional autonomy is the role models play in both representing and explaining concrete processes and phenomena. Hence we'll see that representational and explanatory capacities of models are interconnected and dependent both on the specifics of the case at hand and to the resources available for model construction. This way functional autonomy will present another aspect of (mathematical) irreducibility of models actually used in science to Mt's.

[31] Of course, we can say that the theories are always growing and more sophisticated treatments are always possible, but precisely that is the spearhead the argument – current theories are imperfect and we have to mold our philosophy to them.

Functional independence is connected and usually results from theory-independence in model construction. However, even in cases where there is a close connection with the theory in developing a model it is still possible to have the kind of functional independence that renders the model an autonomous agent in knowledge production [Morrison 1999]. We'll try to illustrate this point in retrospective on the case of the pendulum model we examined earlier as the case for the SA (at the end of Ch. 2.2.). Although the model is a grossly inaccurate Mt, it is nevertheless capable of providing, with the appropriate corrections, an extremely realistic account of the instrument itself and its harmonic motion; which is reflected in its ability to determine the force of gravity up to four decimals [see above Nelson & Olsson 1986].[32] Of course, 'realistic' here doesn't means completely exact (as we saw in the analysis in Ch. 2.2. there are many approximations), but being close enough for the problem at hand – as Nelson and Olsson argue, "if one wishes to push the experiment to six-figure accuracy … [further] efforts must be attempted" [ibid., 120].[33]

Here we see that the accuracy of a model is not necessarily connected with the means of its construction – even an Mt can be realistic when the theory itself can provide essential corrections to the model. But although the pendulum model is theory-driven in its construction, it functions as an autonomous source of information. The model functions as a kind of measuring instrument, in a way that Newtonian mechanic or even the LHO can never do, and provides a feasible way of representing the various forces at play, including the local gravitational acceleration, and eventually provides information on the shape and mass distribution of the earth. The independence of the pendulum model is demonstrated mainly by its ability to facilitate the addition of numerous correction factors and include them in the model. Information about the real system is not gained by simply specifying the force function (which determines the Mt), but also by describing the deviation of the model (for which the laws are true) from the real circumstances, and the model is then used as a hub for organizing the specific mechanisms at play. We saw that the model can be corrected in a variety of ways (Cook's law, Archimedes

[32] A hint which we'll later explore in detail is that whether a model is to be considered as realistic or not depends on which aspect of the system we are aiming. Since we are interested primarily in the calculation of the gravitational constant, the pendulum model is realistic because it accurately, for the given requirement of precision, determines gravitational value.

[33] As Giere [1988] analysis of the pendulum captures (and what we will explore in detail later on) – model will be similar to a target system *only in limited respects*, and even in those respects *only in certain degrees*, and, as Nelson and Olsson demonstrates, what will be enough to call it 'realistic' will depend on the purposes at hand.

principle, etc.) and made more or less exact, depending on what level of accuracy physicist aim. Ultimately, it is the corrected model, not the theory, that answers *why* the gravitational force is what it is and how Newtonian laws govern the motion of the pendulum.

If even Mt's can be realistic, the usefulness of focusing on constructional origin of models becomes spurious for illuminating how models function – that is, the accuracy of a model or its explanatory capacity shouldn't be connected with its genesis. The Pendulum case shows that even in the case of Mt's the theory is only partially responsible for representation and explanation. We do not want to insinuate that Suppes's hierarchy of models doesn't properly depict (at least some of the) relations that bear on a model. It rather that allegiance to fundamental theory as the representational and explanatory vehicle for scientific phenomena distorts our view of how models function in scientific practice.

Furthermore, pendulum-like cases are relatively rare in modern physics, especially in quantum theories, where theoretical equations are often extremely difficult, if not impossible to solve. In most of these cases, modeling does not even approximate the picture it presents, it drastically distorts it – just think of the infinite see of electrons, point particles, chamber wall with infinite potentials, etc.[34] The point is not that there are not realistic theoretical models, but that their prevalence is limited.

There is also the other side of this coin. Beside the realistic Mt's, there are abstract and idealized phenomenological models (Mph's). Morrison discusses Prandtl hydrodynamic mathematical model grounded in observations of the fluid's behavior in water tunnels [for details see Morrison 1999, 53-62]. In spite its phenomenological origins, the model embody abstract or idealized assumptions that makes it a less than accurate (actually, intentionally distorted) representation of the actual phenomena – the fluid is regarded as a continuous medium, first in the sense of divisibility (an idealization of molecules' size), and second in the sense of density (an idealization of the space distribution).[35] Both are inaccurately depicting the real fluids which are discontinuous assemblies of particles, with discontinuous density transitions. In this case it is

[34] We must remember here that even the pendulum case is made tractable only by making idealizing assumptions, among others (inextensible cord, point mass….), that the movements of the pendulum are infinitesimally small (in order to treat it as the LHO).

[35] What is even more interesting regarding the hydrodynamics is that it demonstrates that the existence of a well-established background theory in no way guarantees that one can even provide representational models of a particular system, let alone the kind of accuracy displayed in the pendulum case [Morrison 1999, 52].

the Mph that produces abstract and idealized (nonrealistic) representation and, furthermore, it uses different idealizations in different parts of the system, for the same homogenous fluid.

This argument for disentangling representational accuracy from constructional origin of a model further complicates the picture of scientific models and dwindles the scope of accuracy of the SA picture of models. By emphasizing functional autonomy of models even for the realistic Mt’s the case for the SA has weakened since the representational and explanatory capacity is transferred from theory-derivability and related Mt’s structure, to the manner in which a model is used. Now after separating constructional origins and representational accuracy we start to wonder why actually a model’s relation to the theory is emphasized and how that improves our understanding of how scientific models represent and explain. Contrary to the Mt’s-dominance thesis, we see that models can represent physical systems in a variety of ways and that explanation of specific mechanisms and prediction of phenomena is often performed in a manner, although related to theory, not driven by it.

Looking back on the entire chapter, first we saw that many Mt’s, specifically in quantum theories, are not representational, at least not without specific corrections, because they are in most cases highly abstract and highly idealized. Secondly, we saw that those Mt’s that are representational (like the pendulum model) constitutes only a fraction of actual models used in science for representing. Sometimes, as in the case of intersections of relativity and quantum theories, or electricity and gravitational theory, we don’t even have a well-established background theory from which to derive our models. Even when we do, as in the case of quantum chromodynamics, there is no guarantee it can furnish the models we need since, although breathtakingly exact for certain very special questions, QCD is, in many respects, severely limited in what it can cover [see Teller 2004, 436-8]. These two ways in which the SA fails to produce accurate theory of scientific models negatively reflect the two central questions regarding the application of theories posited in the previous chapter: how scientific models are constructed, and how they represent the world? Contrary to the SA, answer to the question “How do we apply theories?” is that they are often applied in an autonomous way – models that actually do the representing and explaining are often constructed in a non-theory-driven manner. Specific mechanisms in physical systems that are the objects of science are reconstructed piecemeal, according to the characteristics and needs of the situation, in models that are often only constrained by the general theoretical framework, if there is such for the case at hand [Cartwright 1999]. Stated positively, in application of a theory, physicists often have to improvise.

With incorporating this variety in our picture of science, it is no longer clear that there is a unified way to conceive scientific modeling, and only consensus that arises is that we should acknowledge the complexities and diverse natures of scientific models. In this sense, the model theory is today in a similar position as it was in 1960s when Hesse, Hutton, Achinstein, Black, and others, undermined a possibility of a unitary approach to scientific models.

Part II

Chapter 4

Structural Reconstruction Reenvisioned

Partial structures approach. Imperfect representations. Comparison of models. Mathematical reconstruction and scientific content.

Previous chapter revealed that there are essential problems with the SA conception of models. Namely, the way in which proponents of the SA envision models do not agree with how they are constructed and used in scientific practice. It disconfirms the SA's favoring of Mt's in the sense that there are many ways in which models can represent and explain their target systems. Hence it suggests that we should change our ideas how models are constructed and function accordingly, to include the vast space where physicists improvise. However, this essential problem need not necessarily be the downfall of the SA, because, it seems to be, the SA may readjust its understanding of models to accommodate the scientific practice, and yet keep the basic framework of its approach – reconstruct this practice mathematically and try to gain genuine understanding of science through some relation of mathematical structures. This chapter questions this structural reconstruction thesis, by [~?] investigating features of mathematical representation of scientific modeling and evaluating philosophical gains of this approach.

First we will discuss the currently dominant readjustment of the SA – the partial structures approach (PSA) – which can be consider precisely as a response to idealizing and approximating aspects of science highlighted in the previous chapter [French & da Costa 1990; and 2003]. We will question whether the way the PSA includes idealizations and approximation really captures these features of scientific models, and more generally, can these features be illuminated and understood by mathematically reconstructing structural features of models. Examining what advantages structural reconstruction can offer, we'll claim that although structural dependencies are undoubtedly important, especially for scientific explanation, focusing on their mathematical reconstruction blurs certain crucial points regarding modeling, notably its pragmatic side – the roles of agents, contexts and purposes – as well as it fails to provide us with genuine understanding of important conceptual mechanisms of science, notably idealization and approximation.

Later on we will question the structural reconstruction in detail by investigating its capabilities to compare different models [Morrison 1998], to give an account of evolution of model [Portides 2005; and 2006], and what happens with *the content* of scientific theories and models when they are mathematically reconstructed [Muller 2011]. At the end, we will return to the foundations of the SA in a hindsight and compare it with previous theories of models, and from there try to pinpoint the possible mistakes in grounding a theory of models and delineate the basis for the new unified model theory.

Partial structures approach

The key problem of any model theory is to explicate the representational and explanatory relationship between the models and concrete physical systems. Regarding the SA's picture, this question on the nature of models primarily concerns the difference between Mt's and the models actually used in scientific practice. Diversity of the actual models and their uses proves to be difficult to encompass with math structures, either in set-theory or state spaces. Problem is that these reconstructions are structurally too strict (i.e. too simple) to account for the different ways in which models can provide information about the world.

Steven French and Newton da Costa try to incorporate this observation into the SA framework, by weakening the math (to admit looser model/world morphisms) in order to widen the scope of allowed relations between a model and the target system.[36] Namely, including "imprecise, 'loose', vague, open, and ambiguous" model/world relations within the SA's

[36] We've seen this thesis regarding the limits of mathematical reconstruction demonstrated on the real cases from Achinstein [1968] and Hesse [1966] in chapter 2.1, to Cartwright [1983] and Morrison [1999] in 2.3.

mathematical structures, either in set-theory or state spaces, is extremely strained, hence the PSA weakens this stringent formal jacket by introducing partial structures and relations, in order to account for "the incomplete and imperfect nature of the majority of our representations" [French & da Costa 2003, 17 and 19]. Means to this end is a change in the underlying logic (which represents the pragmatic understanding of truth), specifically introduction of simple pragmatic structures on the place of total structures (with all relations defined) of Tarski's framework [Mikenberg, da Costa & Chuaqui 1986].[37] Simple pragmatic structures form partial structures, which then enables a definition of partial isomorphism between different structures. The key feature is that for every relation there is a subset of individuals among which it is unknown whether the relation holds, besides the customary subsets where relation holds and not. Looking from this perspective, the SA's reconstruction, which completely and perfectly maps a given domain, looks like an idealization where all information is known.

The central idea is to stay in Suppes tradition but to properly amend the set-theoretic notion of model – to introduce partial relations, which represent 'partialness' of our information about the model and its target, in order to capture "the epistemic attitudes of scientists themselves" [French & da Costa 2003, 59]. The model/target relation is then understood in terms of partial isomorphism – in the sense that model is only similar to its target (share only a limited set of its properties). Also, various relations among theories and models, including Suppes's hierarchy of models, is captured by partial isomorphisms between partial structures, and, it is claimed, this variety of structures represents a complex web of relationships in scientific practice [French 2008]. Differences among theories[38] and models are, at best, differences in degree of partiality only and "In all cases, they are representational structures, and it is this aspect which the model-theoretic approach is designed to capture." [French & da Costa 2003, 60] In short, representative function of models is interpreted by relationship between partial structures, and the function of structural reconstruction is to indicate the most pertinent relationships, including idealization and approximation, between the various components.

Imperfect representations

[37] That is, if models are going to represent diverse scientific practice, they have to reflect pragmatic view on truth which underlies it. For this reason, Tarski's notion of truth as correspondence is inadequate to capture the relevant relationships [for details on pragmatic truth see French & da Costa 2003, ch.1].

[38] Theories are actually Mt's and are, although in the PSA understood explicitly as non-exact representations, generally rather vaguely characterized in the SA [see Teller 2001, 396, ff9]).

As PSA states the empirical basis we discussed previously: "Both our everyday and scientific beliefs concern representations that are not determinate, not tight, not complete; they are idealizations and approximations, they are imperfect, and they are partial, reflecting our partial knowledge and understanding of the world" [French & da Costa 2003, 17]. We saw that Cartwright's [1983] illumination of problems of idealization and approximation, which brought them in the focus of subsequent philosophy, wasn't the first challenge to the possibility of an overarching account of models in science. Although Cartwright's work connected the two into the notion of 'scientific improvisation' and, crucially, pinpointed the idealizing function of science to center stage (perhaps together with Giere 1979)[39], approximation specifically was discussed in detail already by Redhead [1980], and Hesse (1966) and Achinstein (1968) argued, although against the RV, that scientific practice is far too complex to be captured in terms of mathematical structures.

To answer this problem (and to construct a unified account of models), PSA in the first place incorporates ideas of these different critics and formalize them. They specifically draw on Hesse's idea of models as analogies and Black's idea of models as icons [French & da Costa 2003, 47-8].[40] We discussed Hesse's epistemically significant 'neutral' and 'negative' analogies in Ch. 2.1. which account for differences and unknown relations between model and its target. They account for fallibility and openness of our knowledge of the inquired systems, and also for model abstractness and – by the possibility of features not determined by the model [Hesse 1966]. Black pioneered the relevant analysis by observing that "There is no such thing as a perfectly faithful model; only by being unfaithful in some respect can a model represent its original" [Black 1962, p. 220]. In this sense, he claims, all models, including Hesse's analogies, should be regarded as iconic – as representing features of interest and thereby incorporating both the faithfulness and unfaithfulness of the representation.

These iconic relationships together with Hesse's neutral analogies are unified in the PSA by being "expressed in terms of relevant structural relationships, suitably weakened to include the more plausible similarity … and suitably broadened to cover similarities in both formal

[39] We are concerned here, of course, only with the focused analysis and explicit emphasis of these features. Aside from that, history of science has seen many discussions of idealization and approximation [see Teller 01, 395, ff.5], such as Galileo [McMullin 1985), Duhem [1954], Scrivin [1961]; Shapere [1969], etc.

[40] According to Suppe [1977, pp. 221–230] and van Fraassen [1985b] iconic models were incorporated from early on into the SA.

structure and material properties [French & da Costa 2003, 48]. Since these are already conflated in Ronald Giere's account [1988; and in a lesser extent 1979], what PSA contributes is the *formalization* of this approach, and specifically Giere's account of similarity, by "representing similarity in terms of partial mappings" [ibid, 54]. The main feature of Giere's framework is that model/world relation is understood in terms of similarity in *limited respects* and even in those respects only in *certain degrees* [Giere 1988, 81].[41] For example, "in asserting that the billiard ball model is a model in the relevant sense, one is asserting that the behavior of the gas atoms can be represented by the behavior of billiard balls, in *certain respects* and to *certain degrees* (or, conversely, that the behavior of billiard balls is *similar* to that of gas atoms)." [French & da Costa 2003, 49; italics added]. Similarity is then accommodated within the PSA through a consideration of the number and kind of relations that enter into the correspondence. Therefore the similarity of the billiard ball model to its target system is understood as a *structural similarity*, expressed in terms of a partial isomorphism holding between the respective families of relations.[42]

However, we already know that structural relationships are important – e.g. Morrison [1999] express them in informal terms and hinges scientific explanation on structural dependencies. But how the PSA manages to capture idealization and approximation, and how it promotes our understanding of these mechanisms? It certainly has the advantage over the RV and the mainstream SA because on neither view is there any room for ambiguity, doubt, fallibility, partiality, or openness. Some of them do discuss idealization and approximation along the way, but nobody (except Cartwright and Giere) is promoting the model/world relationship as similarity or inexact fit of some kind. But what about idealization in the sense of the definition from the previous chapter – as deliberate distortion of properties, or approximation - in the sense of being close enough for the case at hand?

So where are idealization and approximation in the PSA picture? Approximation is captured, it is claimed, in a sense of Redhead's [1980] impoverished model (with limited

[41] Following Kuhn, Giere [1988] emphasized the ubiquity and importance of exemplars, both pedagogically and heuristically, what eventually diverted his attention to the role of similarity.

[42] In the first chapter of part III we'll tackle the question whether and why Giere's notion of similarity is not sufficient characterization on its own for purposes of model theory. For present purposes, we'll hold to the assumption that informal similarity claims (as in Giere [1988]) are too vague and rather unclear comparing to the respective structural exposition [cf. French & Ladyman 1999]

faithfulness), which is used as an approximation to a theory in order to facilitate computation [French & da Costa 2003, 50]. Idealizations and abstractions (such as atoms with zero volume, negligible forces of interaction, and so on) are treated in a similar manner – as simplifying assumptions which facilitate the solutions of equations [ibid.]. Functional importance of these procedures are acknowledge, from context to context, as the necessity of limited unfaithfulness of scientific constructs [ibid, 58]. However, all this was already introduced by Cartwright, Giere, Redhead, Hesse, Black, and others. The PSA import is the expression of this inexact fit in set-theoretic terms. The great work was undoubtedly done in connecting all these splendid analyses, but the question is "What is gained by formal reconstruction (when everything is already connected and explained in plain words)?"

Approximation, idealization and abstraction – limited unfaithfulness in general – are captured by introducing appropriate partiality into families of set-theoretical relations which represent a model's structure [ibid, 58]. It is accommodated by taking care regarding which of the Ri are to be dropped, or ignored, to give the impoverished set R'i – this way "the 'degree' of approximation can be measured" [50]. It is then expressed by appropriate partial isomorphism between partially defined model and data model. But this is only a restatement of what we already described in natural language – the question was how this deepens our understanding, beyond that what we already figured out in natural language? What have we gained in our insight into the mechanisms of science by making formal our idealization analysis? Furthermore, for the final word they cite Suppes:

> "It is true that many physicists want to think of a model of the orbital theory of the atom as being *more than a certain kind of set-theoretical entity*. They envisage it as a very concrete physical thing built on the analogy of the solar system. I think it is important to point out that there is no real incompatibility in these two viewpoints [the physicists' and the SA's]. To define formally a model as a set-theoretical entity which is a certain kind of ordered tuple consisting of a set of objects and relations and operations on these objects is not to rule out the physical model of the kind which is appealing to physicists, for *the physical model may be simply taken to define the set of objects in the set-theoretical model*." [1961, 166-167; italic is ours]

However, from the perspective of understanding the reality distorting attributes of models, function of similarity, etc., focusing on restatement of scientific models in terms of mathematical structures seems irrelevant [cf. Suarez & Cartwright 2008]. Looking back on the structural enterprise, the PSA's specifically, it becomes dubious whether the explanations of modeling on the basis of structure that are claimed are really provided. In the next two sections we'll examine this claim from the formal point of view.

Returning to 'analogue' models, the second stepping stone of the PSA, we can see that formalization draws its appeal from the fact that the analogy depends on the relationship between the mathematical structures concerned.[43] We can agree with this point, but that still doesn't mean that explicating this relationship in mathematical terms contribute to our analysis and understanding; structural specifics seems to be better left to scientists. In short, we don't see how the mathematical reconstruction improves the explanation of idealization and approximation – yes it formalize which relationships are idealized or approximated, but that was already explicated by Cartwright and Giere (how else could we made the points from the previous chapter!?). Also, it remains spurious whether the scientific constructs stand in such an ordered relationship as structural reconstruction, even partial, is forced to represent. There simply is no evidence that it can capture the dappled perceptive of modeling that emerged mainly from the research projects of models in physics and economics [Morgan & Morrison 1999; and Cartwright 1999].

Although we think that incorporating partiality of our scientific knowledge is important, as well as is the unification of previous philosophical analysis, we do not consider that the PSA mitigation of the SA has the capacities to properly capture and emphasize the idealizing and approximating aspects of science. Having in mind that a consequence of idealizations and approximations is complicated autonomy of models [Cartwright 1983; Morrison1999],[44] we must observe that the PSA does not deviate significantly from the SA in this respect. Therefore, and because the principal question is concerning the value (or the import) of structural reconstruction in general, we will tackle the PSA further on as a part of the same group as the

[43] Although these are not formal relationships – remember Achinstein [1968] and that mathematical expressions describing, say, the heat conduction and electrostatic attraction are quite different; their analogy entails only similarity [p. 236]

[44] We say 'complicated' in the constructional and functional sense of the previous chapter. We must add that the PSA attitudes regarding this question, and their rejection of the purported autonomy, are somewhat black-and-white. It seems that by concentrating on the point that approximations enable non-deductive derivations from the theory, they miss the slender point that experimental considerations and empirical results play an important role in model construction [cf. French & da Costa 2003, spec. 54-56].

rest of the SA. In this setting, the following section will deal with usefulness of formalization in disentangling the inter-structural relations (i.e. the meta-structural comparison), and the next one with problems of structural reconstruction regarding the model/world relation.

Comparison of models

All arguments against the SA are concentrated around a single axis – the limitedness of mathematical reconstruction to represent the philosophically relevant features of scientific modeling. Consequently, there are essential and numerous similarities between them. In this section, we will discern two main objections regarding the capability of the SA (including the PSA) framework to enable us to successfully compare and evaluate different models. By the 'SA framework' we understand its mathematical (preferably set-theoretical) reconstruction of structural relationships in models and characterization of theory/model/world(Md) relationship in mathematical terms.

Recent years, following Suarez and Cartwright [1995], reintroduced the difference between models and theories.[45] The former are then understood as partially autonomous mediators between theories and phenomena, constructed in such a way to *explain* the specific mechanisms and thereby functioning as independent (autonomous) sources of knowledge about them [Morrison 1999]. According to this approach, scientific models *represent* their target systems, independently of their relationship with the theory, via this explanatory power regarding the specific mechanisms in their targets. On the other hand, the SA captures representational function of models by explicating their structure through morphism relation with Md's (isomorphism, partial isomorphism, empirical substructures isomorphism, etc.). First objection against the SA capability to compare models is that the structural criterion cannot explain representational function because, in many cases, it is not possible to use the criterion to characterize one model as more representative than the other; and this ordering is a necessary condition of any criterion of representation [Morrison 1998].[46]

[45] The difference that even the PSA abolishes: "… from both the extrinsic and intrinsic perspectives, the distinction between models and theories ''per se'' dissolves … the differences are, at best, differences in degree of partiality only" [French & da Costa 2003, 60]

[46] This problem of comparing different models is tightly connected with representation of dynamics in scientific practice – namely, the capability to represent the development of models and theories [Portides 2006].

We will again look at an example from quantum theories; they are usually the locus on contemporary philosophical debates mainly because of their unprecedented exactness. Extraordinary mathematical complexity of these theories makes possibility of a solution for their equations, for most physical systems, practically impossible.[47] Concretely, application of Schrödinger's equation on arbitrary number of nucleons produces the "many bodies" problem, for which there are no analytic solutions available.[48] Physicists are therefore forced to conduct explorations of the structure of atomic nucleus by phenomenological, half–empirical techniques. Phenomenological construction of Hamiltonians contributed in significant insights in the physics of nucleus, among other things the construction of the liquid drop model, the shell model, and the unified model [for a discussion of these models see Morrison 1998; and 2011; Portides 2005; and Bueno, French & Ladyman 2012b]. These models are based on different conceptions of nucleus and nuclear motion – they incorporate different and contradictory assumptions about its structure and dynamics, and therefore it is extremely difficult to mutually compare them. Stated in very simplified terms, the liquid drop models the nucleus as incompressible nuclear fluid (nucleons as closely coupled) and explains only their collective movement. The shell model presumes that nucleons move independently in an average nuclear field, where the nucleus motion is an aggregate of individual nucleon motions. Finally, the unified model postulates independent motion of nucleons in a collective, slowly shifting nuclear potential, thereby accounting for interaction of a collective motion with individual nucleons.

Each of these models explains the features of the nucleus the other two do not. However, the unified model outmatches the others since it explains how collective and particle motions are interconnected in the nucleus. Now, how can we make this comparison within the SA framework? In the SA, this is done by literally counting features (properties and relations) which each model shares with the relevant data, and the best model is the one most similar to the data – the one with the most features shared [cf. e.g. French & da Costa 2003]. However, the problem in the case of nuclear models is that we cannot measure similarity directly, because the models are not cumulative – similarities of a weaker model are not a subset of similarities of the stronger

[47] As Cartwright [1983] has stressed, unaugmented theories and theoretical models can rarely be used for representation of the real physical systems.
[48] Analogue problem exists in Newtonian mechanic, "the three-body problem", for which CM cannot give us exact solutions.

one.[49] Representational capacity of different models cannot be clearly ranked by the structural criterion of the SA because these models represent different aspects of the target system (collective and particle motion) and because they represent it in different ways (by incompatible assumption about its structure and dynamics) [Morrison 2011, 351; also 1998].[50] Since any two of the above models are based on different hypotheses regarding the nuclear behavior, if we rely solely on morphism relation we cannot make the comparison between them as we above did. These models are representing different aspects of nuclear motion, and they do it in different ways, hence the simple counting of shared features will just not do.

The PSA combat this Morrison's objection on two grounds. Firstly, they write off the liquid drop model and the shell model as developmental models. These models, it is argued, are only stages in the development of the unified model and ultimately the quantum chromodynamical theory; their autonomy is only temporary, in the sense that "it is just not clear *yet* how [they] might be related to high-level theory" [French & da Costa 2003, 55; also Bueno, French & Ladyman 2012b, 102]. However, casting out the liquid drop model as 'developmental' is not changing the fact that, because of theirs mathematical incommensurability (and physical incompatibility), the structural criterion (SC) cannot evaluate their comparison. Also, this is connected with the previously discussed problem of 'future science' – actual non-theory driven modeling is cast away by subsequent theory-derivation. There is a subtle PSA point here – the liquid drop model was autonomous at the time, but because it was physicists' firs guess at the nuclear structure they used many improvised technics, such as semi-empirical results, in its construction – they still couldn't find the way to derive it from the theory [Bueno, French & Ladyman 2012b].[51] In this sense the mathematical comparison is not really necessary since the overreaching theory already does that – by being in the stronger derivability connection to her, the unified model is confirmed as the best. But even if we accept that previously autonomous model is replaced by the later, more theory-related one, we are still left with the SC's inability to

[49] By the way, more similar/exact model need not be the better; as the famous case of Copernicus's sun-centered cosmological model forcefully demonstrates (before Kepler 's corrections it was empirically less adequate comparing to the Ptolemaic model). The point is, in certain contexts a weaker, less exact model will be more explanatory [Teller 2004]; to the context relativity problem [e.g. Giere 2010] we'll return later on.

[50] Above all, it is not possible to make a clear distinction among Mt and Md's, because semi-empirical origin of nuclear models – and this distinction is necessary for structural analysis of model representation propagated by the SA.

[51] By derivation from theory here is not necessarily considered the deductive derivation which the PSA explicitly discards. However, the 'good models' are this way still connected with theories.

compare the early models with the final one (to this we will return in the next passage). Finally, we must add that, in present context, the possibility that when a theory is completely formed (and which one is?) we will be able to perform structural reconstruction is not very compelling to us. Regardless of whether this situation of all-and-only Mt's will ever arise, we presently need tools for comparison and evaluation of a pleiad of models of various constructional origins.

Second line of the PSA response regarding the incapability of structural criterion to compare models is acknowledgement that we must use assistance of natural language [French 2008]. In fact, it seems that there is a general agreement that the pure form of structuralism in the context of the SA doesn't do the job when it comes to representation [French & Saatsi 2006].[52] In the purpose of comparison, we must call for additional, informal factors to relate models both to the relevant background knowledge and to the target systems. The PSA claim is that, by allowing for such linguistic factors, they occupy the middle ground between the structuralists (such as Sneed), and those who eschew the formal representation altogether (such as Giere) [French 2008, 275]. This acknowledgment of what we can expect from formal representation returns us to the problems we discussed regarding understanding idealization and approximation: if natural language is necessary, why troubling with the formal reconstruction of a model's relations altogether? How our understanding has deepened by the formal part? From our point of view, it is questionable to what role that addition is actually serving – either the formal part is used for a comparison of models – in which case it simply cannot perform the function [Morrison 1998], or the comparison is done by the non-formal factors – in which case PSA falls back to Giere's account. Also, resorting to the both formal and non-formal factors is simply obscuring. It is a trivial matter that mathematical equations, by which scientific models are generally expressed, satisfy a structure and that consequently structural features of models cannot be ignored. But understanding scientific models, together with their epistemic mechanism, primarily as mathematical structures (subsumed under a theory structure) is highly restrictive perspective that makes us overlook important elements used in the construction of our most successful models [Portides 2008]. From mathematical character of models does not follow that reconstruction of this structure is required for philosophical understanding of models and

[52] In spite of van Fraassen's and Suppe's eschewing of the linguistic elements as entirely irrelevant. Even if we bypass that theories and models need to be described somehow, we must use language to state how the models are related to their target systems [Giere 1988] – since the fit is only imperfect, we must state in which aspects and in what degree we consider a model to match the system.

their epistemic roles. On the other side, when we are reduced to the non-formal reconstruction, that doesn't meant that we are unable to represent this mathematical nature of models – for that we already have the mathematics of the theory – it is just that we do not try to present complex concepts of scientific representation, explanation, idealization, abstraction and even approximation, through restrictive mathematical strictness.

By limiting their expectations of formal representations the PSA comes to a delicate situation, since we saw that striped of structural framework the PSA reduces to Giere's account of models (in a sense, their acknowledgment renders reconstruction useless). Returning to the first line of response to the objection that the structural criterion cannot compare different models, discarding the liquid drop and the shell model as developmental opens up another problem for the PSA closely related to the comparison problem. By rendering certain models as developmental, structural criterion would lead us to undervalue the representational and explanatory function of important models in the history of science (simply because they fail to meet the criterion) [Portides 2008]. If we were to dismiss the liquid drop and the shell model on the grounds that they are only developmental, we would precisely miss their role in the construction of the unified model. Thereby we would overlook the importance of the previous two models in achieving improvements in our representation of physical systems, and fail to understand why the unified model came to be considered so successful [Portides 2006]. Hence we are forced to regard the first two models as representational, in spite of being developmental, and must conclude that, besides its inability to compare different models, the structural criterion fails to represent the dynamics in scientific practice – namely, the development of theories and models. By detaching a model from its evolutionary history, it also blurs how scientific concepts are formed and how they evolve during this process.

The preceding analysis doesn't hiders structural criterion useless for all cases of model comparison, of course, only for the ones where models are not complementary [Morrison 2011]. Considering the widespread usage of inconsistent models to treat the same system (models of fluid dynamics, electrodynamic theory, or models of nuclear physics and quantum chemistry, to mention just a few) in many important cases there are significant epistemic and methodological burdens for the mathematical reconstruction, which is hindered in amount of information it can provide us with, and yet which we are able to grasp in ordinary scientific analysis.[53]

Mathematical reconstruction and the scientific content

Until now we have been investigating the usefulness of structural reconstruction in explicating inter-structural (model/model or model/theory) relations. A mapping relation between structures turns out to be extremely limited in this respect. Now we turn form the comparison of different scientific constructs to how mathematical structures can help us understand the way scientific constructs relate to the world (model/world relation).[54] We already partially tackled this question in the form of Md analysis, but now we will focus on one specific aspect of this relationship, namely to the question "What happens with the content of the scientific theories and models when they are reconstructed mathematically?"

Already Achinstein [1968] observed that the mere identity of formal structure is not enough to confer heuristic plausibility from one structure to another – in the usage of models, their content must also be accounted for (and, significantly, the PSA accepts this importance of content [French & da Costa 2003]). Achinstein's objection is a connection of the comparison problem and the content problem because the reason why we cannot relate models (notably, in the case of analogies), he argues, is that, even when they are mathematically comparable, we cannot capture the heuristic fertility without recoursing to their non-mathematical features.

When we focus on the later problem, what the structural criterion is exactly missing? Firstly, the epistemic aim of science is the acquisition of knowledge about concrete, actual beings in the world. We saw at Suppes [1957] that the empirical step in the set-theoretical axiomatization of a scientific theory is Mt/Md embeddability relation. Since many representational models are not Mt's, we can instead require that for any model to be considered as a part of theoretical scientific knowledge about certain species of concrete actual beings a

[53] In a sense, the problem of idealization, which is central for model theory, is only a variation on inconsistency theme since the idealizations present in models and theories are typically inconsistent with realistic descriptions of the phenomena [cf. Morrison 2011, 343].

[54] It can be sad that 'comparison of models' is an objection that the structural criterion is too narrow to properly individuate models (because it cannot compare inconsistent models), and that the 'missing scientific content' is an objection that the SC is too wide (since it cannot specifies that specifically scientific component of scientific models). Considering the later issue, weakening the required morphism (as with the partial structures) just emphasize the problem of proper individuation of models, because now there are even more available structures to select from [cf. Collier 1992].

necessary condition should be that all relevant Md's are embeddable in it. However, that would require us to formalize, for every scientific model, a new set of structures (contra Suppes who formalized CM and consider every model of CM as belonging to its set-extension – to family of structures defined by Suppes-predicate). This multiplication of structural reconstructions would further shrink the capability of the SA to relate models. Also, even if we bypass this mathematical diversity, we are faced with the problem of a clear distinction between representational and data models, which is jeopardized by the intertwining of theoretical and empirical elements in the construction of many scientific models.

Secondly, the main problem is that, strictly speaking, models are regarded in the SA as structures defined by a Suppes-predicate – its set-extension. Structures meeting the set-theoretical predicate are literally referring to sets only. On the other hand, every scientific theory is about concrete actual beings in the world. How can we take that abstract structures to apply to the real beings, when they are literally about abstract sets in the domain of the set-theory [cf. Muller 2011]?

A way for the SA to dodge this argument is to stress that the main question is not what the scientific models ontologically are, but how can they be best described in order to gain a philosophical understanding of their function – in this sense the SA should be viewed as a rational reconstruction. In this respect, the question is not "What is the nature of models?", but "Can a set-theoretical description represent the way models are used in scientific practice?" [French & Ladyman 1999, 107]. Although we claimed in the previous chapter that the specific SA instances do not manages to properly grasp this function, the principled question regarding mathematical reconstruction remains. Stated somewhat differently, are mathematical structural relations everything what is relevant for describing the mechanisms of scientific representation and explanation?

So what can a set-theoretical structure tells us about the real beings? Specifically, where is the conceptual and propositional content of a scientific theory, as used by scientist, when set-theoretically reconstructed [Muller 2011]? This losing of the scientific content can be seen on the notion of force. In CM force is a real three-dimensional vector $\mathbf{f} \in R^3$, in a sense that it has a direction in R^3, which is its *direction in in 3-dimensional space* when and only when we take R^3 to represent 3-dimensional space, and, secondly, in a sense that it has a *strength, or magnitude*, when and only when we take length $|\mathbf{f}| \in R^+$ to represent this strength [Muller 2011, 101].

Muller's point is that "these explications go beyond set-theoretical sentence '$\mathbf{f} \in \mathrm{R}^3$', because they involve the concepts of *direction in* 3-*dimensional space* and *strength*, whose content clearly goes beyond the language of pure set-theory", in which we can only talk about pure sets [ibid.].

We can try to defend the SA by reiterating the intertwining of set-theoretical reconstruction with the natural language, as we did before [French & Saatsi 2006]. In this manner, the SA proponents could claim that the entire meanings do not need us at all, because what we try to do with mathematics is to represent only the *relevant aspects* of scientific theory – its structure – not to interpret it in total.[55] Namely, the reconstruction does not have to refer to the real beings (i.e. the world) because, in order to talk about these beings, we do not need anything else than what we already have – since it is the scientific theory that should, by assumption, provide us with knowledge regarding these beings. If, however, everything we want to know about the world is not already there, then we should conduct further investigations in order to complement, revise, or replace the theory. Point being that the scientific theory is indebted for the talk about the real beings, and the family of structures satisfying the Suppes-predicate try to tell u something about the scientific theory, not to replace it in its business. Therefore, whether on the level of practice models are set-theoretical entities or not, the question is how they should be represented in order to most appropriately capture the relevant aspects of scientific practice. Analogously, as a reply to the Muller's objection we can notice that the point of mathematical representation is not to stand on its own, instead of the scientific theory (and therefore to be the scientific theory), but to allow us to represent the relevant, structural aspects of the theory in philosophically significant way.

However, this would be begging the question. The initial objection was precisely that the explication of structural aspects is not enough to philosophically represent scientific theory – to grasp the crucial scientific mechanisms, specifically scientific representation and explanation. The SA cannot respond to this objection by claiming that all we intended to do with mathematics is to represent the structural aspects – the problem exactly was that structural aspects are not enough. So how can we accommodate the scientific content? The above PSA answer seems to be

[55] From the logical point of view, complete interpretation would be either (unnecessary) complication (if it is not 1-on-1 mapping), or only a different formulation of scientific theory (its interesting version). As philosophers of science, we do not aim to create a competing scientific theory (that is the job of scientists themselves), but to *understand* its nature – grasp specific scientific mechanisms.

on the right track – restructuring needs to be both in mathematical and in natural (scientific) language. Science itself is already this way – not completely mathematical [cf. Feynman 1967], so combining languages is the obvious choice for the reconstruction. However, underlying problem for the SA is that their conception of models bases on Suppes's idea that the meaning and use of the concept of model is the same in mathematics and empirical science and that, consequently, the set-structures could be taken as what working scientists mean by 'model' [1960]. Yet we see that scientific models contain epistemically significant dimension of content, which cannot be fully grasped by the set-theoretical extension of a Suppes-predicate. Since the scientific representation and explanation uses models in a non-formal way which promotes their content, if we try to disclose it from the picture we'll miss the essential aspects of model-governing mechanisms. Specifically in the case of phenomenological models, which are neither quite realizations of a theory, nor realizations of descriptions of the world, but the specific mediators between the two. These particularly tailored descriptions do not fit nicely in the Tarskian notion of model. Mainly in the aim of grasping the presence of these heterogeneous descriptions, and associated bewildering interconnection of scientific constructs (which is characteristic of non-formal languages), both PSA and Suppes leave room for natural language, and thereby denounce completely rigorous reconstruction [French 2008; Suppes 2011].

Important question that remains is whether this defense is a too much of a weakening of the original Suppes's idea that shaped the initial stages of the model revolution. Namely, have we by adherence to the idea of 'representing the relevant aspects' diverged from the 'rigorous reconstruction' of scientific theory? In any case, it seems to be a significantly weaker thesis that it is generally presented – notably because, in the mentioned accounts, it is not the structural reconstruction that do the job that a model theory is purported to do. These jobs - capturing the main features of scientific models – are performed by the non-formal aspects of the SA accounts. If we understand structural reconstruction only as a philosophical tool in the above sense (only assisting the natural language), we saw that it is extremely limited in what it can do. Furthermore, it is questionable whether there is anything philosophically significant added to the observation that models share parts of their structure[56]; what we already have in a natural

[56] There is a simpler observation on which we'll dwell later on, that a scientific model is a single concrete system (or an abstract system in the case of model templates), and we already mentioned that it is trivial consequence that a system satisfy a structure.

language description. Of course, many aspects of scientific models are described mathematically (and mathematical models are the main topic in model theory), but the philosophical aim is to elucidate the essential scientific mechanisms, specifically explanation and representation, epistemic connection with other structures, and so on. Reiterating the existing mathematical structure doesn't provide the philosophical clarification of the relations among scientific constructs we require – prevalence of mathematical elements do not entail that structural relations clarifies relations among scientific constructs. What we need is an explanation of *why* the shared structure enables us to represent the one system with the other.

Subtle point would be that by considering the math as complementary to the natural language descriptions, 'the rigorous structural reconstruction' is neither rigorous, nor it is really a structural reconstruction. It is a little more than a philosophical analysis (in the case of the PSA, of Giere's similarity thesis), with some of its parts mathematically reiterated. To do the job it is purported to do – provide us with philosophical understanding, structural reconstruction remains short in conceptual resources and therefore proves to be irrelevant for explicating models' relations with the target systems. Simply stated, if explicating, for e.g., Hesse's neutral analogy in the set-theoretical terms cannot work without English descriptions, why do we formalize it at all – hasn't Hesse already explained the entire concept without the set-theory? We already said that, we think, idealization, abstraction and approximation mechanisms, as well as a model's relations with the world, its concept-connections and analogies, are not philosophically clarified by formal relations. The specifically scientific feature of scientific models – their empirical content, remains elusive for the relations among structures understood in the same way as in mathematics.

Part II

Chapter 5

Feynmanian Perspective

Two tracks of development. Retained attitudes.

Opening up the philosophy of science.

Two tracks of development

The last two chapters have presented us with two kinds of problems the SA conception of science is faced with. On the one side, discrepancies with the scientific practice have mounted, specifically since 1983. and *How the Laws of Physics Lie*. Many detail case studies emerged which suggests that the real modeling is much more theory-independent than the SA presumes, and much more entangled with data [Morgan & Morrison 1999; Cartwright 1999; Teller 2004; Portides 2005; Suarez & Cartwright 2008; Gildenhuys 2013; etc.]. On the other side, specifically in recent years, its formal problems became apparent, particularly regarding the capability of mathematics to grasp science specific reasoning procedures. Notably, mathematical representations of approximating and idealizing functions of science were questioned and its role in illustrating the mechanics of scientific representation and explanation [Morrison 1998; Muller 2011; Norton 2012; etc.].

One of the most important outcomes of both the practice and the formal problems is the requestioning of the concept and the role attributed to theory by the SA [Cartwright, Shomar & Suarez 1995; Morrison 2007]. Theory came to be seen as something more than a collection of models, and at the same time, something less than a proper vehicle of scientific representation and explanation. It came to be regarded as a tool for the scientific model construction, a sort of abstract and not necessarily committing instruction for model-building – a construction kit [Suarez & Cartwright 2008; Podnieks 2014]. Also, with abandoning the idea of 'deduction of the *scientific* models from theories', formal strictness traditionally attributed to science is abandoned; science envisioned in the forms of dogmas of meticulously applied inductive logic, or physics talking in the language of mathematics lost its grounds in its stronger reading – an apparent victory for the old Humean struggle against the mathematics and deduction as the model of scientific knowledge and methodology. In general, what especially emerged from analyses of the formal issues of the SA is the question "Is mathematics a proper tool to represent the logic of scientific reasoning?"

Retained attitudes

In asking this question we must not set off from our mind the process how the SA became dominant account of scientific practice. First and foremost, the SA put the models in the center of the stage – all on the observation that scientist generally are model-builders, not the axiomatizors and theorem-provers [Suppes 1960]. Also, in representing science in mathematics rather than in metamathematics, the SA drew philosophical analysis closer to the science which itself generally uses mathematics, and thereby, apparently, outflanked the linguistic problems which plagued the RV. Indeed, the SA explicitly renounced the language problems, and focused to what these linguistic formulations refer to – namely models [Suppe 1989; van Fraassen 1989].

However, when we see how the things have turn out both in the practice and on the formal side, we may, referring to the SA, repeat van Fraassen's words regarding the RV:

> "In any tragedy, we suspect some crucial mistake was made at the very beginning. The mistake, I think, was to confuse a theory with the formulation of a theory in a particular language [van Fraassen 1989, 221]".

Analogously, the mistake, we think, was to confuse a model with the representation of its structural relationships in a mathematical language. The fatal flaw in the model revolution was that following Suppes [1960] the models in science were taken in exactly the same sense as models in mathematics. By this move, the SA kept the crucial feature of the RV that broth their demise – use of a strict language to represent scientific reasoning. Of course, this time it was the informal set-theory instead of the formal logic, but it bypassed the obvious fact that:

> "One reason why mathematics enjoys special esteem, above all other sciences, is that its laws are absolutely certain and indisputable, while those of other sciences are to some extent debatable and in constant danger of being overthrown by newly discovered facts." [Einstein 1923, 27].

Having in mind that together with laws the flexibility of methodologies is also in question [Feynman 1967] we may ask: If mathematics is stricter than physics or biology, how, on the philosophical level, can mathematical relations capture, say, the relationship between a model and the relevant measurements, without distorting the two in an unrealistically stringent picture? In this sense, we must say that the SA presents us with a too much distorted model of the scientific modeling [cf. Teller 2001].

What is really the issue here? As Achinstein [1968] has noted, the comparison between the SA and RV is fruitless – the devil is in the details. Both approaches introduced huge philosophical advances we discussed earlier, but the cost of infelicitous formalizations was the inability to properly individuate the scientific constructs. On its part, this introduced us with the plethora of philosophical conundrums – debates about the proper logical and mathematical means of philosophical representation of theories and models, instead of dealing with substantive problems of scientific representation, explanation, prediction, experiment, etc. The RV conundrums such as Ramsey- and Carnap-sentences or Craig's theories to mention just a few, were a mere artifacts of modeling science by logical formalizations, and this was much discussed in previous decades [cf. e.g. Suppe 2000]. However, the SA produced the same divergence from burning issues of philosophy of science by endlessly debating about the proper mathematical characterization of Mt/Md relationship (is it the isomorphism, homomorphism, injective homomorphism, partial isomorphism, etc.) and thereby engaged in endless readjustments of the mathematics to conform it with the scientific practice (a striking similarity with the RV readjustments in the aim of proper individuation, we discussed in chapter 2.1.). Furthermore, the pragmatics of scientific practice was completely missed by the mathematical approach [Contessa 2011]. Once again, contra early Suppes, the scientists were imagined as, now mathematical, theorem provers. We can wonder: where is his idea of them as model-builders in the static vision of science, perceived through mathematical relations? We must say that some 'received' views apparently remained widespread, in the first place the drive for axiomatization and focus on the philosophical tool of representation, but on the deeper level in retaining the D-N model of explanation – through understanding laws as exact (that is, empirically adequate) instead of as a deliberate distortions of the (phenomenological) truth [cf. Cartwright 1983], and theories (through Mt's) as directly representing the reality, as opposed to representation mediated by the models [Morgan & Morrison 1999]. The retained presumptions of the RV are even more striking when we remember that the RV too was considering the methodology of science as the same as that of mathematics, only with the addition that this methodology can be best represented metamathematically. In this respect we can repeat the words of Gabriele Contessa:

> "The crucial divide in philosophy of science, I think, is not the one between advocates of the syntactic view and advocates of the semantic view, but the one between those who think that philosophy of science needs a formal framework or other and those who think otherwise." [Contessa 2006, 376].

One of the results of this focus on proper morphism search is bubbling of the realism debate inside the SA. It came to occupy the bulk of the advanced research and many SA versions differentiates only over this question, and yet, as Psillos [1999] observes, this has little to do with genuine problems of scientific knowledge. It's mostly philosophical phantasy (a metaphysical pipe dream) with no difference in the perspective on the scientific methodology, but only concerning philosophical agendas – mainly regarding ontological commitments of scientists, which actually bare little or no weight in the scientists' minds, but only in the minds of philosophers, articulating their attitudes on theory-change.

So what was gained by replacing the logical deducibility of data from theories (from the RV) by model-theoretic entailment (the SA)? It seems that the advantages of diverting attention to models the SA has introduced have been blocked by holding to the set-theoretical reconstruction – logically, difference comparing to the first order language is minute [cf. Muller 2011], while substantive problems remains elusive to set-theory almost the same as they were to logic. A miniature of this similarity of formal accounts of scientific constructs is that a theory as a collection of models (which all are already entailed by the definition of a theory) too much resembles the deductive closure view of the RV (where a theory includes all of its logical consequences).

Opening up the philosophy of science

Enthusiasm of the SA surely draw from the fact that scientific models are mostly mathematical models, and therefore came the idea that they can be 'captured' in terms of mathematical structures. The problem is, when we turn from results (i.e. structures) to practice (i.e. methods), the mathematical character of scientific models remains – but not precisely. We have seen that in some crucial steps the scientific derivations deviate from the standard of informal mathematical strictness (in this respect the SA's idea of modeling as set-theoretic entailment is only a rough picture). Not to say that mathematics and logic do not have their place in the scientific enterprise, of course. It's rather that their usage is limited, or, precisely, extremely adapted to scientific needs. Furthermore, informal elements are involved in modeling, such as metaphors and analogies [e.g., Bailer-Jones 2002; Morgan 2012] or interests and purposes [e.g., Giere 2010; Winther 2015], which stand in a loose and ambiguous connection to the final product. It is hard to see how they can ever be captured by sets of structures defined on the Tarskian basis designed for artificial languages. The above mentioned rethinking of the concept of 'theory', which emerges as the key feature of the *new* model revolution, represents exactly this distribution of theoretical knowledge in a lose assembly resembling a natural language:

> "What we know 'theoretically' is recorded in a vast number of places in a vast number of different ways—not just in words and formulae but in machines, techniques, experiments and applications as well." [Suárez & Cartwright 2008, 79]

Turning to the practical consequences, the upshot of the emerging understanding of the theory, and thereby both the formal and the practical problems of the SA, would be that focusing on the structure of theories is disorientating because it is insufficient to account for differences among the scientific constructs (specifically among models and theories), or even that the structure of theories is completely irrelevant for understanding mechanics of scientific representation and explanation[57], because theories simply do not represent at all – only models do [Portides 2008].

In contemporary philosophy of science it is not a question anymore whether idealization and approximation are the central facets of scientific practice. Importantly, the PSA openly

[57] Irrelevant concerning explanations of the specific mechanisms in the target systems. Theories do, however, give explanations of highly abstract and idealized systems, as we saw on the case of QM, which, however, depends on modelers for application to particular physical systems.

endorse this as a fact [e.g. F&L 98, 67-8], although as the burning problem remains what is the appropriate philosophical framework to accommodate it. For that matter, the PSA proves to be an advanced adaptation of the SA to the more recent insights in the scientific practice. This way they perform an extremely important unification of many preceding good analyses, and thereby present a strong example of unification of theory of models. Actually the best so far, since they connect many important traditions from Suppes, Black and Hesse, to Redhead and Giere. Unfortunately, they retain the structural reconstruction requirement, although amalgamated with natural language descriptions, which, on our view marks them not as the middle ground between the structuralists (such as Sneed) and those who eschew the formal representation altogether (such as Giere) [French 2008], but only as the middle step in overcoming the SA from within and liberating the interpretation of science from a formal straitjacket while not losing the benefits of the Stanford model revolution. However, some attitudes of the SA proponents and their critics are much closer than it appears from their academic confrontation. Callebaut [1993], for example, interprets already Lloyd [1988] and Thompson [1989a] as abandoning the deductive-nomological model of explanation by pointing out that scientific explanations typically involve models drawn from a range of domains. In this sense, Darwin's theory is understood only as a general outline for many different explanations [Lloyd 1988], and evolutionary explanations in general incorporates variety of models, without reducing the 'theory' of evolution to any of these [Thompson 1989b]. These case-studies of the SA obviously lie on the line with the 'dappled world perspective' of Cartwright [1999] and Morgan and Morrison [1999]. We are better to concentrate in the future on these aspects of modeling the SA illuminates, than on their drive for the 'perfect structure' to encompass it.

To summarize, in the same sense as we discovered half a century ago that science is not logic, the lesson learned from the 'semantic excursion' is that science is neither mathematics. She incorporates some significant frameworks of the former, and she uses even more of the latter – that's why both the RV and even more the SA had given us enormously important insights, although fairly abstract. Not distancing ourselves from these traditions, we must recognize that to gain *specific insights* into how science works, how she makes concrete representations and provides explanations of specific mechanisms, we must plunge ourselves into more detail than logical or mathematical analytic tools can provide [cf. Cartwright 1999]. In practice, this means that we must use language of science (basically our sharpened natural language) to deepen our philosophical descriptions and understanding of her mechanisms – and in this is neatly summarized the epistemological point of our dissertation. To quote again Einstein: “The whole of science is nothing more than a refinement of everyday thinking” and our reconstruction must reflect this richness (and ambiguity and amorphousness) of a natural language [cf. Fine 1998]

In a sense, it is reminiscent of Hempel’s distinction between pure and applied mathematics, where the later accounts for uncertainties of mathematical laws in scientific usage [Hempel 1945b]. Although with the correction that we today explain this uncertainty on the basis of heterogeneous origin of model-building tools – i.e. modeling by freely organized means [cf. Cartwright & Suarez 2008]. To state it in somewhat freely assembled words which ignited the new informal approach to science and, on the object level – describing the relation of mathematics to physics, sums up in simple terms the bulk of objections the last few decades spawned:

> “The physicist, who knows more or less how the answer is going to come out, can sort of guess part way, and so go along rather rapidly. The mathematical rigor of great precision is not very useful in physics. But one should not criticize the mathematicians on this score. […] They are doing their own job. If you [physicists] want something else, then you work it out for yourself.” […] “Discovering the laws of physics is like trying to put together the pieces of a jigsaw puzzle. We have all these different pieces, and today they are proliferating rapidly. Many of them are lying about and cannot be fitted with the other ones. How do we know that they belong together? How do we know that they are really all part of one as yet incomplete picture? We are not sure, and it worries us to some extent, but we get encouragement from the common characteristics of several pieces.” [Feynman 1967, 56-7 & 83]

In short – science improvises and includes informal, ambiguous components.[58] We cannot depict her flow by a stringent logico-mathematical jacket.

[58] In the next part we will turn to the most important of these – intentional and contextual factors in modeling.

Part III

The Model View

Similarity.

Representation.

Models

Introduction

An extraordinary diversity of results emerged out of critique of the SA. What connects these diverse points is, in the first place, breaking away with the core commitments of the perspective regarding the role and usage of models in the scientific enterprise. Through the works of model revolution at Stanford, the orthodoxy of philosophers' views on modeling became that:

1. models are for the most part explicit mathematical constructs,
2. that they can be easily distinguishable from observational and experimental data, and,
3. they are constructed by clear mathematical entailment from theories.

On the contrary with these positions, its critics can be cluster around views essentially opposite. Illustration of most of them we saw in the part II. Problems with entailment of models by theories (3), which we tackled in the chapter three, showed that in many cases theory structure might be too abstract and idealized to allow us to construct representative models. Consequently, the limitation on theory roles and corresponding autonomy of models is the first point around which the new perspective is formed. Concerning the mathematical nature of models (1), that is their formal character, numerous limitations of presenting these nature by formal (most notably, set-theoretical) tools, discussed in the chapter four of the preceding part, resulted in an awareness that many nonformal aspects, such as scientists' values and policy views, or analogies and metaphors to other disciplines, play an important role in modeling and theorizing [cf. e.g. Savage 1990; Bailer-Jones 2002; Craver 2002]. This way the scientist as users of models, together with their purposes, interest, scientific values and policies, became the second central pillar of the emerging picture. Finally, distinguishability of (explanatory) models from data (models) (2), spawned two levels of responses. In the first hand it was undermined already by analysis of model construction, where autonomous models were found to incorporate both theoretical principles and empirical results [Morrison 1999; Teller 2001]. In the second, by observing the plurality of model components (including values, analogies and policy views, as well as mathematical concepts and theoretical consequences), together with the plurality of different types of models used in science (representative, explanatory, historical, educational, toy models…) [Hacking 1983; and 2009; Frigg & Hartmann 2012], By these observations the *turn to practice* was made and divisions between the theoretical models and data (2) ceased to play a central role in the inquires of scientific modeling.

On these three bases we'll try to delineate the new picture of models which we'll call following Paul Teller [2001] the Model view (MV). The starting assumptions will be constructional and functional autonomy of models, plurality present in scientific practice, with the focus on the nonformal aspect present in it and the place of model-users and their preferences. The challenge which we'll try to tackle in this final part of our dissertation will be to sketch a coherent and unified picture out of diverse multitude of critiques of the SA orthodoxy. In it we'll try to stay in the traditions of the practice turn in the philosophy of science [ignited by Hacking 1983; and Galison 1987; which had its preceding in the social studies of science starting with Kuhn 1970], and specifically its analogue tradition in the model theory developing from Cartwright [1983], through 'models as mediators' project [Morgan & Morrison 1999], to works of an array of contemporary authors, prominently including Teller's unifying work on exactness of models [2003], Contessa's on non-formal framework [2006], and Giere's agent based approach [2010]. This way the Model view, sometimes also called the Pragmatic view due to its focus on extra-syntactic and extra-semantic components of models, will try capture, together with all those mathematical features of models, those informal components and aspect present in any human cognitive activity. By tackling the problems of scientific practice, the MV will place the model theory in a broader framework of science studies aimed at giving a complete and thorough picture focusing not just on its intellectual side, but also on its human factors.

A disclaimer is in place here. By focusing on the informal side of modeling, we'll not succumb to the social views on science suggesting that science is primarily shaped by the normative values of scientific elite and its politics. On the contrary, although these factors certainly take some part, the values and politics that we have in mind are of the epistemic sort and as far as we are concerned this aspect of science should be understood primarily, to use Magnani's term [2012], only as 'epistemic warfare'. The main and pervasive factor, however, stays, to be analytic reasoning and the mathematics as the means of expression. Having sad this, the MV should be perceived in the first place as a further development of the model revolution at Stanford – its supplementation by quirks and nuances of construction and usage of mathematical structures, and their adaptation to specific needs, by human agents. In this sense, due credit should be paid to the SA for advancing our understanding of mathematical aspects of models and illuminating their formal structure. In the same time, by focusing exclusively on the structure it have neglected or even obscured (!) the wider picture of modeling, specifically its practice and nonformal aspects. Since both are significantly contributing to *organization* of scientific endeavor [Wimsatt 2007], as well as to rules of composition of the scientific models, we must try to develop an explicit nonformal framework of modeling. To this aim the MV is devoted – to elaborate the practical side of modeling, together with its mathematical and human aspects.

Since Cartwright lunched a new model revolution in 1983. models became objects of central significance for understanding science. Being positioned between theory and the world, in a pretty direct sense, their role of mediating between the two opened up numerous questions of their relation both to theory, and to the target systems. Most notably, the sense in which models are 'true' of the world – how the messy and complicated phenomenological laws postulated in a model (an abstract system) pertain to the world – became the question where the most is at stake. Also, informal view of model-building remained in a need of further specification. At the same time, if the MV is to give a unified picture of modeling, the vast array of model building components and types of composition – a system for it is justified to presume that will vary, at least according to scientific discipline – together with plurality of model-types that accompany it, all should be subsumed in one abstract description of modeling. When we add to it a flourishing field of modeling through computer simulations [e.g. Winsberg 2010] – a seemingly impossible task.

However, some attempts are already be made in this regard, and the one on which we'll try to dwell is Giere's account of model/world relation in the terms of models being similar to their target systems only in certain respects, and even in these respects only to a certain degree [Giere 1988]. By the same intention to account the broad plurality of model-types and modeling-techniques present in the scientific practice, this approach opened itself to a fatal weakness of being too vague and too general [French & Ladyman 1999]. Along these lines, we'll try to develop similarity approach by making it more specific, as far as it is possible on this level, and complement it in an overreaching picture of modeling in science. In this aim, developing the concept of similarity (i.e. characterizing model/world relation) will be our first concern, followed by a textured description of models and scientific representation.

Of course, the informal framework of the MV remains under construction and we will try to delineate only its basic features necessary for an integrative approach. The focus will be on preserving the diversity and pluralism of models, emphasizing at the same time the human aspects in modeling, notably its fit-to-the-purpose character of scientific constructs and representation only as a means of knowledge acquisition, subordinated to the ends of scientific enquiry. Hopefully, this will attribute to the visions of science as a social enterprise [e.g. Hardwig 1991; Kitcher 1993; or Bird 2007] and illuminate new details of its drive by rationalistic principles and empirical support.

Part III

Chapter 1

Similarity

Problems with similarity. Tversky's features matching account of similarity.

Philosophical account of similarity.

Reduction of similarity to partial structures.

Similarity lies at the heart of Giere's attempt to capture the pervasive use of idealizations and approximations in scientific modeling and it seems apt for describing model/world relationship, mainly due to its informal character and flexibility to account for different types of model explanations [Giere 1988]. We have seen that assuming any kind of isomorphism in this context would be extremely inappropriate, since it is the grossest of category mistakes to assert such a relationship between physical objects on the one hand and essentially mathematical structures on the other [Teller 2001; Muller 2011]. Nevertheless, 'similarity', as it stands, appears rather vague and, without further specification, too abstract to account for delicate mechanisms of scientific modeling. In this chapter, we will investigate some objections against the preconceived notion of similarity [Goodman 1969; French & Ladyman 1999], and try to develop this notion to suit the needs of model theory. Basis for capturing the features of modeling illuminated in the Part II will be Giere's ideas regarding model/world relation integrated with Tversky's psychological account of similarity.

Problems with similarity

As a major problem of the model account often is purported that similarity is too undetermined of a relation to be capable to express delicate relationships between models and their target systems. Arguments of this type have a long history in philosophy, and appeals to similarity have seemed dubious to many philosophers. Most forcefully, W.V.O. Quine argued that similarity is logically repugnant because it can not be explained in terms of more empirically or logically basic notions. Mature sciences, he argued, dispense with similarity relations altogether, and so should philosophers with any general notion of similarity [Quine 1969].[59] Nelson Goodman, inspired by Quine's work, assembled a collection of foundational arguments against similarity and formed a milestone in perspective on similarity, which still dominate philosophical thinking.[60] His main argument is that similarity does not give sufficient characterization of the relationship in question, and that, consequently, similarity alone is not enough for representation [Goodman 1972, 437-9]. Also, the second major problem of similarity, for Goodman, is that any two objects/systems will be similar (and different) in countless ways, and there is no way to determine which aspects are relevant and what degree of precision is enough. Idea behind this objection is connected with the previous problem indeterminateness (that is, logical weakness) of the notion of similarity. Basically, it is a worry (interpreted in the model theory terminology) that if we doesn't make precise the model/target systems relation, we will not be able to clearly understand what is the scientific theory/model, as well as to distinguish them from non-scientific ways of representation. In both cases, further characterization is required if we are going to have an informative account and/or applicable ordering of representations. Logic supporting this theses is that similarity judgments are highly dependent on context, hence the similarity cannot be used for precise and definite accounts of modeling:

> "More to the point would be counting not all the shared properties but rather only *important* properties – or better, considering not the count but the overall importance of the shared properties. […] But importance is highly volatile matter, varying with every shift of context and

[59] We must mention that Quine's position can be interpreted in a quite different manner, as he is objecting not to the principled possibility of the notion of similarity, but against a scientific usage of the ambiguous unrefined natural language notion [see Tversky & Gati 1978].

[60] Influences on the SA can be seen in, for e.g. [French & Ladyman 1999].

interest, and quite incapable of supporting the fixed conditions that philosophers so often seek to rest upon it." [Goodman 1972, 444]

Furthermore, Goodman argues, similarity *in principle* can not be adequately defined, and therefore is slippery and, both philosophically and scientifically, suspecious notion.

Far reaching as it is, problem with Goodman's critique is that his arguments are mainly based on similarity accounts of his time; specifically they are aimed against geometrical model of similarity, independently developed by many authors [for e.g. Carnap 1928; Attneave 1950; Coombs 1954; Torgerson 1965; Shepard 1980]. In the meantime, more advanced accounts of similarity are constructed in psychology and cognitive science. These accounts do not share the same problems with geometrical account and, by the way, surpass some of Goodman's objections. Accordingly, they provide a possible basis for a general philosophical account of similarity required by the model approach, and do so, since they are scientific constructs, in a naturalistic way.

Tversky's feature matching account of similarity

Already in the seventies, Alfred Tversky developed set-theoretic account of similarity based on feature matching, commonly called "the contrast model" [Tversky 1977]. Same as Goodman, Tversky also had a problem with geometrical account of similarity. Actually, motivation for his development of a new similarity account were precisely deficiencies of geometrical model, specifically, empirical evidence mounted by Tversky which undermined all three of its basic axioms: 1) minimality – with consequence of reflexivity; 2) symmetry; and 3) triangle inequality – which represents transitivity of similarity measure in metric spaces.[61] In Tversky's psychological experiments reflexivity of similarity came out to be a problem, since the probability of judging a two stimuli as identical turn out not to be the same for all kinds of identical stimuli [1977]. More importantly, both for Tversky and us, symmetry of similarity is undermined, since experiments revealed that, typically, North Korea is judged to be more similar to China than China is to North Korea [ibid., 334].[62]

[61] For formal definitions of these axioms in geometrical account see, for e.g. [Carnap 1928] or [Torgerson 1965]. For our purposes it is sufficient to know that the three axioms represent respectively reflexivity, symmetry, and transitivity of similarity relation in the geometrical model.

[62] Triangle inequality axiom was also confuted by experiments, which showed that when objects a and b are similar in different respects than b and c, similarity is not always transitive. Precisely speaking: measure of addition of a's

Along with this empirical confutation of (interpretation of) geometrical account's axioms, Goodman's argument regarding context sensitivity of similarity judgments gained experimental support in Tversky's famous 'extension effect': if all objects under consideration share a feature, it tends to become neutral in the comparison, but if an object not sharing the feature is added to the comparison task, the activation of this feature may shift previous similarity judgments between members of the original set. For instance, Germany, the USA, and Russia may be consider to have very little similarity with one another, but adding South Africa to comparison suddenly changes previous judgments since the later country is from the South hemisphere [see Tversky 1977, 344].

Aiming to incorporate these results, Tversky developed a formal account where similarity is neither reflexive, nor symmetric, nor transitive, nor absolute. His 'contrast model' express similarity between objects as a weighted difference of the measures of their common and distinctive features, thereby allowing for a variety of similarity relation over the same domain. Since the contrast between objects' shared and distinctive features is set-theoretically expressed, the similarity relation must satisfy several conditions, most importantly the matching condition and the monotonicity condition.[63] According to the former, similarity is a function of the set of objects' common features, the set of features that belong only to the first, and the set of features that belong only to the second; or, in short, a function of features they share, penalized by a function of features that they don't share. Formally:

$$\mathrm{s(a,b)} = \mathrm{F}(A \cap B, A \backslash B, B \backslash A), \tag{1}$$

where S stands for similarity relation, F for some ternary real-valued function, and A and B for the feature sets of objects a and b respectively. A domain of objects D is presupposed, and objects are then defined as sets of features, both quantitative and qualitative, over predetermined domain of features Δ [Tversky 1977, 329-30]. Second philosophically important condition – monotonicity, states that an object a is more similar to an object b than to object c, if the features a and b share contain the features a and c share, and the features not shared by a and b are among ones not shared by a and c:

$$\mathrm{s(a,b)} \geq \mathrm{s(a,c)} \quad \text{if} \quad \text{(i) } A \cap C \subseteq A \cap B$$

similarity to b and b's to c may be less or equal to measure of similarity of a to c (contrary to the axiom ($S(\mathrm{a,b}) + S(\mathrm{b,c}) \geq S(a,c)$) [see Tversky & Gati 1982]. For e.g. Jamaica is consider similar to Cuba (because of geographical proximity); Cuba similar to Russia (because of their political affinity); but Jamaica and Russia are not judged as similar at all [Tversky 1977, 329].

[63] For other conditions and representation theorem see [Tversky 1977, 351-2].

(ii) $A \backslash B \subseteq A \backslash C$

(iii) $B \backslash A \subseteq C \backslash A$ (2)

If at least one of the above inclusion relations is proper, then sign"≥" can be replaced by ">".

Important consequence of this setup is that, since Δ is only a subset of all possible/imaginable properties, it makes similarity feature sensitive.[64] When a different set of features is chosen for representation of objects in the domain, different similarity relation holds. And which set of features is relevant depends on given purposes and interests, which are, of course, context relative. As can be seen in case of monotonicity, similarity judgments change with the addition of common features or deletion of distinctive feature. If we take for example a red square, a blue square, and a red circle (where each pair has equal similarity[65]) and we add that the first two are made of wood and the last one isn't, similarity judgments will accordingly change.

If S fulfills Tversky's five conditions, then there are similarity scale s and a nonnegative scale f, both interval scales, such that:

$S(a,b) \geq S(c,d)$ iff $s(a,b) \geq s(c,d)$; (3)

$S(a,b) = \Theta f(A \cap B) - \alpha f(A \backslash B) - \beta f(B \backslash A)$, for some $\Theta, \alpha, \beta \geq 0$; (4)

for suitably chosen set of features Δ, a weighting function $f(\text{-})$, and for term weights Θ, α and β. The function f differently weights various subsets of shared and distinctive features and naturally is construed as a salience function, defined over Δ[66]. This is another sense in which s can be context relative – even given a fixed set of features Δ, relative importance of these features may vary with interest, from one context to another. This nicely captures (in a formally precise way) Goodman's observation that:

[64] In principle, any object's possible features are infinite and Δ is always finite, since the theory of similarity pertains to real psychological subjects, with limited mental capabilities. According to Tversky, we extract and compile features of interest from our general knowledge of the world, relevant for our current representations [see Tversky 1977, 329-30]. This is a consequence of limitedness of our representation – at any judgment of similarity only finite number of characteristics is actually in the game (although that number can always, in principle, be expanded).

[65] That is, if we consider relevant both and only the color and the shape.

[66] It is sufficient to define f over Δ, instead of $\mathcal{P}\Delta$, since Tversky assumes that f satisfy feature additivity: $f(X \cup Y) = f(X) + f(Y)$, whenever X and Y are disjoint [Tversky 77, 332].

> "[C]omparative judgments of similarity often require not merely selection of relevant properties [Δ relativity] but a weighting of their relative importance, and variation in both relevance and importance can be both rapid and enormous. Consider baggage at an airport check-in station. The spectator may notice shape, size, color, material, and even make of luggage; the pilot is more concerned with weight, and the passenger with destination and ownership. Which pieces of baggage are more alike than others depends not only upon what properties they share, but upon who makes the comparison, and when. Or suppose we have three glasses, the first two filled with colorless liquid, the third with a bright red liquid. I might be likely to sad that first two are more alike than either of two is like the third. But it happens that the first glass is filled with water and the third with water colored by a drop of vegetable dye, while the second is filled with hydrochloric acid – and I am thirsty. Circumstances alter similarities." [Goodman 1972, 445]

Tversky's representation theorem (4) captures exactly this relativity of weighting the relative importance of properties by relativity of the weighting function $f(\text{-})$. Furthermore, besides feature and salience sensitivity, similarity measure depends on term weights Θ, α, β. The third way objects' similarity is relative in the contrast model reflects dependence on subjects' focus of interest – which can also take the form of modeler's representational ideals. Namely, even with fixed Δ and f, contrast model does not define a single similarity scale, but rather a family of such scales characterized by different values of parameters Θ, α and β. If, for example, Θ, α = 1 and β = 0, then S(a,b) reduces in contrast model to $f(A \cap B) - f(A \setminus B)$, or in the ratio model[67] to $f(A \cap B)/f(A)$[68]. If a and b represents a scientific model and its target system, respectively, then this case, where no system's feature is left unrepresented, express nicely the drive for completely exact (i.e. perfect) models. Another example would be when $\alpha = 0$ and $\beta=1$ where $S(a,b)= f(A \cap B)/ f(B)$, which could express a drive for minimalistic models, where only basic target features are included in the model (for a list of representational ideals that tend to be exhaustive see [Weisberg 2007a]).

[67] The ratio model is only a different mathematical way to write down the contrast model.

[68] Numerator here is $f(A \cap B) + f(A \setminus B)$, which reduces to $f(A)$.

Returning to Tversky's work, we see that his contrast model of similarity encompass both psychological evidence as well as outflanking Goodman's philosophical arguments. What Goodman is actually objecting is that geometrical account lacks context sensitivity and that similarity cannot be made absolute notion; difference is that he is considering this as bad. Question is whether can we make something out of this relativity and use similarity in an orderly and informative way. By the way, value judgments are also not absolutely 'true', but relative to a certain normative system. Emphasizing the lack of symmetry in similarity judgments and its three-way relativity Tversky neatly captured intentions behind the every day usage and provided us with a tool for our philosophical purposes by enabling the advocates of similarity to ground their claims in a developed and well defined notion.

Philosophical account of similarity

There are alternative, formally precise representations of similarity relation, besides Tversky's. Gardenfors has refined original geometric approach by making it asymmetric and context sensitive [Gardenfors 2000], and recently Lietgeb and Mormann presented new mathematical solutions for problems of the original Carnap's account [see Decock & Douven 2012]. We can see from these examples that similarity relation can be made extremely precise in several different ways, all of which contain previously problematic context sensitivity. Also, obviously, all of them are immune to arguments regarding vagueness of similarity, as is generally presupposed in philosophy (based on [Goodman 1972]), and specifically in the semantic approach [e.g. French & Ladyman 1999].

If we were to choose on which similarity formalism to base philosophical notion of similarity required by the model approach, most straightforward option, or so it seams, would be Tversky's account. The contrast model's basic idea of feature matching resembles Giere's notion of similarity in certain respects and degrees, and is easily compatible with ordinary philosophical conception of models. More precisely, definition of objects as sets of features and liberty in feature description (how we define members of Δ) is what connects these accounts and makes Tversky's psychological model highly applicable to scientific-model theory. This way philosophical account of similarity can be based on firm naturalistic grounds. Also, another important ‘similarity’ of Tversky’s notion to Giere’s is that in both accounts the intensity of similarity is accounted – sharing features may come only to some degree.

We cannot use the contrast model directly in philosophy of science because, since it is developed for psychological purposes, it describes similarity relation between objects or among stimuli, not models and the world. Michael Weisberg proposed that if we take the above formula (4) and substitute a and b with m and t (for a model and its target, respectively), and divide domain of features into domain of attributes Δ_a and domain of underlying mechanisms Δ_m, we’ll get following expression for similarity [Weisberg 2012, 789]:

$$S(m,t) = \theta f(M_a \cap T_a) + \rho f(M_m \cap T_m) - \alpha f(M_a \setminus T_a) - \beta f(M_m \setminus T_m) - \gamma f(T_a \setminus M_a) - \delta f(T_m \setminus M_m) \quad (5)$$

Adding a division to the domain Δ is necessary, Weisberg argues, to reflect properties and patterns in a model, on the one hand, and underlying generating processes, on the other. On the other side of the coin, this very reasonable idea of dividing features of models looks much more problematic when pertain to real, target systems. How are we going to assess the similarity of underlying mechanisms in the target system, if not through the attributes and their patterns? Proposed division between properties and underlying generating mechanisms may be just another way of referring to distinction between observable and unobservable entities. Trying not to be drawn into the pitfall of the classical realism debate, we must notice here that this distinction is extremely shifty – what is unobservable relative to current observational methods may not be so in the next stage of development [cf. van Fraassen 1980]. Bigger problem in the context that concerns us here is how we are going to compare model’s mechanisms with target’s mechanisms if these are not observable; yet, if they are observable, why are we stressing them out? It is not entirely clear what Weisberg intention was,[69] but, anyway, for our purposes it is not necessary to enter into divisions of Δ because both the relativity and the vagueness issues are not depending on it. This question can be settled later on. In the mean time we will use the reduced equation:

$$S(m,t) = \theta f(M \cap T) - \alpha f(M \setminus T) - \beta f(T \setminus M) \quad (6)$$

[69] His suggestion that in a more abstract way the proposed distinction can be thought of as a difference between states or state transitions and transition rules looks much more appealing to us [see Weisberg 2012, 789].

Apart from the previous attributes/mechanisms distinction, Weisberg's similarity formula is a rather direct interpretation of the contrast model, in model theoretic terms. It renders model "similar to its target, or to a mathematical representation of its target, when it shares certain highly valued features and does not have many highly valued features missing and when the target does not have many significant features that the model lacks" [Weisberg 2012, 789]. This straightforward solution inherits delicate context sensitivity of Tversky's account we stressed earlier, and with it also its characteristic of defining not a single, but a family of similarity scales. [However, it carries one characteristic that may be philosophically puzzling – by producing a family of scales, the ratio model generalizes several set-theoretical models of similarity proposed in the literature [see Tversky 1977, 333]. This can be very useful in psychology, where psychologists use the formalisms to numerically evaluate similarity judgments of test subjects for specific purposes, but what is the benefit of such family of formalisms in philosophy? If we have a family of similarity scales, which represents different models of similarity relation [Tversky 1977, 332], most natural way to adopt this idea would be to said that we have a family of methodological approaches to modeling, where different representational goals are set, and different kinds of feature matching functions are applied. To express this family of model/world relation in one formalism, occludes the point that modeling in science is extremely methodologically open. However, at the same time, it enables a unified conception of modeling, and thereby explains it in a more fundamental way, from which nonetheless every specific modeling-type can still be obtained by specifying the purposes represented by weighting functions.

I would stress regarding these similarity formalisms that they demonstrate two important points. On the one side, they prove that 'similarity' can be philosophically (and logically) precise and empirically correct, that is, respectful to similarity requirements noticed in the literature: it accounts for non-reflexivity, non-symmetry, non-transitivity, and, most importantly, context relativity of similarity judgments. On the other hand, with the contrast model, and specifically with reduced Weisberg's similarity equation, it becomes apparent that formalism is actually unnecessary for philosophical understanding of model/world relation because even accurate formalism is (and, arguably, must be) too general and also, in a sense, artificial to modeling practice and hence do not allow us to grasp the delicate essence of its specific modes. Above discussed formalisms are important tool for a defense against philosophical objections to similarity, specifically to objection of vagueness, but they fail to present direct, informal way modelers use mathematical constructs to represent the world. Thereby, even the similarity formalism obscures the philosophically relevant aspects of theorizing and modeling, same as they were obscured by the SA formalisms, although it creates space for analysis of modeler's purposes, or representational ideals [cf. Weisberg 2007] – analysis that may, perhaps, lead us to something like types of modeling. Example of artificiality of the proposed solution is a case of weighting in science which in practice is almost always qualitative and informal. Scientists generally do not give exact numeric values to theirs value judgments regarding world/model/theory features, if this is possible at all, even when there is a general consensus regarding the importance of salient features. Here we are drown again to Feynman's point that science does not function in a formally strict manner and that many informal components play a significant role, such as metaphors, analogies, values, and policy views [Feynman 1963; Bailer-Jones 2009]. Most importantly, these informal features often lies implicit or hidden, and thereby present us with a limitation of the general approach – as far as it is useful for delineating and understanding abstract character of modeling, including its general types, concrete and textured variants of these types cannot be just 'feed into the formalism, and get out of it'. For them we must plunge into laborious and detail case-by-case analysis.

In a sense, general formalism is so liberal, that it can be questioned whether it says anything more than Giere's similarity definition in natural language. Formalism can be nice clothing, but we must not overestimate it, because it can make an idea look deeper that it really is. It is important tool for philosophically precise discussion, but we must be aware of its limitation as a tool for representation of scientific reasoning done by real scientists – formal strictness rarely suits psychological subjects scientist obviously are. In this regard, we are more favorable to using a natural language definition of similarity for model theory needs, with having in mind that this definition is corresponding[70] to the formal one and even regressing to it if the semantical problems appear, specifically with vagueness or (unacceptable) ambiguity.

But for the main part, reduced Weisberg's philosophical accommodation of Tversky's similarity account makes sufficiently possible to access account's consequences for model theory

[70] Only 'corresponding' since the rich and ambiguous natural language cannot be 'equivalent', except perhaps partially, to the set-theoretical language in which Tversky's definition is expressed.

more precisely and we will use to supplement Giere's definition where necessary. Specifically, identification of the origins of the Δ, f i weighting terms will enable us to group these in the suitable arsenal of modeling in the course of the next chapter.

Reduction of similarity to partial structures

Now we will address one objection to similarity put forward by the main proponents of the semantic approach. Referring to Giere's original formulation (from [Giere 1988]), Steven French and James Ladyman proposed that similarity relation, when we remove vagueness form it, could be best described in terms of partial structures [French & Ladyman 1999]. Their main argument is that Giere's account appears like offering an alternative representation of model/world relation, only by remaining vague and unexplicated [ibid., 110]. When his ideas are developed, they claim, nothing is gained what we already do not have within the partial structures account.

Their proposition is that, given the partial structure of a model $m = \langle A, R_i, f_j, a_k \rangle$ [71], similarity should be understood as a relationship between families of relations R_i (where properties includes both monadic relations and otherwise). Hence, to say that the model is similar to the target system is to say, at least in part, that the family of relations R_i is similar to the relevant family of the latter [Ibid., 111]. In partial structures formalism terms, this means that certain R_i of the model stand in one-to-one correspondence with certain R_i of the target. Since one-to-one correspondence can stand only between mathematical structures, here we have occurring of the problem of the lost beings [cf. Muller 2011]. Defense is conducted here in standard fashion, through Suppes notion of hierarchy of structures [1962], from data models to theoretical ones. Accordingly, we will surpass this problem since we discussed it in previous chapters, and focus only on capabilities of expressing similarity characteristics through partial structures formalism.

Correspondence between R_i's, it is claimed, captures the notion of similarity, coming in certain respects and to certain degrees, which is accommodated within this view through a consideration of the kind and number of relations that enter into correspondence, respectively.

[71] Where A is non empty set, R_i, $i \in I$, is a family of (partial) relations, f_j, $j \in J$, is a family of functions and a_k, $k \in k$, is a family of distinguished individuals of a.

Respects of similarity are represented through kinds of relation in correspondence, and degrees through number. Thus, similarity comes to be understood in terms of a "partial isomorphism" holding between the families of relations concerned - it is understood and analyzed as an inherently structural notion [French & Ladyman 1999, 112]. Advantages of structural representation, such as clarity and countability of a relation in question, are then naturally following.

However, this formalism, as it stands, clearly cannot express crucial feature of context relativity, observed both by Goodman and in Tversky's experiments. Although it is possible to express certain context sensitivity by adequately choosing members of R_i, crucial salience function is missing – in structural account all relations have the same importance. In scientific practice, however, democracy is not generally applied and often, what many well-known examples demonstrate, few important features 'overvote' many insignificant ones. Along emphasis on features of interest, special weighting of certain *types* of correspondences (aspects of similarity) is also missing. All that can be expressed with partial structures is that certain correspondence exist, not whether it is important, both in general and which parts of it specifically. If we try to express similarity following French and Ladyman's proposition we would simply not be able to capture all of the characteristics set by empirical research – non-reflexivity, non-symmetry, non-transitivity, and context relativity of similarity judgments. First three are, moreover, completely out of the question as a characteristic of ordinary set-theoretical structures. Although all of the lacking features are important, most important to us is non-symmetry, since it express prevalence of model in comparison (we want to say that model is similar to theory or the world, not theory or world to model), and, of course, the most important for representing the practice, context relativity.

We have seen that major drawback of Tversky's formalism is that we cannot distinguish objects within the model (i.e. that a model is not considered as a structure, but as an object). On the other hand, major drawback of partial structures approach is that it cannot express the delicate ways of contexts sensitivity. If we could combine these two approaches, in a way to preserve delicate ways of expressing sensitivity of the former, and viewing a model as a structure of the later, we would get rid of all of the above problems. Most straightforward way to do so is to replace the features of the model in the reduced Tversky-Weisberg's solution (eq. 6) with relations holding between properties of the objects in the model – that is, to replace Δ with *m*= <A, R_i, f_j, a_k>. Similarity will then be capable of expressing relation between the target system and a model conceptualized not as a set of features, but as inherited with objects whose features and relations stand in a similarity relation to entities in the target system. This it will account to cases where multiple objects, say interacting particles in a force field, are modeled within the same system. Formally:

$$S= \theta f(R_m \cap R_t) - \alpha f(R_m \backslash R_t) - \beta f(R_t \backslash R_m) \qquad (7)$$

Not being aware of the above Tversky-Weisberg's formalism, partial structures proponents echoed now obsolete Goodman's arguments and missed the opportunity to develop similarity to its full potential. Instead, due to regarding its oversimplified version, they dismissed it as intellectual cul-de-sac. Opposite is, however, true and we just presented arguments on which we could re-justify Contessa's conclusion that "structural account [the SA, including the PSA] is nothing but a version of a similarity account" [Contessa 2011, 130].[72]

Major difference is that properties in the PSA are properties of *objects in* the model, whereas features in Tversky-Weisberg's account are properties *of* the model. Advantage of the PSA being that they are already representing models as containing respective domain of objects and their relations, while the similarity formalism (eq. 6) lacks the means for representing contained objects. By integrating both approaches we can provide logical space for scientific practice, together with its normative elements, while keeping the advantages of the Stanford revolution – envisioning models as structures. Developing the structural account into a full-fledged account of epistemic representation, we regard, should be attempted exclusively on these lines where relativism of structural comparison plays the central role. To that aim the next chapter is devoted.

[72] A slightly weaker thesis, yet still sufficient for our point, would be that they are complementary [see Chakravartty 2010].

Part III

Chapter 2

Representation

Scientific representations.

Conditions of representation. Its features and potentials

Scientific representations

If we accept the theses that there isn't general problem of similarity, but only numerous specific (solvable) problems, it's now possible to tackle the question ''What are models?" in a completely general way. Surprisingly, this crucial question is commonly asked in modern philosophical papers, but almost never answered. General answer can't be found even in prominent account of Roman Frigg and Stephan Hartmann "Models in Science" [Frigg & Hartmann 2012]. Instead of a general answer, usually, some classification of models is considered and models are analyzed via notions of representation and idealization [see for example Portides 2008]. Disentangling the concept of model by these two terms, it is generally argued, is closest we can get to finding a definition because there exist the disparity of meanings and uses of the term 'model' in sciences [e.g. Portides 2008; Contessa 2011; Frigg & Hartmann 2012; etc.]. Understanding this disparity only as an acknowledgment of mentioned authors that they do not see a possibility of a unified account, we will try to explore in this chapter weather such possibility exist, could it be derived from notions of representation and idealization, and if it can, as we will suggest, what are the consequences regarding related terms 'law', 'theory', and on our picture of science in general.

Discussion regarding the terms 'representation' and 'idealization' and their ability to explain scientific modeling is thriving in the last decade, although with no consensus. On classical account of representation, this relation is generally understood as a two place relationship between the vehicle of representation (a model, theory, picture, graph…) and its target (generally the world; although in special cases a theory or some other construct). When we talk about 'representation' in scientific contexts, we talk about representation that is used for epistemic/cognitive purposes – i.e. learning something about its target. We do not consider, say, esthetic representation (an avant-garde picture for example), where learning and truth about the target system, or explaining it, are not of the essence.[73] Although it is not clear in precisely what sense truth plays a role, it is not expected that scientific representation should be completely truthful.[74]

What is usually expected is that scientific representations are (even moderately) faithful. However, there is some reason for caution here. Namely, it is unclear whether it is appropriate to distinguish faithful representations from mere representations. For example, we can hold that model of the elastic solid ether or vortex theory of gravitation, both of which exemplary false, are none the less scientific representations, although they are not faithful ones. On the other hand, we can hold that although they might were considered as scientific (since they played important role in past investigations), they are certainly not to be considered now as representations, since it turns out they are not - there is no such thing as ether and gravitation is not made by mechanical vortices.[75] This dilemma whether to count unfaithful 'representations' as representations at all becomes significant at the point when we try to set necessary and sufficient conditions for scientific representations. What we will count as these conditions directly depends on our answer on the above dilemma. As Chakravartty points out, if all merely intended scientific representations are genuine representations, then the term 'scientific representation' connotes nothing distinctive than the mere acknowledgment that something is a

[73] It is tempting to mention moral or religious representation here, for which it is desirable to be distinguished from scientific, but is impossibly entangled in truth, explanation, and learning. We mention this ambiguous case here because criterions for 'being scientific' are not even nearly clear as some would want it, and this problem awaits us at every corner (not to say every time we use the term 'scientific').

[74] There are suggestions that obscure notion of 'approximate truth' is best described by 'similarity' which is tightly connected with the term 'model' [see Teller 2001]. This opens up a possibility which we will explore later that 'model' is a more basic notion, or at least equally primitive, yet a clearer one than 'representation'.

[75] There is an echo of scientific realism debate here.

model [2011, 210-11]. If, on the other hand, one insist that some threshold of accuracy is demarcating genuine scientific representation, then we have a huge problem of application of this criterion (if we find it at all) on current scientific constructs – for determining which of the competing scientific theories are 'really representational' would be question begging (in saying which ones are genuine representations we would say which ones are truthful, and thereby solve the competition). Henceforth, the use of the term would be confined only to the history of science; and if 'representation' is not applicable to ongoing science it is certainly not the notion we want to base our picture of science on, since we consider it a live practice. [76]

Middle way would be to count as representations all of those constructs that are intended to represent, but not to conflate completely the term with the term 'model' since not all models are intended to represent – there are models which serves other purposes than representational (such as educational or toy models [Frigg & Hartmann, 2012]). By this approach, 'representation' can be equated with 'representational model', that is a model used for purpose of representing, whether successfully or not. Since this is the most common, and philosophically and scientifically most important usage of models, we may still hope to shed some light on it via analyzing the concept of representation.

Conditions of representation

After clearing the ground, we can now turn to necessary (and potentially sufficient) conditions of scientific representation. Today is widely accepted that to be a scientific representation, there has to be a denotational connection from vehicle of representation towards the target. This minimal condition for any reference is well documented and pointed out by a number of authors [for e.g. see Goodman 1968, Huges 1997, Callender & Cohen 2006]. On presently dominant account, to be a scientific (i.e. cognitive) representation, beside necessary denotational connection, a vehicle of representation needs to allow to the user of representation to perform inferences from its vehicle to the target [see mainly Suarez 2004]. This means that

[76] Also, this usage won't be in accordance with the history of philosophy. Remember the usage of the notion of 'representation' in Kant's work, or in Schopenhauer's masterpiece *World as the will and representation*. Historically there is no truthfulness connected – 'representation' is literally used, specifically in the later work, as an unverifiable model of reality.

vehicle needs to semantically points towards the target and to be a surrogative device for reasoning regarding it.

One can problematize the later condition as whether an object is scientific representation because we can draw surrogative inferences from it, or rather that we can draw surrogative inferences because we take that object to be a scientific representation [see Contessa 2011]. The question is: is the inferential ability fundamental property (or process) that drives scientific representation, or just its emergent feature? Out of this question comes clear the formulation of our quest – we search for a minimalist, deflationary account of representation, meaning that we seek only to describe most general, surface features of representation. Seeking some deeper conditions and constituent relations that will exhibit these features as a by-product, as in [Huges 1999] or [Contessa 2007], will, on our view, only change our basic notion for another – and notion of 'representation' is well entrenched in our views on science. We opt for a descriptive approach and for abandoning the search for necessary and sufficient conditions of representation, and we do it on two related grounds. Firstly, following the historical turn in the philosophy of science [see Hacking 1983] we want to describe how representation functions in science, not to reduce it on some more primitive notion. Secondly, we want to avoid failures of related prescriptive disciplines, specifically classical epistemology which always failed short of giving the definition of knowledge [see Quine 1969, "Epistemology naturalized"]. 'Representation', on our view, same as 'knowledge', or 'truth' is not the kind of notion that allows necessary and sufficient conditions [see also Suarez 2004]. To have a general account of representation, we believe, it is needed (and possible) only to describe how representation functions in science, contrary to the old view of prescribing necessary and sufficient conditions which it must satisfy.

On the descriptive level, the best specification, and most famous one, is above mentioned Suarez's inferential conception of scientific representation, where vehicle of representation (a model) serves to perform surrogative reasoning regarding its target. Strictly speaking:

"*A* represents *B* only if:

(i) the representational force of *A* points towards *B*, and

(ii) *A* allows competent and informed agents to draw specific inferences regarding *B*."

[Suarez 2004, 773] (8)

Representational force in (i) is only a capacity for denotation – that is, denotation non-arbitrarily postulated.[77] Key condition is (ii), where representation is reduced to the ability to perform surrogative reasoning. Crucial point in the above definition, which has previously often been omitted, is that the presence of users is essential for representation. To learn or explain something about a target system we need to account cognitive agents and theirs purposes of inquiry. This little addition changes things dramatically. We mentioned earlier that rep is classically understood as a dyadic relation, now it becomes ternary or quaternary. If we use Suarez definition of representation (8), and amend it by adding that agents use representations for certain purposes, we get Giere's definition of models (!)[78]:

"X uses A to represent B for purposes P" [Giere 2004, 743]. (9)

'X' can be an individual scientist, a scientific group, or a larger scientific community. 'P' is representing different purposes the same group can have in different contexts.[79] As we pointed out in the previous chapter, models (here 'A') represent their targets by being similar to them only in limited respects and in certain degrees. How 'X' uses 'A' for his purposes? Well in the way suggested by Suarez's analysis of representation [2004] – 'X' uses 'A' to derive specific inferences about 'B', according to his interests. On what 'X' bases his inferences and what condition them; or what determines the capacity for representation? On the capacity of similarity relation between structures in question – determined by X's description of structures A and B, and X's evaluation of significance of parts of those descriptions (represented by similarity parameters θ, α, β, and the salience function f from (eq. 7, Ch. 3.1)).

Its features and potentials

Derivation from one definition to another is pretty straightforward, but the trick is that Suarez posited in another paper [2003] five arguments which every account of representation must satisfy. Giere's definition of models hinges on the notion of similarity, and similarity, as is

[77]Simple example of arbitrary postulated denotation is naming, where there isn't any structural connection between a vehicle and the object.

[78] Origins of this approach can already be found in Apostel's work where "The subject S takes, in view of the purpose P, the entity M as a model for prototype T" [1960, 128]/

[79] We will return to precise contextual analysis of agents and their purposes later on in this chapter.

generally understood in philosophy of science, doesn't satisfy these five arguments.[80] I will claim that the notion of similarity as defined in chapter (3.1) is capable to tackle all of Suarez's conditions for representations, and that, consequently, it is possible to base a definition of models on the above definition of representation.

The five arguments are: i) the argument from variety – we have large diversity and range of representational means in science; ii) the logical argument – logical properties of rep are non-reflexivity, non-symmetry, and non-transitivity; iii) the misrepresentation argument – rep account have to deal with inaccuracy and mis-targeting of representations (the taking of the source to have the parts or the whole of the target that it doesn't actually have); iv) The necessity argument – account has to state necessary conditions for representation; v) the sufficiency argument – stating sufficient conditions [cf. Suarez 2003]. Claim is that good theory of scientific representation has to encompass these five points, and the verdict is that similarity understood as a dyadic notion (nether used here or by Giere) fails to fulfill them [see also Suarez 2004].

Regarding the conditions (iv) and (v), we have already stated that we do not believe that representation is a kind of notion that allows for necessary and sufficient conditions. Furthermore, we think that this is not in accordance with *actual diversity*[81] of representational means we have in science (i), together with potential diversity for which we want to keep ourselves methodologically open. Henceforth, it is a further reason for a minimalist approach. The logical argument (ii) is satisfied since our similarity relation is based on Tversky's account which was formulated precisely to encompass logical properties of non-reflexivity, non-symmetry, and non-transitivity of similarity judgments (remember experimental results such as "North Korea is more similar to China, than China is to North Korea"). These empirically observed and tested properties are then directly transferred to similarity account presented in (3.1).[82]

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[80] Suarez henceforth dismiss it.

[81] Much better represented by a 'family' of methodologies, to use a Wittgenstein's term, then by a unified list of conditions.

[82] Misconception of similarity is so pervasive through philosophy of science that, for example, Roman Frigg in his review of recent history of representational accounts discards similarity relation mainly because of the supposed reflexivity, symmetry, and transitivity properties [2006, 54 & 59-60]. Although opposites of which are, as Tversky demonstrates, the essence of ordinary (and philosophical) usage of similarity. Furthermore, even Giere [2010, 274]

The misrepresentation argument (iii) has two parts. Regarding the inaccuracy of representation, similarity is designed especially to grasp this crucial aspect of the scientific representation and it manages by pointing out that not all properties of vehicle are shared by its target – as we sad they are similar only in limited respects (properties) and even that only in certain degrees. Since we incorporated partial structures in (eq.7), inaccuracy is also accounted on the constituent-objects level and distributed throughout the model. Mistargeting is dealt by introducing agents in to the picture (scientists that state similarity and use representations), which as fallible beings are prune to mistakes. Formalism (7) express similarity judgments of these agents and since every individual judgment can be false it is possible also that entire representation is mistargeted by relating the structure 'A' (the model) to the inappropriate physical system.

As for Suarez's condition (i), the variety of representational means present in science, we can see that the definition (9) does not presupposes any limitation on what can be used as a vehicle of representation (and this will prove as a key feature later on) – similarity is just a way in which we characterize which parts of a model are important (representative) for us and which we will use for making surrogative inferences. Since it depends on the users' intentions, vehicle-world relation doesn't need to satisfy a fixed structure and it can take many acceptable forms, relative to the standards and purposes of the given users. In principle, anything can be used as model – material object (as is a cardboard representation of DNA), architectural design of a building, or abstract construct such as mathematical models of the space-time.

When Suarez argued that similarity relation doesn't have the right properties to be acceptable representational account he was explicitly arguing against similarity understood as a two place relation between the vehicle (usually understood as a model) and its target [Suarez 2004]. Based on this misconception, widespread opinion is that Giere's and Suarez's approaches present two incompatible description of scientific representation – the former described as functional and the later as informational view. Informational theories of scientific representation focus primarily on question 'What are scientific representations?', whether they are conceived as theories, models, simulations, diagrams or else. Functional theories focus primarily on the

and Suarez [2003] consider similarity as a symmetrical relation, what has undoubtedly contributed to the confusion in the contemporary philosophy.

question 'What is scientific representation?', where 'representation' is conceived as practice. We have seen recently that these questions are commonly taken separately in philosophical literature [Frigg 2006; Chakravartty 2010; etc.]. Chakravartty, for example, suggests a division on philosophical accounts which answer the first question – finding the knowledge bearing entities, and accounts answering the second one – disentangling knowledge-exercising practices. These two clearly related but different questions are actually, we argue, complementary descriptions, both contributing to a general understanding of scientific representation [see also Contessa 2011]. There is no fundamental dichotomy between information and function since they are considered only as aspects of the scientific practice, integrated in the approach that tends to grasp both its social (normative) and rationalistic features. When we develop similarity like we did in (3.1) we see that similarity in itself grasps both the informational and functional perspective, both structures and their users – and that they are actually two sides of the same coin. We hoped that we have shown how the analysis of representation which implies both of them can be captured within the one view.

Part III

Chapter 3

Models

General definition of models.

Model-building and methodologies.

Picture of science.

General definition of models

At last, with the unified informational and functional perspectives, we are able to directly tackle the question 'What are models?', or, to state it in terms more in accordance with our views yet in a somewhat confusing manner: 'What is modeling?'. As we noticed earlier, answer to this question will build on the results of the analysis of representation and try to develop the model definition presented in (9). It is interesting to notice that, decades ago, computer scientist gave almost identical definition of models (and modeling) as Giere's (eq. 9) [see Podnieks 2010]. Without philosophical foundations such as ours above, they rather based it on practice of computer science, artificial intelligence, and information processing. Marvin Minsky [1965] observed that:

> "We use the term 'model' in the following sense: to an observer B, an object A* is a model of an object A to the extent that B can use A* to answer questions that interest him about A."

Later on Jeff Rothenberg [1989] proposed a definition extremely similar to (eq. 9):

> "Modeling in its broadest sense is the cost-effective use of something in place of something else for some purpose"

Minsky's definition introduces agents into the picture, and Rothenberg's explicates the purposes. We see that both of these computer science definitions have in common with (9) (and with (8)) that model users, together with their intentions, are crucial for understanding models. Based on the above proposals Carl Podnieks formulated the broadest possible definition of modeling:

> "A model is anything that is (or could be) used, for some purpose, in place of something else" [2010, 2]. (10)

Now, what is the purpose of such a broad definition? And isn't it somewhat vacuous?

As Roman Frigg objected for representation, we can notice that merely saying that models are used (for some purpose) is not enough [Frigg 2006; but also Portides 2008 and Contessa 2011]. Of course they do. What we really want to know is what is involved when an agent is establishing a usage. So what we have to understand is how a scientist comes to use A to model (or to represent) B and to this end much more is needed than a blunt appeal to usage intentions [cf. Frigg 2006, 54].[83] However, as Frigg himself argued against the semantic approach, it is a matter of fact that the same structure can be instantiated in different systems [ibid.]: a concrete pendulum and certain kinds of electric circuits instantiate the same structure [see Kroes 1989], but the model of the pendulum represents only the pendulum. In this sense it is indeed intention what ultimately counts. Since, in principle, anything can be a model and it is possible to model, say, electric circuit with the real pendulum (in the same sense that structure of atom is modeled by the Solar system), what makes any of these systems a model is the decision to be regarded as such by the model users. Thus we must agree with what Teller foresaw more than a decade ago that ''it would be a mistake for a general account of the use of models in science to specify more narrowly what can function as a model", since science uses many things as models [2001, 397].

So what gives material to this functional definition; its 'representational force' to use Suarez's term? According to Contessa [manuscript], the most direct answer would be the

[83] When Frigg summarized all objections against similarity approach in recent literature, alongside this argument he also included, as a key objection, that everything comes down to intention. By reduction to intentions the Goodman's [1972] argument that everything is similar to everything else (and only our intention solidifies it) is reintroduced. We already tackled this line in chapter 3.1. To repeat it in somewhat different manner, how is the graph of the Thames's bridge similar to the Copenhagen interpretation of quantum mechanics? If some similarity can indeed be found, nobody would proclaim it as *sufficient for making inferences* regarding some quantum system, on the basis of the graph.

specification of similarity (of the type we attempted in the form of formalism derived from Tversky's model), which integrates the structure in its functional form. Scientists use models by judging them similar in specific respects and certain degrees to their targets, and then investigate properties and relations of the former to reason about the later. The structure is, however, subordinated to users' preferences; hence formalism from ch.3.1. is in accordance with our deflationary, descriptive approach to the scientific modeling. It is based after all on Tversky's work in descriptive psychology. Hence we stay in the Hackingian approach to science, while giving our functional definition material to be filled in.

Model-building and methodologies

Although similarity account is a both direct and fairly detail answer on the question "How scientist comes to use A to model B?", it is still in a need of a development if it is to account for a complete picture of modeling. We think that a more elaborated picture, if it is going to integrate information-function dichotomy in modeling, requires interpreting Frigg's [2006] challenge in terms of model-building methodologies and taking them into the account. Actually, both model-building and similarity are just elaborations of the model definition given in (eq. 9). With this we turn to the pragmatics of modeling. In the 20th century accounts of scientific constructs, following investigations in the foundations of logic and mathematics, pragmatics was just a name for whatever linguistic elements left over out of the syntax and the semantics, characterized as (only) a problem of language application and rarely investigated in detail. Since we tend to base our research on the traditions where even the logic is an emergent feature of a cultural practice [Quine 1960], further confirmed and broaden on the entire level of language in recent years [Clark 1997; Tomassello 1999; Giere 2004], pragmatics now becomes the central theme, far exceeding the problems of application.

When we talk about model-building methodologies in the context of general model theory we have in mind not concrete methodologies, but rather methodological schemes. Instances of these methodologies we demonstrated in Part II. Ch. 3. They generally fall somewhere on the scale between models constructed on the basis, or through a specification, of a theory – the so called homogenous model-building [e.g. Giere 1979]; and, on the other side, model constructed through the less organized means, including phenomenological laws, semi-empirical results, and even data – this is the heterogeneous extreme of model building [Cartwright 1999; Teller 2004; Suarez & Cartwright 2008]. Most of modeling simply falls in the middle, usually including theories or their parts, as well as the less organized means. However, although we present them as the two extremes of model building, many case studies suggest that the most common way of constructing models is actually closer to the heterogeneous option [see Frisch 2000; and 2005]. In both cases modelers try to construct, by any means, an artificial abstract and idealized (to a certain degree) system, which will stand in the place of the real system, and be used to perform reasoning regarding it.[84]

Earlier we claimed that it is possible to ground the definition of models on the analysis of the notion of 'representation'. From the historical point of view, this is very important to us in the sense of continuity of epistemology from the classical idealism onwards. However, this might have been superfluous, since we can use instead of the term 'representation' more intuitive notion of modeling as replacing.[85] The very idea of modeling as replacing implies severe limitations on model-building – some target systems are simply too big and complicated, and it is not possible to model them in detail. It is not just that scientists wish the model to be cost-effective, that is, much smaller and simpler. It is a matter of our representational capability that at some level of size and complexity it becomes principally impossible to create model as a detail copy of the target system. Where we encounter that level? Well already at the level of ordinary everyday objects [see e.g. Podnieks 2010]. For example, the evolution of an isolated container (say a one liter plastic bottle) containing one liter of air is impossible to model in detail, if we believe that air consists of molecules, since for every of approximately 10^{22} molecules a specification of a Hamiltonian is required. Since Schrödinger's equation is already in the case of three particles solvable only by taking certain assumptions which basically reduce the problem to a single-body problem within an energy potential, and since usually there are no analytical solution to the multi-particle Schrödinger partial differential equation, in many cases even for a two-body problem, solving the equations for one liter of air proves impossible both to humans and to known computers.

[84] For the analysis of modeling procedures through the criterion regarding 'modelers', envisioned as a part of the larger project of understanding the modeling practice on which philosophy of science should be mobilized, see Weisberg 2007b.

[85] Strictly speaking: modeling as replacing for cognitive purposes. This way the idea of representation as a vehicle of inference-making is separated from other meanings connected to it, and thus made clearer.

Now we can ask, if it's impossible to exactly model one liter of gas, how large and detailed could be a model replacing the entire history of the universe? What about exactness of our model of the Milky way or the Solar system? How detailed can be a model of the entire history of the planet Earth, including the evolution of life and human race? These questions are, of course, only rhetorical ones. There point is to illustrate that science not just doesn't attempt to give an exact picture of the world, but that in most cases exactness is not even remotely possible.[86] Moreover, returning to the issues of explanation, in many cases exactness is not even desirable and modeling often strikes a delicate balance of introducing us with the world (exactness) and organizing these information in the form in which we can use it for our purposes (explanation). Due to our representational limitations, our models will, in most cases, always fall short of reaching exactness and thus idealizing or abstracting to a certain degree will always be included and thereby constitute the essence of modeling procedures.

Understanding models as abstract entities is considered by many as in a need of a further elaboration and is generally connected with recently thriving views on models as fictions [cf. Suarez (ed.) 2009; Fine 1993; Frigg 2006]. Considering abstractions as epistemological fictions can be misleading in numerous ways, most notably by seemingly depriving them of an empirical base. Anyhow, the status of inexact representations is, we consider, a problem concerning human representation in general, not specifically of approximations, abstractions and idealization in the scientific modeling, and thereby the question more appropriate for general theory of cognition than for model theory. Leaving the question of nature of abstract entities open, it seems sufficient for our purposes to note that every model isolate (or postulate) certain features of the target system, the ones expected to be relevant for making inferences regarding it.[87]

Picture of science

Due to this incompleteness of most of models,[88] we are forced in many cases to make a patchwork of different models in order to gain a deeper understanding of some phenomenon [Cartwright 1999; Teller 2004]. However, the limitation which blocks us to exactly model some

[86] For the theses of exactness of certain QM models, specifically the hydrogen atom model, cf. Hoefer 2003.
[87] For deep yet simple illustration of abstraction see Weisberg's 2007b analysis of Volterra's treatment of post-WWI fishery dynamics.
[88] Arguably all of them except, perhaps, the hydrogen atom model [see Hoefer 2003]; and we stated in ch. 2.3. that even this one is an abstraction from the environment and all the other effects beside the Coulomb's potential [Messiah 1969, 412].

physical systems may not be due to our cognitive limitations. Many systems are simply too unique to be modeled in full detail and a patchwork of models, each very restricted in its application and scope, proves to be the only solution [see also Cartwright 1994]. This cast further serious consequences on the practice of modeling. Along with the problems of cost-effectiveness of detail modeling and limitations cast by our intellectual processing capabilities, there is the problem of justifiability of thesis that nature is encodable in finitely statable universal laws and related models. Also, even supposing these ‘theories of everything’, the initial physical conditions, such the ones regarded in the application of QM to the multiple-body problem, are simply far too messy [cf. Teller 2001]. As Teller notes [ibid, 394], already Laplace recognized that:

> “All our efforts in our search for truth tend, without respite, to approximate the intelligence we have imagined [capable of exact application of Newton’s Laws], but our efforts will always fall infinitely short of this mark.” [1812, 3],

and although many careful advocates recognize the above problems and the need of a refinement of our philosophical positions, few have taken their repercussions to indicate that the idea of modeling as a drive for perfect, exact models should be scraped entirely.

As far as the notion of ‘theory’ is concerned, extreme changes have occurred in the recent decades. Logical empiricists used it to refer to representational capacities in science; the SA proponents used it as an unspecified highly abstract structure from which, together with initial conditions, models are deduced – a theory as a general template for models (which are more specific abstract objects), not considered to be a vehicle for making empirical claims [e.g. Suppe 1989; or Giere 2004]; finally, ‘theory’ was used as an ambiguous term referring to various elements contributing in the construction of models, a catchall for model-building tools [Suarez & Cartwright 1995; and 2008]. Intertwined with the undermining of the notion of ‘scientific law’ understood as universally true generalization which we discussed earlier (Ch. 2.3) [Cartwright 1983; and 1999; Giere 1988; and 1999; Teller 2001], and its replacement by ‘principles’ that, more or less, guide the model-building [Morrison 2000; Giere 2010], the breakdown of the traditional conception of theory liberated modeling from the methodological straitjacket. Not that science is now conducted differently, but our philosophical expectations changed. We do not expect anymore that general philosophical account can provide us with insights into the scientific methodology. For that matter, the Model view should be seen in the first place as the deconstruction of the possibility of a formalistic approach and a clearing of the ground for a pedestrian, naturalistic philosophy of science – a case-by-case development of methodological framework. This ‘framework without a framework’ is there to encourage from-bottom-to-top analyses, not to prescribe acceptable modeling procedures, but to search for them in the concrete analyses and describe them in the above minimalistic sense. As Fine’s enlightened variant of Feyerabend’s motto states: in science, methodologically, “many things go” [Fine 1998, 11], and our best science is devoted precisely to finding those ‘things’, there is no need and place for a philosophy to prescribe them.

In spite of these historicist traditions of the MV dating at least back to Quine [1969] and American pramaticists, most notably Dewey [1923], analytical framework of the MV remains under construction. Emphasizing both the plurality of model-building components, specifically those other than formal and explicit in their nature, and also the diversity in modeling methodologies the MV’s project of understanding the scientific practice remains in a need of a further concrete analyses of modeling strategies employed. At the same time, it stresses that the scientific models are deeply shaped by functions and practices, and that they can be applied and interpreted according to many standards, many times made along the way in the scientific inquiry, not following an approved procedure. Consequently, the MV project is directed at incorporating further analyses of concrete strategies found in the actual scientific modeling, such as the forms which abstraction, idealization and approximation takes, and the delicate constrains of context-dependency, to mention just a few. In this regard, the MV stands in a perspective position to catch on the enormous work performed in the sociology and history of science spawned since the historical turn [Bird 2008], if, of course, it manages to hold on and impose its social and cognitive framework to the bewildering ways of the scientific inquiry.

THE END

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Biography

Stojanovic Milutin was born on 27 January 1987 in Belgrade, Serbia, where he finished junior education. In 2006 he enlisted at Belgrade University, Philosophical faculty, Department of philosophy. He finished graduate studies of philosophy in 2010, with average 9.39, graduating with thesis “Quine’s empirism without dogmas” mentored by Prof. Dr. Zivan Lazovic. He finished master studies in 2011, with average 9.75, by defending master thesis “The role of partially understood models in scientific reasoning” mentored by Prof. Dr. Slobodan Perovic.

Milutin started his PhD studies at Belgrade University in 2011/12 were he is writing his PhD dissertation ever since, under the mentorship of Prof. Dr. Slobodan Perovic. Since 2012 he is working on scientific research project Dynamical systems in nature and society: philosophical and empirical aspects (Динамички системи у природи и друштву: филозофски и емпиријски аспекти) financed by Ministry of education, science and technological development at Government of Serbia. Milutin is particularly interested in logic, methodology and epistemology of science, and has published related articles in international scientific journals.

Statements

Прилог 1.

Изјава о ауторству

Потписани-а ______Милутин М. Стојановић_______

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изјављујем да је штампана верзија мог докторског рада истоветна електронској верзији коју сам предао/ла за објављивање на порталу **Дигиталног репозиторијума Универзитета у Београду.**

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Изјава о коришћењу

Овлашћујем Универзитетску библиотеку „Светозар Марковић“ да у Дигитални репозиторијум Универзитета у Београду унесе моју докторску дисертацију под насловом:

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