Scientific Progress On The Semantic View

An Account of Scientific Progress as Objective Logical and Empirical Strength Increments

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Master Thesis IFIKK, Faculty of Humanities University of Oslo Autumn 2013 Theories are our own inventions, our own ideas; they are not forced upon us, but are our self-made instruments of thought: this has been clearly seen by the idealist. But some of these theories of ours can clash with reality; and when they do, we know that there is a reality; that there is something to remind us of the fact that our ideas may be mistaken. And this is why the realist is right.

- Karl Popper

Abstract

The aim of this master thesis is to make a convincing argument that scientific progress can be spoken of in objective terms. In order to make this argument I will propose a philosophical theory of scientific progress. Two concepts will be constructed with this aim in mind, both which are types of strength measures on scientific theories.

The first concept, that of logical strength, pertains to the way a theory may exclude, or permit less, model classes compared to another theory. The second concept, that of empirical strength, pertains to an objective measure of the informational content of data models, defined in terms of Kolmogorov complexity. This latter idea stems from communication and computational theory. Scientific progress is then defined as the interaction, or the stepwise increases, of these two strength measures.

Central for the conception of a scientific theory is the philosophical framework known as The Semantic View of Scientific Theories. This view can briefly be characterized as an empirical extension of Tarskian model-theory. Another central notion for this theory of scientific progress is the philosophical or metaphysical thesis called structural realism. Both will accordingly be explained and argued for.

Finally, as a test on this proposed theory of scientific progress, it will be applied to two examples of theory transition from the history of physical theory. I conclude that the proposed theory handles these two cases well.

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1. Introduction

Whether we can talk stringently about scientific progress or not is a question that cuts to the very bone of fundamental philosophical problems, like objective knowledge and truth. If objective knowledge is achievable anywhere it should be reflected in the methodology and results of science, which is the very enterprise of, and institution for, epistemological pursuit.

Science is in flux. Now, how can that which changes point to truth? If science were not merely changing but evolving, not only in a state of flux but also developing with an inherent direction, then maybe the pursuit were approaching truth. But one common and influential philosophical view has it that scientific theories are too paradigm dependent to allow any objective standard in terms of which scientific progress could be determined. Nevertheless, I believe this view to be wrong. Scientific progress does occur. And in this thesis I propose a philosophical theory of what such progress consists of.

The logical positivists aimed to construct a unified account of scientific theories, in close accordance with scientific methodology itself. They sought also to explain the progress of science. But their philosophy was haunted by their adherence to inductivist doctrines of confirmation. Neither did their accounts of theory progression by way of theory reduction succeed any better. Karl Popper vigorously criticized the perceived central role of induction in scientific methodology, arguing that the mark of scientific attitude is rather conjectural about truth (but certain as to falsity), with the corresponding emphasis on falsification of theories rather than their confirmation. But neither his conjectures and verisimilitude concept could give an acceptable explication of scientific progress.

The overarching focuses in the philosophy of science then shifted. More emphasis was given to the history and the sociology of science, and to the psychology and idiosyncrasies of the *paradigm*. Another closely related shift was towards more pragmatic philosophical attitudes. A common enumerator being an increasing distance from the original aims and aspirations of the logical positivists. Correspondingly, the project of constructing a concept of scientific progress, in objective and paradigm independent terms, seeming ever more hopeless, ever less attainable. However, the project of giving a unified analysis and account of scientific theories did not, or has not, become extinct. I will argue that The Semantic View of Scientific Theories is a most viable alternative. Not only does this view succeed in accounting for the structure of scientific theories. It also delivers a philosophical-mathematical framework on the background of which a theory of scientific progress can be constructed. That construction is the goal of this master thesis.

The semantic view of scientific theories coheres well with another philosophical thesis which will be explained and defended. This thesis is called *structural realism* and holds that the important features of what scientific theories convey are structural properties, or the relational aspects, of empirical reality. As we will see, once this thesis is incorporated in our more general philosophical framework, issues posed as problems for the conception of science as an accumulative enterprise dissolve, or at least become problems we are able to overcome.

This master thesis has one main ambition:

To develop and give a philosophical theory of scientific progress.

This proposed theory has the aspirations of both being objective and enjoying a high degree of generality.

In order to provide and defend a philosophical theory of scientific progress I will complete the following tasks in consecutive order.

- First, a philosophical account of scientific theories must be in place. This will be done by closely studying two such conceptions. One of which will be rejected, the other one will be used as framework for giving an account of what a scientific theory is, which is needed in order to tackle the question of scientific progress.
- 2. The philosophical (or metaphysical) thesis of structural realism will be explained and defended, a thesis which establishes the continuity across scientific change. And in addition to cohering well with the framework of the semantic view adopted here, structural realism functions as yet another building block needed in order to construct the proposed theory of scientific progress.
- 3. My proposed theory of scientific progress will be given.
- 4. The theory of scientific progress will be applied to and evaluated against two cases of theory transitions from the history of physics. And as will be argued for, the theory of scientific progress handles these two cases in a satisfactory way.

Here follows an overview of the parts and chapters of this thesis.

The first part will deal with philosophical frameworks for scientific theories. The first chapter of this part, chapter 2, will go through the logical positivists' conception of scientific theories, called The Syntactic View of Scientific Theories, or The Received View. It has here been devoted a whole chapter due to two reasons. The syntactic view share certain relevant features with the other framework we will adopt. And much of its philosophical legacy still exist in much contemporary analytical philosophy.

The third chapter will give an account of the semantic view of scientific theories. I explain how this view shares some commonalities with the syntactic view but differs in other important ways, and how the semantic view is more apt as an adequate conception of scientific theories. We will bring with us this framework all the way to the end of the thesis.

The fourth chapter first sums up the discussion so far, then gives an explicit account (or meta-theory) of what a scientific theory is. This chapter ends the first part of the thesis, as we now have an account of scientific theories in place, ready to embark on our project's next step.

The second part of the thesis, dealing with scientific progress, is also the part where the original contribution is provided and presented. First, in the fifth chapter, a serious problem for a *realist* conception of the historical continuity of scientific theories will be explained. Then the thesis of structural realism will be presented as solution to this particular problem. After having argued for theoretical continuity and theory retention via structural realism, we present our theory of scientific progress, accounting for the cumulative aspect in addition to the continuous one. This proposed theory accounts for both logical and empirical strength increments (to be defined) enjoined by scientific theories, which are the two components constituting scientific progress.

In the sixth chapter I discuss two examples of theory transition from the history of physics. The first example will demonstrate an increase in logical strength, dealing with two versions of Newtonian mechanics. The second example will demonstrate an increase in empirical strength, dealing with the transition from Newtonian mechanics to the special theory of relativity.

Ultimately, in the seventh chapter, the thesis is concluded by taking a look at what lays ahead. I propose concrete suggestions for future research and gives some remarks about the generality of this account. Ultimately I make some reflections within a broader intellectual context about the philosophy here propounded.

Part 1

Philosophical Frameworks for Scientific Theories

2. The Syntactic View of Scientific Theories

Although the [Syntactic] View continued to enjoy wide acceptance after logical positivism had been rejected, it is the product of logical positivism and cannot be understood if divorced from the tenets of that movement.

Frederick Suppe, The Structure of Scientific Theories

The syntactically defined relationships are simply the wrong ones. Bas van Fraasen, The Scientific Image

The logical positivists failed in their attempt to give a satisfactory account or an adequate philosophical theory of the structure of scientific theories. Their view is known as the Syntactic View, or the Received View, of Scientific Theories (henceforth called the syntactic view of scientific theories, or simply the syntactic view). In this chapter we will look at the origins of this view, the actual contents of this view, and the problems pertaining to it which ultimately led to its demise. As we will see, the main source of problems was its particular emphasis on linguistic analysis, which mainly focuses on the syntactical properties of theories at the expense of meaning and semantics. Three related problems distinguish themselves: (1) The limits of axiomatization in first-order logic; (2) a problematic bifurcation of the language of science into one theoretic and one empirical part; and (3) the intended role of so-called correspondence rules connecting these two language parts.

The comprehensive and classic account of the syntactic view of scientific theories is Frederick Suppe's contribution to *The Structure of Scientific Theories* (1977). The present chapter leans heavily on his account, but is complemented with some works of the founders of the view themselves, e.g., Rudolf Carnap and Ernest Nagel.

The chapter is divided as follows. In the first section I will present the historical and ideological context relevant for the rise of logical positivism and its view of science. Then in the second section a closer look at the syntactic view of scientific theories and some of the modifications made through its development. In the third and last section I turn to problems leading to the fall of both logical positivism generally and the syntactic view specifically.

2.1 The Syntactic View and Logical Positivism

The Syntactic View of Scientific Theories cannot be seen in isolation from the philosophical movement from which it was spawned, namely *logical positivism*. This movement or philosophical school was developed during the first decades of the 20th century in Germany by a group of philosophers and scientists known as the Vienna Circle. Some of its prominent figures were Hans Hahn, Moritz Schlick, Hans Reichenbach (and his Berlin School), Otto Neurath, and for later times maybe the two most influential ones, Rudolf Carnap and Ludwig Wittgenstein.

Characteristic of this group and for our concerns was the mixture of philosophers, mathematicians and logicians, and scientists, composing this group and developing its particular philosophies. They were generally highly skilled in contemporary science and mathematics, being both aware of and working on different problems pertaining to the different disciplines. This in turn influenced their philosophical views in obvious ways, allowing this school of thought to be dubbed as *scientific philosophy*. Now, these decades being as revolutionary as they were in both science and mathematics, it makes less wonder why this philosophical movement came to be as radical as it was. Furthermore, not only was it radical but also greatly influential of later philosophy, being in some ways the backbone itself of the whole *analytical tradition*. We will now look more closely at some of the ways events and developments in science and mathematics motivated the logical positivists to conceive of the syntactic view of scientific theories.

What's in a name? asked Shakespeare. The two component names of 'logical positivism' pay due to the remarkable synthesis of ideas and attitudes constituting this philosophy. And the syntactic view has clear and direct traces from both. We will start with *positivism*, and then in turn look at the *logic* part.

Contemporary Science Reinforcing the Verifiability Principle

Positivism was the philosophical school of thought extending a strict empiricist attitude beyond natural science to the relatively young social sciences, and is commonly associated with Auguste Comte. *Empiricism* is the more general attitude. (The later phase of the movement is often called logical empiricism.) As empiricists the logical positivists were intellectual heirs of David Hume, holding on to the conviction that the only true source of knowledge is the senses. Whatever bit of our knowledge that cannot be shown traceable back to empirical origins must be tossed to the flames, "for it can contain nothing but sophistry and illusion." (In Ariev and Watkins 2009; Hume, Enquiry, section 7, part 3). This verificationist imperative evolved into one of logical positivism's central ideas, the *verificationist principle*, and is an essential part of the specific philosophical

framework within which their view of scientific theories was conceived (Godfrey-Smith 2003, chapter 2). Let us now look at how some significant events pertaining to physical theory can be seen as both vindicating and reinforcing the empiricist emphasis.

Newtonian mechanics was until the turn of the 20th century regarded as an essentially true theory of physical nature. Although there was some philosophical controversy dealing with the concept of action-at-a-distance inherent in Newton's theory, breaking with Cartesian vortex mechanics of contiguous contact, it is safe to say that, generally, the theory was philosophically *intuitive*. The theoretical framework postulates a three-dimensional (infinite) Euclidean space and an independent linear time dimension (Friedman 1983, pages 12 and 18). A telling example of how this framework is said to be intuitive is how Kant could conceive of this physical framework, indeed, even the laws or axioms themselves, in a purely *a priori* fashion. Newtonian theory could thereby be seen as a precondition for empirical reality itself. No wonder then the controversy and widespread initial reluctance to acknowledge it when a competing theory claimed this framework to be essentially wrong.

Now, the important point here is *how* it was shown to be wrong. In 1919 the physicist Arthur Eddington and his team conducted a fairly simple experiment during a lunar eclipse commonly taken as establishing the truth of Einstein's revolutionary relativistic mechanics over the Newtonian one. As a consequence, mainstream philosophical conceptions of space and time needed to be discarded. *A posteriori* results falsifying *a priori* conceptions. A solid victory of the empirical-scientific approach over armchair philosophizing. This moral was taken to the heart by the logical positivists.

Another field of physics where crucial research and discoveries were made during this epoch, and having even more revolutionary philosophical implications than relativity theory, is quantum mechanics. What quantum theory did in an even more thorough way than relativity theory is forcing the abandonment of the idea that a conceptual framework for the theory should be intelligible or intuitive. This statement is maybe too weak since quantum theory not only have unintelligible concepts but contra-intuitive ones, and, some may say, even contradictory physical consequences (e.g., particles being neither wholly present nor wholly absent.) The moral that was drawn from this and the attitude it provoked, not only for the logical positivists but for the community of physicists more generally, was to pragmatically disregard intelligibility (*If you think you understand quantum mechanics, then you don't understand quantum mechanics*, as Richard Feynman allegedly said) as long as the mathematical formalism *worked*, i.e., gave the right measurable predictions¹. It needed in addition no form of conceivable *interpretation*. And as to the

¹ This attitude is characterized as the agnostic position, and has been widespread among physicists and philosophers

way the formalism worked, quantum mechanics is, after all, the mensural validated science *par excellence*.

Combining the lessons to be learned from the groundbreaking events related to the advent of the two new theories in fundamental physics, we see how these events can constitute a push toward a more thorough empiricist attitude, and one which is more pragmatically oriented when it comes to theories having less intuitive and conceivable concepts and consequences. What is more, since one could argue also that philosophical preconceptions even delayed progress in physical theory, by delaying their acceptance due to philosophical concerns, this motivated a rejection of a goal of philosophy being to explain what the theories are about, beyond the scope of mere measurement, prediction and observation. A step towards a deflationary and a more pure empiricism. As it happens, something analogous was happening in the disciplines of logic and mathematics and the role of pure syntax, as we will see, now as we turn to the *logic* part of our movement's name.

Logicism and Formalism in Contemporary Formal Sciences – The Expulsion of Meaning

Developments in the formal sciences were also highly influential of how the logical positivists came to view what a scientific theory ideally is or should be. Their focus on, and high esteem of, logic must be seen together with the rise of a new type of logic, symbolic or mathematical logic, stemming mainly from Gottlob Frege's analyses on language and mathematics. With this new logical toolbox, serious attempts were made at formulating all of classical mathematics in first order logic (although not completely successful); in other words, the *reduction* of mathematics to logic. This was the project of the *Principia Mathematica* by Alfred North Whitehead and Bertrand Russell from 1910. As had been the case with physical theorizing, with physical theories not cohering too well with some of our preconceptions, similar issues or paradoxes existed in the fields of pure mathematics. Now, with the apparent (at the time) reduction of mathematics to logic, less focus and attention was given to the alleged meaning of mathematical terms and statements, and correspondingly more was given to strictly formal or syntactical aspects, and to consistency rather than *truth*, traditionally conceived. The view that mathematical statements were devoid of meaning, that they were nothing but 'mere' logic and reducible to it, was called *logicism* and gained much ground and respectability during this epoch when much attention was turned to foundational issues in mathematics and logic².

Another important and relevant development happened in the field of geometry, which

alike. Another popular orientation was (and still is) the *orthodox* or *Copenhagen interpretation*. But when these latter are hard pressed on realist issues, they usually fall back on the agnostic position. (See Griffiths 1995, pages 3-4.)

² See chapters 5 and 6 in Shapiro, 2000.

further prompted the step from focusing on meaning to focusing on mere syntax. And in addition to this, development in geometry, together with the work in mathematical logic, paved the way for the logical positivists to hold the view that scientific theories should be axiomatizable. For millennia, actually, geometry was equivalent to Euclidean geometry; there were no other. And a trait most distinctive of Euclidean geometry, influencing also much philosophical theorizing, even mimicking it, like Descartes, was the axiomatic character of it. But it wasn't until the 19th century, when mathematicians first started questioning the axioms of Euclid, nearly taken as givens since the days of antiquity, and as a result developing and working on alternative systems of geometries, that axiomatization itself became a focus of research. This it did by highlighting the axiomatization's role in formulating, determining, and constraining the alternative geometries. (This work also inspired the ideas of model theory, a subdicipline in mathematical logic, which we will return to many times throughout this thesis. (See van Fraassen 1980, page 41)) This way we see how the development of non-Euclidean geometries did more than just prepare the geometries needed for relativity theory in physics; it also gave rise to elaborating on axiomatization and on what it is to be a theory and a model, mathematically speaking. And questions of axiomatization in turn became a central topic in mathematical logic and proved to be a most fruitful approach there.

But why did the work on alternative geometries further prompt the step from meaning to mere syntax? Because, in order to be able to develop these geometries many preconceptions pertaining to the meaning of geometrical terms, like straight lines and triangles, had to be exorcised or thoroughly nullified³. Pasch, working on the foundation of geometry, wrote that "... if geometry is to be really deductive, the deduction must everywhere be independent of the *meaning* of geometrical concepts.." (Quoted in Suppes 1988, page 82). These intuitive geometrical terms lost their presumed meaning, the familiar concept, once they were implicitly defined in various different axiom systems, and what was instead considered important and interesting was what logically followed from the axioms and questions of consistency. This work in geometry was a special case of the more general strategy known as the Hilbert program, after the famous mathematician David Hilbert. In the philosophy of mathematics, Hilbert is known for his adherence to *formalism*, a view that it is the merely syntactic features of logical systems, like deducibility and consistency, which really matters. The parallel to *logicism* when it comes to a kind of abandonment of semantics is obvious, and once again we have arguments *against* meaning considerations and arguments *for* focusing on purely syntactical features or symbol manipulation; pure syntax.

We have now traced some of the philosophical and scientific reasons motivating the two

³ See the section *Alternative Geometries and Their Interrelations*, chapter 9, Nagel 1979.

fundamental doctrines characterizing logical positivism. We see one push towards a strict verificationist attitude where all meaning is empirical meaning, together with the other push towards purely formal considerations when dealing with logical, mathematical, and theoretical frameworks more generally. It is the peculiar application of the combination of these two approaches to empirical sciences that is the syntactic view.

2.2 A Presentation of the Syntactic View of Scientific Theories

First of all, the Syntactic View is not intended as a descriptive account of the structure and the practice of actual scientific theories and scientific work. It is rather a prescriptive or a normative account, meant to give a canonical formulation of the general features that make scientific theories earn their status as such (Frederick Suppe 1977, page 62). It is not necessary that actual scientific theories be thus formulated, although they should in principle be able to be reconstrued in this canonical way. What are these general features?

First, the theory should be axiomatized (or formalized; see also page 37), in a specified language L (paradigmatically, first order logic) associated with a logical calculus. The relevant scientific laws are treated as a set of sentences or axioms, called the theory T (often mathematical equations).

Secondly, this language L is strictly bifurcated into two sublanguages, the theoretical language, L_{τ} , and the observational language, L_o . Pertaining to each of these sublanguages is the corresponding class of terms, theoretical terms, *t-terms*, and observational terms, *o-terms*. A sentence in L_{T} would be one whose terms are exclusively theoretical, and we will call such a sentence a *t-sentence*; and correspondingly for sentences whose terms are exclusively observational, we will call *o-sentences*. A scientific law, like (the mathematical formulation of) Newton's law of gravitation, would typically be a t-sentence while a statement expressing an experimental outcome would typically be an o-sentence. It is important to note that the whole theoretical vocabulary, by implication also the scientific laws formulated in it, have by themselves, or viewed independently, no meaning. "Before the C-rules [see next paragraph] are given, L_{τ} , with the postulates T and the rules of deduction is an uninterpreted calculus." (Carnap 1956, pages 46-47). They are only symbols following the syntactic rules given in the logical calculus.

Thirdly, in order to provide the t-terms with meaning they need to be somehow connected to the observational language. In accordance with the verificationist principle only L_0 can be said to have meaning, or be empirically meaningful (which is equivalent on this view.) Therefore, in order to provide the theoretical vocabulary with meaning, it is necessary to have a set of *mixed sentences*,

that is, sentences containing both t-terms and o-terms, so that we can use our logical calculus in an empirically meaningful and useful way. These sentences are called *correspondence rules* (or c-rules) and they are what incorporate the verificationist principle into the framework of the syntactic view. An example could be sentences establishing the correspondence between the theoretical entities termed 'electron' and certain traces in a Wilson cloud chamber. Another one would be sentences connecting the *theoretical* massive bodies of Newtonian theory with, say, *empirical* celestial and massive bodies like the Moon.

Definition of a Scientific Theory, Prediction and Explanation, and Empirical Truth

The theory *T*, when taken in conjunction with the set of correspondence rules *C*, the conjunct *TC*, is what constitutes *a specific scientific theory*. By making the requisite observations and by use of the c-rules we can provide our theory with initial conditions, then via the logical calculus, and back to the empirical realm once again through the c-rules, we get empirical predictions as logical consequences. This resembles the Covering Law Model associated with, e.g., Carl Hempel (and most adherents of that model were also adherents to the syntactic view.) According to that model prediction and explanation are formally equivalent, so we also get the tools for (or a conception of) scientific explanation by the use of *TC* (Suppe 1977, page 28).

We can further provide a definition of the *empirical truth* of a scientific theory TC. If we let E_{θ} be the class of all observational consequences of TC, then a necessary condition for the empirical truth of TC is that E_0 be true of the actual world (somehow conceived). This is not a sufficient condition, though, since the conjunction of a different component theory T' with C, or the same T with another set of c-rules C', or of T' and C', would on this account be a different scientific theory although it could still have the same class of observational consequences E_0 . This would make the two theories empirically equivalent, conditioned precisely on having this same Eo class (ibid, page 29). What would constitute a sufficient condition for empirical truth, or whether such a condition is even attainable, is dependent on more general epistemological and metaphysical considerations and interpretations, like the schism between realism and instrumentalism in the philosophy of science (More on this in section 5.1). A realist could say that what would constitute a sufficient condition for the empirical truth of TC, in addition to E_0 being true of the actual world, is that the scientific laws in T be empirically true generalizations about the behavior of the relevant entities (especially the unobservable ones); while an instrumentalist, on the other hand, would characteristically avoid the question of truth altogether and rather say that TC is but a tool for making true observations, and if all the relevant true observations is included in E_0 , then TC is *empirically adequate*, and that is as good as it gets.

We have now briefly gone through the three essential features of a scientific theory according to the syntactic view. To repeat, this was the axiomatization or formalization of the scientific theory in a first order language (together with a logical calculus); that this language be strictly bifurcated into two parts: an observational sublanguage and a theoretical sublanguage; and lastly, that these two language parts are connected with each other through correspondence rules, thus providing meaning to the theoretical terms and statements (or sentences). All three of these are problematic, some more than others. The first one will be given some comments in the next chapter (see section 3.2). The second is closely connected to the notorious problem of the observational/theoretical dichotomy, as we will see below, and also to the analytic/synthetic distinction. But it was issues concerning the third feature, the correspondence rules, that, more than the other two features, generated modifications and changes with profound ramifications during the development of the Syntactic View. And it is to these we first turn.

More on Correspondence Rules

Correspondence rules were to serve three functions: "[F]irst, they define theoretical terms; second, they guarantee the cognitive significance [or empirical meaning] of theoretical terms; third, they specify the admissible experimental procedures for applying a theory to phenomena." (Suppe 1977, page 12). Exactly how they came to serve these functions changed considerably over its development, but according to Suppe's account, in the original formulations of the syntactic view, of circa 1930, correspondence rules were supposed to be explicit definitions, of the form:

$$Tx \equiv Ox$$

where 'T' is a theoretical term and 'O' is an observational one (*ibid*, page 12). Explicit definitions give both necessary and sufficient conditions for an identity or equivalence but this requirement in turn proved to be too strong and wrongheaded for its requisite function. We cannot go in detail into the problems with identifying theoretical entities or properties with observational ones here, or merely their equivalence, but will list and briefly explain some of them. First of all, even if explicit definitions of the theoretical vocabulary in terms of an observational vocabulary did actually work (which is not the case), then all of the theoretical vocabulary would thereby be substitutable with the observational one, and thus in principle eliminable, making the whole theoretical part of a scientific theory superfluous⁴. A strange consequence indeed, given that the explicit definition

⁴ Here Craig's Theorem is relevant. This is a theorem meant to show the possibility of replacing a formal linguistic

requirement is demanded in a framework whose theoretical part of language is seen as both essential and *indispensable*.

Explicit operational definitions were also attempted for correspondence rules but had equally severe problems. On this account a theoretical concept, like temperature, is identified with, or is synonymous to, the corresponding unique set of mensural operations. Two problems with this view is that, firstly, there are typically many different experimental ways of measuring the same theoretical magnitude, generating the unwelcome consequence of ending up with just as many distinct concepts. The second problem is that operational definitions are given in a modal language. Like dispositional terms (see below, section 2.3), operational definitions need to be *counterfactually* expressed, and modal notions are notoriously difficult and apparently impossible to accommodate in the language of first order logic (Suppe 1977, page 19). Material implication will not do, as modal notions are typically expressed as counterfactual conditionals. Now this problem pertaining to the limitations of first order logic is a more general one, especially as to how scientific laws are to be interpreted, since laws expressed in first order logic are purely extensional, lacking modality. Yet another important issue generated the same problem of the lack of expressibility in first order logic: Dispositional terms, like the chemical property of being soluble in water, are widespread in scientific terminology and, as with theoretical magnitudes, cannot be explicitly defined in observational terms with the use of this limited logical apparatus.

These problems with correspondence rules as explicit definitions gave rise to two modifications in the original version of the syntactic view. The most important modification was the step from correspondence rules as explicit definitions to something called *Carnap's reduction sentences* (see next section). Reduction sentences did not require necessary and sufficient conditions but merely sufficient test conditions for the theoretical-observational correspondence, and they mimicked modal logic while still being formulated in first order logic. This was simultaneously a step from correspondence rules completely defining theoretical terms to providing only a *partial* definition, since more than one reduction sentence can define the same theoretical term (Suppe 1977, page 22). The theoretical terms were then said to have been given a *partial interpretation* in terms of observational language. When taken into consideration with the second function of correspondence rules given above, to guarantee empirical meaning, it seems reasonable that this modification from complete definition to partial interpretation must have important semantic implications. This will be further inquired in the next section. The second modification was a more positive attitude towards modal logic, thereby making a step away from the strict first order logic

system containing theoretical expressions by another system having no theoretical terms yet having the same

empirical content. It is dubious, though, whether this works with actual (and finite) theories. See Nagel 1979, pages 133-137).

requirement. Whether this step makes the syntactic view more plausible or less so will not be discussed here.

Presentation of the Complete Schema

We can now present the final version in detail of the Syntactic View of Scientific Theories, as of circa 1960, as it is summed up in Frederick Suppe 1977 (pages 50-51):

- 1. There is a first-order language *L* (possibly augmented by modal operators) in terms of which the theory is formulated, and a logical calculus *K* defined in terms of *L*.
- 2. The non-logical or descriptive primitive constants (that is, the "terms") of *L* are bifurcated into two classes:
 - (a) Vo which contains just the observation terms;
 - (b) V_T which contains the nonobservation or theoretical terms.
- 3. The language *L* is divided into the following sublanguages, and the calculus *K* is divided into corresponding subcalculi (the specification of the latter will not be relevant to our purposes):

(a) The *observation language*, L_0 , is a sublanguage of L which contains no quantifiers or modalities, and contains the terms of V_0 but none from V_T ;

(b) The *logically extended observation language*, L_o' , contains no V_T terms and may be regarded as formed from L_o by adding the quantifiers, modalities, and so on, of L.

(c) The *theoretical language*, L_{τ} , is that sublanguage of L which does not contain V₀ terms.

4. *LO* and its associated calculi are given a *semantic interpretation* which meets the following conditions:

(a) The domain of interpretation consists of concrete observable events, things, or thingmoments; the relations and properties of the interpretation must be directly observable.
(b) Every value of any variable in *L*₀ must be designated by an expression in *L*₀.
We may construe interpretations of *L*₀ (and the corresponding subcalculus) as being *partial semantic interpretations of L* and *K*, and we require that *L* and *K* be given no observational semantic interpretation other than that provided by such partial semantic interpretations.

5. A *partial interpretation* of the theoretical terms and of the sentences of *L* containing them is provided by the following two kinds of postulates: the *theoretical postulates T* (that is, the axioms of the theory) in which only terms of V_T occur, and the *correspondence rules* or postulates *C* which are mixed sentences (sentences containing terms from both V_0 and V_T). The correspondence rules *C* must satisfy the following conditions: (a) The set of rules *C* must be finite.

(b) The set of rules C must be logically compatible with T.

(c) C contains no extralogical term that does not belong to V_0 or V_{τ} .

(d) Each rule in C must contain at least one V_0 term and at least one V_T term essentially or nonvacuously.

We can now consider the modifications more specifically. Where (1) originally, or initially, had a strict first order logic requirement, it is now possibly augmented with modal logic. Where (4) had originally simply said that the (observational) terms of V_o be "interpreted as referring directly to physical objects or directly observable attributes of physical objects" (Suppe 1977, page 16), they, or the whole sublanguage of L_o , are now to be given a *semantic interpretation*. And, finally, where (5) originally had required correspondence rules to be explicit definitions, they are now *partial interpretations*.

These modifications are illustrative. A significant difference between first order logic and modal logic is the latter's need for a special type of *semantics* to evaluate its truth conditions, paradigmatically a *possible worlds*-semantic framework, where the first is in no such need, being wholly syntactic. And when we consider this modification together with the other two, they make vivid a developmental tendency from pure syntactics to a more elaborate emphasis on semantics, foreshadowing the other great view of scientific theories, the one we will turn to in the next chapter, The Semantic View of Scientific Theories. But now we need to dig deeper into the inherent problems of the syntactic view, which ultimately led, or at least contributed, to its generally acknowledged demise.

2.3 Problems and Demise

We saw in the last section that correspondence rules as explicit definitions were given up and came to be replaced by reduction sentences. Two motivations for this were adduced. The trouble pertaining to requiring a necessary condition for the definition of theoretical terms, because there are entirely different observational or experimental ways of handling the same theoretical concept; and secondly, limitation problems pertaining to first order expressibility led to the creation of reduction sentences mimicking the counterfactual or subjunctional character of modal notions. Problematic issues concerning this use of a pseudo modal logic will not be further pursued. But we will pursue the step taken from complete to partial definition of the theoretical terms.

The Problematic Reduction Sentence

To make our inquiry more vivid, we will give an example of a reduction sentence, and illustrate the difference between such a sentence and an explicit definition (in first order logic) for a familiar dispositional property, which is in this respect sufficiently analogous to theoretical terms in general. In the previous section the chemical property of being soluble in water was mentioned. As an explicit definition it would be rendered thus⁵:

x is soluble in water if and only if, if x is immersed in water at t, then x dissolves at t,

where 'soluble' (S) is the dispositional property, and 'immerse' (I) and 'dissolve' (D) are taken as straight forward observable properties. Thus, schematically:

$$Sx \equiv (t) \ (Ixt \supset Dxt).$$

The trouble with this explicit definition is that it follows that every item not immersed in water is soluble in water (simple consequence of the material implication on the right hand side of the biconditional.) The corresponding reduction sentence would be:

For any x and t, if x is immersed at t, then (x dissolves at t if and only if x is soluble),

or schematically:

$$(x)(t) [Ixt \supset (Dxt \equiv Sx)].$$

We see that on the reduction sentence version, the immersion of x in water is not a necessary condition, only a sufficient (test) condition. And it does not have the unwanted consequence of all items not immersed in water being soluble in water. The general form of a *bilateral reduction* sentence partially defining a theoretical term Q_3 in terms of observational terms (or logical combinations thereof) Q_2 (the test condition) and Q_3 (the relevant consequence) is simply:

$$Q_1 \supset (Q_2 \equiv Q_3).$$

Even though reduction sentences solved some of the problems pertaining to explicit definitions, and

⁵ This is an alternative variant of the example given in Frederick Suppe 1977, pages 19-21.

disregarding the question of modality, they nevertheless brought problems on their own as candidates for the role of correspondence rules.

First of all, there was the question if theoretical terms are in fact introduced into science in this way, and if they in principle can be. This came to be seriously doubted. For example, the ψ -function figuring in the Schrödinger wave equation in quantum mechanics "do not even seem amenable to definition by reduction sentences." (Suppe 1977, page 23). Furthermore, due to the merely partial definition provided by reduction sentences, the V_T terms could not be completely introduced singularly (in lack of a complete observational definition), but had to be introduced by chains of these reduction sentences. Nevertheless, the chains of reduction sentences did not seem able even to introduce more familiar "metrical theoretical concepts such as 'mass', 'rigid body', 'force', 'absolute temperature', 'pressure', 'volume', 'Carnot process', 'electron', 'proton', etc." (*ibid*). It seems, then, that neither singular reduction sentences nor chains of them can introduce relevant theoretical concepts in the requisite way. Carl Hempel wrote about the theoretical terms that,

Terms of this kind are not introduced by definition or reduction chains based on observables; in fact they are not introduced by any piecemeal process of assigning meaning to them individually. Rather, the constructs used in a theory are introduced jointly, as it were, by setting up a theoretical system formulated in terms of them and by giving this system an experimental interpretation, which in turn confers empirical meaning on the theoretical constructs. [Quoted in Suppe 1977, page 23].

Hempel further argues that it is not simply that these terms happen not to be introduced into science in this way, but rather that they *cannot* be introduced this way.

Secondly, we remember that one of the functions of the correspondence rules is to fix and guarantee the empirical meaning (or cognitive significance) of the theoretical terms. As has been anticipated in the previous paragraph, the syntactical 'entity' intended as sufficient to provide the theoretical terms with meaning did steadily enlarge. These steps from, first, singular explicit definitions to partial interpretation, through chains of reduction sentences; then the second step, from these latter chains of reduction sentences to the whole theory *T* regarded as a unit, both changed the criterion of empirical meaningfulness in profound ways. If, as first proposed, theoretical terms are introduced one by one and completely defined by the correspondence rules, there is no problem as to *how* empirical meaning is *supposed* to be bestowed on them (given that it works, so to speak). But as to partial interpretation, there arises both the *how* question and questions of *to what degree* it leaves the meaning of t-terms underdetermined and, more interestingly, what account is to be given about *whence* the remainder of the meaning of t-terms *is to be imported from*, when the meaning cannot presumably be wholly observational.

Turning Towards Semantics

A third and important question becomes pressing: Can this partial interpretation be done in a way compatible with the central doctrines of the syntactic view? According to some of the syntactic view's critics, e.g., Achinstein and Putnam, the notion of partial interpretation cannot be formulated in a precise way compatible with the requirements of the view (*ibid*, page 87). If we again turn to Carnap, "All interpretation (in the strict sense of the term, i.e. observational interpretation) that can be given to L_T is given in the C-rules..." (Carnap 1956, page 46). But as the problems with reduction sentences, or chains of them, showed, it is not at all clear *how* the c-rules do this job. This problem does not get any better by saying the whole theory *T* as a unit together with *C* provide the partial interpretation. This kind of semantic 'holism', "The unit of empirical significance is the whole of science", inevitably pushes the whole project in a more pragmatic direction, away from the tenets of logical positivism, alá Quine's⁶ (as was the trend in this philosophical tradition in the 1960's and 1970's). And the ones holding the 'whole' unit approach, often ended up giving up the analytic/synthetic distinction, which, in effect, is denying the c-rules their strict role (as we'll come back to below). But if *TC* by itself cannot convincingly be said to give a partial interpretation in any meaningful way, what else is needed?

Again, it is a stronger commitment to semantics. Ernest Nagel, a central figure in the later epoch of the Syntactic View, extended his view of scientific theories, beyond being a logical calculus having its terms provided with meaning via correspondence rules, with the further requirement that scientific theories were given "an interpretation or model" (Nagel 1979, page 90). Carl Hempel wrote that the theoretical part of a scientific theory could be given a semantic interpretation in a suitable metalanguage, providing them with the normal meanings they are assumed to have in natural though scientific language (Suppe 1977, page 91). And Hilary Putnam proposed partially interpreting L_T with the use of a class of intended models. These uses of 'models', and especially the latter two's allusions to model-theoretic notions, will be further studied in chapter 3. And, as with the stronger emphasis given to semantics in general, alluded to in the end of the previous section, the increased focus on model theory also anticipates the semantic view of scientific theories.

But, to repeat the last question from above, how compatible is this step towards a richer semantics with the basic framework of the syntactic view? Not much. Let us repeat the basics, now in the words of Van Fraasen: "A theory is to be conceived as what logicians call a deductive theory, hence, a set of sentences (the theorems), in a specified language." (Van Fraassen 1980, page 55).

⁶ As the conclusion of Two Dogmas of Empiricism bears witness to. The preceding citation is from the same article, page 42 (Quine, 1953).

Letting a scientific theory be dependent on a specific (logical) language in this way severely constrains how we can conceive of what the theory says. This view, strictly speaking, confines what one can say about a theory to its syntactical relationships, like deducibility and consistency, because it puts the syntactic entity, and the focus on it, as fundamental and logically prior to any semantic concern. Instead, on the semantic approach of Van Fraasen, one identifies a theory with *a class of models*, and this in a way that is *independent of a specific language (ibid*, page 44). The models are entities, mathematical structures, describable in a variety of ways, in a variety of languages. This is, among other things, "... important for the comparison and evaluation of theories, and is not accessible to the syntactic approach." (*ibid*.).

It seems, then, that the flirtation with semantics during the later phases of the syntactic view is problematic relative to its *specific* logical language requirement, (1) on page 19, and definitely breaking with its motivation for having a pure logico-syntactical core.

The Problem with Individuating Scientific Theories, and Scientific Practice

Before we preceed with the problems of language bifurcation there is yet one problem with correspondence rules that must be mentioned. We remember that a specific scientific theory on the syntactic view is constituted by TC, the theoretical postulates together with the set of correspondence rules. Accordingly, a change in the set C gives a new scientific theory. The third function of correspondence rules was given above as to specify the admissible experimental ways of applying a theory to phenomena.

What, then, if some new technology, e.g., some new measuring apparatus, makes possible new ways of experimentally testing our scientific theory? (Suppe 1977, pages 103-104). It follows that we are then dealing with another or a new scientific theory, since the set C is now changed. This is a strong argument for not letting the set of c-rules be an essential part of a scientific theory. We have seen, then, that the presumed role and form of correspondence rules generates several problems, not all of which have been dealt with here.

We will end this discussion on correspondence rules by adducing the general observation, given by Patrick Suppes, one of the founding fathers of the Semantic View, that the account of correspondence rules is "far too simple". Further,

The kind of coordinating definitions [correspondence rules], often described by philosophers, have their place in *popular philosophical exposition* of theories, but in *actual scientific practice* of testing scientific theories, a more elaborate and more sophisticated formal machinery for relating a theory to data is required. [Quoted in Frederick Suppe 1977, page 106.; emphasis added]

Thus, problems with language, logic, and meaning notwithstanding, the account of correspondence rules as given in the syntactic view seems inadequate also from the perspective of actual scientific practice.

Problems with the Language Bifurcation

Another consequence of this logico-puritanical motivation, as we have seen, was bifurcating the relevant language into two parts, one theoretical and one observational, where the theoretical part was itself devoid of meaning, but was assigned empirical meaning by being coupled with observational language through correspondence rules. This bifurcation of the language terms is closely connected to the observable/unobservable dichotomy of objects and properties but is nevertheless a separate dichotomy. (It is not clear that what we would classify as *theoretical* is identical with what we would classify as *unobservable*.) First of all, when semantics is taken into the account, from which it was initially banned, and the theoretical terms have some meaning after all (though presumably not individually), the *language bifurcation* seem less warranted, and even less so when seen together with the *role* of correspondence rules. Remember, the bifurcation was a demarcation of meaning, and c-rules the meaning guarantor of the t-terms. So, with the inclusion of semantics, do we then have a kind of *theoretical* meaning in addition to *empirical* meaning, corresponding to the language bifurcation? Maybe it is better to drop the theoretical/observational language bifurcation altogether.

The language bifurcation is intimately connected to the analytic/synthetic distinction, together with the logical positivists' doctrines of meaning more generally, including verificationist principles we've looked at above. These doctrines of meaning have been severely criticized. Most notably, maybe, by Quine (1953) in his Two Dogmas of Empiricism, where he argued that what he called the dogma of reductionism (the verificationist principle) is at root identical with the dogma of the analytic/synthetic distinction (*ibid.*, page 41). Even though it is controversial whether Quine has shown this distinction to be completely untenable, still it is safe to say that no account has so far been given as to how to characterize the distinction in a way that is sufficiently clear and unproblematic. Especially not in way that would somehow ground the strict bifurcation of language, which after all is axiomatic on the syntactic view. To the extent verificationist principles and the analytic/synthetic distinction are problematic, so is the bifurcation of language requirement. And if no clear boundary can be drawn between the theoretical part and the observational part of language, in ways correlative with, or at least intricately connected to, the absence of a strict a general analytic/synthetic distinction, which again cast doubts on verificationist principles, the

central role of correspondence rules is lost. One of their essential functions was to connect the two language parts; another function was to incorporate the verificationist principle. If the language (ideally used in science) is not really bifurcated this way, the connecting function is made obsolete. And if the verificationst principle does not work for individual statements but necessitates at the very least whole scores of auxiliary statements (hypotheses), if not the whole of science, then yet another function of correspondence rules is made obsolete.

We now turn to one last issue dealing with the bifurcation of language. Above we focused on the term dichotomy and found serious problems pertaining to the theory of meaning underlying the split of our relevant language into two parts, one of which is essentially devoid of any empirical meaning. But what about that other dichotomy, the entity and attribute dichotomy? The one that divides nature into two classes, one of which is directly observable and the one which is not? Are there more substantial reasons from the observability and non-observability of our ontology for holding a correlative bifurcation in language? This is a very complex issue, and we will restrain the discussion so as to comment only on the question's implications for the syntactic view. The notion of direct observability is central to the syntactic view and was kept up till the final version, as a necessary requirement for the use of the observation language (see (4a) above). But when it comes to the delineation, no clear boundary can be drawn here either. Carnap concedes this in 1966, "There is a continuum which starts with direct sensory observations and proceeds to enormously complex, indirect methods of observation. Obviously no sharp line can be drawn across this continuum; it is a matter of degree." (From *Philosophical Foundations of Physics*, quoted Suppe 1989, page 56).

Suppe summarizes the problems pertaining to this distinction thus, worth quoting at length.

If we require that an attribute's presence *always* must be ascertainable in principle by direct observation in order for it to qualify as directly observable, then the paradigmatic ones (such as the property of being blue) fail to qualify. Further, if we require only that their presence *sometimes* be so ascertainable, then paradigmatic non-directly observables (such as the property of being a gas) become directly observables.

The problems encountered in attempting to draw a line between observable and nonobservable properties, and so forth, stem from the fact that many attributes of scientific relevance have both directly observable and nondirectly observable occurrences, which makes any natural division ... impossible. [Suppe 1989, page 59]

Still, the language bifurcation, assumed to be co-extensive with the observable distinction, was maintained, though somewhat pragmatically during the later phases, by referring to the scientists

own usages of the distinction. No longer was the language bifurcation correlated directly with the observability distinction but rather indirectly through a referral to standard usages in scientific English. The theoretical/observable term dichotomy was to be drawn on the basis of ordinary usage in natural scientific language (*ibid*, page 57).

But where a natural dichotomy between entities and attributes relative to their direct observability seem next to impossible, can the allusion to scientific English under normal scientific usage fare any better with creating the desired *term* dichotomy? Suppe concludes that if we base the distinction on normal usage, then the term dichotomy will not match the entity and attribute dichotomy. The remaining option would be a rather artificial and arbitrary distinction, necessitating a terminology where directly or nondirectly observable *occurrences* of properties prescribes where to draw the line, that is, by artificially dividing every property into two classes according to the observability (or not) of their occurrences. But this strategy seems to be riddled with no less problems on its own terms (*ibid*, pages 59-60).

We can safely conclude that there seems to be no pressing reasons for us to adopt a strict bifurcation of our language when it comes to what is directly observable and what is not. From early empiricism, what was deemed to be directly observable was taken to be unproblematic relative to questions of truth. This epistemological concern was the original motivation for a language bifurcation. But as with the continuum pertaining to direct observability, it being a matter of degree, so it is with confirmation of scientific hypotheses and theories. No strict linguistic boundary between what is empirically meaningful and not, or no method to syntactically isolate empirical meaning units, seem either possible or necessary. Of course, this does not imply that the question of what is observable and not is not an interesting and important one. And one can still think the syntactic view wrong when it comes to the bifurcation of language, though holding on to the observable/unobservable distinction more generally, as does, e.g., Bas van Fraasen (1987, pages 56-57).

To sum up this section and chapter. We have seen how the basics of the framework of the Syntactic View, especially the requirements of a specific logical language L, the strict bifurcation of this language into two components, and the form and assigned role of the correspondence rules, give rise to several serious problems. And, further, the proposed solutions for these problems did not cohere too well with these mentioned requirements, thus indicating or suggesting some of the Syntactic View's presuppositions to be flawed or at least to be inadequate and inappropriate to give a satisfactory account of the structure of scientific theories. As we saw in the introducing chapter this does not show the attempt of giving such an account to be ill adviced or a non-starter, although

considerably less focus was given to the relationship strictly between scientific theories and nature or reality as logical empiricism collapsed [must check if I can argue for this in the Intro]. Now, within the same tradition there was another approach developing, the semantic approach to scientific theories, which we in the next chapter will turn to.

3. The Semantic View of Scientific Theories

... the semantic approach, as developed by Suppes, van Fraassen, Giere and Suppe himself, does have the distinction of being one of the very few – perhaps the only – global analyses of science in these philosophically fractured, post-Kuhnian times. Steven French and James Ladyman, Reinflating the Semantic Approach

A model consists, formally speaking, of entities and relations among those entities. Bas van Fraassen, Laws and Symmetry

In the last chapter we saw how the Syntactic View of Scientific Theories is seriously flawed. This chapter will present an alternative view, The Semantic View of Scientific Theories. We will argue for both its virtues as a philosophical theory of the structure of scientific theories, and especially how this view can be seen as advantageous vis-à-vis the syntactic view. The most important way in which they differ is how the syntactic view give central importance to the role of pure linguistic analysis, while the semantic view offers a more language independent analysis, with an emphasis on scientific theories as defining mathematical structures and how these structures are mathematically, not linguistically, related to empirical reality. This difference is partly explained by the semantic view's model-theoretic foundation, in contradistinction to the syntactic view's foundation in first-order logic. By placing semantics and mathematics center stage and not language *per se*, I will show how the semantic view can give a more satisfactory account of the structure of scientific theories.

The chapter is divided as follows. In the first section an account of the origins of the semantic view will be presented, showing its close relationships with model-theory and foundational works in the sciences, and its role in the broader philosophical context. Then in section two I will look more closely at the content of this view, particularly at its concepts of theory and model. Finally, in the third section, I will explain how the abstract models are related to empirical reality, through a hierarchy of connected models, and how the concept of truth is conceived of or understood on this view. In all three sections, differences with the syntactic view will be highlighted.

3.1 Origins of the Semantic View

The logical positivists did their philosophy with close attention to problems in science. They saw themselves as following a strict adherence to scientific method, and wanted their own philosophy to be, in a way, scientific. Nevertheless, their project of representing the structure of science was ill-founded due to certain untenable tenets, as we have seen in the previous chapter. During the Second World War the Dutch philosopher and logician Evert Beth complained about the "increasing discrepancy between science and philosophy" and proposed as remedy that "a philosophy of science, instead of attempting to deal with speculations on the subject matter of science, should rather attempt a logical analysis – in the broadest sense of this phrase – of the theories which form the actual content of the various sciences." (quoted in Frederick Suppe 1989, page 6). Logical positivism, and their syntactic view, had to force scientific theories into a frame which form was *ab initio* strongly dictated by their views on logic and verificationism. The *broad* analysis suggested by Beth was meant to counter this tendency of forcing scientific theories into a philosophical straightjacket. And the appropriate means of this analysis was to be the Semantic Method introduced by the great Polish logician Alfred Tarski as early as the 1930's (*ibid*.).

Tarski's work on formal semantics has been of great influence to philosophy (see Patrick Suppes' survey *Philosophical Implications of Tarski's Work*, 1988). In particular, his original contribution to the theory of truth for formalized languages, which grounds the concept of truth in terms of the concept of *satisfaction*, is probably the best known (Suppes 1988, page 86). This theory was key to the development of the subsequent (mathematical) theory of models, which is essential to the semantic view and will concern us a great deal later on. Tarski's model-theory distinguishes two related elements: the sentences or axioms, called the *theory*, on the one hand, and the *models* in which the sentences are *satisfied*, on the other. In a way there are here two entities (or aspects) of theories, where on the syntactic view there is only one. In the logical perspective of the latter, there is only the theory (set of axioms) with its deductive closure (the logical implications of the sentences, syntactically defined.) While on model-theory, "the central thrust ... is to study the mutual relations between sentences of formalized theories and *nonlinguistic* mathematical systems in which the sentences of the theory hold." (*ibid.*, emphasis added). This is an important and significant change of perspective, and will be further elaborated in the next section.

What Beth saw and wanted to exploit, was the potential this view could have for scientific (or empirical) theories. He combined the semantic techniques of Tarski (and other later developers, like Carnap) with the work of von Neumann on the foundations of quantum mechanics, and made some suggesting proposals for semantic analyses of both classical and quantum mechanics

(Frederick Suppe 1989, page 6). Beth did not carry this new project very far, but others soon followed and did just that. The next main developer, now in the 1960's, and presumably the most important one, was Patrick Suppes. He believed that Tarskian model-theory gave the unifying and correct concept of *model* to be used in an adequate description of the structure of scientific theories (see, for instance, his *A Comparison of the Meaning and Uses of Models in Mathematics and the Empirical Sciences*, 1960, page 289). Further, scientific theories were to be axiomatized in set-theoretic terms (a mathematical theory which coheres well with model-theory), thus making explicit and clear all the important relations and structures within the theory. This specific axiomatization, i.e., in set-theoretic terms, is not necessary for the semantic view, as we will see below. Equally important to these abstract and purely theoretic points was his insistence on being true to the complexities involved in experimental work in science. Consequently, he inquired into the nature of experimental methodology and made an account of how these lower layers of experimental research is connected with the higher and more theoretical layers, through a hierarchy of models. (More on this in subsequent sections, especially section 3.3.)

While Patrick Suppes' version of the semantic view is rather neutral to the realism/antirealism controversy about scientific theories, Frederick Suppe has developed a scientific (*quasi*)realist version, as given in his 1989 book, and Bas van Fraassen has developed his antirealist, *constructive empiricist*, version through many of his works (1980, 1990, 2008). This shows the generality of the semantic approach in terms of its independence from ontological and metaphysical positions in the philosophy of science. Also, the resurgence in the last couple of decades of *structuralist* positions in the philosophy of science, especially in the philosophy of physics and the philosophy of mathematics⁷, often go hand in hand with the semantic approach. The foundation of the semantic view is particularly well fitted to explicate and make clear just what is meant by the term 'structure' – the exact meaning of which is often somewhat elusive.

Back in section 2.1 we went through some of the historical and ideological context in which the school of logical positivism with its syntactic view established itself. The semantic view has both some continuities and some discontinuities or divergences from that former tradition. Both use theories and conceptual frameworks from contemporary logic and mathematics as foundations for their respective views on scientific theories. Both have as aim giving a unified account of the structure (form and content) of scientific theories, and to explicate in an objective way, and one independent of factors strictly extrinsic to the content of the theory (like the sociology of science), the relation between scientific theories (as such) and empirical reality. Among the discontinuities,

⁷ Some of these are known as *structural (epistemic* or *ontic) realism* and *scientific structuralism*, associated with, for example, John Worrall, Steven French, James Ladyman, and Bas van Fraassen. For more, see section 5.1.

perhaps the most significant is its view of language. As we have seen, on the syntactic view the language (as such) in which theories are formulated was (is) an essential part of their constitution and *gehalt*. And as we will see, on the semantic view, the idea of describing the content of theories in a way *independent* of the specific language used, is as essential on their view as the language specificity *dependence* is for the syntactic view. Another less important though interesting indication of discontinuity, is how the main developers of the semantic view consistently did not pay much attention to positivistic philosophy during their formative years, instead concerning themselves with actual science (Suppe 1989, page 16)⁸.

3.2 Theories and Models

The semantic view will here be presented more formally. This will be done in slow, successive stages in effort to make the presentation clear and intuitive. The primary division of the account will follow Ronald Giere's own compact nutshell formulation of the semantic view, as "a) the *theoretical definition*, which defines a certain class of systems; b) a *theoretical hypothesis*, which assert that certain (sorts of) real systems are among (or related in some way to) members of that class" (quores in van Fraassen 1990, page 222). Thus we will start with the purely abstract in this section, the theories and models, and then move on to their relation to empirical reality in the next. Although this account leans heavily on, and draws much from, the different authors mentioned and referred to, the selection and the consequent synthesis is my own.

Conceptualizing the Semantic View

Where the syntactic view wanted to dispell meaning from consideration and focus solely on logicosyntactic properties of expressions and formulations (or sentences), the semantic view reemphasizes, naturally, the *semantics* 'behind' the former purely linguistic entities. In a quite intuitive way, the focus is not on the logico-syntactic form of linguistic expressions but rather on what they *stand for*; what they are meant to *present* or convey to us; what they, as symbols, are *symbols for*. Or said in another way, we now focus on what is described rather than the description itself. The

⁸ It is my contention that the semantic view is both important to the philosophy of science and that it has not been given the attention it deserves in the philosophical literature. Generally, this may be a consequence of the fall of logical positivism and the subsequent popularity of the *Weltacnhuung* approaches, with the corresponding negligence of a close study of the theory/empirical reality relation.

The semantic view or approach is highly mathematical in nature, and must be that way given its modeltheoretic framework (see Patrick Suppes 1967, page 57). Now, if Galileo was right in that mathematics is the language without which it is impossible to understand this Grand book (the Universe), it may also be that way when it comes to proper understanding of what scientific theories, describing and representing this Universe, are and what they convey. If that is the case, a highly mathematized philosophy of science may be required for an adequate representation of science, at least of certain parts of it.

semantic view incorporates the fact that we can convey or describe the same thing in different ways – a trivial fact when it comes to *natural languages*; this thesis might have just as well been written in Norwegian – a point mentioned earlier in connection with its emphasis on language independence. But this many-to-one relationship between descriptions and 'thing described' does not pertain merely to natural languages, but also to formal and exact ones, like mathematics – language of science *sine qua non*.

When it comes to both scientific and purely mathematical theories, what is described by way of theory are abstract models. To illustrate this thought, and distinguish it from the syntactic view, we will look at a geometrical example given by van Fraassen (1980, pages 41-44). Consider the following (jointly contradictory) axioms:

- A1 For any two lines, there is at most one point that lies on both.
- A2 For any two points, there is exactly one line that lies on both.
- A3 On every line there lie at least two points.
- A4 There are only finitely many points.
- A5 On any line there lie infinitely many points.

From these axioms we can make theories and explore them as on the syntactic view. Let T_{θ} have axioms A1-A3; T_1 have A1-A4 (or, T_{θ} plus A4); and T_2 have all axioms minus A4. By modern symbolic logical analysis (the essential and 'only' tool for the syntactic view) some consequences immediately follow. "Each of the three theories is *consistent*: no contradiction can be deduced. Secondly, T_1 and T_2 are *inconsistent* with each other: a contradiction can deduced if we add A5 to T_1 . Thirdly, T_1 and T_2 each *imply* T_{θ} : all theorems of T_{θ} are clearly also theorems of the other two." (*ibid.*, page 42). These are of course very important results. But does these logical conclusions exhaust what is implied by the axioms? Van Fraassen continues and draws our attention to this: "Yet, it will also be noticed that these logical notions have *counterparts* in relations expressible in terms of what the theory *says*, what *it is about*, and what it could be interpreted as *being about*." (*ibid.*, emphasis added.) Let us, then, examine this counterpart. Here is a depiction of a finite geometrical structure, the so-called Seven Point Geometry (Figure 1, below).



[Figure 1. The Seven Point Geometry. Source: <u>http://gmfbrown.blogspot.no/2010/03/rational-abstraction.html</u>. Visited 26.08.13.]

This structure consists of seven basic elements, the 'points' 0, 1, 2, 3, 4, 5, 6, and the seven lines (though not necessarily straight, obviously) connecting these. Further, this is a structure of which axioms A1-A4 are true; in the language of model-theory, this structure *satisfies* the theory T_i (axioms A1-A4) and is therefore considered as a *model* of that theory. And since a contradiction cannot be true of anything, the (abstract) existence of a model implies consistency of its corresponding *theories* (theory-model relationship admits a many-to-one relation, analogous with the relation between descriptions and the thing described, referred to above.) T_0 is also a theory for this model, but not T_2 . More generally it seems fair to argue that the theory-model relation is, in a way, a many-to-many relation, since a theory actually picks out not one model but a class of models (the class of models satisfying the theory); and, conversely, a model (or a class of such) constrains in a determinate way its many possible theory-formulations. We see here the two entities 'doing work, instead of only one, as indicated in section 3.1. And it is this model-theoretic framework that enables the semantic view to transgress the tight limitations of the syntactic view, in the following way.

How The Semantic View Goes Beyond The Syntactic View

We could prove consistency of the theories by providing a model for them, as we saw, which is often simpler (*ibid.*, page 43) than demonstrating consistency purely syntactically. With the notions of *model* and *truth*, which belongs to semantics, van Fraassen reminds us, we can go further than

logic. Logic tells us that the theories T_1 and T_2 are inconsistent, "and there is the end to it". (*ibid*.). But thinking about these theories in terms of mathematical structures, we can say more. That the first theory is only satisfied by finite structures, the latter theory only by infinite structures. And further, the finite Seven Point Geometry structure can obviously be represented on a Euclidean plane surface (as with the figure depicted above), showing the interesting property that this structure can be *embedded* in another structure. The (finite) Seven Point geometry is a sub-structure of the (infinite) Euclidean plane structure. These embedding relations are mathematical morphism relationships, and will be further explained below.

By studying models with a broader mathematical tool set, instead of studying theories only within the narrow confines of logic, we see that we can compare and evaluate theories in ways not accessible to the syntactic view (*ibid.*, page 44). The change in focus is encapsulated in Patrick Suppes' simple idea, that "*to present a theory, we define its class of models directly*, without paying any attention to axiomatizability, in any special language," (quoted in van Fraassen 1990, page 222), and is further shown in the practice of modern mathematics. Today's presentations of, say, Euclidean geometry, is not so much focused on Euclid's axioms, as on the Euclidean spaces themselves, directly (*ibid*). Neither is this restricted to pure mathematics. A similar shift can be seen in Suppes' work on reformulating the foundations of Newtonian mechanics, replacing Newton's axioms with the definition of a Newtonian mechanical system (class of models) (*ibid*).

This shift in focus, corresponding to the shift from the syntactic view to the semantic view, can also be viewed as distinguishing an intrinsic from an extrinsic characterization of scientific theories. "The formulation of a theory as a logical calculus ... gives an intrinsic characterization." (Suppes 1967, page 60). We have already seen some of the limitations with this characterization. Even in the context of logic, questions such as whether a theory can be given a first-order axiomatization require "some extrinsic way of characterizing the theory" (*ibid*). This latter way, as we have seen, defines instead its models directly. In order to do this, one first designates a particular model and then characterizes "the entire class of models of the theory" (we return to how this is done below). This latter characterization pertains to properties *extrinsic* to a particular model (i.e., "formulate[s] a set of axioms that will characterize this class of models without referring to the relation between models.") (*ibid*). The question of axiomatizability can then be posed quite simply as the possibility of stating "a set of axioms such that the models of these axioms are precisely the models in the defined class."

Let us see another example which shows how the semantic view goes beyond the syntactic view. This time an example from science, from classical particle mechanics, with important general

results.

The axioms for classical particle mechanics are ordinarily stated in such a way that a co-ordinate system, as a frame of reference, is tacitly assumed.

One effect of this is that relationships deducible from the axioms are not necessarily invariant with respect to Galilean transformations. We can view the tacit assumption of a frame of reference as an *extrinsic* aspect of the familiar characterizations of the theory. From the standpoint of the models of the theory, the difficulty in the standard [first order] axiomatizations of mechanics is that a large number of formally distinct models may be used to express the same mechanical facts. Each of these different models represents the tacit choice of a different frame of reference, *but all models representing the same mechanical facts are related by Galilean transformations*. [ibid., page 61, emphasis added.]

This specific equivalence class of the relevant models is on the intrinsic characterization not afforded any theoretical significance, yet it represents the Galilean relativity principle⁹, precursor of Einstein's theory and maybe the most important concept in the history of physics. That is, considerations extrinsic to the theory (extrinsic to the axiomatic framework of particle mechanics) are required in order to distinguish what is special about this class of models. Suppes comments on this that,

It is certainly possible from a philosophical standpoint to maintain that particle mechanics as a scientific theory should be expressed only in terms of Galilean *invariant* relationships, and that the customary formulations [intrinsic characterization] are defective in this respect. [*ibid.*, page 62, emphasis added].

More generally, from a scientific standpoint we can definitely appreciate the fruitfulness for modern physics of describing theoretical *invariants*, together with the closely related concept of symmetry. Group theory in mathematics is especially apt for studying this kind of invariant structure. The theory of groups was conceived by way of generalizing the concept of symmetry in geometry, where one sought to formulate the invariant, *structural* properties pertaining to these as such. As these invariants started popping up in fundamental physics, a deep connection between group theory and physics was revealed (see Ian Stewart 2008, *Why Beauty is Truth – The history of Symmetry*).

An analogous example is given by the Lorentz invariants in Einstein's special theory of

⁹ That this class of models (or class of reference frames) representing the same mechanical facts are related by Galilean transformations is equivalent to the Galilean relativity principle, stating the requirement that all the laws of physics have the same form in all admissible frames of reference. (In Newtonian theory, only inertial frames of reference. In Einsteinian theory, even gravitationally accelerated ones (the *general* theory of relativity.). See also section 6.1.
relativity. Also in quantum mechanics, similar relationships are of profound importance. This is not a result attributable to the semantic view. But the latter is particularly well suited to explain or make explicit just why these invariants matter, elucidating the theoretical significance of certain mathematical relationships in ways the syntactic view could not.

Theoretical invariants also have significance of a more philosophical nature, pertaining to *objectivity*, by locating and describing what is constant over varying contexts, aspects, and perspectives. (Suppes 2002, chapter 4; James Ladyman and Don Ross 2007, section 3.3). This consideration is important both for explicating specific scientific theories and the nature of their (theoretical) ontology (at least on structuralist approaches), and also important to a proper evaluation of theory change and theory preservation over the history of science, especially during scientific revolutions. The latter is a topic we dive into in chapters 5 and 6.

Formalization, Models, and Mathematics

The semantic view places no less emphasis on formalization in dealing with scientific theories than the Syntactic View did on axiomatization. Although these notions are often used interchangeably, it will here be constructive to clarify and distinguish them. "*Axiomatization* consists in the establishment of an axiomatic calculus, and thus consists in an essentially syntactic formalization. *Formalization* encompasses both the syntactical techniques of axiomatization and the semantic techniques of model theory." (Frederick Suppe 1979, page 113. Emphasis added). Among the many advantages (to the philosophy of science) pertaining to formalization, according to Patrick Suppes, one is to bring out the meaning of a connected family of concepts in an explicit fashion; a second is to provide a degree of objectivity impossible without it (*ibid.*, pages 111-112). In the context of the semantic view, we could say that formalization is a means to make explicit and clear the concept of model. A concept which is ubiquitous and presumably indispensable in scientific thinking. And it is here the formal sciences come to our aid, through model theory and mathematics more generally.

The overly mathematical way of dealing with models as used in science, characteristic of the semantic view, has received criticism for not being able to provide an adequate and unitary account of the diverse uses of models in scientific theorizing and practice (e.g., Downes, 1992). We will be lead astray if we go too deeply into this, but it will be illustrative to our purposes here to view some of the response given (Steven French and James Ladyman 1999; Bas van Fraassen 2008, pages 309-311). Some of the criticism betrayes a certain confusion by treating mathematics and (formal) semantics as *subject matter* instead of useful *vehicles for communicating* abstract patterns and relationships.

The use of iconic models are well known in science. These are heuristics, visualizable

thought devices meant to shed light on something not known, by way of analogy to something that is better known (Frederick Suppe 1979, pages 96-97; French and Ladyman 1999, page 107). Consider, for example, the model of gas atoms as small colliding billiard balls. This *iconic* model has both positive, negative, and neutral analogies to what we know about gas atoms. Now, the ways in which these models are used do not at all suggest that they are mathematical models. The same can also be said of material models, like scale models in engineering or DNA-models in biology classrooms. These models are not 'semantic' models. But the issue here is rather if the relevant *function* of these uses of models, *qua models*, can be appropriately captured within the semantic view. On the latter's conception of models this function, which is to represent through certain similarities, is captured with mathematical precision and explicability (through partial isomorphisms, see below). "What these models represent is the *structure* of the entity concerned...," (French and Ladyman 1999, page 109) and the structure is on this account something abstract and objective, transgressing what is merely visualizable to us (as on the *iconic* use). And the upshot of the argument is that these different uses of models can indeed be captured in model-theoretic terms; in particular, in set-theoretic descriptions. We will now look formally at abstract models and their inner relationships.

Formalizing Scientific Theories in Set Theory

Suppes' set theoretic view on formalizing scientific theories ('axiomatize' in his vocabulary – we continue with ours) is the simplest and most general way within the semantic approach¹⁰. We start our formal exposition with the general way of defining a mathematical structure. Basically, it consists of a set of elements *A* and a set of relations *R* defined in terms of the elements, conjoindly denoted $\langle A, R \rangle$. Relations are also sets (or, strictly speaking, subsets of *cartesian products* on the set of elements¹¹) but where the elements *have an ordering*. Such ordered sets are also called tuples, where *n*-adic relations consist of *n*-tuples. One important type of relation is the common mathematical *function*, here denoted *f*. This simple schema, entities and their relations, has

¹⁰ There is no set-theory fundamentalism inherent to the semantic view. If the objection is made that the semantic view reduces science to set theory in a vicious way as did the syntactic view with a logical calculus, an important point is overlooked. On the semantic view the theory is not identified with its syntactic apparatus of formulation. The 'thing described' is in focus, not the specific description. First of all, there are several different set theories which one may use to describe the same structures (French and Ladyman, 1999, page 116). Secondly, other mathematical theories may be equally appropriate; for example category theory (Landry, *Shared Structure Need not be Shared Set-Strucure*, 2007).

¹¹ A *cartesian product* is a mathematical operation which returns a set from multiple sets. That is, for sets A and B, the Cartesian product A × B is the set of all ordered pairs (a, b) where a ∈ A and b ∈ B. (<u>http://en.wikipedia.org/wiki/Cartesian product</u>, entered 01.09.13.)

Let us make an example for a mathematical structure. Let the set of elements on which we define the relations be the real numbers \mathbb{R} . A cartesian product $\mathbb{R} \times \mathbb{R}$ (the real numbers 'crossed with themselves') can then be represented as the familiar (cartesian) coordinate system (x,y). Relations defined on this product picks out subsets of it, e.g., subsets of points x and y defined by a mathematical function (or formula.)

tremendous general expressivity. Further, describing in a clear way any subject matter fundamentally means describing some entities and relations.¹²

Mathematical structures describes the pattern of relationships holding between the members of its set. What the members are *qua* individuals does not matter. As such, the ontology of A is very liberal. It can be a discrete set of objects, like genes, or it can be continuous, like the real numbers; it can be finite or infinite. *It can be any manifold whose constituents can be structurally characterized* – like the notion of a spacetime or a quantum field.

Then we proceed to theories. Here we will consider group theory as an example, as given in (Suppes 2002, pages 31-32.) This is a very simple theory, and in standard formulation usually presented as the three axioms, where \circ is a binary operator (a function), *e* is the identity element and ⁻¹ the inverse operator (another function).

B1: $x \circ (y \circ z) = (x \circ y) \circ z$; (The function is associative.)
B2: $x \circ e = x$; (The function has an identity element.)
B3: $x \circ x^{-1} = e$; (Every variable has an inverse.)

Suppes comments that

The difficulty with these axioms, taken in isolation, is that one does not quite understand how they are related to other theories, or exactly how they are related to mathematical objects themselves. These uncertainties are easily cleared up by recognizing that in essence the axioms are part of a definition, namely, the definition of the predicate 'is a group'. The axioms ... tell us the most important properties that must be possesed by a mathematical object which satisfies the predicate 'is a group', or in other words, by a mathematical object that is a group. [Suppes 2002, page 31]

The first thing to notice here is that – in addition to the limitations pertaining to viewing theories axiomatically, or instrinsically, when it comes to explicating relationships between mathematical *objects* (outside of what is logically contained within the theory at hand, its deductive closure) – also *inter-theoretic* relationships stand in the same need. The second point concerns an *extrinsic* way adequate for these concerns, namely the definition of *a set-theoretic predicate*, which on Suppes' view is the correct way of formalizing scientific theories.

In this more general framework, not confined to the deductive closure of its axioms, adding

¹² This schema also incorporates the possibility of naming some of the elements; these are called the *constants*, denoted *c*. In number theory this can be the numeral '0', in theories of measurement it can be some appropriately selected points (like freezing and boiling temperature on the Celsius temperature scale). This fixes a certain order of the structure, but is not necessary in every structure.

the axioms of set theory to the framework of elementary logic, and with emphasis on the models rather on the axioms, we can describe the theory of groups by defining the class of its models directly as follows. Let U be a structure, and we give this set-theoretic predicate:

U is a group if and only if there exists a nonempty set *A*, a binary operation \circ on *A*, an element e of *A* and an inverse operation on *A* such that $U = \langle A, \circ, \neg^1, e, \rangle$ and for every *x*, *y*, and *z* in *A* the three axioms given above are satisfied. [ibid.]

Every structure satisfying this predicate is a model for group theory. And as is clear in the definition above, when we formally describe the relevant properties pertaining to (or constituting) the model, we define a whole class of models; all and precisely those models with the sought properties – the same *type of structure*.

Sameness or similarity of structure between models can be clearly and rigourosly defined by way of mathematical morphism relationships. Suppes writes that "one of the most general and useful set-theoretic notions that can be applied to a theory is the concept of two models or structures of a theory being isomorphic." (*ibid.*, page 54). The intimate connection between group theory and fundamental physics has been mentioned earlier. For example, one particular symmetry group, called the Lorentz group, corresponds perfectly to the transformations of frames of references, in special relativity, which leaves both the kinematical laws of that theory, Maxwell's field equations in the theory of electromagnetism, and Dirac's equation in the theory of the electron, all invariant. This holds by virtue of the mathematical structure (or models) of the coordinate transformations and of the group being isomorphic to each other. We see here that theorizing about structures, at a more general or higher level, so to speak, permits us to connect entirely different, though structurally similar, theories in a most fruitful way¹³.

Now, how are these morphism relationships established? They are established by so-called *representation theorems*. Simply put, given two structures, one finds a function which maps the constants, if any, and all the relations (including functions) of the one into the other. Let the two structures be

$$\alpha = \text{ and } \beta = .$$

Let f be a function with domain A and range (co-domain) B, and for any element picked out by a

¹³ Above we emphazised the importancy of the specific-langauge independence on the Semantic View; here we see also how this approach can be said to be *specific-theory independent*. It shows the unifying character of model-theory by handling metascientific analyses.

constant c_{α} in A, $f(c_{\alpha}) = c_{\beta}$ (the constant pick out a corresponding element in B); for each relation Rand set a of elements in A, if $a \in R_{\alpha}$, then $f(a) \in R_{\beta}$. This mapping is then a homomorphism from α to β (Hodges 1997, page 5). All the structure in α has a structural-identical counterpart in β (but not necessarily the other way around). A stronger relation is given by a function which has the same properties but is also *injective* (or one-to-one), then the function is an *embedding* from α to β . A function which is also *surjective* (bijective: one-to-one *and* onto) gives an *isomorphism*. The two structures are then said to be isomorphic, meaning they are, in a sense, identical; they are structurally or mathematically indistinguishable. Although their basic sets (A and B here) may be different, their relations are identical. The weaker morphisms are also called *partial isomorphisms*.

The Mathematical Landscape

Now, by the very act by which one has characterized a structure or model, one 'peaks' into a mathematical landscape or a logical space consisting of all possible models, delineating the scope of classes of models bearing these morphism relations to it and excluding all the rest. More concretely, by giving a representation theorem for a theory, Suppes writes that,

[a] certain class of models of a theory, distinguished for some intuitively clear conceptual reason, is shown to exemplify within isomorphism every model of the theory. More precisely, let M be the set of all models of a theory, and let B be some distinguished subset of M. A representation theorem for M with respect to B would consist of the assertion that given any model m in M there exist a model in B isomorphic to m. In other words, from the standpoint of the theory every possible variation of model is exemplified within the restricted set B. [*ibid.*, page 57].

All theoretical models, then, bears certain relations to one another, both logically and mathematically. And these relations indicate some of the ways we can reason about them and compare them. This approach of inquiry into and generalizing over models of theories does seem promising as a perspective for the philosophy of science. What is more, conceptualizing and reflecting on *the totality of possible theoretical models*, and how this complex whole must somehow be (meta-)structured, is a tantalizingly interesting thought.

Admittedly, this model- and set-theoretic foundation may seem distant to the structure and content of our familiar scientific theories and the ways they model the phenomena. But the foundation needs to be as abstract as it is in order to cover the general pattern of scientific modeling in all its variety. One of the most common ways is to portray a system developing over time (dynamical systems). Parameters of same and/or different dimensions of the system are related by certain equations as functions of time, viewed continuously or discreetly. Mathematically, this

defines a class of different trajectories in an abstract space of all possible system states, a so-called *state space*, a space of as many dimensions as the number of independent variables (parameters). On this conception there is nothing special about the time; the equations could instead be functions of spatial dimensions, pressure, temperature, or parameters from economic theory. The essence is the interconnections between the parameters, the structures *imposed* on this space by the equations. The relevant point for us here is that the partitioning of the phase space by *all possible solutions* is equivalent to set-theoretic partitions, thus fitting perfectly with the above foundation. The different solutions correspond to different classes of models for the theory, and here the state space view offers a way of conceptualizing some of their connections to one another.¹⁴

Scientific Laws on The Semantic View

An account of laws has been conspicuously absent, considering its high esteem and importance with respect to scientific inquiry and discovery. This has been intentional; laws do not have an inherently important status on the semantic view. This is due to the fact that laws are inescapably theory relative (though *objective*). Their *formulation* and exact form is dependent on the broader theoretical framework, both linguistically and semantically. Newton's mechanical laws, for instance, are not properly expressible without the differential calculus constructed by himself and Leibniz in the 18^{th} century. *We* can compare these laws with Aristotelian physics *in retrospect* – a comparison in principle not open to Aristotle – which demonstrates this asymmetry.

This does not mean the central role of laws in science is in any way diminished, only that they are secondary relative to theories. As we will see in the next section, good theories map successfully (though partially) onto the structures of empirical reality; and some aspects of this structure we may, if we so choose, call laws. Considering forms of state spaces from above, Frederick Suppe writes that,

... various configurations can be *imposed* on that space, such as trajectories, subspaces, probability measures, and the like. In particular, we can impose configurations on the points of such a space which correspond to laws of succession, laws of coexistence, and laws of interaction. [Suppe 1989, page 106. Emphasis added.]

These imposed configurations are indeed important. Laws of coexistence are laws which typically prescribe certain equilibria or possible states of a system (restrict possible positions in a state

¹⁴ Neither is the Semantic View restricted to sciences representable in this state space approach. Frederick Suppe has shown its applicability to problems in Biological Speciation and Taxonomy (1989, chapter 7) Considering, as explained initially, that this semantic framework fundamentally is about making explicit and systematizing relations between entities, the high degree of universality of its applicability should come as no surprise.

space), like Boyle's ideal gas law PV = rT; laws of succession correspond to the state space trajectories of temporal evolution described above, like Newton's laws of motion or the laws of population genetics; laws of interaction correspond to the mathematics describing interacting systems.

But which mathematical relations we decide to call laws are mathematically arbitrary. In the absence of successful, *unified* and objective criteria for what constitute laws, what distinguishes the particular relations so chosen, as laws, is the pragmatic significance they may have for us (and may often be the result of historical coincidences.) The status of law is something 'imposed' by us, not something inherent to nature. Van Fraassen writes about laws that, all the while they are "important features by which models may be described and classified", nevertheless, "[t]he distinction between these features [which we call laws] and others that characterize the model equally well is in the eye of the theoritician; it does not, to my mind, correspond to any division in nature." (van Fraassen 1990, page 223). The latter is admittedly somewhat instrumentalist in attitude, nevertheless, as explained above in section 3.1, the semantic view does not presuppose such an attitude but also admits of views with more realist inclinations. The point to be stressed, though, is that in evaluating theories on this view, what matter is how (good) the theories as such relate to empirical investigations. And that this evaluation should be done without any metaphysical bias or preconception as to the form of, or any special place for, so-called *laws of nature*. Mathematical measures of fit between different models are what matters. That is after all what gives science its *objectivity*.

In this long section we've seen the central aspects of theories and models on the semantic view, along with some elaborations on formalization and on what models are, and briefly commented on the role of the traditionally important notion of law. In the next and shorter section we will complete the presentation of the semantic view by connecting the abstract models with empirical reality, thus making them ontologically relevant (See also section 4.2).

3.3 Theories and Empirical Reality

We have now come to Giere's second component, the *theoretical hypothesis*, which asserts that certain *real* systems are among, or somehow related to, members of the class of models pertaining to the theory at hand. What does this mean? Or maybe we would like to ask, What makes a theory *true* according to the semantic view? Simply though inaccurately put, a theory is true if the world, or at least a part of it, is identical to one of its models. This section will elaborate on this assertion, explaining the relation between theory and world more accurately.

First of all, we need to dispense with some of the connotations to the concept of truth (or knowledge, depending on differing philosophical conceptions). As the semantic view can be considered as an empirical extension of Tarskian model theory, the concept of truth is defined in terms of satisfaction (see 3.1) through the existence of abstract models. For a concept of *empirical truth* we add to the theory-model satisfiability conditions, the condition of connecting models of theory with models of empirical data. As we will see, this concept of 'truth' is both constituted and constrained by the very methodology and form of scientific research. In a slogan, *scientific methodology is epistemology enough*. And the limits of scientific knowledge and truth is isomorphism of structure (see the ultimate subsection of this chapter).

Empirical Structure and the Models of our Theories

Let us give another example from van Fraassen (1980, pages 45-46) illustrating two things. First, it shows the basic idea of empirical structures being related to models of theory. Second, it clearly demonstrates how theories can contain structure which is superfluous. Newton, in his theory of mechanics, conceptually distinguished between true and apparent motion, relative to a postulated absolute space (We will come back to this example in section 6.2). True motion is motion relative to this absolute space; apparent motion is motion *as observed*, motion relative to the observer. However, relative motion could in principle always be identified as *differences* between true motions. When we measure and record celestial apparent motion by measuring relative distances, time intervals, and angles of separation, we form data as *relational structures*. In the vocabulary of van Fraassen, these are called *appearances*.

For Newton's theory to be *empirically adequate*, the theory must have some model such that all actual appearances are identifiable with (isomorphic to) true motions in that model. Thus, by interpreting or properly configuring the apparent motions, translating them onto the general model space of the theory (an inertial frame of reference), so that they can be evaluated in accordance with Newton's general equations of motion, we can compare the *structures* of the two. (That is actually the general way data is said to corroborate the theory. More on this below.)

There is a sense in which the class of mathematical structures constituting the models for Newton's theory is larger than the class of structures of *appearances*, which we will call *empirical structures*. When this class matches in the requisite way a model of the theory, the empirical structure can be embedded in the larger theory structure. When this happens, the empirical structure is called an *empirical substructure* of the theory (*ibid.*, page 43).

In attempting 'to save the phenomena' Newton made a theory hoping to make all appearances be empirical substructures of his theory. And as we know it was for a long time successful at this. What challenged it later on was discordances between empirical structures (data) and the theory's models. But, not only does discordances between theoretical and empirical model influence theory construction and modification; *superfluous theory structure indicates what is theoretically arbitrary from an empirical point of view*, and consequently what may be scientifically dispensable. Newton was challenged by his contemporaries as to the notion of absolute space, most notably by Leibniz. The notion did not make sense because it, in principle, did not make any (empirical) difference. It can be shown that Newton's theory is empirically equivalent for the center of the universe having an arbitrary constant motion in any direction (*ibid.*, page 46). These model universes are all structurally identical¹⁵.

The models of a scientific theory, then, stand to trial in at least two types of ways having to do with empirical considerations. The first and direct way has to do with failure of structural fit between models of the theory and *actual* models of data. The second, indirect and almost transcendental way has to do with considerations about whether *possible* empirical structure would constitute any theoretical difference (empirical equivalence defined structurally). Both ways narrow down the class of acceptable models, sharpen our theories. (We return to this in discussing our theory of scientific progress in chapter 5.2.) The structural fit between theory and data and the notion of a hierarchy of models will be the object of inquiry for the rest of this chapter.

The Hierarchy of Models

Patrick Suppes introduces the concept of a hierarchy of models in two seminal papers (1960, 1962). We've mentioned earlier the spectrum of this hierarchical ordering as from purely abstract models to empirical models of data. We will designate the abstract models or pure theory as the higher end, empirical models as the lower. Although theory construction pertains to the whole hierarchy (the lower levels are also thoroughly theory-laden), there are still some essential differences dividing the hierarchy in a pure theory part and a data part (see figure 2 on page 47 below for a graphical depiction.).

Theories of data always pertain to measurement, where a measurement, in the words of van Fraassen, essentially

... is a physical interaction, set up by agents, in a way that allows them to gather information. The outcome of measurement provides a representation of the entity (object, event, process) measured, selectively, by displaying values of some physical parameters that - according to the theory

¹⁵ This type of consideration, completed by Mach and Einstein, expelled all superfluous theory structure in mechanics with respect to motion (See, for instance, Hans Reichenbach 1958, §34). So we see once again the implicit importance of invariance of theory structure in further developing and sophisticating mechanics, guided by the principle of relativity. (For further discussion, see sections 6.1 and 6.2)

At the level of data, then, we are already soaked in theory. In a more compact formulation, "by measuring we assign an item in a logical space." (*ibid.*, page 172). This space may be some scale appropriate for the magnitude in question, like a weight or temperature scale, or a more complex coordinate system of location or magnitudes. Further, they are always finite and discrete. More importantly, as we saw in the quote, the outcome or result are (theoretical) representations of physical interactions. These can typically be considered as the dots on a graph or some relative frequency – what we would simply call the data. The relevant theory of measurement and the data collected, somewhat misleadingly called *model* of data, constitute a model of a *different* logical type than the model-theoretic type (Suppes 1960, page 253). The data model does not *satisfy*, in the logic-technical sense of the term, the theory of data, but rather represents (or presents) it in a different way.

In contradistinction to the data or measurement part, the theoretical part is ordered modeltheoretically. These models are the mathematical structures we've been studying. Completely abstract and independent of any empirical relations; inter-related by morphisms; often with functions being infinite and/or continuous; satisfying or realizing the corresponding theories (logictechnically.)

How, then, are these two types related to each other? The specifics will vary considerably from different scientific inquiries, and the number of theories related may be arbitrarily large; but this is the general schema: In addition to the lowest data level and highest theoretical level, we have intermediate levels, both theoretical and experimental. We can think of the highest level as the general theory, i.e., the general principles and equations. Below that, the *theory of experiment* defines *surface models*. (The surface models is where theory meet the data from below.) These, the surface models or experimental models, are connected to the higher level models by morphism relationships, as explained earlier. When the high level theory is constrained to the experimental set-up, the theory of experiment is defined and constructed (together with its models). This stage is analagous to providing initial and boundary conditions; conditions constituted by the particularities of the experimental set-up. On our imagined data model from above, the dotted graph, the *surface models* would define certain trajectories relative to a specific frame of reference (while the graph *dots* or the data model will here correspond to the concrete observed positions over time).

Now we can finally connect theory with empirical reality – the surface models will meet the data models from below. Here we use the machinery of *statistical methods* to compare *goodness of*

fit between, say, the dots and the curves (*ibid.*, page 256). Formulated more generally, goodness of fit is compared between the data collected and appropriately represented in data models, and the theoretical-structural expectations, that is, the theory models.

The above paragraph is a simplification, of course, and did in addition portray the process 'from above'. That is, starting from the theoretical level and moving downwards. But the influence runs both ways. It may be that the data as collected cannot simply be represented 'as dots on a graph' ready to be compared with theory, but needs to be further structured and extrapolated before a comparison and evaluation can be made. For example, by going from relative frequencies to probability measures, which in effect is to extrapolate a surface model from a greater number of



[Figure 2. A graphical depiction of the hierarchy of models. The design is by the author. Thanks to Natale Hugvik for the graphical presentation.]

data models (van Fraassen 2008, pages 169-170). The relevant theory guiding the extrapolation would define a class of models, corresponding to empirical structures, which will be sought embedded in higher level theory structures.

It could also be the case that the relevant contiguous, higher level theory still was under construction, or even non-existent. These empirical structures would then both suggest and constrain theory construction at higher levels 'from below'. New technology giving better instruments, improved experimental design (including statistical methods) would also influence this, further constraining the class of models fitting the data. Considering this two-way, or reciprocal, theory construction influence, van Fraassen comments that,

... reflection on the possible forms of structures definable from joint experimental outcomes yields constraints on the general form of the models of the theories 'from below'; that class of models can then be narrowed down by the imposition of postulated general laws, symmetry constraints, and the like, 'from above'. [van Fraassen 1990, page 228].

Formal Vertical Path as Correspondence Principle

It is the existence of a certain formal, *vertical path* through layers of models, from the highest theory to lower data models, which bestows empirical relevance to our scientific theories. (This marks a clear distinction from the syntactic view and its problem ridden correspondence rules.) The formal path is constituted by the similarity connections the models of each two (in the hierarchy) adjacent theories bear to each other; either by mathematical morphism relations when the two theories are of the same, appropriate logical type, or else by measures of statistical fit. Theoretical models which are not connected down to the data level in this way, are just that – abstract, theoretical models with no empirical relevance. On the other hand, the class of theoretical models which do have a formal path down to the data says something about the structure of reality, or our ontology. (These models distinguish *empirical theories*, see section 4.2). Exactly what this formal path, implying structural similarity, says, depends on the kind of theory they are models of. But the essential point here is the nature of this connection and what it connects.

Implications for Empirical Truth

We end with some implications. In science, we cannot assert something about empirical reality without stating it within some theoretical or conceptual framework. Within the semantic view there

is no way of talking about *truth of theories* but through structural match between the models and the empirical structures our best scientists can present us with [footnote: abstract truth]. So we see that the concept of truth and scientific methodology, at least for scientific theories, conflate. There is no way to evaluate (empirical) truth claims but relative to a theory the models of which coheres well with the structures of empirical investigations, i.e., that the claim coheres well with our knowledge acquired by following scientific methodology.

And now we are finally in a position to elaborate on the *identity relation* mentioned initially, namely that a true theory has a model identical to, or at least to a part of, the world. Our scientific theories determines their models only up to isomorphism. No matter how much empirical data structure we attain, or no matter the level of precision of our representations of this structure, our theories will always be underdetermined up to isomorphism. True theories, *in some absolute sense*, and for us to know that they are true in this way, is therefore principally impossible. But this fact gives no reason to despair. There seems to be no upper limit to how sharply our theories can grasp the structure of reality, empirical and instrumental constraints notwithstanding.

This concludes our account of the semantic view. We have seen how the semantic view with its different attitude towards linguistic analysis as such, as compared with the syntactic view, and with its model-theoretic foundation, does not get entangled up in the difficulties pertaining to the limitations from first-order logic and the troublesome language bifurcation. Further, we saw how the requirement of a vertical path through the hierarchy of models is parallel, although superior, to the correspondence rules of the syntactic view. But must importantly, we have a promising framework for an adequate philosophical theory of the structure of scientific theories.

4. What is a Scientific Theory?

Science is the final arbiter of truth. W. V. O. Quine, Two Dogmas of Empiricism

This chapter will function as a bridge connecting the two parts of this thesis, where Part 1 is of a philosophical-historical character concerned with the structure of scientific theories, while Part 2 will be of a more theoretical character concerned with scientific progress and will contain what is my original contribution. In order to tackle the question of scientific progress an account of scientific theories must first be provided. Such an account was sought for in the preceding chapters; here we present our findings. That is the function of this chapter.

The chapter is divided as follows. In the first section I present an overview of our present position in the thesis, both what we have gone through up to now and what lays ahead. In the second section I present explicitly a working concept of a scientific theory, following the semantic view presented in the previous chapter. This is necessary groundwork for what is to follow on scientific progress along the rest of this thesis.

4.1 Overview of our Current Position

Looking back

In chapters 2 and 3 we have looked thoroughly at two distinct conceptions of what a scientific theory is. Although there are in some ways radical differences between them, they can also be seen as two somewhat overlapping programs within the same philosophical tradition, having the same overarching goal of trying to establish and explain the relationship between scientific theories (as such) and empirical reality.

First, in chapter 2, we went through the conception of scientific theories associated with the logical empiricism (or logical positivism), called The Syntactic View or The Received View.

Strongly influenced by contemporary mathematical logic, with its deflationary or reductive stance with respect to *non-linguistic*, abstract entities, like mathematical and theoretical objects, the syntactic view concerned itself heavily with technical analysis of language and with the construction of formal, artificial languages (like first order logic), with a special emphasis on the *syntactical* properties of the languages. A theory was seen as a collection of sentences, the axioms, together with the deductive closure of these axioms, i.e., all sentences logically implied by them. At this juncture, this is a mere logical calculus (with inference rules defined), without connection to the empirical world. In order to connect the logical calculus to the empirical world the logical empiricists, guided by the verification principle, construed correspondence rules, that is, sentences connecting the theoretical vocabulary with an empirical vocabulary, in order to bestow empirical significance on the theory. This was highly problematic, as we saw. Nevertheless, one can argue that this project of reducing philosophical analysis of scientific theories to linguistics and mathematical logic was successful in the important sense of showing the limits of the purely syntactic approach. Its failure demonstrated how philosophy of science is more than philosophy of language.

Then, in chapter 3, we looked at another conception of scientific theories called The Semantic View, or as it is sometimes called, for good obvious reasons, The Model-Theoretic Conception of Scientific Theories. Tarskian model-theory is a discipline within mathematical logic concerned with the classification of structures, and the relationships between theories and models (which are structures satisfying the theory at hand), mathematically speaking. As a form of, or a discipline within, mathematical logic, model-theory distinguishes itself in its focus on *extra-linguistic* entities, that is, mathematical entities, as opposed to the more syntactically focused disciplines. *Not meta-mathematics but mathematics*, as Patrick Suppes declared (paraphrase from van Fraassen 1980, page 65). The semantic view is based on an empirical analogy to or empirical extension of model-theory.

In model-theory a set of mathematical axioms or equations relates to, or 'picks out', a corresponding class of models. The project of the semantic view is to handle scientific theories in the same way. Where the theories in pure mathematics deal with models equally mathematically pure, scientific theories deal with models which, their abstract nature notwithstanding, still bear relationships to empirical phenomena. And the nature of this relationship is of utmost importance for distinguishing the semantic view from the syntactic view. On the linguistic framework of the latter, the theoretical content of a scientific theory were to be linked to the empirical world by connecting their respective *languages* in a formal way. On the other hand, the semantic view considers language to be of secondary importance. Parts of the theory are not *linguistically* connected to experience or experiment, as on the syntactic view. Rather, the theory as a whole is

mathematically evaluated against empirical reality, through the morphism relations explained in chapter 3. This fact removes from the domain of the philosophy of science many conundrums pertaining to technicalities of linguistic analysis. That is a virtue.

I find the model-theoretic approach to scientific theories to be attractive and on the right track. Further, I find it promising as a framework not just for giving an account of what a scientific theory is *per se*, but also as a broader framework in which questions of philosophy of science may be formulated in a way receptive of much constructive work – and maybe even answers.

Looking ahead

For the rest of the thesis we will concern ourselves with issues relevant for thinking about *scientific progress*. It is my contention, or conjecture, that this model-theoretic framework suggests some ways in which we can talk clearly and objectively about scientific progress. Our route to scientific progress is via a theory of scientific progress presented in chapter 5, building on two important concepts that we will develop in sections 5.2.1 and 5.2.2: logical strength and empirical strength. Having these two concepts defined we conclude the presentation by explaining the interaction of the two concepts relative to scientific progress. In chapter 6 we will evaluate if and how we can talk meaningfully about scientific progress by going through two case studies of theory transitions from the history of physics and see what is implied by the theory change relative to our theory of scientific progress stands trial to many challenges from the philosophy of science. Constructing a proper theory of scientific progress would accordingly be of great philosophical value. Chapter 5 can be seen as a prolegomena towards that end.

Before that, however, we will need to state more clearly what our working concept of a scientific theory is. The following section concludes Part 1 of the thesis, being the last component needed in order to begin on Part 2, where we evaluate our concept of a scientific theory against real cases of theory transition relative to a suggested conception of scientific progress.

4.2 Our Working Concept of a Scientific Theory

First we will explicate our notion of *theory* together with the closely associated notion of *models*. This will first be done in a purely theoretical setting before we take empirical considerations into account. Theory and scientific theory will be distinguished in a way analogous to how the logical positivists distinguished between theory and scientific theory: A theory was considered a scientific theory once it was connected to empirical phenomena through correspondence rules. Here, once a theory becomes *empirically relevant*, it will be called a scientific theory. This is a divergence from ordinary use of the concept of a scientific theory. Nevertheless, it does fit our purposes and that is

what counts, as this account is more a suggested theoretical construction than an conceptual analysis of ordinary or paradigmatic use. More on this below.

An explicit definition of *theory* will not be given, but we will instead say something about its role vis-a-vis other closely related concepts. A theory defines a class of models. In order to do this a theory must have some (structural) semantic content in terms of which the relevant class of models can be said to satisfy the requirements given by that content. This is a necessary condition for some set of sentences and mathematical expressions being called a theory. Otherwise the candidate theory is simply too vague to be of any use. For a theory to recognized *as such* (technically speaking) this requirement of defining a class of models is also sufficient. On our account a theory is not to be *identified* with the associated class of models. It is preferable to keep these two entities, theory and class of models, separated due to their different natures and the different ways we use them. Theories describe and *prescribe*, models are and *obey*. The two manifest the distinction between the properties to *describe* and be *described*. Now, if models are, what are they?

As we saw in section 3.2, models are mathematical structures, still it is important to separate the two concepts. What distinguishes them is that models are models correlative to, or co-defined by, a theory (or a set of theories). In the previous paragraph we started with a theory and referred to the class of models defined by it. The converse characterization is also possible, and may sometimes be more appropriate. We could construct or discover (depending on our metaphysical stance on mathematics) some interesting mathematical structures and then, in turn, try to formulate theories having these particular mathematical structures as their class of models. Another important way in which we talk about theories and models where we emphatically cannot identify them is when we find that theories, although being different, still define the same class of models. This feature of theories tells us that they have interesting structural properties in common. The commonality may suggest, for example, that they are different aspects of a larger theory incorporating both. A third consideration for keeping theory and classes of models distinct is the occasional need for talking about hypothetical theories or models, respectively, without invoking its counterpart. We may theorize meaningfully about models of the world in contexts where considerations pertaining to a theory of the world as such would be irrelevant. Maybe we have in mind a theory governing only a part of the world-model (though, of course, being consistent with the hypothetical world-model as a whole.) On the other hand we may talk about a hypothetical theory. As an example, we may reflect on a theory of everything, pondering the hypothetical relations holding between it and General Relativity, say, without considering the hypothetical class of models defined by a (hypothetical, assumed) theory of everything. Consequently, theories and

models are appropriately and accordingly distinguished and separated.

A theory can be logically and mathematically related to other theories. Both these, relations and the related theories, can be interesting to study for purely theoretical purposes. For theories to be scientifically interesting they need to either themselves be amenable to empirical testing or they need to be formally connected to other theories amenable to empirical testing. In both the purely theoretical context and in the scientific one, formally related theories constitute a systematic hierarchical structure. In a corresponding way the classes of models defined by formally related theories constitute *a hierarchy of models*, as we explained in section 3.3. When a theory has models either amenable to comparison with empirical structures or formally connected to models which are amenable to comparison with empirical structures, then the theory at hand has the property of being *empirically relevant*. A theory which has the property of being empirically relevant, or simply put, *is* empirically relevant, we call *a scientific theory*¹⁶. Here we see the analogy mentioned initially in this section: the formal connection between the classes of models in the hierarchy of models for empirically relevant theories, has a similar role on this account of scientific theories as correspondence rules has on the syntactic view.

The nature of these formal inter-theory connections was explained in 3.3 (and see figure on page 47) as consisting of both mathematical morphism relations between the corresponding classes of models, and the different type of relationship, statistical fit, between the surface models (lowest level theory-models in the hierarchy) and data models (empirical structure).

When it comes to assessing the scientific virtue of a scientific theory we will confine ourselves with a measure or desiderata (the only?) that compels the belief that a given theory is a true representation of some domain of the world 'out there'. This is the virtue of a scientific theory being *empirically adequate*. As we recall (see page 44), a theory is empirically adequate when all

¹⁶ The reader may understandably find this condition puzzling. First, is it sufficient for a theory being empirically relevant for it to be a scientific theory? Wouldn't that include too much, i.e., include all kinds of pseudo science, provided only they have some empirical consequences? In that sense our concept would differ in important ways from standard use. But this fear is ungrounded. For one, our concept of a scientific theory is primarily a technical one, constructed for some specific philosophical purposes. Neither is the condition very liberal. The way we have grounded our concept of theory in model-theory, that is, in a way that poses certain mathematical requirements, will exclude most pseudo science gibberish. Nor is the condition too conservative. It is a virtue that our concept of a scientific theories, and old and/or false empirically relevant theories.

Secondly, wouldn't treating empirical relevance as a necessary condition for being a scientific theory exclude theories of which science communities treat as serious scientific theories, although they do not have empirically testable consequences, like string theory? Whether this is in fact a vice or a virtue is itself a controversial scientific and philosophical question. (The physicist Lee Smolin argues powerfully that string theory is indeed unscientific in his *The Trouble with Physics* (2007)). That string theory or any other untestable theory would not be called scientific on my account should not be understood as of any derogatory significance. The theory is simply treated as on a par with other mathematical theories. The motivation for defining scientific theory via empirical relevance is to have a general and objective notion as independent as possible of contingent facts about the beliefs and attitudes of scientists and scientific communities.

the data models of our up-to-date scientific research, that class of empirical structures, are embeddable in the theory's models (equivalently, in the structures of our theory); and in that case the former are called *the empirical substructure of* the latter. The reason why empirical adequacy should compel our belief that a theory is a true representation of empirical reality is the objective coincidence (literally speaking) between structural features of our theoretical models and the structural features of empirical reality (represented by the data models).

Does our criterion of empirical adequacy for commanding adherence to a scientific theory, or to the belief that the theory is a true representation of the world, imply a sort of radical empiricism? No. It rather indicates a form of a sound verificationism in the sense that, to use Quine's dictum, science is the final arbiter of truth. To make my point clearer, we should reformulate: *Experimental* science is the final arbiter of truth. With this qualification and emphasis on experimentation I want to forestall a possible objection that this leads to a view where all that scientifically counts is getting the actual data right and that the underlying theory does not have any value or significance beyond being instrumental at this enterprise. One could argue that science can and does say more than this, e.g., that it explains counterfactual relations and dependencies beyond mere empirical regularities. And further, that this is what experimental science essentially helps to uncover, almost by definition. Experimental design seeks to unravel and establish the complex network of dependencies that is empirical reality, from everyday laboratory testing to impressive particle accelerator collisions. Data models emerging from experimental research, then, already incorporate modal relations going beyond strict empiricism¹⁷.

There is another consideration by way of which our account can be said to go beyond empiricism. Even though our concept of a scientific theory is constructed by use of empirical notions like empirical relevance and empirical adequacy, one can still embed this concept in a stronger metaphysical background framework. One such framework that is here assumed (but will be explained and argued for in the next chapter) and previously mentioned (in section 3.1) is *structural realism*. Given two empirically adequate and empirically *equivalent* theories, that is, both theories having the same empirical substructure, would that imply there is nothing more to be said about the theories *modulo* which describe reality most truthfully? Structural realism goes further. It is not as if the purely abstract component of a theory is independent to and separable from the empirical part. The two parts are mathematically and logically connected. And there is a specific sense in which two equivalent theories must share some common structure in terms of which they have, and beyond their simply having, the same empirical substructure. That structure which they

¹⁷ This is a point I will not argue for. Neither must one hold such a view on modality while adhering to the semantic view. Nevertheless, there are good reasons why one should incorporate modality, thereby going beyond empiricism. For a thorough discussion of this, see chapter 2 in Ladyman and Ross (2007).

have in common beseeches realistic acknowledgment. Importantly, this is true only of that shared structure which is necessarily connected to the shared empirical substructure. In that sense, it is only to the extent to which theory structure can embed empirical structure that they command commitment of any realism. (Now, how to *interpret* this theory structure metaphysically or ontologically is a question outside the scope of this thesis.)

After having summed up the two philosophical views on the structure of scientific theories in the previous chapters and formulated our working concept of a scientific theory, we are now ready for the second part of this thesis. The second part includes accounting for the problem of scientific progress, proposing a theory for scientific progress, and lastly, applying this theory to two examples of theory transition from the history of physics.

Part 2

Scientific Progress

5. A Theory of Scientific Progress

There was continuity or accumulation in the shift, but the continuity is one of form or structure, not of content. John Worrall, Structural Realism – The Best of Both Worlds?

It seems that the principle of the shortest code length is about as fundamental and objective as any criterion can be. J. Rissanen, Complexity of Models.

In this chapter the theory of scientific progress will be presented. This will be done in section 5.2, where we will first explicate our two concepts of a theory's strength, logical strength and empirical strength (5.2.1 and 5.2.2, respectively). After that, in 5.2.3, we will explain and account for scientific progress in terms of the interaction of these strength concepts.

But before we can take on scientific progress we need to turn to a problem for the notion of theoretical continuity over transitions in the history of science. The reason why we need to account for this before giving our theory is twofold. First, our theory must overcome and transgress, and can be seen partly as a response to, this problem. Second, the problem of theoretical continuity motivates the philosophical thesis of structural realism as solution. The solution of which is a precondition to the theory of scientific progress to be given, and a thesis which is an essential building block for the same theory. The first section will accordingly deal with this problem and the proposed solution.

5.1 The Problem of Scientific Progress and Structural Realism

To explain the more or less obvious progress of science while keeping an attitude of realism has proved to be surprisingly difficult. Surprising because, before one encounter the problems pertaining to realism and progress, most of us have an impression of science as a more or less steady cumulative enterprise. Not just in engineering aspects and technology, but also of a lasting, continuous growth of knowledge about how the world *really is*. Although the development is not without some transitions, for this naive view this does not pose a challenge. Our scientific theories

presumably answer questions such as: How does the world work? What is it made up of, what are nature's fundamental constituents? Further, these descriptions, given by our scientific theories, are getting better and better at describing the ontological inventory of the reality wherein we reside. And these scientific theories are characterized as being at least approximately true.

Now, philosophically there has been a severe tension between two approaches to describing the enterprise of science. On the one hand we have the systematic approach in the philosophy of science, where the study is of how our contemporary scientific theories, or how scientific theories in general, relate to empirical reality. This approach has epistemological concerns as its central aim. Logical positivism with its syntactic view and its successor the semantic view are instances of this approach. The other approach, with the works of Thomas Kuhn as the paradigm example, is characterized by its emphasis on the historical study of the scientific enterprise, with a particular focus on scientific revolutions. The trouble with combining the two approaches has generated much of the debate pertaining to the philosophy of science for the latter half of the last century.

Any successful philosophy of science must be able to account for scientific revolutions, or *radical theory change*, in a satisfactory way. The problem of radical theory change can be characterized as follows. How can we explain both the success of science, demonstrated by our cumulative ability to predict and explain phenomena, and the fact that our theories change from time to time, sometimes in radical ways? Especially, can we explain this seeming paradox without having to abandon realism?

The realism in question here is scientific realism. Scientific realism is the view that our scientific theories are essentially correct. Not just in getting the empirical facts right, or saving the phenomena, but that they correctly describe how the world is. It asserts that "the *nature* of things is correctly described by the metaphysical and physical content of our best theories." (Ladyman and Ross 2007, page 93). This could be described as a full-blown scientific realism. A more modest view is that we ought to believe that our best current scientific theories are *approximately true*. Scientific realism also claims that the central theoretical terms of our current best theories successfully refer to the unobservable entities they posit (*ibid.*, page 68).

The No-Miracle Argument

Scientific realism has a very strong argument supporting it. This is called the *no-miracle argument*. If our scientific theories were not true descriptions of the world, i.e., if they were false, how could they still be so successful *as scientific theories*? In the words of Hillary Putnam: "The positive argument for [scientific] realism is that it is the only philosophy that doesn't make the success of science a miracle."(*ibid.*, page 69). This argument is an inference to the best explanation. And as

such it has both local and global versions. If we look at a particular theory and consider its success in explaining and predicting phenomena, scientific realism is used as the *explanans* for the success. It is in terms of the scientific theory being an approximately true description of our world that the theory is so successful.

An especially impressive form of prediction is the theoretical prediction of novel phenomena. Newton used the irregularities (relative to his theory) of Uranus's observed trajectory to predict the existence of a hitherto unknown planet, and also inferred exactly where it could be found. Astronomers pointed their telescopes where Newton's deductions dictated, and there was Neptune. How could this be the case if Newton's theory were not true? This is an instance of the local version. The global version looks at the scientific enterprise as a whole, marvels at its long list of achievements, and asserts that the best explanation for this long successful career is that scientific theories are approximately true.

The Challenge From History

Enter the skeptic. Or rather, the instrumentalist or anti-realist. They claim the history of science shows this view to be untenable. Superficially, the realist may get away with certain instances of significant theory change. There are intuitive senses of how Newton's theory 'approximates' the theory of Einstein. The development of science has been essentially cumulative and that "the deposed theories themselves, *and not just their successful empirical consequences*, have generally lived on, albeit in a 'modified form', after the 'revolution.'" (Worrall 1989, page 105). This attempt at securing scientific realism stands on shaky grounds. It relies on vague notions like 'approximate' and 'essentially', and on 'maturity' (as on the post-hoc maneuver of trying to differentiate mature scientific theories from precisely the ones that are problematic.) Nevertheless, the vagueness of the notions notwithstanding, the problem goes deeper.

There are theory transitions in the history of science where the content of the succeeding theory represents *a radical break* with the preceding theory, and cannot be said to approximate it. If theory T is approximately true and theories T and T' are radically at odds with each other on the theoretical level, then how can T be said to approximate T'? At the theoretical and ontological level there are radical shifts from Newton's theory to Einstein's. In Newton's theory space is infinite and Euclidean, time and simultaneity are absolute, and the inertial mass of a body is constant. While on Einstein's theory space is finite (though unbounded), time and simultaneity are not generally absolute, and the mass of a body increases with its velocity (ibid., page 109). At the theoretical level, then, it seems nothing "essential" is preserved.

The radical shift in ontology is especially vivid in the history of optics. According to

Newtonian optics light consists of rectilinear streams of light particles obeying geometrical rules. After the 18th Century light was most certainly not considered as particles but as waves, according to Fresnel's wave theory of light. These waves propagate through an all evasive mechanical medium called the Ether. This was the theory Maxwell perfected; the electromagnetic field theory, containing the four fundamental equations bearing his name. He tried in vain to base these field properties in the properties of the Ether. Then came Einstein's electrodynamics and the Ether was exorcised. Not only did the medium disappear, now light was again seen as particles, as photons. The empirical facts of particle behavior did not suddenly disappear (of course), so through the development of quantum mechanics, light was seen as some strange synthesis between particle, a discrete entity, and 'a smeared out object' having wave properties. How could we reconcile the view that scientific theories describe how the world is, in terms that they are approximately true, with these instances of radical and fundamental theory change?

The Pessimistic Meta-Induction

This challenge to scientific realism is known as *the pessimistic meta-induction*. Through the history of science we have believed several successful theories to be (approximately) true that subsequently were shown to be false. Especially, their theoretical terms do not refer to anything real, according to later theories. But our contemporary scientific theories do not differ in kind from these earlier theories. Then, by induction, we have no good reasons for believing that the current best theories will fare any better. Consequently, we should not believe in scientific theories' approximate truth. The attempts of trying to qualify or distinguish certain theories by maturity and so forth have not been successful (*ibid.*, page 113). As we mentioned, these notions are too vague and their applications seem entirely post-hoc. To state the argument against the no-miracle argument precisely:

P1) Successful reference of its central theoretical terms is a necessary condition for the approximate truth of a theory.

P2) There are examples of theories that were mature and had novel predictive success but whose central theoretical terms do not refer.

Then, from P2 and modus tollens by P1, we get,

C1) So there are examples of theories that were mature and had novel predictive success but which are not approximately true.

And what simply follows from P2,

C2) Approximate truth and successful reference of central theoretical terms is not a necessary condition for the novel-predictive success of scientific theories. [Ladyman and Ross 2007, page 84.]

The no-miracle argument is undermined. We see that a scientific theory's ability to predict novel phenomena does not presuppose approximate truth and successful reference of its central theoretical terms. The latter is then clearly not the best explanation of the former. Are we then doomed to be radical empiricists or instrumentalists?

Structural Realism

John Worrall, in his article *Structural Realism – The Best of Both Worlds*? (1989), set out to establish a way of retaining a kind of realism, thereby not making the success of science a miracle, yet being able to account for, or explain, radical theory change. Is there an account of science that manages to overcome this difficulty and 'have the best of both worlds'? Structural realism is supposedly, and arguably, such a position. In the words of Worrall,

There was an important element of continuity in the shift from Fresnel to Maxwell – and this was much more than a simple question of carrying over the successful empirical content into the new theory. At the same time it was rather less than a carrying over of the full theoretical mechanisms (even in the 'approximate' form) ... There was continuity or accumulation in the shift, but the continuity is one of form or structure, not of content. [1989, page 117]

The important clue here for what kind of view structural realism is is the emphasis on *form* or *structure* over *content*. The content of a scientific theory, meaning its description of how the world is constituted, its ontology, may be changed to the unrecognizable in a succeeding theory. Nevertheless there can be similarities in *the form* of the theories, retention of some structural features they have in common¹⁸. Much philosophy of science's almost exclusive focus on the meaning and reference of the theory's theoretical terms has occluded what are truly the relevant features of theory change.

Imagine a big tree with many branches filled with green leaves. Although the leaves may

¹⁸ The *form* of a theory is not something contrary to or fully independent of its content. Retention of form also means partly retention of content, only content at a less superficial level, i.e., the relational or structural aspect of the content.

change form and color from spring to fall, the trunk and the branches look the same. Well actually, they may grow some from one year to the next but always retaining the overall form. Borrowing a paleontological term, the trunk and the branches have grown homologically, meaning they grow continuously without abrupt change from the time it breaches the ground to the day it dies. In describing the tree as such would the varying size and color of the leaves be of primary importance? This would be to fail to to see the tree for its leaves. In the same way, to explain or account for our theories and their development, the structural properties are more important than their particular inventory of theoretical terms.

What is structural realism? Worrall traces the origins of this view back to Henry Poincaré and Bertrand Russell, though arguing that the first was less a *conventionalist* than is often assumed. Van Fraassen adds Rudolf Carnap to this list (van Fraassen 2008, chapter 10). There are differing accounts but some features are common for them.

Intuitively, the structural features of a scientific theory are to be contrasted with its ontology, where structure is understood broadly as the relations between elements. In the mathematical sciences, structure is often described as the relations that are captured in the theory's equations. So, rather than being committed to the existence of electrons (the ontology of the theory), the structural realist is committed to the relations between electronic phenomena [or real patterns, see 5.2.2] that are described by Maxwell's relations, *and arguably those relations are preserved in the move from classical electrodynamics to quantum electrodynamics*. [Bokulich and Bokulich et.al. 2011, page xi, *emphasis added*.]

Worrall's important achievement for our concerns is using this view to overcome the problem of scientific progress.

But first let us take a closer look at this structuralist view. It distinguishes itself by placing an emphasis on structural, mathematical relations over ontology, at least ontology as it is traditionally conceived. The nature or the constitution of the things that exist are of minor importance relative to the structural properties underlying, or manifesting themselves through, our epochally changing ontological inventory *of things that exist*. What matter are the interrelationships systems of objects show forth or even constitute. Focusing on a thing's nature, or simply on what we call it, is to focus on something transient, if not illusory. *A rose by any other name would smell as sweet*, as says Juliet. The converse is as commonplace in science. The meaning of 'atom' changes as the theoretical framework changes, and today's meaning is something completely different from the time of Democritus. The same can be said of the electron – what matter are the "*relations between electronic phenomena*." And although phenomena *as registered and formulated* by science may vary over time and over technological advance, their relations are remarkably invariant.

Retention of Structure Over Radical Theory Change

To take a concrete example, let us again turn to the history of optics. Here we saw the ontology going through the most radical of changes. The type of thing light was went from particle to wave and back to particle again before metamorphosing into some kind of synthesis between the two. The background ontology changed no less. An all-permeating mechanical substance, the ether, went from existence to non-existence. Was there anything cumulative or invariant over all this change? Yes: The mathematical relationships in each theory stage. Maxwell equations in electrodynamics, the fundamental equations for that discipline of science, are identical both pre- and post-ether (Worrall 1989, page 119). A laser beam, although its objects are now taken as quantum wave-particles, still obeys Newton's laws of geometrical optics. The same goes for the mathematics of the wave phenomena. It is in the attitude of structural realism to regard the structural-mathematical level of analysis as the real and profound one, while the traditional 'thing ontology' is something superficial. At this structural level conspicuously much invariance presents itself over scientific revolutions.

The cumulative aspect, in addition to the invariant aspect, is indicated mathematically through asymptotic relationships between succeeding theories in physics. We will extend and substantiate this cumulative notion model-theoretically below (see section 6.3, especially pages 92-93). The new theory 'reduces' to the previous theory when we consider limit values of certain physical parameters. When this is the case for two theories we say they obey a *correspondence principle*.

We will briefly explain two such instances. The special theory of relativity (STR) asymptotically approaches Newtonian mechanics (NM) in the following way. In describing and analyzing any system of objects we need to use a frame of reference. But all frames of reference describe the same physics (Galilean relativity principle), provided we use some specified set of rules or schema transforming one frame of reference to another. In STR this transformation schema is dependent on the relative velocity in a way fundamentally different from NM – especially, the time coordinate is frame-dependent on STR while not on NM. Nevertheless, if we let the relative velocity v between two frames of reference approach zero, the transformation schema, and consequently most of the physics, become asymptotically identical. And their time coordinates coincide and become identical (congruent).

The second instance concerns quantum mechanics and classical statistical mechanics. Quantum phenomena display themselves when we observe small systems on a small scale, i.e., systems of a (relatively) small number of 'particles' *N*. At this level the two theories are radically and fundamentally different. Particles do not have a specific location or velocity. The number of particles may even vary. Still, if we let *N*, the number of particles, increase, the mathematical formalism asymptotically approaches that of classical statistical mechanics (Ladyman and Ross 2007, page 95). As with relativity theory, the two sets of theories become *empirically indistinguishable*, i.e., empirically equivalent *at certain segments of the relevant scope and resolution*. (More on this below and in the next chapter.)

Structural Realism as the Solution to the Problem of Progress

We can now outline why structural realism solves the problem of scientific progress. That is, how this view does not make the success of science a miracle and how it can explain, or at least be consistent with, radical theory change. First the no-miracle argument. According to structural realism our scientific theories are successful because these theories describe the *structural properties and relations* of empirical reality. More concretely, successful reference of central theoretical terms does not matter inasmuch as the theoretical role the object terms have makes sure the *theory as a whole* is empirically adequate (or the systematic relationships they stand in, relative to other object terms, in the broader theoretical framework.) The emphasis on successful reference of theoretical terms is akin to the expected role of the correspondence rules on the syntactic view as given in chapter 2 (see section 2.2 on correspondence rules). And similarly, how the alternative structural way of emphasis is reminiscent of the semantic view from chapter 3. (We will return to the latter connection directly below.)

Instead of talking vaguely about theories being 'approximately true', we would on the semantic view say our scientific theories latches onto empirical reality by being *partially isomorphic* to it (see section 3.3). By substituting approximate truth with partial isomorphism to empirical reality the pessimistic meta-induction argument is undermined. By the retention of successful theory structure, i.e., preserving theory structure isomorphic to empirical reality, radical theory change does not prompt the abandonment of realism. Rather, the continuity and cumulativity of the structural aspect of scientific theories could be said to once again force the acceptance of realism by a resurgence of the no-miracle argument.

Now, structural realism is a version of philosophical realism, a metaphysical thesis. And the semantic view of scientific theories, as accounted for in chapter 3, is a broader philosophical framework in which structural realism fits aptly. Structural realism highlights the mathematical-structural aspects of scientific theories as the important ones, and the semantic view builds its framework on and around the mathematical notion of a structure. This sameness of focus or

emphasis makes them highly commensurable. And as we have seen, structural realism shows great promise as a philosophical view of science with respect to overcoming the problem of the pessimistic meta-induction. It is within this pair of philosophical views, the semantic view and structural realism, that we will present our theory of scientific progress.

5.2 Our Theory of Scientific Progress

This section, giving our theory of scientific progress, is divided into three subsections. The first two gives the strength concepts to be used in the third part, where we provide our final theory. In each of the two first subsections the definitions are not given immediately. The theoretical content of each concept must first be motivated and thoroughly explained along with their construction.

5.2.1 Logical Strength

Our concept of logical strength (to be given) is an attempt to cash out our intuitive notion of how strong a theory is. It makes sense saying that theories can differ in how strong they are (or maybe, rather, how *strict* they are.) As a first approximation we can look at how this measure is used in mathematical and propositional logic. A theory is here seen as a set or collection of axioms or sentences, with their deductive closure. A straight forward candidate measure is the number of independent axioms. The more axioms, the stronger the theory. But as this measure says nothing about the content of the axioms, we will reject it as superficial and insufficient as to explicate or adequately represent the theoretical content.

When we take the semantic or model-theoretic perspective another candidate presents itself. Corresponding to a set of (non-contradictory) axioms viewed as a theory T within mathematical logic, a class of models M is defined (although not thereby being explicitly given.) It follows immediately that by adding an (independent) axiom to this theory, the new theory being called T', some model in the set M is no longer a model for T'. The size of the set of models defined by a theory, or the cardinality of the set (a measure of a set's size in set theory), is then inversely related to some particular property of the theory. Let us call this property *logical strength*.

Above we discarded the number of axioms being the measure of a theory's strength. There is an interesting relation between this property of a theory, logical strength, and the size of the class of models defined by it, but this property is not given by the number of axioms. In accordance with the semantic view we reject the latter measure due to the language-specificity pertaining to a particular formalization determining the number of axioms (see sections 3.1 and 3.2). Formalized differently the number of axioms may vary. Consequently, in accordance with the pronounced languageindependence of the semantic view, it is something other than the number of axioms that determines this relation (theory's strength/size of class of models). We will now try to further characterize our concept of logical strength.

Karl Popper's Notions of Range and Logical Content

We have seen that within our semantic framework, beyond the confines of mathematical logic, a theory is something else and more than a set of axioms. But a theory is neither a set of propositions on this account. Nevertheless, a model-theoretic approach to propositional semantics (or logic) displays an analogous concept or measure of logical strength worth taking a look at. Karl Popper, in his classic *The Logic of Scientific Discovery*, introduces a concept he calls 'range', defined in this way: "The class of basic statements permitted by a statement *p* may be called its 'range'. The 'range' which a statement allows to reality is, as it were, the amount of 'free play' (or the degree of freedom) which it allows to reality." (Popper 2002, page 108).

As we see, Popper deals with *propositions*. Basic statements are observational reports as propositions. The truth maker is here regarded as the constitution of reality (the set of all facts, or something similar). Let p be *the trajectory of the Earth around the Sun is an ellipse*. The range of p includes the (hypothetical) basic statement q stating *the trajectory of the Earth around the Sun is a circle*. If we instead regard the range of q, then the converse does not hold, i.e., *the trajectory of the Earth around the Sun is a circle* is not compatible with *the trajectory of the Earth around the Sun is a ellipse* since any ellipse is not a circle while a circle is an ellipse. q is then not included in the range of p. It then follows that the range of p is greater than the range of q. q is in a sense a *stronger* statement than p (ibid., page 106).

In propositional logic a theory is a set of propositions, or the conjunction of them. To any given theory, in Popper's sense, corresponds a set of (potential) falsifiers, that is, observation reports contradicting the theory. Let the size of this set of potential falsifiers be denoted as the *logical content*¹⁹ of the theory. Consequently, the stronger the theory, the less range it has. And symmetrically, the stronger the theory, the more possible falsifiers it has. We see that 'range' and logical content are inversely related. So we define, as does Popper, our *logical strength* to be the inverse of range. Popper's logical content and our logical strength are then analogous.

¹⁹ Popper defines first *empirical content* of a proposition *p* as the class of its potensial falsifiers, then he defines *logical content* as the concequence class of *p*. (Popper 1959, page 103).

Our Versions of Popper's Notions

Let us then try to formulate Popper's concepts model-theoretically. Range could then be: *The class* of models permitted by theory T may be called its 'range'. The 'range' which a theory allows to reality is, as it were, the amount of 'free play' (or degree of freedom) which it allows to reality. And logical strength is taken as its inverse also on this model-theoretic reading. Remember that on Popper's version reality, interpreted as something like the set of all facts, is the truth maker. But on our version of the semantic view there is no way to refer directly to *reality*, our counterpart would have to be specified model-theoretically, combined with empirical constraints (see below, and also section 3.3). Also, the first clause, "the class of models permitted by theory may be called its 'range'' does not work well model-theoretically, since a theory simply *defines* a class of models irrespective of the logical strength of the theory. And we do not want our concept of logical strength be the number of models defined by a theory *simpliciter*. Range, then, must be relativized to something extrinsic to the theory. This something will be some specified (class of) empirical structure.

Popper, as we saw, relativized his notion of range to reality *qua* truthmaker. Our counterpart will be to demand of a scientific theory that it is *empirically adequate*, relative to the relevant empirical structure. Consequently we would get: *The class of models permitted by an empirically adequate theory may be called its 'range'. The 'range' which a theory allows to (a part of) empirical reality is, as it were, the amount of 'free play' (or degree of freedom) which it allows to (a part of) empirical reality. Our concept of a scientific theory's logical strength will be related to the inverse of this notion of range. But we also want our strength concept to be comparative, that is, a relational property between two theories <i>T* and *T'*. Therefore we require that the empirical structure relative to which we will compare the theories' respective logical strengths, *is shared by both theories* as empirical substructures. The relevant empirical structure is embeddable in both theories.

Our Definition of Logical Strength We finally get:

Definition 1: A scientific theory T' is logically stronger than another scientific theory T, relativized to some shared empirical structure, if and only if T' permits less models than does T. When this holds for T' relative to T, we denote this as $T \leq_L T'$.

The relativization to shared empirical structure is essential to isolate what is meant by *logical* strength in contradistinction to *empirical* strength (the latter to be explicated below). This

relativization does not imply that the theories so compared must be empirically equivalent – the theories may just as well be empirically non-equivalent, relative to any given empirical structure. However, what is implied, and in itself important to notice, is that logical strength thus defined is relative to some specified set of structures and not an intrinsic property of the theory as such.

An interesting special case must briefly be discussed. Another way we could have defined 'logically stronger than' would be to require that *all* the models of one of the theories be a proper subset of the models of the other theory. Consequently, the first theory has strictly fewer models than the second theory, thus it would be regarded as the logically stronger theory. This is a special case of our concept, since, if we let the relativizing class of structures be the (empirical) models of one of the theories, then that theory is the logically stronger one, according to our definition. The reason why we did not choose this definition is precisely its lesser degree of generality. Further, if we translate our concept into this way of formulation, we would say that some *subset* of theory *Ts* (empirical) models is a *proper subset* of some *subset* of theory *T's* (empirical) models. So we see that not only is our concept more general, in not restricting comparison to all and only the models of one of the theories compared, but our formulation is arguably also simpler.

To proceed we need to clarify what is here meant by empirical reality. That is a step which leads naturally to our next strength concept, empirical strength.

5.2.2 Empirical Strength

Our informational source to reality, or to empirical reality, is experimental science. Through careful experimental design, and theories of experiment and measurement, science generates data models of empirical reality (see section 3.3). These models are sometimes called *surface* models, adequately highlighting the edge where theoretical abstraction meets raw empirical stimuli or impingement. These data models are some times the correctives for existent higher level theories, other times inspirational hints for theories yet to be discovered and constructed. The measure of logical strength is attributed to these higher level theories. But we equally need a measure on the content and the quality, in some sense *the strength*, of these data models, or empirical structure.

We will describe our measure of empirical strength in objective terms. Evidently our ability to gather in and articulate empirical structure of increased strength (alternatively, content) is closely tied to technological constraints. As technology progresses, so does our ability to make measurements increase, both in scope and in fineness of grain. One could plausibly argue that given our universe's temporal arrow and gradient of technology, empirical strength increases almost by necessity. This is indeed a lucky fact for science, and the highly exponential rate of technological development has made the topic of the limits of science practically relevant²⁰. For our concern, which is how to describe progress in science objectively, we need some way to measure the comparative empirical strength of different sets of empirical structure. This measure must grasp several moments and combine them adequately: the amount of data collected, the scope of the measurement (width), and the resolution of the measurement (depth), together with a non-redundancy measure, to be explained below.

Dimensionality and Data Content

In chapter 3 (pages 45-46) we followed van Fraassen and characterized measurement as assigning an item in a logical space. The outcome of a measurement provides a representation of the entity measured by displaying some (physical) parameters that characterize that object relative to the theory of measurement. Further, data collected by measurements are necessarily discrete and finite, making them practically quantifiable. The logical space wherein the measurement's outcome is determined has a dimensionality. Dimensionality is the number of parameters needed to pick out an element in the data set. This could be a point in an *n*-dimensional geometry. The dimensionality would then simply be *n*. Or imagine a weather report giving today's different temperatures at different locations, say, the capital cities of Europe. Here we assign (attribute temperature) to different items (Europe's capitals). The logical space is here simply the cross product between the set of capitals and temperature (an appropriate section of the real numbers). This *data set* expressed formally (a simple *empirical structure*) has dimensionality 2. A measurement element, say, 23 degrees Celsius in Oslo, is given by two parameters: (*city, temperature*).

We could expand the logical space in the following way. Imagine a grid or coordinate system laid out on the map of Europe and that every city measured its temperature (at some arbitrary time. The temporal dimension not to be included – yet). This data set would have dimensionality 3: two spatial parameters (longitude and latitude) and temperature assigned to each city. Expanding further, we could bring in measurements of pressure, precipitation, wind strength and direction, and we could measure all these parameters over time (yet another parameter.) When viewing the moving picture of the weather forecast over the weekend to come we are observing a data set with at least dimensionality 4: two spatial parameters, one temporal, and, say, pressure or temperature. This, I hope, has illustrated what is dimensionality.

Data content given by measurement can then in general be partly quantified in terms of the product of the dimensionality D with the number of objects N measured, like the capitals above, or

²⁰ Some authors have called this technological development 'the law of accelerated returns'. See for instance Ray Kurzweil 2006, *The Singularity is Near*, chapter 2.

with the parameter scales used (like a grid or coordinate system) – depending on the measuring context. The parameter scale used factor in both scope s and resolution r; that is, every given 'dimension' (parameter scale) has a certain scope and resolution. Call this product d.

Scope and Resolution, and Redundant Information

Technological advances of measurement apparatus increase both the scope and the resolution of the corresponding logical space. Scope and resolution, like the reach of our telescopes in astronomy, and the level of detail visible in cells through the microscopes in microbiology, are involved in partly determining the measure of empirical strength of empirical structures, as we said above. Now we need to explain why d is not the measure we look for. There is a sense in which increases in scope or resolution need not be relevant to a measure of empirical strength. Altitudinal measures of the moon's craters should not increase the empirical strength of the data models relative to a theory of celestial mechanics. This *information* is *redundant*. More obviously, neither should an arbitrary increase in the dimensionality – adding in color as a dimension is also redundant in celestial mechanics.

How can we adjudicate what information is to be considered redundant? Can we minimize *d* while not loosing valuable information? The question of dimensionality is (relatively) straight forward. The (measurable) parameters in the data models should correspond to the number of parameters used in the higher theory. It is somewhat trickier with scope and resolution. How do we proceed to establish the most adequate compromise between increasing scope and resolution on the one hand, and *relevant* information on the other? And can this be done in a general and objective way? It seems it can.

Real Patterns and Algorithmic Compressibility

Keeping our metaphysical presuppositions at a minimum, and continuing within a structural realist approach, we can say without much controversy that what measurements generally are intended to discover or uncover are *patterns*. In explaining what is meant here by patterns we will make use of Ladyman and Ross' (2007) information-theoretic elaboration on Daniel Dennett's account of 'patternhood' in his *Real Patterns* (1991).

In recording data we search for informative data that is often enmeshed in uninformative data (or noise). This problem is identical to the problem of extracting a signal from noise in information and communication theory, and we can therefore attack it using the same methods. Intuitively, a signal is meaningful while noise is just randomness. Claude E. Shannon, an American mathematician and engineer – known as "the father of information theory" –, devised a

mathematical theory accomplishing the separation of signal from noise (Floridi 2010, chapter 3). What is informative, or meaningful in this context, is what is not (mathematically) random. While this technique was designed for dynamical signal detection in radio communication, it applies also to abstract, static mathematical objects like data sets or models. In the latter case *algorithmic compression* techniques is used to eliminate what is random instead of recourse to probabilities, as in the former (Zurek 1990, page 74.) By algorithmically compressing data models, that is, encoding information using fewer bits than in the original representation, two important ends are achieved: 1) We get rid of uninformative noise in the data; 2) We get an objective measure for the information content called the *Kolmogorov complexity* (also called *logical depth*).

Let us take a step back to patterns and measurements, and connect these with the notions from the above paragraph and to the problem of adequate scope and resolution. Patterns, then, is the subject of measurement. Patterns are to the experimental scientist, or the philosopher of science, what signals are to the communication theorist. We will let the patterns experimental science seeks to uncover constrain our data models by the following two criteria for being a *real pattern*, following Ladyman and Ross (2007, page 233):

A pattern *p* (any function from data to a model of data) is *real* iff

- 1. it is projectible;
- 2. it has a model that carries information about at least a pattern P in an encoding that has an algorithmic compressibility higher than the bit-map encoding of P^{2l} , and where P is not projectible by a physically possible device²² computing information about another real pattern of higher algorithmic compressibility than p.

This definition needs some clarification. The first criterion establishes an objective modality intrinsic to the pattern at hand: A projectible pattern support counterfactuals. This excludes mere coincidental regularities. *The target has been located*. The second is an implementation of Occam's razor, which secures that the selection of *scope* and the *resolution* is thorough enough to include all relevant (real pattern) information, *but only to that extent*, preventing the inclusion of redundant

²¹ A bit-map encoding is simply the copy of every information bit from source *C* to representation *C'*. For example, a bit-map encoding of a picture would be the copy of every pixel, even within larger patches with the same color. A representation with some degree of compression would instead copy whole same colored patches as to reduce the total amount of data (simply a description of the area and the color code), since the copy of every equi-colored pixel in an equi-colored area would be redundant data.

²² A simple projection could be to make a regression analysis over some data points and extrapolate with the given best mathematical function describing the relevant data pattern. Any projection is a form of *computation*, and as such a *projectible* pattern needs a computer to be projected. The qualification 'physically possible device' is made to exclude restrictions coming from our here-and-now technological capabilities, leaving the only relevant constraints be the more abstract and general ones from mathematics and physics. (See Ladyman and Ross 2007, page 208.)
information. Also, although not relevant to our concerns but still interesting, the second clause restricts ontological commitment to only that which is *required* for an empirically adequate science *(ibid.*, pages 233-234).

The Strength Measure on Empirical Structure

Now we will relate the constraints on the data, defined through real patternhood, to the objective measure for empirical strength. We will henceforth postulate that our actual data models (empirical structure) are constrained by real patternhood, or something very similar to it, being the result of an actually existing pragmatic heuristic, which partly defines and constitutes experimental science. This fixes the appropriate size of data models in a non-arbitrary way, which we will now explain how to quantify.

Given the fact that data collected by measurements are finite and discrete, they are quantifiable. Data sets for measurements have dimensionality D, depending on the theoretical context. Our measurements are directed at a certain number N of objects (or patterns). Depending on our technological situation, the scope s and the resolution r may vary arbitrarily within a certain interval (we do not have the appropriate scope and resolution at this juncture). The content of a data set d is directly related to s and r. We then propose a formula for the measure of the (as yet uncompressed) data content of an arbitrary data set, d:

$$d \approx N \times D \times [s_1 r_1 (D_1) \times s_2 r_2 (D_2) \dots \times s_n r_n (D_n)]$$

The content of the brackets is the product of the scope and resolution pertaining to each dimension (parameter).

To compress the data in order to remove redundant information, thereby fixing the appropriate scope and resolution, and restricting our targets of research to modal patterns, we apply the *real pattern constraints*. First, N is now devoid of coincidental correlation patterns. Second, by algorithmically compressing our data sets, variable with respect to scope and resolution, we end up with the appropriate data set. We call this algorithmically compressed data set, AC(d). It is a straightforward procedure to apply this compression to tensors (a tensor can be represented as a multi-dimensional array of numerical values). The procedure is defined for *strings* of symbols. An array or vector is a string of numbers. A higher dimensional tensor can be represented simply as a set of greater dimensionality of strings which are equally compressible (Zurek 1990, page 78.) Finally we can get a measure of the data set, the *Kolmogorov complexity K*, given as the set-theoretic "length" of AC(d):

$$K(d) = |AC(d)|^{23}$$

We now translate this back to our semantic view framework. Henceforth when we talk about empirical structure we mean data models constructed and compressed in this way. Empirical strength is then defined as follows:

Definition2: Empirical strength of a theory T is the Kolmogorov complexity of the theory's empirical substructure.

Recall, the empirical substructure of a theory T is the empirical structure embeddable in the theory's models. A quantifiable strength notion is then adequately defined for each scientific theory.

It is often desirable to compare two theories relative to, or in terms of, their empirical strength, when the empirical substructures of the theories are model-theoretically related. This could be when there is some overlap in the data models, e.g., if the empirical substructure of one theory is a *proper subset* of the empirical substructure of another. This relation is given as a theorem, with a useful corollary, inspired by Van Fraassen:

Theorem 2^{24} : *If*, given two theories *T* and *T'*, for every model *M* of *T* there is a model *M'* of *T'* such that all empirical substructures of *M* are isomorphic to empirical substructures of *M'*, then *T* has empirical strength of equal or stronger degree than does *T'*. We denote this as $T' \leq =_E T$.

From this it follows that:

Corollary 2: If the above holds, i.e., *T* is empirically at least as strong as *T'*, but the converse does not hold, i.e., *T'* is not empirically at least as strong as *T*, then *T* is strictly empirically stronger than

²³ The algorithmically compressed data set is then a minimally redundant data set of the real pattern information. The *Kolmogorov* complexity, defined as the length of this data set, is the number of bits required in the shortest possible description of the informational content. "Algorithmic information content of a binary sequence *s* is defined as the size of the minimal program ... which computes *s* on the universal computer" (Zurek 1990, p 76).

A qualifying remark may here be wanted. Any inherent feature of a description is necessarily dependent on the langauge used. Nevertheless, there is an invariance theorem stating that the *Kolmogorov* complexity of any data compression is nearly always insignificantly dependent on the specific language used. The numerical contribution of the specific langauge used to the final value is just an additive constant, while the compressed description length is usually of a much higher order. (See Zurek 1990, p 76; en.wikipedia.org/wiki/Kolmogorov_complexity (01.08.13)).

²⁴ This theorem is essentially identical to a theorem given in Van Fraassen (1990, p 67.) The original formulation is as follows: "If for every model *M* of *T* there is a model *M'* of *T'* such that all empirical substructures of *M* are isomorphic to empirical substructures of *M'*, then *T* is *empirically at least as strong as T'*." An important difference being that Van Fraassen only gives a relative, model-theoretic ordering of strength, not any *absolute* value as given by our *Kolmogorov* complexity. (See definition2.)

Proof: Let *R* be any partial ordering relation on a set, an antisymmetric relation (and reflexive and transitive) $A \leq_R B$, where *A* is either equal to *B* or *A* is strictly ordered relative to *B*. If the converse relation does hold, i.e., $B \leq_R A$, then A = B. If the converse does not hold, i.e., it is not the case that $B \leq_R A$, then it follows that *B* is strictly 'greater than' *A*, that is, $A <_R B$.(A strict partial ordering is irreflexive, transitive and antisymmetric.) (Suppose 1957, § 10.3)

To complete our theory of scientific progress we must now combine these two strength concepts in the requisite way.

5.2.3 Empirical Minimality – Logical and Empirical Strength Interaction

Above in the first section we found an objective, that is, mathematically demonstrable, criterion for a theory being logically stronger than another theory. A transitive relation given by: *A theory which permits less models than another theory, relativized to some structure or class of models, is the logically stronger theory*. For *scientific* theories this criterion is relativized to *empirical* structure or classes of *data* models. In the second section we constructed a quantifiable concept of empirical strength of scientific theories. Empirical strength was defined as: *Empirical strength of a theory T is the Kolmogorov complexity of the theory's empirical substructure*. We are now ready to formulate the upshot of our argument, a theory of scientific progress.

The same kind of twofold logic that was used in constraining our data sets (include as much as necessary yet no more) will be repeated, this time at the higher, inter-theory level. Science can be characterized as the endeavor of having empirically adequate theories. There are times we have empirically adequate theories and times we do not. The lack of empirically adequate theories stimulates the search for them. Kuhn's *anomalies* are one form this lack may take. But at any time *t* in a given field there is (usually) some theory or theories which are accepted as the dominant ones. This is a mere sociological fact. Nevertheless, a scientific community can in fact be in error of which theory or theories are the best ones, known at a given time.

A scientific community is right if they prefer the theory or theories that combine empirical strength with logical strength in the following way. In a given field let's say we have a set of theories T_1 , T_2 ,... T_n Relative to these theories and the field of research we have the corresponding (minimally redundant) empirical structure or data models. If one of these theories is the empirically stronger one, it is the theory closest to empirical adequacy and therefore the most correct and

preferable theory. If two or more of these empirically strongest theories are empirically equivalent (consequently having identical empirical strength) another consideration must be applied. Which of these empirically equivalent theories is the logically stronger one? If one theory is logically stronger than the rest, then this is the theory to be preferred. A scientific theory meeting these requirements we partly follow Van Fraassen and call *empirically minimal*.

Definition 3²⁵: A scientific theory is empirically minimal if it is equal or stronger in terms of empirical strength to all logically stronger theories.

Which implies:

An empirically minimal theory cannot increase its logical strength without losing empirical strength.

The preference for empirically minimal theories is due to the fact that it minimizes superfluous theory structure (see below and pages 55-56 in chapter 4). This makes explicit our scientifically motivated aim of having theories that save the phenomena without making theoretically unnecessary metaphysical claims.

Further, if two or more theories are both empirically and logically equivalent, i.e., they have both the same empirical and logical strength (they have the same class of models and empirical substructure), then there is no method or desideratum for selecting the preferable, most correct theory at time t. Let this be a formulation of our verifiability principle.

What About Scientific Progress, Then? This is our theory:

Definition 4: Scientific progress consists in either an increase of empirical strength in a given theory (an existent or a new) or an increase in logical strength due to theoretical innovation.

And an alternative formulation with focus on empirical minimality:

Scientific progress consists in the stepwise endeavor towards attaining empirically minimal

²⁵ Van Fraassen (1990, p 68.) formulated this as follows: "We may call a theory *empirically minimal* if it is empirically non-equivalent to all logically stronger theories – that is, exactly if we cannot keep its empirical strength the same while discarding some of the models of this theory." (Remember that Van Fraassen does not use our concept of empirical strength, but another one, less explicit than ours.

theories; that is, a zigzag progression of logical and empirical strength increments driven by the quest for empirical minimality.

Empirical Strength in Scientific Practice

What in scientific practice increases empirical strength? This happens either by measuring some newly discovered phenomena, or if by measuring a known field only with increased scope or resolution, something not previously registered is thereby unraveled. Then we establish what is known as a scientific *novelty*. Relative to previous empirical structure or data models, the incorporation of this novelty in empirical structure increases the Kolmogorov complexity of the enhanced or extended empirical structure²⁶. This extended empirical structure is either embeddable in contemporary theory, or it is not. If it is embeddable in a theory *T*, this increases the theory's empirical strength. If it does not, this novelty generates no effect on the strength of our best theories. Only when a new theory *T'* which can embed it (as empirical substructure) does the empirical strength increase. This constitutes one of the two ways in which science progresses.

Logical Strength in Scientific Practice

Theoretical innovation, on the other hand, happens on the theoreticians' desk. Relative to the same empirical structure we may have, as we have seen, several empirically adequate, or empirically *most* adequate, theories. Let us say we have one best, empirically adequate theory *T*. This theory then has the entire scientific field's empirical structure as a substructure in the theory's models. A theory always has some theoretical structure over and above its empirical structure²⁷. This purely theoretical structure we call *surplus structure*. (Not to be confounded with *superfluous* structure – surplus need not be superfluous.) Comparatively, theories may have more or less surplus structure. More surplus structure translates into the theory having more models, and is consequently the logically weaker theory, given our definition. Let us then say an ingenious scientist comes up with a new theory *T'* which is empirically equivalent to *T* but demonstrably has less models (i.e., less surplus structure, surplus structure which is now *superfluous*), then this new theory is logically stronger.

Should we therefore prefer it? Yes, this we did say some sections above. But since it would be ontological frivolity or bias to infer that the simpler (logically stronger) theory is therefore more truthful, we will not draw that inference. As naturalists we must deny the underlying presupposition

²⁶ The inclusion and representation of a novel phenomena in a data set is non-redundant information, and as such increases necessarily the Kolmogorov complexity. If the phenomena represented was redundant, it would either not be real (*nothing*) or not novel.

²⁷ This is the case even if we cannot sharply separate and distinguish the strictly empirical from the strictly theoretical part. (See section 2.3 on the discussion of the observational/non-observational distinction.)

in question. Now, we could argue that the theory with less surplus structure enjoys greater probability of being true since its theoretical postulations are fewer or thinner. But, logically, the logically weaker theory may still be true. What is more important is that the logically stronger theory tends to be more general and as such more amenable to further theory construction²⁸. (An example is how the relativization of space in Newtonian mechanics with Galilean spacetime is theoretically more similar to relativistic mechanics than is the Newtonian version with absolute space.)

As we saw in chapter 4 (pages 55-56), *what we can say* is that the two empirically adequate theories are both tracking the truth as far as they both can embed the relevant empirical structure. A new and empirically stronger theory may be more similar to the one or the other (often to the logically stronger theory) or it may be completely different to both. *But what we can be certain of is that the new theory must also embed this particular empirical structure, in addition to the inclusion of the novel empirical structure.* Although a theory *T* being logically stronger does not imply it is the one closest to the truth, we will still characterize *the discovery* of a new alternative theory as scientific progress. Having more theories or theory formulations increase our theoretical knowledge the same way further mathematical development does. It just may not point towards empirical truth*fulness.* Still, there is scientific progress.

We now conclude the presentation of the theory of scientific progress. We have argued that empirical strength is quantifiable and that comparative logical strength is demonstrable. Therefore, this theory delivers an objective measure of the progress of science. And the way any set of theories in a given field may be compared in terms of logical and empirical strength establishes its generality. In the following chapter we will apply this theory to two examples of (radical) theory transitions from the history of physics.

²⁸ I would like to thank Anders Strand for directing my attention to this.

6. Two Examples of Scientific Progress

In this chapter we will apply (or test) our theory of scientific progress on two examples from the history of mechanics. Both the theoretical framework and results used and obtained stem in large parts from Michael Friedman's book *Foundations of Space-Time Theories* (1983), which is a classical and authoritative text on the subject, at least in its purely mathematical parts. What our chapter provides is the connection between Friedman's and ours theoretical and philosophical contents, and the application of this synthesis on the two examples chosen.

In the first section we will explain why the examples were chosen from mechanics and why they are important. A mathematical framework and some explanations needed for the demonstrations will also be given. Then in the second section I go through the first example, demonstrating an increase in logical strength. Ultimately, in the third section, I go through the second example, demonstrating an increase in empirical strength.

6.1 Why Mechanics Matter

Mechanics is the branch of science concerning the behavior of physical bodies when subjected to forces or displacements. As such, the field of study is as general as is conceivable – the scope is universal and of unrestricted jurisdiction, at least in terms of spacetime. An ideal, true mechanics would prescribe precisely how matter moves in space and time. Some notion of what space and time are, and how they relate to matter and its properties (or objects generally, or bundles of properties and relations), are therefore included in mechanics as presuppositions, that is, as a necessary part in any theory of mechanics. Mechanics, then, is not only general to the highest degree but also fundamental in being presupposed *by any other theory of natural science*. This is one of the reasons why mechanics was the subject chosen on which to apply and evaluate our theory of scientific progress. A second reason is that although spacetime theories of mechanics are quite abstract and difficult subjects, they are still relatively simple *as theories*. A third and related reason is that these theories are particularly amenable to model-theoretic formulations and

considerations.

The history and development of mechanics in all its diversity, from Newton's classical and original theory as formulated in his *Principia* to Einstein's relativistic theories and the quantum revolution, offer a well of insights pertaining to key philosophical concepts such as progress and objectivity. In addition, a whole range of metaphysical and ontological concepts. From Newton's theory with absolute conceptions of space and time, and classical electrodynamics (essentially the same mechanics extended to electrically charged bodies), onwards with Maxwell's unification of electronic and magnetic theories, and further to the radical shift to the special theory of relativity, dispensing with the Ether together with the absolute conceptions of space and time; and ending, for now, with the curving of the very spacetime fabric according to the field equations of general relativity. This history is philosophically rich.

We will restrict our focus to two important theory transitions. The first will be the transition from Newtonian *kinematics* (disregarding gravity for the sake of simplicity) with absolute space to the Newtonian kinematics without absolute space, having instead a *Galilean* spacetime. This example will demonstrate an increase in logical strength. The second will be the transition from the latter theory to the special relativistic theory of kinematics. This second example will demonstrate an increase in empirical strength. Finally we will see how the conjunction of these transitions can be seen as being propelled towards empirical minimality.

Relativity Principles

Through these theory transitions we will also see how the philosophically motivated principle of relativity was guiding the development in mechanics. In addition to being interesting in themselves, the relativity principles (there are various versions) used in physics have interesting and close connections to our concepts of logical and empirical strength, as we will see. In chapter 3 we mentioned the Galilean relativity principle and expressed it in footnote 2 as: *All the laws of physics have the same form in all admissible frames of reference* (page 36). Below we will further work out the exact meaning of this, and in addition correct it by giving two more precise versions.

Also, in chapter 3 (page 37), we mentioned the connection between the relativity principle, with its emphasis on what is theoretically invariant, with the broader philosophical notion of objectivity. There we expressed this connection *as locating and describing what is constant (or invariant) over varying contexts, aspects, and perspectives*. This formulates at least one of the meanings objectivity is normally understood as having. Formulated as the opposite of *subjective* we see that what is objective is what is constant inter-subjectively, or independent of the particular subject. This sense of objectivity gets an exceptionally clear and explicit meaning in the

mathematical framework of physics. Here the subject may be taken as an observer in the form of a *frame of reference*. We would not count as objective what would vary arbitrarily from one frame of reference to another. Rather, that which demonstrates or manifests the opposite property, namely invariance over different frames of reference, we *would* count as objective (on this meaning). As relativity principles in physics values or accentuates what is invariant over all frames of reference of some specified, admissible class of frames, we see the inherent strive for this sense of objectivity in the notion of relativity. Invariance relationships have also similar connections to objectivity in other sciences than physics, though that will fall outside our current scope. Objectivity and relativity in physics, then, both concern and tend toward what is less and less, or not at all, arbitrary.

Theoretical Framework for Our Examples

We start with explaining the mathematical framework we will use. The general schema for characterizing spacetime theories is given following Friedman (*ibid.*, page 48). A spacetime theory of mechanics *T* picks out a class of (dynamically possible) models:

$$< M, \Phi_{I}, \Phi_{2}, \dots \Phi_{N}, T_{C} >$$

As we recall from chapter 3 (page 38), the form of this schema is in accordance with the standard way of defining mathematical structures (or models) in model-theory. The first element, M, is the set of entities (which can be either finite or infinite). M is here a four dimensional (structureless) differential²⁹ manifold. A manifold is a kind of primitive term in spacetime physics – think of it as an amorphous and abstract kind of primordial 'fabric'. This manifold of spacetime has *a topology*, which means that for any point p in spacetime we have the notion of a neighborhood of all points 'close' to p. Further, the space time is *coordinatizable* by the set of all quadruples of real numbers, i.e., we can define and use coordinate systems: "Such a coordinate system enables us to translate statements about geometrical entities into statements about real numbers: for example, we can describe curves in space-time by numerical equations." (*ibid.*, page 33).

The Φ s are the different geometrical objects postulated by *T* and obeying *T*'s field equations³⁰, and are defined on the manifold³¹ In order for a spacetime to have a *geometry* the

²⁹ In mathematics, a differentiable manifold is a type of manifold that is locally similar enough to a linear space to allow one to do calculus. (<u>http://en.wikipedia.org/wiki/Differentiable_manifold</u>, entered 23.08.13.)

³⁰ Field equations describe mathematically how the geometrical objects and other objects are interacting. An example: A mass density, mass spread out in spacetime, are represented by *source variables*. This defines a gravitational field which is represented by *field variables*. Further, *field equations* relate the source variables with the field variables. *Equations of motion* define trajectories of other objects in terms of the field variables. (Friedman 1983, page 44.)

³¹ Set-theoretically or model-theoretically these objects are constructed by relations.

manifold must have sufficient structure imposed on it. This is given via the geometrical object of an *affine connection* (*ibid.*, page 39). An affine connection gives 'directions' to the manifold. Essentially, the affine connection prescribes which lines are to be considered as straight. Now that we know which directions, or lines, are straight and which are not straight, we need some measure of distance. A *metric* provides just that. In addition to this, we have some curves T_c , usually particle trajectories, defined on M, which obey the equations of motion of T.

We now have sufficient machinery to describe our spacetime theories (*ibid.*, page 44). But first let us summarize: A spacetime theory T postulates some geometrical objects on a manifold M, gives field equations imposing satisfaction requirements on the geometrical objects, and finally, equations of motion particle trajectories must satisfy (*ibid.*, page 48).

Newtonian Mechanics with Absolute Space – An Example

These abstract notions will become somewhat clearer as we give a concrete example. We will choose the spacetime probably closest to our (non-physicist) intuitions: Newtonian spacetime theory with absolute space (which is also the first spacetime we will study in the next section.) As in any spacetime here considered, it consists of a four dimensional manifold M. The first geometrical object imposed on this manifold is a flat affine connection, denoted C. First considering only the spatial part of the manifold, the flat affine connection corresponds to the intuitive three dimensional Euclidean space. The three spatial directions (dimensions) orthogonal on each other always and anywhere³². And for each time instant t, the length between two points is given by a Euclidean distance function (obeying the Pythagorean Theorem) or metric, denoted as m. Where the spatial part of the manifold is flat, we say that the temporal part dt is linear. The d before the t means the *difference* of t – there is no absolute *position*, only *durations* between two relative instants. That is, any temporal *interval* is implicitly defined for, and independent of, all reference frames (equivalent to the property of absolute simultaneity).

To achieve or *to rig* the absoluteness of space we need a further geometrical object, a three dimensional vector space V, without which *absolute rest* and *absolute motion* cannot be defined. Any vector space has a defined zero vector. All reference frames with zero velocity relative to this vector space are then by definition at rest. Newtonian spacetime theory T of kinematics, with absolute space, (NMAbs), is then associated with the following class of models:

Models for NMAbs = < M, D, dt, m, V >

³² This means that the spacetime derivative of the affine connection is zero. It is flat and isotropic.

Following our schema from above we would also include a set of curves $T_{c.}$ However, for characterizing a *spacetime* theory as such this is not necessary to do explicitly. We are then dealing with models at a higher level of generality: This class of spacetime models includes all the models of the normal and lower level of generality, that is, the distinct models corresponding to all the possible and different particle curves defines on them. (Think of the curves as giving initial conditions. Distributions of masses and so forth, considered together with the specific spacetime, constitute boundary conditions.) Since we are excluding gravity for the sake of simplicity, thereby having merely a kinematic and not a dynamic theory (a flat spacetime), there are no masses in this 'universe', i.e., no mass density functions, and hence no gravitational fields (nor any other force fields.) Our particles are mass- and charge less³³.

Models and Frames of Reference

We will now consider how models are related to frames of reference. First we need to dispense with a common inaccuracy and source of confusion. Although it is more a terminological issue than a logical one, frames of reference are occasionally equated with models³⁴. This is not the case here. In the framework we use, corresponding to a model (of a particular spacetime theory) there is an infinite set of possible reference frames. Different frames of reference may 'belong' to different models, or they may not. A frame of reference and a coordinate system are coextensional in this context of physics. A specific frame of reference, relative to a spacetime theory of the above form, corresponds to a coordinate system. The particular *kind* of coordinate system depends on the theory at hand, i.e., depends on the properties of the particular spacetime. As such, whole sets of coordinate systems, or whole sets of reference frames, corresponds to each model of the theory.

Now, relative to a specific theory and its associated models, there are certain inherent rules determining classes of *privileged* or admissible reference frames, such as *rest frames*, *inertial frames*, and *geodesic frames*. These classes are determined by the theory, given by exactly the set of transformations, from one frame to another, which satisfy certain *invariance* conditions. The first class corresponds to the class of all reference frames that are at rest relative to some absolute space. The second to frames of reference having a constant, relative velocity. The third, having only

³³ This simplification will not affect our examples and arguments. Mass converges as does the other parameters as the different theories are asymptotically compared.

³⁴ Patrick Suppes (1967) sometimes writes in this way, e.g., in the quote in chapter 3 (page 36): "We can view the tacit assumption of a frame of reference as an extrinsic aspect of the familiar characterizations of the theory. From the standpoint of the models of the theory, the difficulty in the standard axiomatizations of mechanics is that a large number of formally distinct models may be used to express the same mechanical facts. Each of these different models represents the tacit choice of a different frame of reference, but all models representing the same mechanical facts are related by Galilean transformations." In our terminology and framework we would in the last clause rather say that *all frames of reference, within one and the same model or across different models, representing the same mechanical facts, are related by Galilean transformations.*

gravitational acceleration. These classes are theory and model dependent; still, there are infinitely or arbitrarily many of them. And what makes them special, what distinguishes them, as we will further explain in the following sections, is that the laws of each theory is such that all reference frames in these classes are rendered *physically equivalent*. Yet they are different, i.e., not identical according to its corresponding theory. This means a principle of relativity would hold; the laws would have the same form.

Frames of reference may also be *empirically equivalent*, as may different theories. Again, as with theories, it is important to specify relative to what they are empirically equivalent. Different reference frames in one and the same theory may be empirically equivalent, and different reference frames pertaining to *different theories* may be empirically equivalent, *relative to some but maybe not all possible empirical structure*. When relevant, this information will henceforth be explicitly given. Empirical equivalence between frames of reference means that there is no mechanical experiment by which one could tell the one from the other (*ibid.*, pages 151-152). Simply speaking, in these reference frames everything looks the same.

Frames of Reference, Equivalences and Group Theory

Finally we will make some comments about group theory and frames of reference. Group theoretic considerations will be included in presenting our two examples of scientific progress. This is because group theory articulates what is relevant and important in these examples and for our concerns, and it simplifies our ways of talking about them. We spoke above of transformations of reference frames having certain properties. We can reformulate that with increased clarity. To compare what is going on from the vantage point of different reference frames we must be able to switch from a particular reference frame to a particular other. This switching we call an operation. A mathematical group consists of a set of specified operations defined on a set of elements, obeying certain requirements (See chapter 3, page 39). In our context, the frames of reference are the elements while the set of operations are the different ways in which we can transform one reference frame to another.

Let where you now sit be a reference frame. All you see around you have coordinates relative to that reference frame. We will look at three types of transformations. If you walk anywhere else and sits down, that reference frame is given by a *translation* relative to the first. It is simply moved to another place. Your previous position has definite coordinates relative to your new position. All possible translations correspond to a certain group, the group of translations. The second group consists of *rotations*. Imagine suddenly your office gets tilted 45 degrees clockwise for you. That is also a transformation of reference frame, which transforms the coordinates

systematically. The third group is the group of velocity boosts. You now leave your office building and immediately enter the tram, which is moving with a constant velocity. The way this new reference frame of yours, the tram, is connected to the first, is by a velocity transformation in a certain direction. These three groups exhaust all possible, non-accelerating reference frames.

Above we mentioned classes of reference frames that were *physically equivalent* and *empirically equivalent*. We will now connect these notions to group theoretic notions. But first of all, we need to explain how they differ. The class of reference frames that are physically equivalent means all those reference frames that are equivalent *according to the relevant theory*. It is an intrinsic property of the theory at hand. This class of physically equivalent reference frames corresponds to *the symmetry group of the theory* (Freedman 1983, page 56 and 154). And the symmetry group is precisely the set of those transformations that leave physics invariant, i.e., the set of transformations which leaves reference frames physically equivalent. We will explain what the symmetry group is for every theory we work with in the sections to come. The second class of reference frames, the ones who are empirically equivalent, correspond to a group we will call the indistinguishability group (*ibid.*, page 154). This group is not determined by theory alone, in contradistinction to the symmetry group. The content of the group is theory relative but partly, yet essentially, determined by empirical reality. The 'gehalt' of an indistinguishability group is not a matter of pure mathematical construction, but also a contingent fact.

We will see that the more general aim of science to render theories that are empirically minimal is in the context of mechanics equivalent to making theories such that the indistinguishability group is contained within the symmetry group. More on this below. The quest for empirical minimality is also intimately linked to the exact role of *relativity principles* driving the development from classical mechanics to special relativity. (*ibid.*, section IV.5, especially pages 158-159). We now turn to our two examples.

6.2 Loosing Absolute Space – Gaining Logical Strength

Our first example regards the transition from Newtonian mechanics with a conception of absolute space to a Newtonian mechanics without such an absolute conception. But first a few words about the history of this transition. In Newton's own formulation, his theory included both absolute time and absolute space. As we saw in chapter 3, Newton made a fundamental distinction between true and apparent motion. True motion was motion relative to space itself, while apparent motion was motion as observed relative to an observer. Acute thinkers did point out from the very beginning, as did Leibniz, how this absolute sense of space was both unverifiable, 'speculative', and anyway

unnecessary. Newton conceded the unverifiability of absolute space, but stood by it as a postulate nonetheless.

Two parallel theories of Newtonian mechanics were consequently elaborated and formulated. The historical-sociological details about the transition is irrelevant to our purposes, what matters is that both theories are clearly formulable and that it is a fact that one of them is theoretically more similar to later theories than is the other. (Later conventional theories of mechanics have not re-introduced absolute conceptions of space.) We will now present the two theories.

Our Two Theories

The first theory, Newtonian mechanics with absolute space, was given in the previous section. We repeat the essentials. It consists of a flat spacetime manifold with a vector space 'rigging' the space such that *there is some structure in space relative to which objects may have true rest or true motion*. And the temporal duration is the same for all possible reference frames. We then have the class of models of this theory *T* given as

Models for
$$T$$
 (NMAbs) = $\langle M, C, dt, m, V \rangle$.

(*M* is the manifold; *C* is the affine connection; dt is the temporal duration; *m* is the metric; *V* is the vector space.)

Our next theory, Newtonian mechanics with *Galilean spacetime* (NMG), is characterized as follows. It is identical in all aspects to the previous theory except the inclusion of a vector space V. As a consequence there is no true motion, all motion is relative. Relative here, as is more often the case than not in physics, does not mean any loss of objectivity. The absolute notions of *position*, *rest* and *(true) velocity*, have been replaced with the relative counterparts, where the relativization is simply to a frame of reference. Any length, relative velocity and acceleration are still *invariant* measures. It is the latter fact which makes possible that NMG has the same laws of motion as NMAbs. The spatiotemporal parameters the equations are dependent on are only these reference frame relative yet invariant ones. The class of models for Newtonian mechanics with Galilean spacetime T' is then given as,

Models for
$$T'$$
 (NMG) = $< M, C, dt, m >$

The Logically Stronger Theory

We will now demonstrate how this latter theory T' is logically stronger. In order for that we need to show how T' permits less models than does T, relative to some empirical structure. In this particular example it doesn't matter which empirical structure we pick out for our theory evaluation; we know for a (contingent) fact that the two theories are empirically equivalent. This is due to the (again, contingent) fact that absolute space is undetectable. We will let the empirical structure be the astronomical data before the advent of relativity theory, before anomalies in the trajectory of Mercury were discovered. On that assumption both our theories can be considered empirically adequate.

Here is the proof (all mathematical results from Friedman 1983, pages 152-154). Let f be a transformation of reference frames, of the velocity boost kind. Think of f as letting everything in space have a constant velocity in a given direction. Such a transformation operates on both the affine connection and the vector space. Here are the two respective classes of models of NMAbs, where the latter is transformed:

Models for T (NMAbs) = < M, C, dt, m, V >, and the transformed class = < M, fC, dt, m, fV >.

The corresponding two classes on NMG are given as follows:

Models for T' (NMG) = < M, C, dt, m >, and the transformed class = < M, fC, dt, m >.

Now, it can be showed that the affine connection is left invariant over such a velocity transformation, such that

$$fC = C.$$

Equally demonstrable, this equality relation does not hold for vector spaces, meaning that

$$fV \neq V$$
.

Substituting this identity and non-identity into our transformed theory classes, we get, respectively

Transformed models for T (NMAbs) = < M, C, dt, m, fV >

and

transformed models for $T'(NMG) = \langle M, C, dt, m \rangle$.

We see that the transformed model class for $T_{,} < M_{,} C_{,} dt, m, fV >$, is a different model class than the untransformed class, $< M_{,} C_{,} dt, m, V >$. The two model classes, although *physically*³⁵ *equivalent*, are theoretically distinct. On the other hand, the two model classes for T', the transformed $< M_{,} C_{,} dt, m >$ and the untransformed $< M_{,} C_{,} dt, m >$, are identical. *They are (sets of) identical models*.

Models which are distinct on Newtonian mechanics with absolute space are identical on Newtonian mechanics with Galilean spacetime. Since the two theories are otherwise identical, it follows that T' permits less models than does T:

$$T$$
 (NMAbs) $\leq_L T'$ (NMG).

The theory T', Newtonian mechanics with Galilean spacetime, is logically stronger than T, Newtonian mechanics with absolute space.

Given our Definition 4 from the last chapter (page 76): *Scientific progress consists in either an increase of empirical strength in a given theory (an existent or a new) or an increase in logical strength due to theoretical innovation.* It follows that this theory transition was one of scientific progress.

(This result may appear counter-intuitive. A possible source for this must be explained. One could easily think of these theories in mechanics in such a way that to add another theoretical constraint by extending the mathematical structure with yet another geometrical object would render the theory stronger and not weaker, which is implicated by the argument. This consideration would be analogous to how adding an axiom would make a mathematical theory stronger. Intuitive as this is, the analogy is ill-conceived. Remember that in addition to these geometrical objects given in the mathematical structure there are also the (field) equations governing the interaction of the theory's objects in general. Now, increasing the constraints posed by this set of equations would make the theory stronger, and would be analogous to adding axioms to a theory. But the mathematical structure having more objects defining it, can rather decrease rather than increase logical strength.

³⁵ Remember, *physically* equivalent means that the laws of the theories at hand prescribe the same results or consequences (trajectories). And as such, physical equivalence is independent of empirical considerations. (See also Friedman 1983, page 152.)

In this example, by adding the vector space rigging spacetime, whole new classes of models are constructed ('created') beyond the original model classes, thereby making the theory, as we saw, logically weaker.)

The conception and theoretical incorporation of absolute space in Newtonian mechanics is a prime example of superfluous theory structure. We have seen how a theory which frees itself of this extra structure gains logical strength, according to our theory. Increasing logical strength may also be treated as the *result* of adhering to and applying some metaphysical principle of parsimony like Occams's Razor³⁶. There are close similarities between our concept of logical strength and this principle. Nevertheless, our constructed concept is *model-theoretic*, and the concrete form and justification are different enough for us to distinguish them (see section 5.2.3, especially pages 77-78).

Next we will look at the connection between this result and relativity principles and group theory.

The Principle of Relativity and Group Theory – An Alternative, More Specific View

As promised in the first section, we will account for how this theory transition, and the next, can be seen as the result of the application of philosophically laden principle of relativity, and also see how what we call the quest for empirical minimality translates, in this context, to another, group theoretic notion.

Our first formulation of the relativity principle stated that the laws of nature are the same (or take the same form) in all inertial frames. This we mentioned is inaccurate and we will provide the necessary corrections. Actually there are two components in what is taken to be *the relativity principle (ibid.*, pages 149-153). The first is given thus:

(R1) All inertial reference frames are physically equivalent or indistinguishable.

This is the content of a philosophically happy intuition, which drove physicists toward constructing theories which did not depend on specific reference frames. On our meaning of objectivity (pages 80-81 above) this drive toward R1 can also be seen as motivated by considerations on objectivity. The second states that:

(R2) If two frames of reference are indistinguishable according to *T*, theory should be theoretically

³⁶ I would like to thank Anders Strand for pointing this out.

This is more of a methodological norm than R1 is; and it implies an increase in logical strength when possible. Newtonian mechanics with absolute space satisfies R1 but not R2 (*ibid.*, pages 153-154). We saw that frames of reference which were both empirically and physically (theoretically) equivalent were still given in non-identical models. On the other hand, the shift to the Galilean spacetime version made Newtonian mechanics satisfy both relativity requirements.

There is a parallel and interesting group theoretic consideration pertaining to this theory transition, increasing logical strength and adhering to the relativity principles above. We explained in the previous section how the frames of reference that are physically equivalent are described by the transformations corresponding to the symmetry group of the theory. In T (NMAbs) the symmetry group consists of the two groups of all translations and rotations. Nevertheless, the theory has a different indistinguishability group. A velocity transformation, we saw, is empirically undetectable. That means the indistinguishability group consists of the group of velocity boosts in addition to the groups of translation and rotation. Accordingly, on T (NMAbs) the indistinguishability group is larger than the symmetry group (see *ibid.*, pages 154-155).

Again, on the other hand, things are different and more 'progressive' on the Galilean version. Here the symmetry group of the theory is the product of the groups of translations, rotations, and velocity boosts. All these transformations generate physically equivalent reference frames. The indistinguishability group does not change of course (the theories are empirically equivalent), so that group is still the same. Accordingly, the two groups are identical; the indistinguishability group is contained within the symmetry group (*ibid*). The identity of these groups implies, relative to our theoretical and empirical context, that the logically stronger theory is also the *empirically minimal* theory. We know of no alternative theory which is empirically equivalent yet logically stronger than Newtonian mechanics with Galilean spacetime.

We now proceed to our next example.

6.3 Loosing Absolute Time – Gaining Empirical Strength

Before turning to the demonstration, some preliminaries must be given about the example's history and particularities.

Some Background for the Transition

The evidential situation underlying the transition from Newtonian mechanics with absolute space to

one with Galilean spacetime can be characterized as *negative*. It was the lack of any evidence corresponding to some theory structure, together with the insight that this particular structure was both empirically and logically independent of the remaining theory structure, which led to its abandonment. As a result, increased logical strength was achieved. The evidential situation underlying the transition from Newtonian mechanics (with Galilean spacetime) to the special theory of relativity (STR), on the other hand, was *positive*. There was an accumulation of systematic discrepancies between models of Newtonian theory and data models coming from experimental research.

Evidence started making trouble for the deeply held conviction of the validity of the Galilean principle of relativity. This principle implies that different velocities are additive. If you walk inside of a train with a velocity v and the train has a velocity u (in the same direction), then relative to a reference frame at rest by the railroad you have the velocity v + u. This is intuitive but strictly false (Taylor and Wheeler 1992, section L7). Since the physical laws of mechanics deals with *changes in velocity*, that is, accelerations, the generality of the laws of physics over all inertial frames holds. But trouble ensued when the successful results of measurements of the speed of light started coming in. For one, the results were not compatible with the additivity of velocities. Secondly, and even stranger, the speed of light as recorded was the same no matter the velocity of the source of the light emission (*ibid.*, page 60). Was there some privileged rest frame after all? But at rest relative to what – absolute space? Actually, many physicists attributed this constancy to the ether but all attempted explanations failed.

Einstein managed to save Galilean relativity *and* explain the apparent constancy of the speed of light. The speed of light is indeed constant. More specifically, it is constant relative *to any inertial reference frame*. But this is clearly contradicting Galilean relativity; if you press your laser pointer in the direction of the train, then the velocity of the laser beam c, relative to a reference frame at rest on the ground, should be the sum of c and the velocity v of the train! Now, this inference is valid but one of the assumptions, albeit tacitly made, is false. The time coordinate is not absolute, but is relative to different inertial frames of reference. Any such frame has its distinct time flow or duration.

This theoretic change corresponds to a change of spacetime, from Galilean to *Minkowskian*. Although this spacetime is as flat and four dimensional as Galilean, it has a fundamentally different geometry (Friedman 1983, page 127). In Galilean spacetime the geometry is Euclidean and space and time dimensions are independent and separable. Not so in Minkowskian spacetime. Here the space and time aspects of the spacetime fabric are interwoven, or intertwined. They are mutually dependent. Nevertheless, the special theory of relativity has a theoretical framework which makes

all distances and durations perfectly objective although frame dependent. The *spacetime interval* is invariant while each of the contributions of the temporal and spatial part *co-vary* over reference frames. This covariation is expressed in the transformation laws pertaining to the theory. The transformation laws are of the same form as the Galilean, only incorporating the covarying spatial and temporal contributions which depend on the relative velocity between two reference frames (*ibid.*, chapter IV).

Asymptotic Relations and Model-Theoretic Relatedness

There is a common belief that NM is asymptotically related to STR. We explained this relation with respect to the transitions from Newtonian mechanics to special relativity and quantum mechanics in chapter 5 (pages 64-65). And that there is a methodological principle in physics called the correspondence principle, which states that the mathematical solutions to the previous theory should be retrievable within the new theory at certain limit values. We explained further that regarding the consecutive theories NM and STR this happen when the relative velocity between two reference frames approaches zero. For the purpose of demonstrating STR being empirically stronger than NM, we will now deliver upon our promise from chapter 5 of giving this asymptotic relation some model-theoretic substance.

Asymptotic relation between theories implies isomorphism of some of their respective model classes. Whether this implication is general is a question outside the scope of this thesis; here we will argue for it in this particular case, between NM and STR. When transforming frames of reference with respect to a velocity boost, the other spatial directions orthogonal to this velocity direction remain identical, both in NM and STR. We will let the velocity be in the (arbitrarily fixed) x-direction. Thus, on the Galilean transformation scheme in NM, from x to x', where x' moves with velocity v relative to x, we get (Ladyman and Ross 2007, page 94):

$$t' = t$$
$$x' = x - vt$$

To follow upon our earlier example, we will let x be the coordinate length as measured from the ground while x' is the corresponding length measured from inside the train which moves with velocity v in the x-direction. The corresponding transformation scheme pertaining to STR, the so-called *Lorentz* transformation, is more complicated, given how the temporal and spatial aspects of the Minkowski spacetime are interwoven. They are given by

$$t' = \gamma(v) (t - vx)$$
$$x' = \gamma(v) (x - vt),$$

where $\gamma(v) = \frac{1}{\sqrt{1-v^2/c^2}}$. Here the asymptotic relation is readily seen if you put v = 0, implying that $\gamma(0) = 1$. The Lorentz transformation then 'collapses' into the Galilean. The parameters that would otherwise vary in STR but stay constant on NM are on this special case as constant on the first as on the latter. The two theories then become *physically* equivalent. This is not a simple matter of a mathematical curiosity. Actually, in STR the spacetime locally for every inertial frame of reference is also Galilean with Euclidean geometry. "Within any single inertial frame, things look precisely as in Newtonian kinematics: there is an enduring Euclidean three-space, and global time, and the inertial law of motion..." (*ibid.*, page 127). From the Lorentz transformation scheme it follows that for two (or more) inertial frames of reference in STR, with no relative velocity, the Galilean transformation scheme holds and Newtonian mechanics is valid. This fact makes some of the respective theories' model classes isomorphic to each other. The two theories, then, share at least one class of models.

Our Demonstration – Increasing Empirical Strength

Our demonstration consists of two parts. First we will explicate how STR has empirical strength absolutely greater than NM (in terms of Kolmogorov complexity), given the fact that the empirical substructure of STR has (non-redundantly) greater scope than does NM. Secondly we will show how STR is comparatively, in model-theoretic terms, empirically stronger than NM, according to our Theorem 2 and Corollary 2 of theory comparison (page 74)

The data models, or empirical structure, in a given field of science is not given and constant but vary in accordance with the relevant mensural technology. And mensural technology, or measurement generally, always has a margin of error. Up to an extreme point of accuracy NM and STR are empirically equivalent for relative velocities more or less below one tenth of the speed of light, *c*. Above that limit STR diverges from NM exponentially. Let our data models be of measurements of some physical parameter, dependent on velocity according to STR, pertaining to an object moving relative to an observer frame. On trial one we measure this parameter over the velocity interval between zero and $1/10 \ c$. That is, the scope of the parameter (dimension) velocity, $s_{\rm F}$, has this velocity interval as its value. According to the empirical equivalence between NM and STR mentioned above, it follows that this empirical structure is embeddable in both theory models. In trial two we let the velocity interval increase up a certain significant point, relative to the margin of error, over 1/10~c. This second empirical structure is embeddable in the models of STR but not NM. All else equal, the empirical substructure of STR has one parameter scope, s_{ν} , of a larger interval than does NM. Since the measure of data content *d* is positively related to the scope factors, and this increase in scope is by assumption non-redundant information, it follows that the Kolmogorov complexity of STR is greater than the corresponding value of NM. The special theory of relativity, then, has greater empirical strength than Newtonian mechanics.

Now we will give the alternative proof. Since the two theories are model-theoretically related, i.e., they share at least one class of models, they are susceptible for this kind of comparative evaluation. Theorem 2 says that,

If, given two theories T and T', for every model M of T there is a model M' of T' such that all empirical substructures of M are isomorphic to empirical substructures of M', then T' has empirical strength of equal or stronger degree than does T. We denote this as $T \leq_E T'$.

Let NM be *T* and STR be *T'*. Above we saw that for models where the relative velocity between any two objects is zero, the respective theory models are isomorphic, i.e., model-theoretically identical. We also saw that for velocities up to one tenth of *c* the respective models are empirically equivalent. In this 'parameter space' where NM is empirically valid is the domain of the empirical substructure of NM. Given the limited empirical equivalency between the two theories, the empirical substructure of NM is equally embeddable in the models of STR, i.e., the now dually embedded structure is one and the same empirical structure, and identity implies isomorphism. It then follows that for every model *M* of NM there is a model *M'* of STR such that all empirical substructures of *M* are isomorphic to empirical substructures of *M'*, and consequently NM \leq_{E} STR; STR has empirical strength equal to or greater than does NM.

To get the relation to be *strictly* greater, comparative empirical strength, we need to show that the converse relation does not hold, according to Corollary 2. This is simple. Pick one model of STR where the empirical substructure corresponds to measurements with relative velocity significantly above 1/10 *c*. This structure is not isomorphic to any empirical substructure in NM. Consequently STR \leq_E NM does not hold. Which finally implies that NM \leq_E STR. That is, the special theory of relativity is strictly, comparatively empirically stronger than Newtonian mechanics.

As was the case in the theory transition accounted for in the previous section, given our Definition 4 from the preceding chapter: *Scientific progress consists in either an increase of empirical strength in a given theory (an existent or a new) or an increase in logical strength due to*

theoretical innovation. It follows that also this theory transition was one of scientific progress.

In the alternative formulation of our definition of scientific progress it was stated that, scientific progress consists in the stepwise endeavor towards attaining empirically minimal theories; that is, a zigzag progression of logical and empirical strength increments driven by the quest for empirical minimality (pages 76-77). Through our two examples we have seen such a stepwise endeavor. In the first case, with Newtonian mechanics with absolute space, we had an empirically adequate theory. Nevertheless, the theory had superfluous theory structure and another theory, Newtonian mechanics with Galilean spacetime, came to the scene, increasing logical strength and thereby negating the first theory as empirically minimal. This virtue, empirical minimality, was then enjoyed by the Galilean version. After some time, though, recalcitrant measurements were registered, and empirical structures from experimental science were no longer embeddable in Newtonian theory. Einstein's theory of special relativity was superior in this respect and consequently empirical strength was increased. A manifestation of the progression of science through the zigzag motion between logical and empirical strength increments.

7. Conclusion

I have argued for and showed how there are two ways in which scientific theories make progress. We saw how scientific theories may increase their logical strength relative to some given empirical structure by cutting down its model classes in order to crystallize the similarities between the theory's models and empirical structure. This circumstance, scientific theories becoming one with empirical reality, as it were, is both our epistemological goal and, in terms of structural isomorphism, our epistemological limitation. And we saw how scientific theories grow in empirical strength by being able to embed data models containing or expressing a corresponding increase in *meaningful* informational content (in terms of Kolmogorov complexity.)

What lays ahead?

Much more work, obviously. One special type of scientific progress should be investigated within the proposed framework. That is unification of different scientific theories. My contention is that these specific cases can be seen as milestones for our theory to be applied and adjudicated. In theory unification both model-theoretic considerations about the respective theories and their consequent synthesis, and considerations pertaining to the Kolmogorov complexity of the converging data models, are pertinent. The unification of the theories of electricity and magnetism into electromagnetism is a good candidate for such an investigation. And the hypothetical resolution of the incompatibilities of relativity theory and quantum mechanics no less so.

Further, specific examples of the formal connection between high level theory models and down to the data models should be sought explicitly articulated, so as to convince us of the adequacy of this view of science, lest it be restricted only to the most abstract philosophical analyses. Herein may also lay particular problems which stand in need for philosophical resolution. Also, in order to test our theory of scientific progress the Kolmogorov complexity of the empirical structure, or data models, of some discipline's research should be sought calculated, albeit not in exact terms, for just the same reason.

Much is gained for philosophy by working closely with science. As is seen in the sequential order of this thesis, a thorough and close analysis of what scientific theories are, and an analysis

true to scientific practice, is a precondition for philosophical theorizing about scientific progress. And while it is more difficult to say what philosophy has to offer, or should offer, to science, apart from how some philosophical literacy among scientists would be beneficial for both parts, philosophy arguably has much to gain by close collaboration with on-going science. Given the dynamics and progress of science there is always new and ontologically relevant material on which philosophy can be applied, and for philosophy to grow upon. The semantic view of scientific theories distinguishes itself as a promising philosophical framework for this assignment. Especially vivid is the tendency among philosophers of science most in sync with contemporary fundamental physics to adhere to the semantic view, or views very similar to it. But as I have argued, the framework of the semantic view is not seem restricted to physics or fundamental physics.

The great diversity of science poses a challenge for any unified account of it, and here the semantic view is no exception. While posing a challenge, the diversity of science should not be seen as an obstacle rendering the attempt futile. How general this view of science is and how adequately it can represent different types of scientific theories, must be investigated by scientifically well-informed philosophy. Scientific theories generating dynamical models for representing the discipline's phenomena or objects of research are perfectly amenable to model-theoretic articulation. But what about historical sciences, like evolutionary biology and cosmology? Are there here superimposed two kinds of classes of models, models of short time span on top of models of a much longer time span? Short time span in cosmology would be the mechanics of a certain period, defined in terms of the there and then operating physical laws and constants. Long time span would be models of the evolution of the universe as a whole, that is, evolution of the physical laws in interaction with the boundary conditions themselves. Analogous considerations pertain to evolutionary biology. Can we reconcile these two ways of theorizing model-theoretically, or are they principally different?

Another important locus of philosophical research is the extension of the semantic view to the social sciences. Especially the mathematized sciences like economics and parts of political science and sociology appear well fitted to this semantic treatment. There are clear ways which these sciences are not different from physical sciences with respect to the methodology of the semantic view. The mathematical foundation defines models which are connected to, and tested against, models of data stemming from empirical research. But if the physical and social sciences still differ in important ways, it would be appropriate to investigate just how they part ways with respect to semantic approach. My own contention, and reflecting my own stance on the subject, is that to the degree the science in question is well founded *as a science*, it is compatible with the semantic view. Everything else is story telling. Which of course can be of great value, just not valuable in these scientific terms.

I will end on a broader and more speculative note. Answering to the motivation for working with the question of scientific progress, to embark on this project and discover that we actually have some solid ground on which to stand, is a victory vis-à-vis the relativism about objective truth inherent or implicated by the Weltanschauung perspective of so much philosophical thinking since the time of Kuhn and continuing on with Feyerabend. This is important not just for having a correct philosophical attitude towards science and scientific growth and knowledge, but also for other areas such as ethics and politics. Philosophical doctrines about science with such relativist consequences have dire implications for our thinking about ethics. Relativism about truth and corresponding skepticism about, and even hostility to, science, easily leads to relativist attitudes in ethics (as the menace of post-modernist thought is the prime example.) Conversely, solid and well-founded belief in the objectivity of science and its progress reinforces the conviction of the validity of universalist ethical positions, i.e., that there actually are distinctions between right and wrong in the fabric of social reality. Of course, the view of science here argued for is not construed in such a way as to reflect strongly held ethical convictions. Rather, the connection, in attitude if not in logic, between universalist ethics and the semantic view of science is more a happy coincidence worth appreciating, and worth defending.

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