Theory Change and Structural Realism. A General Discussion and an Application to Linguistics

Inaugural-Dissertation

zur Erlangung des Doktorgrades

der Philosophie an der Ludwig-Maximilians-Universität

München

vorgelegt von

Thomas Meier

aus Schwäbisch Gmünd

23. Februar 2015

Erstgutachter: Professor emeritus Dr. Carlos Ulises Moulines

Zweitgutachter: Professor Dr. Otávio Bueno

Datum der mündlichen Prüfung: 23. Januar 2015

Acknowledgements

First, I want to thank my parents for all their support during my studies. Furthermore, I want to thank Hannes Leitgeb, Carlos Ulises Moulines and Otávio Bueno for their fantastic supervising and their guidance through the different stages of my doctoral studies. Since the time of my fellowship at the Munich Center for Mathematical Philosophy and at the LMU Munich, I have had the privilege of discussing my work with so many brilliant persons, at the MCMP and in global academia. I am thankful for this and I want to thank Jeffrey Ketland, Michael Friedman, John Collins, Branden Fitelson, Peter Lewis, David Chalmers, Stephan Hartmann, Pablo Lorenzano, José Díez, Xavier de Donato Rodríguez, Axel Barceló, Ana Rosa Pérez Ransanz, Fernanda Samaniego, Victor Peralta Del Riego, Jorge Tagle Marroquín, Gerhard Schurz, Chris Menzel, Ed Zalta, Armando Cíntora Gómez, Mario Casanueva, Álvaro Peláez, Gil Sagi, Howard Lasnik, Dietmar Zaefferer, Barbara Partee, Adolfo García de la Sienra, Charlotte Werndl, Karim Thebault, Radin Dardashti, Paul Dicken, James Ladyman, Steven French, Richard Pettigrew, Christian Damböck, Georg Schiemer, Erich Reck, Catarina Dutilh-Novaes, Maria Caamaño, Richard Dawid, Jan Sprenger, Peter Ludlow, Kristina Liefke, Dominik Klein, José Luis Falguera, Conor Mayo-Wilson, Johannes Korbmacher, Marta Sznajder, Catrin Campbell-Moore, Pascal Ströing, Hans-Christoph Kotzsch, Thomas Schindler, Christian Meyer, Albert Anglberger, Norbert Gratzl, Martin Fischer, Sebastian Lutz, Holger Andreas, Tilmann Massey, Georgios Karageorgoudis, Roland Pöllinger, Monica Livier Aguilar, Gerardo Martínez De la Torre, Juan Rokyi Reyes, Philipp Schwind, Claus Beisbart, Tim Maudlin, Erik Curiel, Alessandro Torza, Fred Muller, Vincenzo Crupi, Mathias Frisch, Christian Feldbacher, Holger Lyre, Olivier Roy, Julien Murzi, César Escobedo, Ralph Cahn, Lena Hofer, and Wilken Steiner. Special thanks go to the Alexander-von-Humboldt-Foundation, for their generous support by granting me a MCMP Doctoral Fellowship for the duration of my dissertation project. Apologies to all those I have not mentioned.

For Alicia and Leonardo

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Chapter 1

Introduction

The general topic of this doctoral dissertation is the debate on scientific realism, structural realism and theory change, with a primary focus on linguistics. Broadly speaking, scientific realism is the view that our scientific theories are true, that the terms in our theories do refer to entities which exist in the "real" world and that our best scientific theories can give us correct predictions and explain the phenomena correctly. As opposed to scientific realism, the so-called antirealist view consists of the negation of the realist theses. For the antirealist, it is enough to assume that our scientific theories are tools which help us to obtain certain results through experiments. There are no statements made about the truth of a theory, the reference of the terms which occur in a theory, nor is there made any statement about predictive success of our theories. They are merely seen as a tool for obtaining certain results.

Given the fact that our scientific theories change over time and some of them turn out to be false, the view of scientific realism is challenged. Two main accounts of how to conceive a scientific theory have risen from this discussion: the so-called *syntactic*, and the *semantic* or sometimes also called *model-theoretic* conception of theories. In the former, a theory is understood as the deductive closure of a set of formulas in first-order logic. In opposition to this view, the semantic conception holds that theories are to be represented best as a class of mathematical structures. The latter, as its adherents argue, provides a more detailed representation of inter-theoretical relations and is therefore more adequate to explain structural continuity between different theories through theory change.

Structural realism, as a form of realism, asserts that our knowledge of the world can best be provided by knowledge of structures, which aim to represent the world adequately, this view is also called the *epistemic* form of structural realism. In a different way, *ontic* structural realism states that the fundamental ontological entities are in fact not individuals but structures. Objects, as we speak usually of them, just occupy places in a structure, in which they occur. One assumption I make is that, by adapting a semantic conception of theories, much of the subject concerned by structural realism changes in an important way, for it would now be the case that the semantic approach is especially useful because it contains an emphasis on structures. One form of semantic views on scientific theories that has been developed, is often known as the *structuralist view of theories*, or the *structuralist meta-theory*. A special emphasis in this dissertation will be on this form of structuralism.

One central research aim of this dissertation is to investigate, by reconstructing model-theoretically some exemplar scientific theories, whether structural continuity between models of different scientific theories does exist. Specifically, I will search for the applicability of structural realism to the field of linguistics. A research of this kind would provide new insight and indicate possible developments for both epistemic and ontic structural realism. As an exemplar case for my reconstruction, I will search for connections between the following three theories that had been developed in linguistics: Leonard Bloomfield's structural linguistics, Zellig Harris' transformational theory and Noam Chomsky's early theory of generative grammar.

In the linguistic scientific community, the common view is that these theories are different in many senses, and that the shift from Bloomfield's theory to Harris' theory, and more famously, the shift from Harris to Chomsky has been a groundbreaking event in the history of linguistics[1]. It seems at first sight that important concepts were introduced in each of the three theories, and that there were radical shifts in the ontologies of each theory. I will analyze this in detail in my dissertation.

What exactly changes from Bloomfield to Harris, and from Harris to the early Chomsky, is not easily recognizable. Specifically, it is not sufficient to provide a conceptual analysis of the theories and their historical development only, as it has been done by Seuren and Newmeyer. From the perspective of the philosophy of science, and especially within the framework of the structuralist meta-theory, it is a core assumption that only a rational and logical reconstruction of empirical theories can provide full insight into both their inner- and their intertheoretical relations. In this sense, I will reconstruct these theories and answer the question whether there is a form of structural continuity from Bloomfield over Harris to the early Chomsky, or not. If any structural continuity can be found, it will give us reasons to argue for structural realism, in this specific case for structural continuity in linguistics.

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[1]See Seuren's 1998 and Newmeyer's 1986 and 1996 analysis of this episode of the history of linguistics.

Until now, it has not been investigated whether structural realism also applies to a branch of science like linguistics, where theories predominantly contain qualitative concepts and do not employ a developed mathematical apparatus, as in physics, economics, geology, and many other scientific disciplines. In the sense of Ladyman and Ross (2007), I will also argue for the applicability of structural realism to non-mathematized fields of science. The logical reconstruction of the above mentioned theories of linguistics will give further results and, if successful, it can strengthen a version of structural realism that is focussed on the notion of structural continuity. It would be possible to show its applicability to the field of linguistics and therefore also to theories radically different from those of physics.

With this, the immediate question of a certain "primacy" of physics as *the* exemplar science arises. I will discuss in which sense it might be fruitful to search for structural continuities in most fields of empirical science or whether it might be more plausible to argue for a form of reductionism to physics, or at least of a primacy, which physics might occupy. The complex question of reductionism plays an important role because we might think of fields of empirical inquiry that are far different from physics, as linguistics. In consequence, it might become extremely difficult to motivate a "primacy of physics position" and to show in which sense a linguistic theory might be reducible to physics, or at least somehow "dominated" by physical theories.

In chapter 2, I will trace the origin of the debate on structural realism by outlining and discussing problems of traditional scientific realism. I will present the main obstacles for scientific realism. Furthermore, the historical development of structural realism will be discussed. I will present the views of Poincaré, Russell, and with great detail, the view of Carnap and his early epistemology.

Chapter 3 of this dissertation concerns the formal framework in which I aim to work, *i.e.* the structuralist meta-theory. I present the formal framework of this view, before I present a discussion of the historical development of this view, and how the early epistemology of Carnap and Kuhn's views on scientific change influenced this view. I argue that these influences make the structuralist meta-theory already a predestined view for applying it to questions of structural realism and theory change. After this, I outline one formal notion of this view (the notion of reduction). I argue that for the ellucidation of structural continuity, it will be especially helpful to employ this notion.

After that, I will discuss the so-called Newman objection in chapter 4. This objection presents a core problem for structural realism. As will become clear below, I present a way out of the Newman objection, by modifying structural realism in a pragmatic way.

Chapter 5 addresses exclusively the view of ontic structural realism. I discuss this view and show how it could be formulated plausibly for linguistic theory. For this purpose, I will argue that ontic structural realism can be formulated in the framework of Harris' transformational theory. In this chapter, I also respond to a critique that has been made against structural realism in the special sciences.

In chapter 6, I present the already mentioned case study of theory change from the discipline of linguistics. There, I first present arguments which support my view that I chose the case study. Generally, the phase of theory change in linguistics I have chosen presents precisely a phase of theoretical development when a scientific discipline passes from a premature status, to a status of full maturity, as I will show. After this, I present a conceptual and logical reconstruction of the three theories under issue. I will show the logical structure of these theories, by representing them in the framework of the structuralist meta-theory. For the case of the reconstruction of Bloomfield's theory, I also show that Bloomfield's theoretical approach is strongly analogous to Carnap's method of quasianalysis, developed in the *Aufbau*. After the structuralist reconstruction, I will show how structural continuities between these theories obtain, by referring to the formal notion of reduction.

After this, I will respond to open questions and critiques that have been made to structural realism in chapter 7. I have characterized the most important critiques that have been made to structural realism, and my aim will be to respond each of these critiques in detail.

In chapter 8, I discuss how structural realism leads us to a form of *neutralism* with respect to the debate on scientific realism. I will argue that, after moving to the camp of structural realism, and after employing formal tools for the description of the structure of our knowledge about scientific theories, one should take a neutral stance in the debate. In this sense, I propose to move first from scientific realism to a form of structural realism, then to a form of neutralism.

Chapter 2

Towards structural realism

2.1 Introduction

Worrall's (1989) approach is the locus classicus to the debate on structural realism in contemporary philosophy of science. This view affirms the following. Given that our theories change, what is retained is their structural content and that there is structural continuity between our theories, even through revolutionary theory change. After Worrall's original proposal, there have been several developments concerning a formulation of mainly two views within the debate on structural realism, so-called epistemic and ontic structural realism. The epistemic form of structural realism affirms that our epistemic access to the world occurs through structural descriptions and that all our knowledge of the unobservable world is structural. Historically, this view can already be found in Poincaré (1905 and 1913), Russell (1912, 1927), Carnap (1928), or Maxwell (1962), amongst others. According to ontic structural realism, all that exists *is* structure and individual objects do not exist independently of the structures in which they appear. For the ontic structural realist, the ontologically basic entities are structures, not objects. On the one hand, there is a discussion about structural realism that focusses completely on contemporary physics, for instance Lyre (2004), Esfeld (2004), or French and Ladyman (2003). On the other hand, works from Psillos (2001, 2006), Worrall (1989), or Votsis (2003, 2004) approach it in a way that clearly has a focus on general philosophy of science and epistemology.

In this chapter, I will discuss and outline the historical development of structural realism, before providing a general characterization of structural realism. For this purpose, I will first discuss the development of the scientific realism debate, and argue that the formulation of several open questions in this debate lead to structural realism in its modern version. Secondly, I will discuss the development of early structuralist epistemologies, specifically those of Poincaré and Russell, and with more detail, the position of Carnap. I end this chapter with a brief characterization of structural realism.

In a first approach, we can say that structuralism is a position that gives more importance to relations than to objects. We can think of a structure as a system of relations. A structuralist position is then one which is more interested in the whole system of relations than in single objects, one that emphasizes on the whole structure. For a structuralist, it is the case that one cannot study whatever domain of knowledge by individuating single objects and not taking into account the whole structure. There exist many different *structuralisms*, in fields such diverse as linguistics, literary theory, psychology, anthropology or philosophy. This first intuitive understanding of structuralism is what can be called a starting point for all forms of structuralism. Soon, it gets more concrete and the different forms depart quickly from each other.

Whenever I will talk about a structure, I will rely on the standard formal notion that can be found in logic textbooks: A structure, in a precise sense, is the following entity: $\langle D_1, ..., D_m, R_1, ..., R_n \rangle$. The D_i are so-called basic sets and the R_j are relations constructed on these sets. The D_i contain what is taken to be the ontology, *i.e.* they contain the *objects* of which one speaks. Note that these objects are part of a structure. The existence of the entities which are taken to be the elements of the basic domains of our structures is merely a posit. For the structuralist, objects do only exist within these domains, which are itself a constitutive part of a structure and specified by the R_j, which are usually functions. As a simple example, one could think of objects such as chairs, as elements of a domain D, and then define relations R_1 to R_n that describe in which spatial relation the chairs are located to each other. This would be a structural description of our knowledge of the spatial location of these chairs.

2.2 Scientific realism and structural realism

2.2.1 Components of scientific realism

To begin this section, I will characterize the components of traditional scientific realism. As usually formulated (see Putnam 1975a and 1975b, Boyd 1983, or Psillos 1999), scientific realism is presented as a view about the aim of science, with accompanying semantic and epistemological theses. Scientific realism is the view that our scientific theories are true (or at least approximately true) descriptions of the relevant aspects of the external universe. As is well known, the classical formulation of scientific realism involves the combination of three different, simpler realist positions, namely, metaphysical, semantic, and epistemological realism. For a comprehensive formulation of scientific realism, these three components are needed. The first component, *metaphysical realism*, is the most general and intuitively the most basic feature of realism.

Metaphysical realism: According to this doctrine, the world exists mind-independently. It is not constituted by our imagination or our inquiries, it is not a construction or a creation of our thoughts. Metaphysical realism is not only presupposed by all scientific realists, it is normally assumed also by standard antirealists.

Semantic realism: This view states that all terms of a (scientific) language refer, whether they stand for observable or unobservable entities.

Epistemological realism: On this view, scientific theories provide an accurate description of the world. In this sense, we obtain knowledge of the world (in part) through the accounts provided by our scientific theories, which in turn depend on particular interpretations of these theories and their conceptual underpinnings.

The term 'approximately true' is used, because scientific realists acknowledge the fact that our scientific theories change over the course of time, sometimes they are replaced by radically different successor theories. To accept a scientific theory is not the same as believing that its truth is certain. It should also be clear that our scientific theories are open to refutation by rival theories at all time, or by the discovery of some new evidence that can be incompatible with a currently held theory. Scientific realism usually applies degrees of acceptance to theories. For example, to accept the scientific theory of molecular biology is not the same as believing that it is impossible for the theory to be false,. Instead, it means to assign to it a high degree of acceptance, since it is a theory that gives truthful predictions and that explains certain phenomena.

Furthermore, scientific claims are always open to falsification. In scientific realism, it is believed that our successful theories reflect the true “nature" of reality. In this sense, all scientific realists are aware of the fact that our theories are fallible, but nevertheless sometimes we gain correct accounts of how the universe is.

Scientific theories can be wrong, but they still give us an accurate model of what there is. In the nineteen-seventies of the twentieth century, scientific realism was famously developed by Hilary Putnam (1975b). At his time, Putnam aimed to defend a realist picture against historicist, relativist and constructivist arguments by Thomas Kuhn (1962) and Paul Feyerabend (1975), amongst others.

Putnam (1975b: 69-73) argued for scientific realism by claiming that the following central assumptions are true:

1. Sentences of our scientific theories make true or false claims. These sentences are true because of something external to us.

2. The terms of our scientific theories typically refer.

3. In a mature science, accepted theories are taken to be at least approximately true.

4. With our theoretical terms, we can refer to the same entity, even when it occurs in different theories.

One further adherent of classical scientific realism is Richard Boyd. According to him, reality is prior to thought, and the theories of mature sciences (such as physics or biology) approximately represent the features of the world. We can think of such a representation by means of a successful act of reference. We will now take a look at several core assumptions and also problems for scientific realism. This is required for the purpose of formulating structural realism and for the understanding of its development out of this context.

2.2.2 The no-miracles-argument

The main arguments for scientific realism make use of the so-called "Inference to the best explanation" (IBE). Ladyman and Ross (2007: 69) explain these arguments as the 'local' and 'global' appeals to the IBE to defend scientific realism. This means that, once we make an inference, we seek to explain a fact, by positing another fact, where the first is a sufficient condition for the second one, and the first is the 'best' option from the set of sufficient conditions for the second. This criterion gives plenty of room for interpretation, because there might not be a consensus regarding the question of what the 'best' option from the set of sufficient conditions for the second inference is.

In oder to determine which are the 'best' options, one usually makes use of notions such as coherence with other data, or plausibility. These criteria are similar to criteria of the debate of theory choice in epistemology. IBE-arguments are clearly not sufficient for the scientific realist, as is famously addressed by one of the most important opponents of realism, Bas van Fraassen (1980). He argues that any theory can in principle be interpreted in pragmatic terms such as coherence or plausibility, and that we are not obliged to infer that a given theory is true, only because it is the best explanation for some phenomena. Any given theory could also be regarded as an empirically adequate explanation of the phenomena. Such an empirically adequate explanation is an explanation that accounts for the empirical data "as if it were true". This would be more elegant than the realist view, given that one need not commit to the theory being true. One would only continue to investigate the domain with the resources of the theory at hand, and remain agnostic regarding the existence of the objects and processes of the theory.

Let us turn the focus now to what has been known as the strongest argument in favour of scientific realism (according to Psillos 1999 and Worrall 1989), the so-called *no-miracles argument* (NMA): In the discussion about scientific realism, the NMA is known as a strong argument for standard scientific realism. This global version of an IBE-argument goes back to Smart (1963, 1979), Putnam (1975b) and Boyd (1983). As a logically valid argument, the NMA can be formulated as follows:

(i) Our empirical theories are successful.

(ii) If our empirical theories are successful and our empirical theories are false, then it is a miracle that our empirical theories are successful.

(iii) It is not a miracle that our empirical theories are successful.

∴ Therefore, our empirical theories are true.

The basic assumption that is made by the NMA is that it would seem a complete miracle if we would obtain correct predictions from false theories, if for example, an airplane would fly, but the theory that the engineer would use to build it would be false, etc. The NMA assumes implicitly that science has formulated already plenty of successful theories. The issue of novel prediction needs to be scrutinized further, since it is in principle possible that a theory gives us accurate descriptions and correct predictions merely by chance. But NMA is not explaining the fact that our scientific theories can in fact produce novel predictions. For a scientific realist, if a theory gives us a correct model of reality, it follows that it will also entail novel predictions. But for the anti-realist, on the other side, this is not clear. From the anti-realist perspective, it is required that one gives an alternative explanantion.

The scientific realist could argue that our scientific theories should be understood to consist of two parts. First, there is an empirical part that collects the data from the phenomena, so to speak. The other part could be called theoretical. In this part, we build a model of the phenomena, which connects the results of our measurements and attempts to explain them. If the theoretical part of the theory contains a correct explanation, the predictions that are entailed by the theory will be correct as well.

The fact that our theories produce novel predictions is widely accepted. In order to illustrate this, we can think of a new piece of technology as a particular predictive success of the theories on which its construction is based. For example, an airplane would not fly, or a smartphone would not work, if certain underlying theories of fundamental physics would not be correct. In a sense, the inventor of a new technological device predicts that a certain assemblance of parts will construct a device that will have certain functions. In this sense, technology gives us many concrete examples of novel predictions in science.

An objection to NMA

One of the most important criticizers of scientific realism, Bas van Fraassen (1980: 38), rephrases NMA as follows: Since science needs to explain its success and certain regularities in the world need to be explained as well, the remaining question is if the realist explanation for the success of science is the only non-miraculous explanation that is available. He suggests that one could also think of a Darwinian explanantion, and explain this success without committing to scientific realism.

From the Darwinian account, science is successful in a selectionist sense. This means that, analogously to evolutionary biology, where certain organisms with more favourable traits than others produce more offspring than those with less favourable traits, the dynamics of scientific theories work the same way. During the course of time, favourable traits proliferate, and the less favourable traits disappear. At the end of an evolutionary process, one can observe a set of organisms with many favourable traits. Analogously, science gives us many predictive theories, and those with the most favourable traits will proliferate as well. Traits in this sense would be virtues such as coherence, elegance, economy, explanatorial success or simplicty, amongst others. In this sense science is full of predictive theories, but this does not mean that we are in need of a realist interpretation, or a miracle. According to van Fraassen, it only means that the success of science can be explained as well in a Darwinian sense, without a need of NMA. There is no need of considering it miraculous that there are rival theories. Why not think that there are many theories out there which can give us correct predictions, but that some of them are, “evolutionarily speaking", more fit than others?

We have seen that NMA gives scientific realism a strong ground, but that there is also a way to criticize NMA, as van Fraassen did. However, I consider van Fraassen's critique of scientific realism as weak, since it stays at a metaphorical level with the Darwinian account. His critique falls short of giving more detail. In any case, NMA itself is not enough if one aims to defend scientific realism. Unfortunately, there are still more problems for scientific realism that would need to be addressed, if one wants to defend this view.

2.2.3 Holism and underdetermination

The discussion on the general problem of underdetermination of theories by data is older than the philosophy of science. In a sense it can be understood as a modern and scientistic version of a *Cartesian Demon*, so to speak. In more recent times, John Stuart Mill was the first who formulated the problem of underdetermination, a problem which has been famously formulated by Willard Van Orman Quine in the proper context of epistemology and the philosophy of science. Mill states:

> Most thinkers of any degree of sobriety allow, that an hypothesis ... is not to be received as probably true because it accounts for all the known phenomena, since this is a condition sometimes fulfilled tolerably well by two conflicting hypotheses ... while there are probably a thousand more which are equally possible, but which, for want of anything analogous in our experience, our minds are unfitted to conceive. ([1867] 1900: 328)

For standard scientific realism, certain theoretical mechanisms within a successful scientific theory help us to refer to “reality" successfully. The problem of underdetermination undermines this realist thesis. Following this argument, it is not possible to specify one single theory, but many of them. This means that we are in principle not able to formulate one particular theory that reflects reality. This argument goes back to Quine (1975) and we can explicate it as follows.

According to Quine, for every observation in science, there is the possibility to formulate an infinte number of theories and these theories can in principle be formulated in a way that they are contradicting each other. Let us think of a hypothesis H, and of an observational statement B. If we aim to revise this hypothesis, we can infer a prediction from this hypothesis, let us call this prediction P, such that $H \Rightarrow B$. We observe in the following step if B is in fact the case. If B does not obtain, we can infer the falsity of H, by *modus tollens*. However, in order to infer B, we need background assumptions. This means that we are not able to infer from the falsity of the observational statement B the falsity of the hypothesis H. We are only in the position to infer the falsity of H and at least one background assumption A. Formally speaking, this means the following:

$$\cfrac{\cfrac{H \wedge A_1 \wedge \cdots \wedge A_n \Rightarrow B \qquad \neg B}{\neg(H \wedge A_1 \wedge \cdots \wedge A_n)}}{\neg H \vee \neg A_1 \vee \cdots \vee \neg A_n}$$

Quine argues that we can never pick out a hypothesis in isolation from others, but only in the context of *the whole* of a system of background assumptions. This thesis is widely known as the Duhem-Quine-Thesis, going back to Pierre Duhem (1914) as well as to Quine. Despite the fact that Quine's critique is now older than half a century, the problem of underdetermination is still an open question. Holism as a thesis has proven to be sustainable, but in a restricted way, since Quine's original form was too radical (see Fodor and Lepore 1992, and Moulines 1986 for an exhaustive discussion of holism).

For a scientific realist, the thesis of underdetermination presents a problem. Since there is always a logical possibility for this problem to arise, we can not put it beside in our debate on scientific realism. The main consequence of this is that the scientific realist is not justified in asserting that one particular theory uniquely reflects reality, but several of them.

In order to overcome the problem of underdetermination, it is useful to distinguish between weak and strong underdetermination. The first case is called weak, since it occurs only in situations when a theory is consistent with all the evidence, and another theory is consistent with the same evidence as well. But since it is usually about two theories that are both consistent with the evidence, one is not driven to reject scientific realism. Weak underdetermination is not strong enough, since one could easily choose between two theories. One theory might just be fulfilling an epistemic virtue in a better way, and the scientist can easily choose. Theories that are weakly empirically equivalent can be distinguished by such a move.

In the case of strong underdetermination, this problem becomes bigger. The stronger form of underdetermination relies on a stronger notion of empirical equivalence, where this latter notion is understood as follows. Strong empirical equivalence means that two theories produce exactly the same empirical consequences, including all possible future data, not only the actual ones. More precisely, this means that for every theory there exists an infinite number of strongly equivalent, but incompatible theories. There is no way that any evidence could ever distinguish these theories. In this way, theory-choice is not possible anymore. Thus, it follows that there is in fact no way of justifying the selection of a particular theory as *the true* representation of the world.

Objections to the problem of underdetermination

However, the scientific realist can defend her view by alluding to the following problems for underdetermination. First, from the fact that theories can have the same evidential support, it does not follow that they also have the same *epistemic* virtues, such as their simplicity, the explanatory power, elegance, etc. In this sense, one could still choose between theories. However, this appears to be too weak for a fully fledged realist, and these epistemic virtues can also be used by an instrumentalist or a constructive empiricist, such as van Fraassen.

Secondly, when we test our theories, we are in fact able to use auxiliary hypotheses in our derivations. By doing so, the empirical equivalence of our theories may be affected as well. Quine showed us that it is highly plausible to understand theories as complex constructs, normally including many auxiliary hypotheses. In this sense, one can think of two theories that are empirically equivalent at first sight, and taken in isolation. But once the respective auxiliary hypotheses are included in the evaluation of these theories, they might turn out to be actually different.

Thirdly, for two theories to be empirically equivalent, it is required that we have a clear distinction between the observable and the unobservable consequences of a theory. In fact, the distinction between the observable and the unobservable is not always sharp and can sometimes not be established. In this sense, on could undermine the problem of underdetermination by just appealing to this fact.

Despite these possible answers to the problem of underdetermination, it is not entirely clear that we are still scientific realists if we approach underdetermination in one of these ways. After all, the standard scientific realist picture is significantly weakened. I will offer an answer to the problem of underdetermination from the perspective of structural realism in chapter 7.

Concerning the debate on underdetermination and epistemic virtues, one problem is that scientific theories are distinguishable by these virtues, even if the empirical equivalence between theories tells us otherwise. For instance, we might even prefer a theory with less empirical relevance over another one, only because it might have a higher explanatory power, or more elegance or simplicity. In this sense, relying on such epistemic or superempirical virtues does not appear to help the scientific realist. An anti-realist could use these virtues as well, since from that perspective, such virtues can be seen as being entirely pragmatic, and in this sense not realist. We could choose between theories without any reference to the notion of truth. In this sense, underdetermination cannot be fully undermined by appealing to epistemic virtues from a realist perspective.

2.2.4 Theory change

In the scientific realism debate, what has been taken to be a strong indicator against scientific realism, is the fact that our theories change over the course of time (see Laudan 1981). Before the challenge of answering issues connected with theory change, scientific realism could be defended in its classical way by the NMA. To recall it quickly, the central claim of the NMA is that the best explanation for the predictive success of our theories is that they are (at least approximately) true, for it would be a miracle if the predictions would be correct and rely on false theories. However, Laudan challenges NMA by arguing that there is no necessary connection between reference and predictive success. A theory can be predictively successful, but still not refer to anything, Laudan argues. Furthermore, he argues that approximate truth cannot be defined coherently. He also argues that there are many cases in the history of science where theories that were believed to be true turned out as false. This last part of Laudan's arguments is famously known as the pessimistic meta-induction (PMI).

The pessimistic meta-induction

Laudan (1981) formulated PMI and referred to the history of science. He argues that by looking at the historical development of our scientific theories, it is clearly recognizable that the empirical success of a theory does not entail its (approximate) truth. His argument can be stated as follows:

(i) In the course of history, all our past empirical theories have turned out to be false. They have always been abandoned and/or replaced by successor theories.

(ii) If this has always been the case in the past, it is at least very likely that this will also be the case for actual and future empirical theories which we currently hold as being true/correct.

$\therefore$ Therefore, it is very likely that our currently held empirical theories are not true and that our future empirical theories will turn out to be false as well.

The debate on scientific realism has taken several different directions. By formulating arguments such as the NMA or PMI, the classical scientific realist picture is only upheld if one develops a promising account of reference of scientific terms, as Psillos intents to do (1999). However, theories of reference of scientific terms ultimately have to be accomodated within the group of theories of direct reference (such as Kripke 1970, 1980, or Putnam 1975a, 1975b, 1981), or within the group of its opponents, such as classical theories of indirect reference (Russell, 1905). This leads to different problems in the philosophy of language, and moves the debate into a different branch of philosophy. In the light of this results, in order to defend scientific realism, one needs to come up with a new account of the reference of scientific terms and, furthermore, should be able to answer to the strong problem of underdetermination as well. This task seems nearly impossible, I argue. In this sense, one might be better off with moving towards a stance that has been famously introduced by John Worrall in 1989, namely, structural realism.

Worrall develops structural realism with the background of taking into account what he calls "the best of both worlds", by including parts of both the NMA and PMI. Within the proposal of his version of structural realism, it does not seem a miracle that our theories make correct predictions on the one hand, and it does not seem problematic that our theories often change radically over time on the other hand either.

Before I will discuss contemporary structural realism in Worrall's sense in the following chapters, I will now outline what I consider to be the most influential work that has been done on structural realism and structuralist epistemologies within the analytic tradition. This becomes important if we aim to formulate a version of structural realism that can be upheld in the contemporary debate, and especially in the last chapter of this dissertation, where I argue that structural realism leads to what has been called neutralism by Psillos (1999).

2.3 Early structuralist approaches

In this section, I will outline what I interpret to be the figures which developed structuralist positions in epistemology and in the philosophy of science. These developments gave birth to our contemporary structural realist view.

2.3.1 Poincaré and Russell

We can think of Poincaré as the first structural realist. Of course, the name *structural realism* was introduced many years later to the debate (by Maxwell in 1962). But this does not really change the fact that many of Poincaré's positions concerned with general questions on theory change in the philosophy of science are very similar to contemporary structural realists. Let us consider the following, famous passage:

> At the first blush it seems to us that the theories last only a day and that ruins upon ruins accumulate... But if we look more closely, we see that what thus succumb are the theories properly so called, those that pretend to teach us what things are. But there is in them something which usually survives. If one of them taught us a true relation, this relation is

> definitively acquired, and it will be found again under a new disguise in the other theories which will successively come to reign in place of the old (Poincaré 1913: 351).

Here it becomes clear that Poincaré was already focussing on relations that we can find again through radical theoretical shifts. He refers to *true relations* and states that these will definitely be found again in later theories. This is exactly what was later called *structural continuity* (see Worrall 1989). And it is exactly the polemical point about this position, since what is needed is more detailed case studies of theoretical change where it is shown that such relations are in fact found again. Even more important and clearer, Poincaré states in 1905:

> Fresnel's theory enables us to do today as well as it did before Maxwell's time. The differential equations are always true ... they express relations, and if the equations remain true, it is because the relations preserve their reality. They teach us now, as they did then, that there is such and such a relation between this thing and that; only, the something which we then called motion; we now call electric current. But these are merely names of the images we substituted for the real objects which Nature will hide forever from our eyes (Poincaré 1905: 160-161).

This famous quote is also the passage to which Worrall (1989) refers when he mentions that structural realism can already be found in Poincaré. I fully agree with Worrall that Poincaré was in fact one of the first structural realists, without calling himself so. Anyhow, there is no difference in Poincaré's and Worrall's positions concerning theory change. Both appeal to the continuity of structure. It is also clear that both refer to equations as structures.

A primarily epistemological approach to structuralism was developed by Bertrand Russell. Already in his 1912 *The Problems of Philosophy*, it becomes quite clear that he endorses an epistemic structuralism:

> Assuming that there is physical space, and that it does thus correspond to private spaces, what can we know about it? ... That is to say, we can know nothing of what it is like in itself, but we can know the sort of arrangement of physical objects which results from their spatial relations ... We can know the properties of the relations required to preserve the correspondence with sense-data, but we cannot know the nature of the terms between which the relations hold (*ibid.*: 15-16).

We can see that Russell focusses on a direct epistemic structuralism, by not mentioning any empirical theory concretely, as Poincaré does. It is worth to note that both Poincaré and Russell mention a *nature* or *reality* of the things as they are, which is unknowable. This is a strong similarity to what Kant called the *Ding an sich*, which was also unknowable in his epistemology. Though it might not be possible to defend the view that even Kant was a structural realist, the similarity between these authors is remarkable and it can at least be said that Poincaré and Russell were at least influenced by what could be called a Kantian *air*. More obviously and less surprising, the same holds for Carnap, as we will see in the following section.

2.3.2 Carnap: Objectivity and structure

The early philosophy of Rudolf Carnap suggests a structuralist epistemology as well. Carnap calls his project in the *Aufbau* constitutional system. Carnap's idea was to extend the logicist project of reduction of all mathematics to logic to the field of empirical science. According to Carnap, a pure relational theory is best applied with his constitutional system.

For his project, what counts as *real* is the content of our basic experiences. This reality can only be individuated through the introduction of formal operators that build objects. It is through the construction of classes and relations, how this succeeds. By applying this procedure of analysis, different types of objects are constituted. These types of objects represent the different domains of the particular sciences.

According to Carnap, all propositions of science refer to structural properties of the relations that define their objects. To this, one might immediately object that in mathematics, we formulate propositions only about structures, but the empirical sciences have to be capable of giving criteria for distinguishing the different entities they speak of. This means that the propositions of empirical science need to serve as criteria for individuating particular phenomena. By only giving a structural description, we are not able to do so, it appears. Carnap's aim was precisely to develop a system for the description of objects where one is able to individuate the objects of a domain via purely structural descriptions.

Carnap mentions that the purpose of the constitutional system is not only to classify concepts, but to construct all concepts of science from certain basic concepts. Its task is to establish the nature of each concept that belongs to what we call science. In this sense, constitution is a form of reduction. The logically complex things are constituted by logically simpler ones. The idea of reduction consists of finding a general rule that indicates the way in which a statement about one object can be transformed to a proposition about other objects.

Influences on Carnap's structuralism

Carnap's structuralist epistemology was influenced by several other authors. In this section, I will outline and discuss those influences. Michael Friedman (1999) explains the historical context of Carnap's early structuralism as follows:

> Carnap's conception of knowledge and meaning is Kantian . . . objective meaning can only be derived "from above" - from formal or structural relations within the entire system of knowledge. The conception plays an important role, for example, in Moritz Schlick's (1918) Allgemeine Erkenntnislehre, Russell's (1927) The Analysis of Matter . . . All of these works disengage meaning and knowledge from ostension, and lodge them instead in the system of logical relationships among our concepts . . . All of these writers, including Carnap, are clearly indebted to the notion of "implicit definition" deriving from Hilbert's (1899) axiomatization of Euclidean geometry (*ibid.*: 99-100).

What Friedman calls "disengage" meaning from ostension is a central aspect in Carnap's structuralism. In the *Aufbau*, §16, Carnap introduces his purely structural definite descriptions of knowledge. Following him, only such descriptions guarantee objectivity:

> Every scientific statement can in principle be so transformed that it is only a structural statement. ... For science wants to speak about the objective; however, everything that does not belong to the structure but to the material, everything that is ostended concretely, is in the end subjective ... If we aim, in spite of this, at agreement in the names given for the objects constituted on the basis of the experiences, then this cannot occur through reference to the completely diverging material but only through the formal indicators of the object-structures (*ibid.*: all translations from the *Aufbau* in this dissertation are mine.).

By disconnecting descriptions from ostension and by introducing his purely structural definite descriptions, Carnap aims to provide a method that guarantees objectivity in the language of science. Whatever is experienced and then formulated as a statement, is only described in terms of purely structural definite descriptions, *i.e.* statements about its relation to other events.

This bears an interesting connection to David Hilbert. In his formalization of geometry, Hilbert said that concepts such as 'point' or 'line' were whatever make the axioms of euclidian geometry true. In this way, the concepts used in the axioms are implicitly defined by general truths that hold of these concepts. Hilberts method of implicitly defining consisted in the postulation of certain axioms. In these axioms, there appear concepts that are not independently defined. They state that those concepts are simply whatever makes those axioms true. For example, 'point' and 'line' are partially defined by the general truth that, for any two distinct points there is a line, such that those points lie on that line. Implicit definition determines concepts in relation to one another. But implicit definitions are not meant to be used for the determination of the connection between such concepts and empirical reality. In other words: The concepts mentioned in the axioms are defined by their role in the axioms.

As an early logical empiricist witness for Hilbert's influence, I mention Schlick's *General Theory of Knowledge*, where concepts of geometry are understood as entities that are bearers of the relations laid down by the system of axioms. Schlick (1918) makes it clear that implicit definitions are in no connection to empirical reality:

> Implicit definition, . . . never stands in community or connection with reality, it denies this intentionally and in principle, it remains in the realm of concepts. A framework of truths constructed with the help of implicit definition never rests on the ground of reality, but, as it were, floats free, bearing, like the solar system, the guarantee of its stability within itself. None of the concepts appearing therein designate, in the theory, a real thing; rather, they mutually designate one another in such a way that the meaning of one concept consists in a determinate constellation of a number of the others (*ibid.*: §7, p. 37; the translation is mine.).

The mere abstract mathematical structure is given by Hilbertian implicit definitions, but Carnap's purely structural definite descriptions are explicit definitions: they pick out a definite object in the domain of the constitutional system by specifying it as the unique such object satisfying purely formal conditions (see also Richardson 1997 for this). Carnap was certainly inspired by implicit definitions. Nevertheless, with his purely structural definite descriptions, he aimed to distinguish his method from Schlick's and Hilbert's, since he also pursued a different aim, *i.e.* the description of a purely empirical domain by making use of purely structural descriptions.

In the *Aufbau*, the empirical domain is given as the set of elementary experiences on which the basic relation of "recollection of similarity" (Ähnlichkeitserinnerung) is defined. In contrast to a system of implicit definitions, then, Carnap's constitutional system is concerned with the description of the empirical world from the beginning. By relying on the fundamental relations (Grundrelationen) (§75), Carnap argues that by taking these as the undefined basic concepts of his constitutional system, it becomes possible to unify positivism and neo-kantianism:

> The achievement of the revelation of the necessary basis of the constitutional system is therefore due to two totally different philosophical positions, which are often hostile to each other. Positivism highlighted that the only material of knowledge lies in the unprocessed and knowledgeable given; the fundamental elements of the constitutional system are to be searched there. Transcendental idealism, especially in the style of neo-kantianism (Rickert, Cassirer, Bauch) has argued rightly that these elements do not suffice; because posits of order need to be added, our "fundamental relations" (*ibid.*: §75).

Carnap's neo-Kantian motivation for his system in the *Aufbau* becomes clearer here, §177:

> Constitutional theory and transcendental idealism agree in representing the following position: all objects of cognition are constituted and, the constituted objects are only objects of conceptual cognition in the sense that they are logical forms, constructed in a determinate way. The same holds finally also for the fundamental elements of the constitutional system. They are put as undecomposable units at the basis, but then, as the constitutional system proceeds, they become occupied (belegt) with several properties and decomposed into quasi-components (*ibid.*).

Carnap aimed to overcome traditional philosophical disputes with his constitutional theory, (see his §5) where he argued that his theory uses a neutral language and does not fall into either, realism or idealism. He argues for a neutralist stance in such philosophical disputes. Still, the introduction of the fundamental relation is inspired by the neo-kantians. Carnap's neutralism and its relation to structural realism and to the structuralist meta-theory will be discussed in detail in chapter 8 of this dissertation.

2.4 Structural realism characterized

After having discussed the development of structural realism, I will now characterize the general view of structural realism, as it is widely accepted in a standard version. It should be clear that besides Poincaré, Russell and Carnap, there have been many other authors that made significant contributions to the development of structural realism, such as Grover Maxwell (1962). For the purpose of this dissertation, I only mentioned these three authors, since I consider only their views to be directly relevant to my own work.

However, despite a considerable sophistication and bifurcation of the debate on structural realism (see Frigg and Votsis 2011, or French 2014), I think that there are still two main currents of this view, *i.e.* the ontic and the epistemic current. On the one hand, the epistemic variant of structural realism (abbreviated as ESR) can be expressed as follows:

All we can know of the unobservable world is its structure.

On the other hand, there is the perhaps more controversial and metaphysically more polemical view of ontic structural realism (abbreviated as OSR), which states that:

All that exists at the ontologically most fundamental level is structure, and there are no individual objects.

After having characterized structural realism and after having shown where one should place its development in the history of the philosophy of science, I will discuss what is often seen as the main problem for structural realism, the so-called Newman objection in chapter 4. In what follows, I will outline the framework of the structuralist meta-theory and its possible applicability to questions of structural realism.

Chapter 3

The structuralist meta-theory

In this section, I will outline and present the formal framework of the structuralist meta-theory. In this view, empirical theories are not understood as sets of statements closed under deduction, it is a non-statement-view. Moreover, empirical theories are understood as classes of model-theoretic structures. As it is expressed in the following passage:

> The fundamental intuition underlying our approach is that the smallest significant or interesting parts of empirical science - things like empirical laws - are best characterized, not as linguistic entities, but as model-theoretic entities - classes of set-theoretic structures (Balzer, *et al.* 1987: xxi).

First applied and developed by Joseph D. Sneed in his 1971 *The Logical Structure of Mathematical Physics*, this approach was called *Emended Ramsey-View*, since Sneed referred to Ramsey as a predecessor, especially concerning the question of theoretical terms. It was also Wolfgang Stegmüller who developed the structuralist approach, later together with Sneed and others. The central motivation of the development of the structuralist meta-theory was a critical stance towards the received view (or statement-view) of scientific theories, which had been developed and held by philosophers of science since logical positivism. Opposing to the conception that scientific theories should be understood as sets of statements, inferentially connected, Stegmüller (1973/1976) introduced the name *non-statement view* of theories. The main difference to the received view is that in the structuralist meta-theory, scientific theories are axiomatized by means of set-theoretic predicates (this method goes back to Patrick Suppes' *Introduction to Logic*, from 1957).

3.1 The framework

The development of this approach goes back to works with a primary focus on physics by Sneed, and an analysis of the Kuhnian approach to scientific change by Stegmüller. Historically, there have been mainly two influences on the structuralist meta-theory. First, the set-theoretical approach in the philosophy of mathematics, developed by the group of French mathematicians, called "Nicholas Bourbaki" (1970), and second, Suppes' method of defining set-theoretic predicates.

The structuralist meta-theory shares important assumptions about the logical structure of scientific theories with other well known approaches like those of Bas van Fraassen (1980), or Steven French and Newton Da Costa (2003), all being part of the so-called *semantic* conception of scientific theories. All versions of the semantic conception reject the view that scientific theories are best conceived as sets of statements, closed under deduction. The adherents of the different semantic approaches rather argue that an empirical theory is considered to be given by its models.

The structuralist meta-theory has developed detailed case studies of different areas of empirical science, such as physics, biology, chemistry, economics and linguistics, that have been worked out during the last decades (see Diederich *et al.* 1989, 1994 for an overview). But it has also diversified and developed promising approaches to the discussion about the dynamics of theories (see Moulines 2011), theoretical terms and belief revision theory (see Andreas 2011, 2013), and the debate on scientific realism (see Sneed 1983).

As I have already mentioned above, the structuralist meta-theory makes use of set-theoretic predicates. We can characterize a set-theoretic predicate P as a predicate that specifies:

- the type of a structure $\langle D_1, \ldots, D_k, R_1, \ldots, R_n \rangle$, where k is the number of base sets, and n the number of relations;
- the typification of the relations $R_1, \ldots, R_n$;
- the axioms that the relations $R_1, \ldots, R_n$ need to satisfy.

As a result, the structure $\langle D_1, \ldots, D_k, R_1, \ldots, R_n \rangle$ will eventually satisfy the set-theoretic predicate P. This methodological tool allows one to represent the structure of scientific theories. In the structuralist meta-theory in particular, a theory is given by its models, which are sequences of the following form:

$$\langle D_1, ..., D_m, R_1, ..., R_n \rangle$$

The D_i are so-called basic sets and the R_j are relations constructed on these sets. The D_i contain what is taken to be the ontology of the theory, *i.e.* they contain the *objects* assumed by the theory as *real.* The objects in the respective domains are specified by the R_j, which are usually functions. In empirical theories which make use of quantitative tools, they usually are functions mapping empirical objects into the real numbers, or some other mathematical entities.

Potential models

In the structuralist meta-theory, theories are understood as tuples. More specifically, an empirical theory is understood to consist of sets of models: A so-called set of *potential* models (M_p) fixes the general framework, in which an actual model of a theory is characterized. All entities that can be subsumed under the same conceptual framework of a given theory are members of the sets of the potential models of this theory. Let's consider an example of such a potential model, expressed as a set-theoretic predicate, which makes the structure of an empirical theory explicit. We will look at a potential model for *Classical Collision Mechanics* (see Balzer *et al.* 1987: 26-27), which is a simplified system of mechanics, such as a billiard game. We can think of colliding billiard balls:

> A potential model of *Classical Collision Mechanics*: $M_p(CCM) = \langle P, T, \mathbb{R}, v, m \rangle$

1. P is a finite, non-empty set
2. T contains exactly two elements
3. $v : P \times T \rightarrow \mathbb{R}^3$
4. $m : P \rightarrow \mathbb{R}^+$

The intended interpretation of this example is as follows: P is a set of discrete bodies that can be called “particles" (in our example, we can think of two billiard balls), T is a set of two instants, one time instant before the collision, and the other time instant after the collision. v is the velocity function, assigning to each particle p and point of time its velocity as an element of $\mathbb{R}^3$. Velocity is a time-dependent vectorial function whose range are triples of real numbers. It assigns a three-component vector (one component for each direction in space) to each particle at each time. m is the mass function, assigning to each particle its mass.

Theoretical terms

As in many other approaches within the philosophy of science, the question of theoretical terms is addressed within the approach of the structuralist meta-theory. In this approach, the criterion for theoretical terms is relativized to theories. By doing so, its adherents aim to escape some of the problems that other approaches to theoretical terms have (as famously discussed in Sneed, 1971). The essential point in Sneed’s criterion is that terms can be theoretical for one theory, but non-theoretical in other theories. In the structuralist meta-theory, “The "empirical basis" of a given theory T should include those concepts which, in a sense still to be made precise, are not specific for T. T’s other concepts will belong to T’s theoretical "superstructure" (*ibid.*: 48)". This criterion relativizes the notion of theoreticity and makes the status of theoreticity of a term always dependent on a concrete empirical theory, in which it occurs. More specifically, the structuralist criterion for theoreticity is as follows: A term is T-theoretical if the set of all T-determining models is included in $M(T)$. More loosely, we can say that a term is theoretical if it can only be determined if the fundamental laws of a theory are presupposed.

Partial potential models

Once the potential models are determined, the next step is to determine the so-called *partial* potential models (M_{pp}). Partial potential models represent the framework for the corroboration or refutation of the theory in question, they represent the framework of data, which shall corroborate or refute a theory. The concepts in M_{pp} can be determined independently of a theory. Terms which are theoretical (and proper to T) in the potential models of the respective theory are cut out. In other words, this means that partial potential models are the pure data-models within the framework of the structuralist meta-theory. These sets of models do not contain any theoretical terms or functions.

Actual models

Models which belong and which in addition satisfy the laws of a theory, are called *actual* models (M) of a theory. The following is an example of an actual model, (Balzer *et al.* 1987: 27):

> x is an actual model of classical collision mechanics ($x \in M(CCM)$) if and only if there exist $P, T, \mathbb{R}, v, m$, such that:

1. $x \in M_p(CCM)$
2. $\sum_{p \in P} m(p) \cdot v(p, t_1) = \sum_{p \in P} m(p) \cdot v(p, t_2)$.

In the actual model of CCM, the fundamental law of this theory is added, the *law of conservation of momentum*, expressing that the sum of the products of mass and velocity of each particle must remain the same before and after a collision.

Global constraints

Furthermore, local applications of a scientific theory may overlap in space and time. For this purpose, the formal notion of *global constraint* is introduced. Global constraints (GC) are formal requirements that restrict the components of a model in dependence of other components of other models. Constraints express physical or real connections between different applications of a theory, *i.e.* the *inner*-theoretical relations. To explain it intuitively, we can think of a physical object that is part of a system. This object, say, a certain train wagon, must have the same weight, no matter to which physical system that wagon belongs. It may stand on a railroad somewhere in Nebraska, or on a railroad close to Berlin. The same wagon will have the same weight, if we think of physical systems on earth (and under idealized conditions). Because of such overlaps, the notion of constraint is required in the structuralist meta-theory. In what follows, I will give the formal definition of a constraint in the framework of the structuralist meta-theory.

> The definition of a constraint, (see Balzer *et al.* 1987: 47) is as follows:
>
> If M_p is a class of potential models, then

(a) C is a constraint for M_p, iff

1. $C \subseteq \mathcal{P}(M_p)$

2. $C \neq \emptyset$ and $\emptyset \notin C$

3. $\forall x \in M_p : \{x\} \in C$

Some empirical theories deal with the same or very similar domains of objects. Particles will have the same mass in classical collision mechanics and in classical particle mechanics. Anyhow, these are different theories.

Global links

The global links (GL) represent the intertheoretical connections between such theories. They are important for the determination of theoretical concepts, since non-theoretical terms can only be determined by means of other theories which do not presuppose an earlier theory. This has as a consequence that the partial potential models of a theory can get their interpretation and their meaning only through other theories, which in some sense "precede" that theory. The links represent some transfer of information between theories. This information consists mainly of data which are obtained in the course of some determination of a term which is non-theoretical in a theory. Such transfer of data contributes to the interpretation of the partial potential models. It is part of the determination of the meaning of the terms occurring in the partial potential models, and therefore it is an essential component of a theory itself.

To take into account the functions linked with each other we shall not denote these functions directly but rather the places they occupy in the tuples constituting the potential models. I will now quote the definition of a link (Balzer *et al.* 1987: 61):

It is expedient to introduce the following notation: For any given natural numbers $i_1, \ldots, i_n$ let $\pi(T, i_1, \ldots, i_n)$ denote the class of all tuples $\langle R_{i_1}, \ldots, R_{i_n} \rangle$ for which there is some $x \in M_p(T)$ such that for $j = 1, \ldots, n : R_{i_j} = R^x_{i_j}$

That is, $\pi(T, i_1, \ldots, i_n)$ is the set of all sequences of functions (or predicates) appearing in the places $i_1, \ldots, i_n$ of potential models of T.

(a) L is an abstract link from M_p to M'_p, iff $L \subseteq M_p \times M'_p$

(b) L is a (concrete) link between M_p and M'_p, iff:

1. M_p and M'_p are classes of potential models with m and m' relations, respectively
2. There are $i_1, \ldots, i_s \in \{1, \ldots, m\}$ and $j_1, \ldots, j_t \in \{1, \ldots, m'\}$ such that
3. $L \subseteq M_p \times \pi(T, i_1, \ldots, i_s) \times M'_p \times \pi(T', j_1, \ldots, j_t)$

After having introduced the core parts of what an empirical theory is taken to be, within the structuralist meta-theory, I will pass on to present the global concept of an empirical theory in this approach.

3.1.1 The concept of an empirical theory

In the structuralist meta-theory, an empirical theory T consists of its core K and of the intended applications I. K is itself a complex structure and consists of sets of potential models M_p, partial potential models M_{pp}, actual models M, global constraints GC and the global links GL. In the structuralist meta-theory, a single empirical theory is usually called *theory-element*, since it is always a part of a bigger structure, a so-called *theory-net*. Furthermore *e.g.* the theory-holon of classical mechanics would have several theory-nets and theory-elements, such as classical collision mechanics, rigid body mechanics, and others (see Balzer *et al.*: pp. 167-205 for more detail). A theory is then, formally, the following tuple:

$T = \langle K, I \rangle$, where $K = \langle M_p, M_{pp}, M, GC, GL \rangle$. The I are the sets of the intended applications of a theory. Formally, they are defined as follows:

> Definition 1: I is a set of intended applications for K only if:
>
> (1) $K = \langle M_p, M_{pp}, M, GC, GL \rangle$ is a theory-core.
>
> (2) $I \subseteq M_{pp}$.

The I are not formally characterized. Their determination depends on pragmatic constraints. The pragmatic motivation behind the definition of the intended applications of a theory are best explained by this quote:

> It is most economical and most natural to assume that the intended applications of T have the structure of partial potential models ... This assumption is not only reasonable on the basis of general considerations, it also is backed by studying how scientists talk and how they argue in the context of an "application" of a theory. In the light of the previous discussion M_{pp} might be called the set of all possible applications of a theory. But among these there always will be undesired applications which have really nothing to do with the theory in question ... We can say something precise, namely that an

> intended application is a partial potential model, but we cannot be precise about every feature of intended applications (Balzer *et al.* 1987: 86-87).

This reflects that the authors were aware of trivialization objections to structuralist approaches (see Newman 1928), which is why they decide to take a pragmatic viewpoint on the question of the intended applications. I will elaborate a response to the Newman objection by alluding to this point in chapter 4.

3.1.2 The empirical claim of a theory

In the framework of the structuralist meta-theory, theories have empirical claims. This means that all the intended applications can be supplemented by theoretical terms such that the resulting structures are models. If we want to avoid trivialities, constraints and links become important. This is why normally, mature empirical theories do have links and constraints (as I mentioned above), and are usually understood as *theory-nets*:

> What we need in addition, and what in most cases renders empirical claims non-trivial, are the constraints and links. They express cross-connections among potential models and among theories, and they allow only for combinations of potential models subject to certain restrictions (*ibid.*: 90).

Empirical claims are usually expressed after the logical reconstruction of a particular empirical theory. This is considered to be the formal result of a structuralist reconstruction, and therefore a central part within the whole approach. If no empirical claim is formulated, it is still informative to have a structuralist reconstruction of a specific theory, since the general conceptual framework of any theory can be outlined in clear terms by the tools of this approach (in the case study of linguistics I provide in chapter 6, we will see this with more detail. There, I only formulate an empirical claim for one theory).

3.2 Historical influences

In this section I present a historical investigation of the methodological and conceptual connections that exist between Carnap's early work in the Aufbau, Kuhn's conception of theory change in empirical science and the structuralist meta-theory. This becomes relevant, especially because one aim of this dissertation is to analize the relations between structural realism and the structuralist meta-theory. And since Carnap was a strong influence on both structural realism, and on the structuralist meta-theory, his influence on the structuralist meta-theory is a crucial point that needs to be analyzed. Furthermore, as it will become clear in chapter 8, Carnap's supposed neutralism plays a crucial role in the development of my own neutralist account, and in my interpretation of the ontological commitments of the structuralist meta-theory.

The connection between the structuralist meta-theory and Kuhn's philosophy of science is better known than the connection between Carnap's early work and this view. At first sight, there might not be an obvious connection between Carnap's early work and the structuralist meta-theory, I will argue here that the program of the *Aufbau* should be understood as one mayor influence on Stegmüller and other adherents of the structuralist meta-theory, such as Moulines. This will show that the early structuralism of Carnap has not only a connection with contemporary epistemic structural realism, but also is deeply connected with the methodology of the structuralist meta-theory. Considering this connection, it gives us reason to believe that the structuralist meta-theory has a high affinity to represent structural realism.

I think that Carnap's program of knowledge description from the *Aufbau* is analogous with the structuralist program of knowledge description of scientific theories, in at least two senses. First, from a methodological point of view, it emphasizes on relational descriptions of our knowledge. Second, both Carnap and the adherents of the structuralist program declare themselves to be objective and neutral in questions of realism and antirealism, both generally and more specifically in the philosophy of science. My second thesis is that Kuhn's ideas find their precise formal reformulation in the framework of the structuralist approach.

3.2.1 Carnap's influence

There exist many interpretations of Carnap's *Aufbau*. Clearly, Richardson's (1997) reconstruction stands out for its rigor in clarity and exhaustiveness. Since I will focus on Carnap's structuralist epistemology, I argue that the *Aufbau* is, generally, a program for logically reconstructing our knowledge of the world in structuralist terms. This means that the descriptions of our knowledge are purely structural definite descriptions, as explained in §16.

The principal aim of Carnap's method is, as he says, to provide an objective way of describing our knowledge of the world. Only what can be described in relational terms, can be known objectively. All that is not described in purely relational terms, is in the end subjective and should not be the subject-matter of scientific inquiry. For my purpose, I will now turn the focus on a connection between Carnap's method and the structuralist meta-theory. This is a connection that has been left implicit so far. I argue that this connection constitutes indeed an essential part of the methodological motivation of the structuralist meta-theory. There is only sparse textual evidence of structuralists referring directly to the *Aufbau*. Interestingly, two main figures of the structuralist view, Stegmüller and Moulines, have both written monographies on Carnap's early epistemological program. Early work by Stegmüller (1969) and Moulines (1973) provides a detailed analysis of Carnap. This reveals at least that some main figures of the structuralist view had a strong interest in Carnap's early epistemological pogram.

Considering the date of publication of these works, it is easy to notice that the structuralist meta-theory had been developed later, mainly between 1971 with Sneed's *The Logical Structure of Mathematical Physics*, and closing a first phase of consolidation with Balzer, Moulines, and Sneed's 1987 *An Architectonic for Science*. As one main figure of the structuralist program, Moulines expresses as follows about the Aufbau:

> To be more precise, the use of Carnap's Aufbau I propose here consists in reinterpreting Carnap's "Konstitutionstheorie" as a formal explication of the notion of an ideal observer, *i.e.* an epistemic subject provided with the essential constituents of an ideal "observational language" to check any empirical statement made in theoretical science (Moulines 1991: 265).

This interpretation of Carnap is strongly analogous to the purposes of the structuralist meta-theory. Moulines' interpretation of Carnap's program in the *Aufbau* is similar to the motivation of the structuralist meta-theory in the following sense. Both approaches have as a central aim to explicate the constitution of our experience about the world, with formal methods, and in a scientistic spirit. The early Carnap developed a formal framework in the *Aufbau* that allowed him to provide a system of definitions in purely relational terms. The adherents of the structuralist meta-theory aim for the same, but they start by analyzing empirical theories, not our experience directly, as Carnap did. This is the main difference between both approaches.

The structuralist meta-theory aims mainly to provide a program for analyzing science, by means of a detailed logical analysis of scientific theories. In addition to the logical reconstruction of theories, this approach also intends to describe the social phenomena of the scientific enterprise, by incorporating many of Kuhn's original interests (see Balzer *et al.* 1987). In the framework of the structuralist meta-theory, at no point we can find the postulation of an ideal observer in the sense of Carnap. However, in the structuralist program there exists also the motivation of applying logical tools in order to reconstruct our knowledge of the world in purely structural (*i.e.* relational) terms.

Let us now turn the focus on Carnap's structuralism in the Aufbau. In §66, he gets more concrete on the background motivation and the aim of pursuing an epistemic structuralism:

> How should science come to objectively valid statements, if all its objects are constituted by an individual subject? ...the solution to this problem lies in that of course the material

> of the individual streams of experience is completely diverging ... but certain structural features agree of all streams of experience. Science has to restrict itself to such structural properties, since it aims to be objective. And it can restrict to structural properties, as we have seen earlier, for all the objects of knowledge are not content, but form, and they can be represented as structural entities (*ibid.*).

Following Michael Friedman's (1999) interpretation, I understand the *Aufbau* as an outline of one version of epistemological program of early logical positivism. In the *Aufbau*, as in the structuralist view, there is an emphasis on structural descriptions of our knowledge of the world. In the structuralist meta-theory, these descriptions are made of our empirical theories, but in Carnap's program, structural descriptions are provided directly of our knowledge, as I have already mentioned above.

Still, it is of central importance to acknowledge the fact that both share the view that our knowledge should be best described in form of structures. Where Carnap starts with structurally describing our knowledge of the world, the structuralist meta-theory describes this knowledge indirectly, through the structural description of our empirical theories. Carnap starts at the lowest level, *i.e.* the level of an ideally constructed epistemic agent, but in the structuralist view, the starting level is that of scientific theories.

3.2.2 From Kuhn to Stegmüller

Let us now consider the connections between the structuralist meta-theory and Kuhn's (1962/ 1970) view on the dynamics of theories and of theory change. First, I will briefly mention the core ideas of Kuhn's proposal of theory change, before I outline the connection to the structuralist meta-theory.

A central aspect in Kuhn's work is the concept of scientific community. In Kuhn's system, a scientific community is a group of people that shares and uses the same paradigms. He introduced the notion of normal science as the activity of "puzzle-solving". In normal science, scientific research is guided by a paradigm. Anomalies can occur; this can lead to a crisis in a certain field. Such a crisis can, but must not lead to what Kuhn called extraordinary science.

If a crisis leads to extraordinary science, it happens that one paradigm is substituted by a new one, and, after some time, a scientific revolution occurs. The scientists that were applying the old paradigm cannot successfully communicate with the scientists of the new paradigm anymore. This is what has been famosuly called incommensurability by Kuhn. There is a potential impossibility of successful exchange of information between these groups of scientists, and therefore, an apparent problem for scientific rationality.

Furthermore, Kuhn described the four components of a paradigm as follows:

The four components of a paradigm:

1. **Symbolic generalizations:** In order to comprise knowledge, certain symbols are introduced and generalized. One concrete example are equations as they occur in practically all fields of mature science.

2. **Models:** There are heuristic models and ontological models. Heuristic models are mere fictions; the ontological models do correspond partially with the world. As an example: we usually imagine planets and stars to be actually round celestial bodies, but we do so only for practical reasons.

3. **Values:** The methodological values shall guide the scientific research and raise questions of technological applicability, ethic questions and also questions of the coherence of the research. For instance, certain research areas might not be addressed for ethical reasons (genetic engineering, nano-technology, etc.). Another example is that, normally, scientists and philosophers of science accept the methodological values of theory-simplicity, formal elegance, coherence, and economy.

4. **Exemplars:** These are the paradigmatic applications, the concrete instantiations of a paradigm. Such concrete cases show how a paradigm actually works. These are the especially well working intended applications of a paradigm.

I turn the focus now on the relation between Kuhn and the adherents of the structuralist meta-theory. We will see that, despite Kuhn being the one who influenced the development of the structuralist meta-theory in an important way, it is also him who received a certain influence back. Sneed (1971) explicitly mentions the relation between his work and Kuhn's proposals as follows:

> It is certainly plausible to think that the initial successful application of the core of the theory is essentially the same for

> all those who have the theory. Different people who have the theory at a later time in its development may believe different statements. They may be more or less clever in seeing ways to extend the theory, and more or less successful in convincing their colleagues what evidence supports the claims they make with the theory . . . it is quite clear that Kuhn's thesis strongly suggests that we should modify our notion of what it is to have a theory of mathematical physics so as at least, to require that everyone who has the theory has it "because of" the same initial success (*ibid.*: 292-293).

One can see here that Sneed recognizes the fact that radical theory change can occur in some cases, and that he aims to include it to his framework. Sneed and the other developers of the structuralist meta-theory did exactly this, they modified the notion of what it means to have a theory of mathematical physics. As we have seen above, by identifying a theory with the formal notion of structure, a first step towards such a Kuhnian aim is done. It is clear that one formal representation of a model of classical collision mechanics does not suffice to incorporate Kuhn's thesis of theory change into the structuralist framework, but within the structuralist meta-theory, it was a first step towards an incorporation of Kuhn's ideas. This incorporation became stronger once the structuralist meta-theory developed. Today, there exist several formal notions that serve to model Kuhn's ideas. An example of work in this area is Moulines (2011), where four types of theoretical development are presented, with the mentioning of concrete historical examples.

Let us consider again the interpretation that Sneed gave to Kuhn's proposal:

> Again in Kuhn's terminology, we have said very little about "scientific revolutions" as they occur in mathematical physics. I confess, at the outset, that this is a subject about which I find it extremely difficult to say anything that is both precise and interesting. Nevertheless, the view of the logical structure of theories of mathematical physics I have been defending does appear to have some consequences relevant to such questions . . . (*ibid.*: 296).

Sneed expresses his lack of interest in questions of radical theory change. Nevertheless, he is clear that he thinks it is his apparatus of set-theoretic formal reconstruction of physical theory, that leads to a better understanding of Kuhn.

The relative a priori

The notion of relative a priori has also been shaped by Kuhn, as I will outline in what follows. In the structuralist meta-theory, besides the purely formal aspect, Stegmüller's (1973/1976) notion of a relativized a priori reveals a close connection to Kuhn's ideas. He expresses as follows:

> The reason that we may only speak of a relative a priori is that no core, be it ever so sophisticated and yield it ever so many successful expansions, can be guaranteed never to get caught in an a priori conflict with some future alternative and go down before it because this opponent can "deal with anomalies which it cannot". . . Kant claimed that his theory reconciled rationalism and empiricism, the a priori and

> the empirical components in the scientific process. The reconstruction of Kuhnian theory dynamics with Sneed's conceptual apparatus is perhaps a better candidate for this job (*ibid.*: 218).

One can make an intent and translate this view into the framework of the structuralist meta-theory. This goes as follows. The relativized a priori, would be the theory-core K. And what is actually subject to changes are the intended applications I of a theory. The applications change, but a certain stable core always persists, despite any radical changes. Interestingly, the theory-core K is itself a complex structural entity, built up of complex structures as well. This fact has to be taken into account by structural realism. The framework of the structuralist meta-theory appears as being idoneous for representing structural realism in this way. For the structural realist, "something" structural has to remain, even in phases of radical theory change. This "something" can not only be equations, as Worrall claimed, since structural realism should aim to be applicable as a general view, and not only to highly mathematized scientific disciplines. I propose that, if we adopt the underlying framework of the structuralist meta-theory, the conception of empirical theories as the tuple $\langle K, I \rangle$ gives us an ideal starting point in the debate on structural realism.

Besides Stegmüller, also Friedman (2001) mentions a relativized a priori in the philosophy of science. His conclusion about a stable core, which can also undergo changes, is very much alike to the view advocated by Stegmüller.

Kuhn and formal methods

Furthermore, a connection between Kuhn and the structuralist meta-theory can also be found in a paper by Kuhn (1976), where he refers to the structuralist meta-theory and its influence on his own approach, and recognizes explicitly the enriching contribution to his program provided by Sneed and in a more systematic way by Stegmüller:

> To a far greater extent and also far more naturally than any previous mode of formalization, Sneed's lends itself to the reconstruction of theory dynamics, the process by which theories change and grow . . . Sneed also suggests and Stegmüller elaborates the possibility that at least some cases of change of core correspond to what I have called scientific revolutions . . . Though the Sneed formalism does permit the existence of revolutions, it currently does virtually nothing to clarify the nature of revolutionary change. I see, however, no reason why it cannot be made to do so, and I mean here to be making a contribution toward that end (*ibid.*: 184).

Kuhn leaves it open for future work to develop the structuralist meta-theory more in the sense of his proposals. He recognizes explicitly that it is the structuralist meta-theory as a formal approach in the philosophy of science that gives tribute to his program and that allows to express his ideas in a formal framework. In this sense, one can see that the structuralist meta-theory was inspired by Kuhn in the beginning, especially Stegmüller's work. During the last decades, it has become more and more apparent that one central research topic in the structuralist meta-theory is indeed the dynamics of theories (see Moulines 2011, amongst others.).

3.2.3 Conclusions

I have argued that in the *Aufbau*, Carnap's main aim was to provide a logical method for the reconstruction of our knowledge of the world. Such a description is provided by purely structural descriptions. The structuralist meta-theory aims to describe the logical structure and the dynamics of scientific theories. As scientific theories are taken to be our most sophisticated, elaborated and systematized descriptions of our knowledge of the world.

It is, as in Carnap's, a proposal for reconstructing our knowledge of the world. It only starts from the reconstruction of our empirical theories and is in this sense not a direct, but an indirect description.

Furthermore, Kuhn's ideas about revolutionary theory change are reformulated in a logically precise sense in the framework of the structuralist meta-theory. Within this framework, it becomes clear how theories actually change through time and how these are interrelated in every single case, from a formal perspective. The dynamics of scientific theories are modeled logically, not only metaphorically, as in Kuhn. Stegmüller alludes to a relative a-priori, which he associates with the structuralist conception of what a scientific theory is. The whole structuralist program is addressing a wider range of questions than modeling theoretical change. But one core part of Stegmüller's contribution to the development of structuralism is his analysis of Kuhn's ideas on theory change and also his application of a structural view of our knowledge about empirical theories. It is clear that at no point, the structuralist meta-theory refers explicitly to Carnap's *Aufbau* as their primary source of information and motivation. We have seen that the direct motivation for the methodological tools for structuralism can rather be found in Suppes'

(1957) method of defining set-theoretic predicates, and in the work of the group of French mathematicians called Bourbaki. Notwithstanding, there are many indices to suppose a connection between the early Carnap and the structuralist program. Although Carnap's logical method of expressing our knowledge in purely structural terms is strictly speaking not equivalent to what in structuralism is usually taken to be the right logical tool for analyzing empirical theories, in both approaches, there is a primary focus on structures. Carnap aims to describe our knowledge of the world in purely relational terms. Structuralism describes our knowledge of scientific theories in structural terms.

3.3 Discussing structural continuity:

3.3.1 The Ramsey-view

Before I will start my explication of structural continuity by analyzing it within the framework of the structuralist meta-theory, I want to argue why I see this step as progressive in the debate. The main reason for this is that an approach in terms of the Ramsey-sentence does not suffice to capture structural continuities adequately. One can represent the structure of an empirical theory in terms of a Ramsey-sentence, but it is not nearly as expressive as it would be in terms of the semantic conception. The main problem for the Ramsey-view is that it is not developed to show the dynamics of theories. It is of course a tenable view if we aim to represent the structure of our theories, but for the modeling of theory change, it seems to be too limited. Especially because it can not give any account of scientific practice, theory change and general questions of diachronical studies of science.

I will quickly recall the Ramsey-sentence: Given an empirical theory with theoretical and observational terms: $TC(t_1...t_n, o_1...o_m)$, we substitute the terms for variables and existentially quantify over them:

$$\exists x_1...\exists x_n TC(x_1...x_n, o_1...o_m).$$

By doing so, one gets rid of the surplus meaning of theoretical terms, and generally does not have to make any ontological commitment to the existence of theoretical entities, such as *gene*, or *electron*. The Ramsey-sentence, as Worrall and Zahar (2001) argue, gives us a sufficient representation of the cognitive content of a theory. However, it is not entirely clear how a Ramsey-view representation of our theories in question could provide a representation of structural continuity. Nevertheless, Zahar argues how such a representation should look like:

> Whereas referential continuity demands that as we move from one hypothesis to the next, we continue to talk about roughly the same objects, structural continuity more reasonably requires that we talk, in similar terms, about one thing-in-itself which is not directly accessible to us . . . there ought to be a translation of the old system into the new one such that all observational functions and predicates remain unchanged, and the old axioms are transformed into theorems, or else into limiting cases of theorems of the new theory (Zahar 2001: 54).

However, to do as Zahar proposes is to move beyond a pure Ramsey-view and to apply more complex formal tools for the purpose of theory reconstruction. This step is necessary and goes towards the direction of semantic conceptions of theories, where theories are generally taken to be more complex entities, and where a significant sophistication of a Ramsey-view of theories is realized.

3.3.2 Intertheoretical relations

One central methodological claim of the structuralist meta-theory is that, after a logical reconstruction of some theories under issue, we gain results about their relations to other theories. In the intertheoretical relations, it is possible to identify structures that might appear in both related theories. The respective potential models (*i.e.* their general frameworks) of different theory-elements can be related through such relations. Such intertheoretical relations can count as one version of structural continuity. In the Fresnel-Maxwell case, the continuity would be that some differential equations appear in both the older and in the successor theory.

What could be said against this proposal is that, from the fact that certain intertheoretical (and inner-theoretical) relations can be shown, it does not follow that these structures are in any form *real*. This concern seems in principle correct. Anyhow, I reply as follows: If certain structures persist again and again during radical theory change, this tells us that at least our formal representation of the persisting structures reflects the world correctly. The parts of a structure which would persist, would then show that we got at least *something* of the world right.

It is correct that the persistence of some structures needs to be linked somehow to the empirical level, *i.e.* the structures need to be empirically grounded. Within the approach of the structuralist meta-theory, this occurs only if one builds such structures up, based on concrete empirical theories. This means that the formal structures are always a metatheoretical representation of a concrete empirical theory. We will see concretely how this looks like in chapter 6, where I consider examples of structural continuity in linguistics.

In the structuralist meta-theory, the general methodological approach is to reconstruct some representative empirical theories logically. By doing so, the aim is to provide clarity about the logical structure and the ontological commitments of the reconstructed theory. Reconstructions show the logical structure of scientific theories. These are identified with structures in the above mentioned formal sense. I aim to show that the methods of the structuralist meta-theory contribute to a clarification of structural realism, for they enable us to show what structural connections between empirical theories look like in a precise formal sense.

The somehow too restricted or insufficient approach of Worrall, which is only about mathematical structure in the sense of equations (or in the sense of the Ramsey-sentence), can be abandoned or, even better, amplified. Despite the fact that the structuralist meta-theory has been formulated principally as a program for outlining the logical structure of our empirical theories and to model the dynamics of scientific theories, and that it is usually understood as being neutral to debates on scientific realism, the acceptance of its framework as an instrument for logically reconstructing our empirical theories, combined with the epistemological assumption made above, help to model structural realism, especially the notion of structural continuity. However, one can still be a representative of the structuralist meta-theory without committing to structural realism. The step of accepting both structural realism and the structuralist meta-theory requires a stronger ontological commitment, and does not necessarily follow from my proposal.

3.4 Structural continuity

In this section, I will address the question of structural continuity, especially in the framework of the structuralist meta-theory. Before we can move to this, I will discuss the relation between ESR and OSR, since I see no consensus in the debate on this question. It is of central importance to clarify this relation, if one wants to provide results in the debate on structural realism.

3.4.1 The relation between ESR and OSR

As I have already mentioned above, Worrall's structural realism is introduced in the context of including *both worlds*, *i.e.* he aims to incorporate both, the no-miracle-argument (NMA), which goes back to Smart (1963, 1979), Putnam (1975b) and Boyd (1983), and the pessimistic-meta-induction (PMI), and to offer in consequence a *new* proposal to scientific realism, namely, structural realism. In this sense, Worrall's approach seems to be much more on the side of epistemic structural realism, since it focusses on theory change and the dynamics of theories, and ultimately on epistemological questions concerning our knowledge, more specifically, our knowledge of scientific theories.

To go even further, I argue that OSR can be criticized independently of ESR. Since the adherents of OSR argue that at the fundamental ontological level, there are no objects, but structures, it is primarily a metaphysical view on what there is. It is not necessarily connected to debates on theory change and scientific realism.

One major objection to OSR is the problem of "relations-relata". This problem can be formulated as follows. There can be no relations without relata, in other words "one cannot intelligibly subscribe to the reality of relations unless one is also committed to the fact that some things are related" (Chakravartty 1998: 399). So if it is only relations that there are, it can't be without having relata as a necessary constitutive part of the relations. However, some adherents of OSR offer the following answer to this problem:

> The best sense that can be made of the idea of a relation without relata is the idea of a universal. For example, when we refer to the relation referred to by 'larger than', it is because we have an interest in its formal properties that are independent of the contingencies of their instantiation. To say that all that there is are relations and no relata, is therefore to follow Plato and say that the world of appearances is illusory (Ladyman *et al.* 2007: 152).

We can see this answer as an example of how the OSR-debate has shifted away from ESR and generated its own problems. By appealing to the debate on universals, one moves away into the field of metaphysics, which is something entirely different from the focus on epistemic structural realism. Now, the question would not be any longer about the structure of our theories. It would be a question of purely abstract, logical possibilities of formulating relations, etc. If we aim to stay within structural realism in the philosophy of science, our aim is to focus on scientific theories and theory change over time. It might be the case that relations can be formulated without relata, but this, however, is unconnected from the debate that on structural continuity, and gives us reason to think that in the end, OSR is entirely different from ESR.

One further pivotal aspect is that OSR seems to be heavily grounded on contemporary physics. In ESR, what is central is the claim that what we can know is the structure of the world. At no point, ESR-adherents claim that at the ontologically fundamental level, there are no objects. For an adherent of ESR, it can be perfectly fine to accept that at the ontologically fundamental level there exist objects. The crucial point is that we can't know them. What we can know, are the structures that describe the behaviour of these objects. These structures can be mathematical equations, scientific theories as a whole, Ramsey-sentences, model-theoretic structures, or something else that one would need to specify.

Moreover, if we want to address Worrall's original proposal and if we want to contribute to the debate on scientific/structural realism and theory change, one can work separately on ESR, since OSR does not have a concrete relation to this. The main questions about OSR do not bear with theory change, but with the foundations of physics, causality and the metaphysics of science. In any case, one can try to formulate and to discuss OSR with respect to the special sciences (I will dedicate chapter 5 of this dissertation to this question). On the other hand, ESR is about our epistemic access to the world, and about the structure of our scientific theories, but OSR is about the structure of the world at its most fundamental ontological level.

In his original work, Worrall bases his position on a case study of optical theory. He mentions the theoretical change from Fresnel's to Maxwell's theory of optics. He states that, though the referents and names of the postulated entities in our theories change, the mathematical structure is preserved through theory change. Furthermore, what is continuous is the mathematical structure, that is, some equations that reappear in successor theories after theoretical change. Differential equations of Fresnel's theory of the ether reappear in Maxwell's theory of the electromagnetic field. Worrall (1989: 118-119) illustrates this position:

> Although Fresnel was quite wrong about what oscillates, he was, from this later point of view, right, not just about the optical phenomena, but right also that these phenomena depend on the oscillations of something or other at right angles to light. Thus if we restrict ourselves to the level of mathematical equations - not notice the phenomenal level - there is in fact complete continuity between Fresnel's and Maxwell's theories (*ibid.*).

What is asserted by Worrall, is that our empirical theories are not completely overthrown when they change, the structural parts are retained. Nevertheless, Worrall is not entirely clear on this point. He argues that we do not notice the phenomenal level. But if we would not notice the phenomenal level, there would be no connection to the empirical anymore, it could be a purely mathematical structuralism, dealing with equations that are not attached to reality in any form. This is why it is important to note that the phenomenal level does also matter for the structural realist. But there can be important changes at this level, and still, there can be a structural continuity through theory change, even if the semantic status of the referents of the unobservable entities changes radically. It is central that, for the structural realist, we do not have epistemic access to the referents of the (theoretical) terms that appear in our empirical theories. But we can have knowledge of the mathematical structures (equations), in which the (theoretical) terms appear that we use to refer to these entities. If it occurs that the same equations are part of radically different theories, for the structural realist it seems reasonable to assume that what represents the world as best possible are these equations.

But equations should not be necessary for structural realism to work. Many mature scientific disciplines make no use of equations, or only very little (*e.g.* linguistics, biology, or psychology). Disciplines which deal more with qualitative concepts and are less mathematized need also be reconstructable from a structural realist perspective. If this can't be done, structural realism cannot be fully extended to the whole of empirical science, unless one wants to be a strong physicalist reductionist about physics.

What we know in the discussion on structural realism, is that certain structures are preserved through theoretical change (see Worrall's 1989 case-study), and that other parts of our theories get lost. Now, the underlying epistemologically fundamental idea behind this whole approach can be made explicit as follows: If only structure is preserved, and if we aim to be structural realists, only the structures can bear the connection to the world.

Since it is the structures that appear again and again through theoretical change, only these can transmit the *real* part of our theories.

This just follows if we accept that all other parts of our theories get lost through time. So, the epistemic structural realist concludes: our best epistemological access to theoretical entities is through the structures of our theories about them.

For Worrall, structural realism is defensible, for having shown that at least in the case of theory change from Fresnel's to Maxwell's theory, certain mathematical structures persist. However, this is not enough, since one representative case-study does not convince us about the existence of structural continuities in all areas of mature science. For epistemic structural realism to get strengthened, it is necessary that further representative case studies are carried out. The structural realist will have to find such structural continuities in many other cases of theory change. As I will show below, I aim to contribute to this by providing a case-study of theory change in linguistics. This will be done by the application of the formal framework of the structuralist meta-theory.

3.4.2 Reduction

In order to represent structural continuities between our theories, I propose to make use of the structuralist concept of reduction, as it is discussed and defined by Díez and Moulines (2003: 391-396):

Definition 2: Let $Mp(t), M(T), I(T)$ respectively be the sets of potential models, actual models and intended applications of T.
Analogously, let $Mp(T^*), M(T^*), I(T^*)$, with respect to T^*. T is reducible to T^* iff there exist the relations ρ and ρ_e such that:

1. $\rho \subseteq Mp(T) \times Mp(T^*)$.

2. $\forall x, x^*(\langle x, x^*\rangle \in \rho \wedge x^* \in M(T^*) \rightarrow x \in M(T))$.

3. $\forall y(y \in I(T) \rightarrow \exists y^*(\langle y, y^*\rangle \in \rho_e \wedge y^* \in r[M(T^*)] \rightarrow y \in r[M(T)])))$.

The first condition establishes that both theories are connected through their conceptual frameworks, their sets of potential models. Condition two expresses the derivability of laws from the reducing theory T^* to the reduced theory T. And condition three expresses the preservation of the successful applications of the theories, where ρ_e is the relation ρ at the t-non-theoretical level.

I want to propose that, if two theories are related through the structuralist reduction relation, it can be seen as a case of a structural continuity. This criterion of structural continuity is of course not a necessary one, but sufficient. It would be absurd to claim that all cases of structural continuity should be cases of theory-reduction. As examples of reduction in the structuralist framework, one can consider the cases of the reduction of Classical Collision Mechanics to Classical Particle Mechanics, and the reduction of Rigid Body Mechanics to Classical Particle Mechanics shown in Balzer, *et al.* (1987: 255-284).

In chapter 6, I will show that the structuralist concept of reduction can be used to show structural continuities in the specific case of theory change in linguistics that I discuss. Since the structuralist meta-theory offers more notions that can represent the dynamics of theories, we can not exclude the possibility that more technical notions might be applicable as well to structural realism. This, however, is a task of future investigation.

Chapter 4

The Newman objection

The Newman objection is considered to be the strongest argument against structural realism, more specifically, to structuralist epistemologies (see Demopoulos and Friedman 1985, Ketland 2004, or Ainsworth 2009). In this chapter, I will outline this objection. Then I discuss some responses that have been formulated to this problem, before I develop my own response, with which I aim to give a general solution to the Newman objection in the debate on structural realism.

4.1 The objection

One can express Newman's objection in the following way:

Structure is not sufficient to uniquely pick out relations in the world.

More specifically, the Newman objection can be expressed as follows: Suppose that the world consists of a set of objects, and its structure is W. W includes a family of relations R, but nothing else is known about it. Given any collection of objects, only the formal structure of these relations can be established—provided that there are enough objects to begin with. Given only the formal structure, it is not possible to identify a unique referent for this class of relations. We could stipulate that we are talking about the intended class of relations. But, as will become clear below, this move goes beyond a purely structural description. Newman himself expressed this very clearly:

> Any collection of things can be organized so as to have the structure M, provided there are the right number of them. Hence the doctrine that only structure is known involves the doctrine that nothing can be known that is not logically deducible from the mere fact of existence, except ('theoretically') the number of constituting objects (Newman 1928: 144).

I will now outline Newman's theorem formally:

Let $S = (U, R_1, \ldots, R_k)$ be a structure, and D be a set. We can formulate an injection $\rho : U \to D$. It follows that there exists a structure S' whose domain is D, and which has a substructure isomorphic to S.

The proof for Newman's theorem[2]: First we define the image of mapping ρ as $\rho(U) :\leftrightarrow \{x \in D : \exists \alpha \in U, \rho(\alpha) = x\}$. We know that $\rho(U) \subset D$. Furthermore, ρ is injective, and we know that $\rho : U \to \rho(U)$ is a bijection. The inverse is $\rho^{-1} : \rho(U) \to U$. It is now possible to define a relation $R_i^{'}$, for each n-place relation R_i on U, on the set $\rho(U)$ as follows:

[2]See Ketland 2004 for a detailed discussion of Newman's theorem and the proof.

$R_i^{'} :\leftrightarrow \{(x_1,\ldots,x_n) \in D^n : x_1,\ldots,x_n \in \rho(U)\&(\rho^{-1}(x_1),\ldots,\rho^{-1}(x_n)) \in R_i\}$. In other words, $R_i^{'}$ is an n-place relation on D. It follows from the definition of each $R_i^{'}$ that $\forall \alpha_1,\ldots,\alpha_n \in U, (\rho(\alpha_1),\ldots,\rho(\alpha_n) \in R_i^{'}$ iff $(\alpha_1,\ldots,\alpha_n) \in R_i$. If we repeat this for every relation R_i on U, we can define relations $R_i^{'}$ on D and hence build a structure $S^{'} = (D, R_1^{'},\ldots,R_k^{'})$. If we now take the restriction of $S^{'}$ to the subdomain $\rho(U) \subset D$, we can see that it is the substructure $(\rho(U), R_1^{'},\ldots,R_k^{'})$, and this substructure is isomorphic to S. Hence, we can impose any structure on a set, save for cardinality constraints.

It can now be seen that this result presents a problem of trivialization for structuralism. There have been several proposals of how to avoid Newman's objection in the literature. For the purpose of this dissertation, I will address three answers that I consider relevant for my own account.

4.2 Three answers to Newman:

4.2.1 French and Ladyman

For Steven French and James Ladyman, the Newman objection does not arise in the framework of the semantic view of scientific theories: "the Newman problem is obviated if one does not think of structures and relations in first-order extensional terms (French and Ladyman 2003: 33)." Though it is possible to show logically that at least for one specific form of the semantic view, the Newman objection does arise as well (see Ainsworth 2009: 150-152), I will show that the structuralist meta-theory does not get affected by the Newman objection. After all, Newman's formal result holds regardless of the mathematical framework that is in use. In principle, it is important to note that the epistemic and the ontic versions of structural realism are entirely different and that it is easily possible that objections that affect the epistemic variant do not affect the ontic one, and viceversa.

4.2.2 Votsis' account

In his doctoral thesis (2004) and in his 2003 paper, Ioannis Votsis proposes that we should distinguish between structures we build up *a priori*, and structures we somehow 'arrive at' a posteriori, after an empirical investigation. I agree with Votsis that such a distinction is indeed helpful, since in the philosophy and epistemology of science, one is primarily concerned with empirical theories, and not just with the formulation of *any* kind of abstract, purely mathematical and therefore a priori structure:

> First of all, it should be made clear that if all the structural realist is arguing for is the claim that there exist relations with particular structures, then this is obviously trivial for the reasons Newman mentions. But no structural realist makes such a claim! (Votsis 2004: 122).

According to Votsis, a structural realist claims more. The structural realist would indeed claim that there exist relations with particular structures, but furthermore, it would also be claimed that we can know these structures, and that they represent a certain empirical domain.

Votsis gives the following example that highlights the limit of Newman's objection:

> Take the numbers 133 and 123. I can, restricting myself solely to arithmetic, perform various operations on these numbers. One such operation is addition. Similarly, if I had two collections of 133 and 123 physical objects respectively, I could count them one by one, and would reach the same result. Despite the similarities, there is an important difference between the two cases. The latter case is one in which the result is a property that is then ascribed to the physical world, in particular to the physical objects under consideration, and not merely an exercise of arithmetic. This claim is warranted by the employment of an empirical method to arrive at the given number (Votsis 2003: 886).

By mentioning this example, Votsis aims to show that there is a difference between structures that are formulated in purely logico-mathematical terms, and structures that are formulated in order to represent empirical knowledge. I agree with Votsis that this distinction is indeed necessary if we want to make structural realism a coherent position in the epistemology and the philosophy of science. Performing operations on structures in purely formal terms permits the Newman objection to succeed, but this changes once we specify structures about physical systems, that are part of the world.

Nevertheless, Peter Ainsworth claims that Votsis' proposal is untenable, for the following reason:

> It is not obvious why the fact that some structures have been arrived at a posteriori guarantees that these structures are more important than those structures that have been arrived

> at 'merely' a priori. Simply being arrived at via an a posteriori method does not seem to be sufficient to make a result important, especially if that result could have been arrived at a priori. After all, if the claim 'eggs is eggs' had been discovered to be true a posteriori, it would not thereby be more important than those identity claims that had been arrived at 'merely' a priori (Ainsworth 2009: 167).

In my view, Ainsworth fails to give a counterargument to Votsis' proposal. In general, but even more importantly in the debate on scientific and on structural realism, it is absolutely crucial to distinguish between empirical and non-empirical systems, and hence between a priori and a posteriori structures. This fact seems to be unacknowledged by Ainsworth. There is a fundamental difference between structures we can formulate a priori, and structures we 'arrive at' a posteriori. The former can be formulated analytically and be based on logic and mathematics, where the latter depend on how the world is. It is also the a posteriori structures that should be of interest for the structural realist, since the aim is to contribute to the debate on scientific realism, and in general to the philosophy of science.

Votsis' argument is in the same spirit as my own account, since I also stress the importance of the fact that it is not the same to claim that epistemic structural realism does not hold, based entirely on a mathematical result on the one hand, and to apply structural realism to concrete cases of description of empirical phenomena. It is true that Newman's objection is a formally correct mathematical result, however, once we move to the application of formal methods in order to describe empirical phenomena, this result shows its limits. Newman's formal result simply does not have any impact on the actual reconstruction of an empirical theory by means of a set-theoretic predicate, more specifically, by applying the methodology of the structuralist meta-theory.

4.2.3 French and Saatsi

Before proposing their own account, French and Saatsi back up Votsis' proposal: "What saves the Ramseyfying structural realist is the fact that the theoretical content captured by her use of Ramseyfication goes well beyond the mere formal, logical structure of the unobservable world (French and Saatsi 2006: 551)."

Furthermore, it goes along quite well with my own view that French and Saatsi state "... we take it that in reality there has never been ... a purely structural view of theories (*ibid.*: 557)". It is in fact a misinterpretation by the criticizers of structural realism to suppose that structural realists account for a 'purely' structuralist view of theories. We always need to say more about the empirical claim and the intended applications of a theory, this feature makes my own structuralist view in fact 'only' partially structural.

The misinterpretation that I have identified can be explained well in historical terms, since Newman's original critique was addressing Bertrand Russell's (1927) theory of perception. This debate originated in an entirely different context than the current debate on scientific (and structural) realism. It seems that one is better off with acknowledging that all structuralist views of theories and of structural realism in general are quite different from Russell's causal theory of perception, most importantly, they are always concerned with concrete empirical theories. For French and Saatsi:

> It is not the case that any unobservable domain of suitable cardinality can be extensionally ‘carved out’ into isomorphic structure in a way that satisfies this constraint on the members of the domain. Yet, provided that this constraint is satisfied, it is meaningful to speak of representing the structure of the domain ... the theoretical variables for which the simultaneous values and their change are given by the structure are theoretically interpreted: they refer to physical properties and relations. It is not the case that all the theoretical content about these variables is encoded in the structure of their interrelations at each moment of simultaneity and over time - there is interpretational content about these properties that is captured linguistically (*ibid.*: 557-558).

I share the intuition that for any structuralist representation of an empirical domain, there is always a reference to real existing physical relations. What this exactly amounts to will de beveloped below, by making reference to Carnap's concept of *founded relations.*

4.3 Towards a pragmatic account

All the different answers to the Newman objection, as well as my own, have in common that they acknowledge that one should say more than just that there exist relations within particular structures. One should be clear by stating explicitly that one accepts, of course, the formal result of Newman's Objection, nevertheless, one can wonder about its philosophical impact for structural realism. We can draw this picture as follows:

1. In debates on structural realism (mostly on ESR), we search for cases of structural continuity between apparently radically different theories.

2. Given these cases, one can employ a variety of different formal tools to reconstruct them—such as Ramsey-sentence views, or semantic approaches.

3. When we write down the structure of an empirical theory in a specific formal framework, Newman's problem doesn't emerge. After all, we need to state explicitly what the domain of the theory consists of (particles, genes, markets, etc.), and it's no longer a trivial matter whether structures of the appropriate kind can be constructed.

One could ask if this does not imply that structuralism is left behind. I argue that it is a pragmatic move, which allows one to incorporate the content of Newman's result, while questioning its philosophical import. However, if one stays at a very abstract level (disregarding the content of the theories in question), Newman's objection arises.

Concerning ontic structural realism, this view is indeed unaffected by Newman's objection, since it is not an epistemological position, but rather a view about what there is. It seems that, since in ontic structural realism there are no explicit claims about knowledge, and about knowledge representation (such as the Ramsey-view, semantic views, etc.), the Newman objection is in principle unable to affect ontic structural realism. OSR is based on interpretations of contemporary physics. It's basic claim "all that there is at the ontologically fundamental level is structure" is grounded on the results of our contemporary physical theories.

In this sense, OSR is more a metaphysical position, a metaphysics of science, rather than a view that makes claims about our knowledge of the world. Most importantly, in OSR, there are no explicit claims about 'the structure of our knowledge' or, more specifically, about the structure of the formal representations of our knowledge of the unobservable parts of the world. Such questions are fully accomodated within the debate on epistemic structural realism.

A pragmatic way out of the Newman objection

I will now procede to develop my own account of what I call "pragmatic structural realism". As a first step, I will recall the concept of set-theoretic predicates, as they were introduced in chapter 3. By making use of such set-theoretic predicates, one is able to represent the structure of our theories. This tool can be applied to represent the structure of our theories, also in discussions about epistemic structural realism. For the case of OSR, this view does not have to rely on any form of logical (or formal) "meta-representation" of our empirical theories, since its adherents normally directly interpret the claims of physical theories and give metaphysical interpretations in this way (see Esfeld 2004, or Lyre 2004, amongst others).

A standard example of a meta-representation within the formal framework of the structuralist meta-theory is the one I have outlined in chapter 3, namely, the potential and actual models of classical collision mechanics.

One can ask now: Why is the information provided by a theory representation using a set-theoretic predicate trivial, as Newman would have claimed? I do not see such a supposed triviality of information that is given by the potential model illustrated in chapter 3. The information is not trivial for the following reason: one states explicitly what the elements of the basic domain are—in the case of our example from chapter 3, the elements are particles.

Furthermore, to provide a structuralist representation of an empirical theory, with a set-theoretic predicate, is no trivial task: a structure of the relevant kind (about the objects in question) may not be available. Hence, my proposal is that, if the structural realist wants to be safe from a trivialization charge, a pragmatic stance is needed. In more detail, the pragmatic move consists in specifying explicitly the empirical system that is being represented, and to determine concretely the domain of objects and their relations. This is required to make sense of any structural formulation of a scientific theory; otherwise, the theory becomes just a piece of mathematics.

In the framework of the structuralist meta-theory, the pragmatic part of a structuralist view is represented by the set of intended applications of a theory, I (also introduced above in chapter 3). The determination of the intended applications depends strongly on the scientists that actually do make use of the respective theories, and is therefore pragmatical. For instance, we can think of a physicist in a laboratory, trying to carry out an operation of measurement. She might, in a specific case, have something similar in mind to the potential model of Classical Collision Mechanics, when she is actually working, even as an underlying background assumption. But she will be certain about what the “things" that will be measured actually are. These are not apples, shoes, or elephants, but particles. In this sense, the elements of the basic domain are individuated, and in this sense, my proposal is a pragmatic move.

In purely formal terms, Newman's objection presents a challenge to those who make the abstract claim that "all our knowledge is structural", since a pragmatic component is required. In fact, the proper understanding of the relevant theories requires attention to their pragmatic counterpart. Thus, attention to particular case studies is central. The crucial point is the following: by specifying explicitly what our set-theoretic predicates represent in each case, the information about the structure of our theories is not trivial anymore.

4.3.1 Carnap's founded relations

To further outline my pragmatic proposal, I will now turn the focus on Carnap. In §154 of the *Aufbau*, he introduces *founded relations*: "we want to call relations that correspond to an experienceable, 'natural' relation, whose corresponding parts do have something experienceable in common, 'founded relations' (*ibid.*)". I argue that the relations specified by set-theoretic predicates are experienceable and "natural" in Carnap's sense. Following Carnap, founded relations are *erlebbar*, which means that they are experienceable. Given this feature of founded relations, it is possible to specify only those structures that are relevant for the description of our empirical knowledge, namely, those corresponding to experienceable relations. Thus, one begins by picking out *real*, existing, physical relations, which means that when a structuralist representation of a theory is provided, one has to restrict to the relevant, real existing physical relations, and not to any kind of abstract structure. In this sense, a description of such real relations in terms of set-theoretic predicates is provided, and one is able to select those set-theoretic relations that stand for the appropriate experienceable relations.

This Carnapian proposal provides an answer to the Newman objection, for it restricts the possible structures only to *experienceable* relations. These are any relations that one *can* experience. Some are experienced directly (directly observable relations); others are experienced indirectly (via instrumental access). But in none of these cases, one is concerned with *any abstract* structure. But there is no doubt that, as soon as one tries to talk about structure in a more general and abstract way, the Newman objection may rise again. However, this presents no problem for either, pragmatic structural realism, or the structuralist meta-theory, for the reasons I have explained above.

4.3.2 Structural scrutability

David Chalmers (2012) proposes a way out of the Newman problem that is similar to my pragmatic proposal. He identifies Carnap's original intent of pure structuralism as insufficient, and argues as follows:

> ... this leaves open the possibility of weak structuralism, on which the basic vocabulary may include a limited number of expressions for relations (such as phenomenal similarity) plus logical expressions. Both theses have analogs in the domain of scrutability. The analog of pure structuralism is Logical Scrutability: the thesis that all truths are scrutable from truths using logical vocabulary alone. The analog of weak structuralism is Structural Scrutability: roughly, the thesis that all truths are scrutable from truths using logical vocabulary plus structural expressions, where (to a first approximation) a structural expression is one that expresses a

> basic relation. Logical Scrutability is undermined by Newman's problem, but Structural Scrutability remains on the table (*ibid.*: 409).

Chalmers identifies an analogy between what he calls *weak structuralism* and *structural scrutability*. Whereas my pragmatic structural realism matches well a weak structuralism, it is clearly distinct from structural scrutability, since my structuralism does not rely on any notion of truth, nor does it aim at connecting any truthlike notions with structural realism. This is an important difference. Once we include the notion of truth, we would lean too much into the realist side. My aim, however, is to focus on the structural descriptions, by alluding to a specific logical vocabulary, in the above outlined, pragmatic sense.

Later on in the same section, Chalmers argues that by explicitly specifying the physical system under issue, we are able to save our construction from Newman's problem (*ibid.*: 411). This move is very much analogous to the one I propose in this dissertation, since I aim to specify the relevant physical system under issue in each case, and argue that such a specification endorses pragmatic virtues, such as *relevance, objectivity* or *elegance* for a given physical system.

4.3.3 Structuralism, empiricism and realism

By making such a Carnapian move, one might ask whether pure structuralism is given up by moving to experienceable relations? This depends on what the requirements are on structuralism. If pragmatic considerations are not considered part of the structuralist framework, then structuralism is left behind. Without a pragmatic component, pure structuralism makes very little sense, since one cannot specify what the structures that are introduced in one's theories are *about*. The intelligibility of structuralism ultimately demands this pragmatic feature.

Consequently, one might then ask whether realism has been given up by moving to experienceable relations? I have emphasized the empiricist requirement on experienceable relations, but I have understood such relations broadly (some are directly experienceable, others are indirectly so). It is possible to distinguish the following three interpretations:

1. *Constructive empiricist reading*: The experienceable relations (those to which one should assign an epistemic role) are restricted only to directly observable relations.

2. *Broadly empiricist reading*: The experienceable relations include directly observable relations and indirectly observable relations—as long as one knows that the relevant instruments satisfy epistemic conditions akin to observation (see Bueno 2011).

3. *Realist reading*: The experienceable relations include directly observable relations and indirectly detectable relations (that is, one can detect them with instruments that need not be akin to observations).

Thus, depending on how one interprets the experienceable relations, realist and anti-realist views can be accommodated within the framework of pragmatic structural realism.

4.4 Conclusions

I have argued that Newman's objection holds if we make just abstract structural claims. But understood in this way, the objection has no connection to structural realism in the philosophy of science, where concrete cases of actual theories are at issue. By invoking a set-theoretic predicate formulation of the structure of a physical theory, we need to state explicitly the domain of objects the theory is about. By doing so, one is forced to leave behind the territory of abstract knowledge claims. One has then to emphasize the crucial role of experienceable relations, and provide a broad framework in which these relations can be multiply interpreted, such as the framework of the structuralist meta-theory. This leads to different philosophical views (from constructive empiricism through a broad form of empiricism to realism). In this way, we can acknowledge Newman's place, and claim that Newman is of course right, although the philosophical impact of his result is, in the end, limited. His objection presents a problem for the very abstract form of structuralism, when it is applied to epistemology (such as Russell 1927), but not to structuralism about scientific theories.

Chapter 5

Ontic structural realism

5.1 Introduction

So far, we have seen that since Worrall's introduction of structural realism to the debate on scientific realism, there have been several developments concerning a formulation of mainly two views within the debate, so-called *epistemic* and *ontic* structural realism. As I have already developed above, ESR affirms that all our knowledge is structural, but we remain ignorant with respect to the properties of the entities that are part of these structures, where ontic structural realism asserts that all that exists *is* structure. Following this view, objects only fill places in structures and do not exist independently.

In this chapter, I will concentrate on the ontic version of structural realism. The aim is to discuss the applicability of OSR in domains of science other than physics, specifically in linguistics. I will show that in fact, OSR is well applicable to linguistics. More precisely, I will show that in Zellig Harris' transformational theory, the predecessor theory of Chomsky's generative grammar, it is of central importance to have a structural ontology. Otherwise, I argue, it would be difficult to give the theory a correct underlying metaphysical fundament. Before discussing the case of OSR in linguistics further, I will outline Harold Kincaid's and Don Ross' accounts of OSR in economics and the social sciences, as well as Steven French's proposal with respect to OSR in biology.

Since its introduction, OSR has been discussed vastly within the context of the philosophy of physics, by numerous authors such as Cao (2003), Dorato (2000), Esfeld (2004), Frigg and Votsis (2011), Lyre (2004), Psillos (2006a), and Redhead (1999), to only mention a view. There is vast literature on the topic, and the discussion has diversified and developed into several views. Some of these views on structural realism don't have much in common anymore, besides that they all employ the term structure. This surely presents some problems, since our goal in the debate should be to provide universally acceptable answers to the debate on OSR.

Generally speaking, OSR states that what exists at the ontologically most fundamental level are structures. For the adherents of OSR, there exist no individuals, and all that there *is*, is structure. It is also well known that many authors have been arguing in favor of this position by relying on concrete results of contemporary physical theory. It is not my aim to discuss OSR within the debate in the philosophy of physics. Moreover, I aim to outline how OSR can be expanded to the special sciences, such as biology (see French 2014), or in this case, linguistics.

Furthermore, the fact that OSR almost completely focusses on physics brings the discussion of reductionism to the debate. Especially Ladyman *et al.* (2007) and Lyre's (2013) positions on a supposed "primacy of physics" and some form of reducibility of all other sciences to physics are at issue here. The same holds for Ladyman (2008). In what follows, I argue that ontic structural realism finds applicability in other domains of empirical science as well.

5.2 The *primacy of physics*

Motivated by a naturalistic viewpoint, Ladyman and Ross propose a constraint on how to understand the relation between physics and the special sciences, the so-called primacy of physics. In this section, I discuss this principle, which is formulated as follows:

The primacy of physics contraint: *Special science hypotheses that conflict with fundamental physics, or such consensus as there is in fundamental physics, should be rejected for that reason alone. Fundamental physical hypotheses are not symmetrically hostage to the conclusions of the special sciences* (Ladyman *et al.* 2007: 44).

Here it is argued that physics has a more important status than all other scientific disciplines. This claim is made in order to lift physics on a higher level than all other disciplines, by relying on a supposed *scientistic* position á la Neurath and other adherents of the *Wissenschaftliche Weltauffassung*. Ladyman (2008) explains the motivation for this principle as follows:

> Some forms of physicalism imply that everything that exists is physical . . . Thus understood, physicalism is in tension with standard scientific realism. This kind of physicalism is also in tension with naturalism, since naturalists allow questions

> of ontology to be decided by science. Hence they ought to be realists about such entities as markets, fixed action patterns, mating displays, episodic memories, evolutionarily stable strategies, and phonemes, because successful explanation and prediction have been produced by special sciences that refer to such entities, and such success is sufficient for ontological commitment within science. Similarly, naturalism seems to demand that the causal claims of the special sciences be taken at face value, not least because, as mentioned above, it is the causal relations in which the entities posited by the special sciences feature that give us grounds for inferring their existence (*ibid.*: 745).

For Ladyman, we are only justified to believe in the existence of those entities of special science that are used to make causal claims. Any other entity in the special sciences is not acceptable for a naturalist, as he argues. This position is attractive. Nevertheless, sometimes it is better to postulate (or to accept) the existence of such "more special" entities. For instance, if we don't accept the existence of entities such as phonemes or markets, just because we don't have an account that explains their causal status, from a scientific realism perspective we might get into trouble and end up at a point where we would be unable to affirm sentences in economics, like 'free markets exist'.

Also in linguistics, the question of the *real* status of the entities postulated is complicated. Seen from a physicalist point of view, one can easily ask questions like "What is *real* in linguistics?" This is a rather difficult question to answer, and might depend on the type of linguistic theory that one endorses. One could answer "The speech sounds that speakers produce are the real existing (and physically measurable) entities", or “Some mental structures that allows us to be competent speakers of a language".

We should be worried that by endorsing Ladyman’s view, we might end up loosing too much entities in our ontology. I suggest that one should rather go along with certain entities, despite their possible lack of a direct causal relation between them. To assume the existence of phonemes is central for linguistic theory, no matter if one is a structural, a cognitive or a generative linguist. Furthermore, a central part of theory-building and of the postulation of entities occurs through idealization and abstraction. And if we want to stay with the strong demand of the causal role that all our *real* entities need to play, we might be left without almost all entities we currently take to exist, in a common-sense view. We might only be left with elementary particles. This would fall too short if the aim is to draw a general picture from the perspective of scientific realism.

I rather propose that we don’t think of a primacy of physics in a narrow sense, but more loosely. Our different scientific disciplines postulate the existence of certain entities, and they further state that such postulations are required in order to explain the phenomena that we aim to describe. No matter if a theory is about molecules, particles, genes, acids, animals, stock markets, or social institutions. It is precisely an ontic structural realist view that helps us to accommodate the existence of such entities, no matter if these entities are causally relevant or not. Once it can be shown that such entities only come into existence as parts of bigger structures, or that these entities are themselves of structural form, it is reasonable to accept their realist status, from the perspective of OSR.

Let us think about the justification for the primacy of physics step. There appears to be a conflict of motivations in the beginning. First, by appealing to this principle, Ladyman and Ross aim to strengthen the view that OSR does not have to be explored outside the realm of physics, since all other sciences are in a sense subordinate to physics, and if something changes in physics, something might have to be accomodated in the special sciences, but not viceversa. Secondly, in a later attempt, both authors explore the possibility of OSR in the special sciences, and both suggest that positive applications of OSR in the special sciences could be discovered (see their 2008 papers).

In his paper, Harold Kincaid argues in favor of OSR in the special sciences as well. He argues that one can have a structural ontology in a special science without even thinking of the relation to other fields, such as physics. As he rightfully points out:

> One clear sense in which social theories may be about structures and not individuals is that social structure can relate organizations, classes, groups, practices, and so forth without any explicit reference to individuals. Of course, organizations are in some sense composed of individuals (Kincaid 2008: 722).

I agree with Kincaid that we should accept that in social science, there are structures at a fundamental level as well. He goes on:

> Furthermore, social structures suggest that token identity will have to be identity at a time, since over time the same institutional token will be realized by different individuals so long as the structure persists. Finally, explanations in terms of social

> structure can be sufficiently detached from the details about the individuals bringing it about that it is indeterminate exactly which individuals at a time compose the institution. This is in principle no different from the problem of trying to decide which molecules make up a particular table (*ibid.*: 729).

Kincaid draws an interesting analogy to physics here. If we want to focus on structures, in the sense of OSR, what is then the difference between social science and physics? Both explore structures. One difference lies in the status of "ontological fundamentality" that we aim to attribute to the entities that are postulated in both disciplines. No one would reasonably argue that structures in social science are ontologically more fundamental than structures in quantum physics. However, there is a difference in accepting that molecules are more fundamental than societies on the one hand, and in saying that it does not make sense to postulate OSR outside of physics on the other hand. Consequently, I argue in favor of a moderate version of the primacy of physics, that can now be formulated in a slightly modified version as follows:

The moderate primacy of physics constraint: *Special science hypotheses that conflict with fundamental physics, or such consensus as there is in fundamental physics, should be revised and might be rejected for that reason alone. Fundamental physical hypotheses are not symmetrically hostage to the conclusions of the special sciences, however in certain cases, hypothesis of the special sciences should be accepted despite their apparent conflict with fundamental physics.*

This weakens the primacy of physics constraint and allows us to draw a more pluralist picture with respect to the special sciences.

5.3 OSR in the special sciences

In order to discuss the applicability of OSR in the special sciences, one might in principle argue that, since the general motivation for structural realism has been the pessimistic meta-induction and the problems for traditional scientific realism (see Worrall 1989), based mainly on cases of physics, such an expansion of structural realism bares any justification, or motivation. There is no doubt that the original motivation of structural realism has its origin and justification in cases of radical theory change in physics. Anyhow, we have seen so far that exploring the possibility of formulating OSR in other disciplines might actually become a fruitful task.

5.3.1 Lyre's critique

I will now focus on Lyre's views on structural realism. He states: "Structural realism is first and foremost an ontological framework that provides us with a tailor-made metaphysics for modern physics (Lyre 2013: 2)". In my view, Lyre's position is too restricted and can actually give rise to a confusion. This is mainly so because Worrall's (1989) paper, the one that brought structural realism into play, is settled down in the debate on scientific realism, and not in the discussion of the foundations of physics. At no point, Worrall aims to provide such an ontological framework for physics. Instead, Worrall talks about the continuity of mathematical structure through radical theory change. As it is well known, he developed his version of structural realism with the motivation of providing a solution to arguments that had been raised against scientific realism, such as the pessimistic meta-induction. His paper proposes that certain mathematical structures are continuous even through radical theory change. Yet, there is never a reference to fundamental ontological questions of physics, as Lyre understands structural realism. Nevertheless, Worrall's aim was clearly within the context of epistemic structural realism, settled down in the discussion about scientific realism.

Nowadays, we are all aware that OSR has been developed years after Worrall's 1989 paper, and that Lyre's view of structural realism is motivated from the perspective of OSR. Lyre's approach addresses the important point that OSR might in principle be applicable only to physics, just by its nature, *i.e.* it is about the metaphysics of the world. However, I see his view as being too restricted, since the exploration of OSR in the special sciences is at least an open question that needs to be addressed. It might well be the case that Lyre is right, however, we will see this only after a detailed look at several scientific disciplines.

What makes the epistemic part of structural realism entirely different from Lyre's proposal is the fact that the *early* epistemic structuralists like Russell or Carnap did not talk about the ontology of physics at all, they rather emphasized about what we can *know* of the world, and *how* we can do so. It is clear that even Worrall mentions a case-study of physics, when he discusses structural realism. But the general focus on structural realism in physics started after French and Ladyman (2003). It started to bring contemporary physics into play. It is no doubt true that physics has a primordial status in the discussion on the metaphysics of science.

However, the price of not studying OSR in the special sciences might be too high, since it is not entirely clear if OSR does not play a role there.

Furthermore, Lyre's position against OSR in the special sciences can be best expressed by the following quote:

> My point is the following: while I do believe that structural descriptions and structural laws play an *exclusive* role in the special sciences, I fail to see that they play an exclusive role or that they should give us any reason to believe that all that there *is* on the various levels is structure (Lyre 2013: 3).

Lyre rightfully attributes an importance to structural descriptions and structural laws, but denies their exclusiveness. This sounds reasonable, but it fails to make a strong point about OSR, since structural descriptions are mainly the concern of the epistemic structural realist. In OSR, structural descriptions are at most of a secondary importance. The OSR-adherent interprets a scientific theory without logically reconstructing it previously, and argues that what is in fact postulated there, are structures at a fundamental level. The way in which one should represent these structures is primarily a concern of epistemic structural realism. In the discussion about forms of representation, one can make use of the Ramsey-view, the structuralist meta-theory, and other frameworks.

Furthermore, it might be true that an exclusive role of structural entities in the sciences is not justifiable at present. But if we want to have a broad and universally acceptable answer to this, more case-studies are needed.

Lyre concludes:

> Nothing commits us to the existence of genuine, non-reducible higher-level-structures. I've fortified my points by arguing that structures are global entities and that the assumption of higher-level structures as genuinely global or holistic entities is even more arcane (*ibid.*: 7).

This view seems to be misleading, since for OSR, it is not necessary to postulate structures as being only global and non-reducible. What matters is the role such structures play in our theories and the way in which they are connected to the entities of our theories that we take to be objects. For OSR, a structure should play the central role in any empirical theory, as opposed to objects.

Moreover, what matters first is the level of ontological fundamentality, so to speak. Is it more reasonable for a biologist, an economist or a linguist to postulate objects with certain properties, when they formulate a theory, or is it in fact the case that in the disciplines of the special sciences, the postulation of certain objects with certain properties makes only sense if these are interpreted as parts of bigger structures? I suggest that for OSR to hold in the special sciences, there is one central requirement, that is, its ontological fundamentality. If an entity has relational properties and if it plays a crucial role within an empirical theory (causal, or explanatorial), we can say that this entity is ontologically fundamental in this theory. Such entities, however, can be conceived as individuals, or as structures, such as it is the case of contemporary physics and ontic structural realism.

The central task for adherents of OSR in the special sciences will be to identify such entities with relational properties and to convincingly argue in each case that such entities are better understood as structures.

Furthermore, if such entities turn out to play a fundamentally important role within a theory, one can say that these entities are the ontological basis of a theory, and that therefore, OSR makes sense in the respective field. Below, I will argue that this is the case with the notion of *transformation* and *kernel sentence* in Harris' and Chomsky's early theories.

5.3.2 OSR in economics

As mentioned above, Ross (2008) explores OSR in economics. He argues positively about the application of OSR in the special sciences, as follows:

> People are to economic theory roughly as tables and rocks are to physical theory. OSR as applied to physics doesn't deny that there are tables or rocks, and it explains how physical theory can provide (ever improving) explanations of their behavior despite denying that they are good models of fundamental reality. (I.e., it denies that fundamental reality is a collection of objects like rocks, but smaller.) Similarly, economic theory is not a set of propositions about entities like people, but meaner. In this respect, economic theory exactly resembles physical theory .... (*ibid.*: 742).

As in all other scientific disciplines, in economics a specific domain of empirical phenomena is investigated. Further, economists study the way in which the objects of their domain interact and are put into a structure. Left interdisciplinary and intertheoretical connections between economics and other sciences such as physics beside, it is not clear why one should not assume an ontic structural realist position for economics, just as for any other scientific discipline, where it can be reasonably argued for the existence of structures in which the objects of inquiry appear as subordinated parts of a structure. However, in order to affirm that OSR is not only possible, but also plausible in economics, we are in need for a detailed case study that gives us a clear result.

So far, the possibility of OSR in economics has been outlined by both authors, Kincaid and Ross, but such a structuralist stance for economics has yet to be developed sharply. One might start with discussing the ontological status that the principal entities of economics have. These entities could be markets, rational agents, but also many other entities. In the scope of this work, I aim to emphasize that the general task of exploring OSR outside of physics is a promising task for future investigations.

5.3.3 OSR in biology

For the case of biology, French (2014) argues that structural realism in biology has a high plausibility. By relying on Beatty (1995), French suggests that there are no laws in biology and that "biological structures would be temporally specific, changing in their fundamental nature under the impact of evolution (French 2014: 331)". This would in principle present a problem for the structural realist, since especially OSR relies on laws and symmetries. However, a structuralist ontology for biology seems plausible for French. He comments on *Price's Equation*:

> Although obviously not a symmetry principle, this covariance equation is independent of objects, rests on no contingent biological assumptions, and can be understood as representing

> the modal, relational structure of the evolutionary process (*ibid.*: 338).

This famous equation of biology relates the change in average value of character from one generation to the next with covariance between fitness and character, and the fitness weighted average of transmission bias: $\Delta z = Cov(w, z) + Ew(\Delta z)$. Price's equation can be understood to "represent the structure of selection in general (*ibid.*: 338)", French argues.

Despite the obvious lack of mathematization in biology, we are still able to identify mathematical structures, such as Price's equation. The exact level of mathematization that is needed to make structural realism applicable is not clear. In the previous chapter, I have already argued that once an empirical theory is formalizable, we are already in a position to represent its structure. In this sense, whenever we are in a position to reconstruct a mature empirical theory within a meta-framework (such as the semantic conception of theories, specifically the structuralist metatheory), the structures "of the world" can be represented formally.

French (*ibid.*: 339) lists four topics of biology that suggest OSR in this discipline:

1. Gene identity
2. Gene pluralism
3. Metagenomics and biological individuality
4. The heterogeneity of biological objects

These four issues in biology all involve the consideration that a relational and structural understanding of biological entities and processes should be taken into account. Since the concept of gene has changed dramatically during the history of biology, and since genes could be understood as DNA sequences, the question of the individuality of genes arises. As well as the fact that individuals in biology would rather be conceived as "emergent entities, dependent upon the appropriate structures (*ibid.*: 345)."

Within the scope of this dissertation, it is not possible to address and to discuss French's OSR in biology abundantly. I see his proposal as a promising approach of how to apply OSR outside of physics. The aim in this section is merely to show that applications of OSR in the special sciences have been discussed, and that the exploration of OSR in linguistics is one further contribution to this enterprise. A proper discussion of French's version of OSR in biology is due to future research.

5.4 OSR in linguistics

Other than in physics, in linguistics, a lower level of mathematization is achieved. This is principally due to the subject domain. Whereas in physics, most of the claims that theories make are best expressed in terms of equations, in linguistics, equations hardly can express what the respective theories claim. Notwithstanding, we are able to represent the structure of language in formal terms, especially in the field of syntax. For this purpose, formal methods such as mathematical logic and graph theory are frequently applied (I will discuss the question of mathematization in linguistics, and how it relates to the maturity of a scientific discipline in the following chapter). Once a formalization of certain subdomains of linguistics is given by these methods, we are presented with mathematical structures that represent the phenomena, quite analogously to what physicists have, once they have formulated equations.

I see it as a central point to respond to the critiques of OSR in the special sciences by providing a concrete and detailed case of existing structures in a special science; structures that are ontologically more fundamental than the individuals that are postulated in that special science. In what follows, I will discuss the case of one subdomain of linguistics, namely, transformational theory.

5.4.1 Transformations

For my analysis of OSR in linguistics, I will consider transformational theory, since it gave syntactic theory a significant progress and it can be understood as having achieved a common acceptance in the field of syntax. Before Chomsky, Harris gave linguistics (especially the disciplines of morphology and syntax) a strong formal fundament. By doing so, he helped linguistics to become a more *mature* science. This is the principal reason why I chose this theory for my purpose of arguing for OSR in linguistics. Harris (1968) outlines the motivation of his work broadly as follows:

> The only body of data required for the whole analysis of language is the indication that certain sound sequences, out of some large sample, are utterances of the language ... while others are not, and that certain ones are repetitions of each other. Structural Linguistics shows how these utterances can be characterized as a set of constructions on certain discrete elements. Mathematical Linguistics shows that the character-

> ization can be made in terms of other sets, defined by certain relations among these linguistic elements, and that the entities in the new sets are arbitrary and are defined only by the relations among the new sets (*ibid.*: 1).

In this quote, we see that Harris has a structural linguistic theory in mind, *i.e.* he emphasizes on what he calls 'sets defined by certain relations among these linguistic elements'. This means that he was aware of the importance of structural and relational properties in language. More concretely, he defined several linguistic entities as such relational entities. He makes it clear that the entities we defined in the new sets are arbitrary entities and that they are defined only by the *relations* among the new sets. This is a pivotal point in favor of a structuralist ontology in Harris' theory of linguistics.

In order to illustrate this, I will focus on one central notion of transformational theory, the so-called *kernel sentence*. I propose that kernel sentences should be understood as structural entities, for reasons that will become clear below. In order to introduce the notion of kernel sentence, Harris expresses as follows:

> The kernel is the set of elementary sentences and combiners, such that all sentences of the language are obtained from one or more kernel sentences (with combiners) by means of one or more transformations . . . our picture of a language, then, includes a finite number of actual kernel sentences, all cast in a small number of sentence structures built out of a few morpheme classes by means of a few constructional rules;

> a set of combining and introducing elements; and a set of elementary transformations . . . (*ibid.*: 335-339).

Let us consider the following example: The kernel sentence:

(i) *Tom saw Mary*

in active, is obtained from the passive

(ii) *Mary was seen by Tom*

through a transformation. Harris' formalism in this case is as follows:

$N_1 V\, N_2 \leftrightarrow N_2 V^*\, N_1$

where N_1 is 'Mary', V the verb 'saw', V^* is 'was seen by', and N_2 is 'Tom'. The $\leftrightarrow$ is the transformation operator.

A kernel sentence is a complex structure, since it is composed by words, which are composed by the smallest units of meaning, the morphemes. However, morphemes on their own do not suffice for the formulation of an ontology of natural language, based on individuals. We can agree that morphemes, such as $'-ing'$, $'-hood'$, or $'des-'$ are (fairly abstract) objects. Anyhow, morphemes alone are not the entity that suffices to compose our ontology of linguistics. Most importantly, we are in need of some ordering elements, such as rules of formation for well-formedness, and rules of transformation, as Harris explored in detail. A sentence should be seen as a fundamental entity of language, since it bears meanings and it is what speakers utter. Of course, many times, sentences can be uttered erroneously, or they can consist of only one word, and there are many possibilities for other special cases.

I propose that we conceive sentences to be relational entities, since they are composed by relata, *i.e.* words (and morphemes). In this sense, at the ontologically fundamental level of transformational theory, we have to deal with sentences, to be more precise, with kernel sentences. These kernel sentences are the ontological fundament of this branch of linguistics, and they are structural entities, since they are composed by words and morphemes.

The fact that kernel sentences can be transformed, such as in the case of active-passive transformation, indicates that they should have an ontologically more fundamental status, in comparison to other linguistic entities. Sentences can always be subject of transformation, while words standing alone can't. This indicates the relational status that sentences have in transformational theory and is a further indicator that we do best by conceiving such sentences as structures, instead of individuals.

5.4.2 Ontological fundamentality

So far, I have argued that sentence-structures are ontologically fundamental and of central importance in Harris' transformational theory. With his theory, Harris provided a method of describing how complex sentences are in fact built out of simpler ones, down to the level of kernel sentences. Kernel sentences are the central entity in his theory, and clearly, in linguistics, a sentence is understood as a structure, not as an individual. A sentence is a structure, composed by certain simpler elements by certain syntactical and other rules. These simpler elements are phrases, then words at the next lower level, then morphemes at the lowest level. But for Harris' transformational theory to work it is central that kernel sentences are postulated, not morphemes, as single, individual entities.

Morphemes only occur as parts of bigger structures, namely, words, and words only occur as parts of the bigger structures, *i.e.* sentences. Kernel sentences need to be postulated in order to be able to explain how we build up more complex sentences out of simpler ones, by means of transformations.

To point out the different levels of complexity in Harris' theory, we can proceed as follows: The following example seems to be trivial, but since the discussion about OSR in the special sciences is far from trivial, and given the fact that there is a lack of concrete evidence, I will focus on this quite explicitly. Given the sentence:

(iii) *The student reads the book*,

we can decompose this sentence-structure into its phrases, *i.e.* the noun-phrase *The student*, the verbal-phrase *reads*, and the noun-phrase *the book*. At the next lower level, one can decompose the phrases into words, then, one can proceed in decomposing the words into their morphemes.

Furthermore, from the perspective of OSR, it is the importance of the rules that can generate well-formedness which makes the notion of phrase structure rule (as I will outline and discuss in chapter 6) interesting. These rules give the notion of sentence more "ontological weight". Only with a sentence, composed by the underlying phrase structure rules, an information is given in a correct, complete and typical sense (for native speakers).

The notion of *formal grammar* also gives us information about how to connect OSR to linguistics. According to Chomsky (1957), a formal grammar generates a formal language, which is a set of (usually) finite - length sequences of symbols (*i.e.* strings) that may be constructed by applying production rules to another sequence of symbols which initially contains just the start symbol. A rule may be applied to a sequence of symbols by replacing an occurrence of the symbols on the left-hand side of the rule with those that appear on the right-hand side. From the perspective of OSR, the symbols are individuals, but for themselves, they are incapable of composing a sentence. What is needed are the production rules, which manipulate strings of symbols, and by doing so construct a sentence. I will discuss the phrase structure rules for English with more detail in chapter 6.

5.4.3 Transformational rules

Transformational rules only make sense if they are applied to sentences. And sentences are understood as structures, rather than individual objects. It does not make any sense to transform a word or a morpheme. Since this shows us that sentences are the ontologically central notion of Harris' theory, and that sentences are structural entities, and not individual objects, it is reasonable to assume OSR for linguistics. Concretely, it is reasonable to assume OSR for Harris' transformational theory.

Transformations are of central importance for linguistic theory, since they show us the relations that exist between sentences. More specifically, they show relations among sentences which are not immediately detectable. To mention one further example, consider the example of a transformation involving a relative clause:

(iv) *The employee who sold the computer to me was friendly.*

and

(v) *The employee sold the computer to me.*

Such examples lead us to think that transformations are of central importance in syntactic theory, since they illustrate how certain sentence forms (such as active and passive) are related, and how one form of sentence can be generated by applying certain rules of transformation. The set of transformational relations can be conceived as another fundamental entity of syntactic theory. Harris (1968) explains this set as follows:

> The base operators $\phi : A(x) \rightarrow B(x)$, where A, B are propositional forms, and x a particular word class or subclass ... present in $A(x)$, can be taken as generators of the set of transformational relations among sentences (*ibid.*: 100).

This suggests that only by individuating the relational properties of sentences and their transformations, we can get a correct picture of syntactic theory, and hence it motivates an ontic structural realist picture.

5.4.4 OSR in one branch of linguistics

However, it is a fact that linguistics is a highly diverse science. There are many linguistic sub-disciplines such as phonetics, phonology, semantics (formal or cognitive), neurolinguistics, or sociolinguistics, among others. And there are many different ways of working in all these disciplines, *e.g.* hand in hand with cognitive science, or with a focus on statistical evaluation, together with psychologists, in a speech-laboratory, etc. Anyhow, since Harris' transformational theory was an influential step in the development of syntax, I argue that it can be seen as a representative case for a mature and influential part of linguistics.

It might be rightfully argued that the case of Harris' theory is not fully representative for the whole of linguistics. I see no problem in this, since linguistics has a high level of ramification and sophistication in the most diverse ways. It might well be that OSR cannot be plausibly formulated for other parts of linguistics, but as far as Harris' syntactic theory is concerned, I argue that OSR can be upheld successfully.

Furthermore, I see Harris' theory as an influential part of linguistic theory in the twentieth century, since it marks specifically the change of paradigms, from the Pre-Chomskyan era to the Chomskyan phase of linguistic thought. In that sense, it will also be interesting from the perspective of theory change, since some parts of Harris' theory might well be overtaken from successor theories, such as structural linguistics. Geoffrey Pullum argues that transformational theory is not only indebted to Chomsky's original research, and that Harris' and also Carnap's contributions should be given its place:

> TGG is generally assumed to have sprung entirely from Chomsky's work, specifically his large unpublished manuscript . . . and the brief undergraduate lecture digest of it that was published as SS[3]. While linguists are aware that the term "transformation" comes from the work of Chomsky's mentor Zellig Harris, and some have noted that Harris probably took the term from Carnap (1934), it has gone almost entirely unremarked that the underlying mathematics is largely present in much earlier work, overlooked by linguists because Chomsky never cited it (Pullum 2011: 284).

However, whether OSR is also reasonable in other fields of linguistics is a question of future investigation. The aim in this chapter is to formulate

[3]By 'SS', the author refers to Chomsky's *Syntactic Structures*.

OSR in one concrete theory of syntax. As other authors that I have mentioned above (Kincaid, Ross) have argued as well, I see interesting analogies to the approach ontic structural realists have, when they look at contemporary physics.

If one wants to formulate OSR in a special science, one has to argue that there exist good reasons to believe that the structures that are postulated within a concrete theory are ontologically more fundamental than the individuals that are part of the same theory. The relation between the respective special science and physics should be left beside. In strict terms, it is clear that "the world" ultimately is made of elementary particles, but within one concrete empirical theory (such as transformational theory), the fundamental entities can well be seen as structures, and as "real".

Nevertheless, there is no doubt that there cannot be a general claim for the applicability of OSR to all fields of special science. What is needed are concrete case-studies and the discussion of concrete examples, as I have provided here.

5.5 Conclusions

In this chapter, I have mentioned that OSR has been weighted mostly in connection with results of contemporary physics. We have seen that the primacy of physics can be better formulated in a weaker version that allows to pay more tribute to the special sciences. Furthermore, I have argued that Lyre's position on OSR is problematic, by highlighting that there is in fact a way of formulating OSR in the special sciences. For this purpose, I have shown a concrete case of linguistics, where it is more reasonable to assume OSR than any other ontology, in order to give the theory a philosophically well-formulated fundament. The kernel sentences and transformations in Harris' linguistic theory are the ontological fundament of this branch of linguistics, and they are closer to be structures, than to be understood as individual objects.

As I have mentioned above, it is necessary to outline more cases of OSR in the special sciences. It might well be the case that OSR is applicable only to some empirical theories in some branches of science. This might not be problematic. OSR can well work in physics, in some parts of biology and in some parts of linguistics, maybe also in certain branches of social science. This shall not present a contradiction or a problem for OSR. Once we have accumulated more specific examples of OSR in the special sciences, and case studies are worked out, the relation between the different scientific disciplines with respect to OSR can become clearer. With this chapter, I aim to make a contribution towards this end.

Chapter 6

A case study from linguistics

6.1 Mathematization and maturity

From the perspective of the philosophy of science, it is a central question to ask when a scientific discipline achieves maturity. The notion of mature science has been used in a widespread manner, but it is rather unusual that philosophers who employ the term also define quite well in which sense they employ it. A science is typically taken as mature when there is a significant level of unification and coherence, as well as some level of mathematical sophistication within a discipline. In a mature science, no one seriously questions fundamental postulates, *e.g.* in contemporary physics, no one would ever doubt of the laws of thermodynamics.

Linguistics is a polemical case in this context. It is not entirely clear that there is an absolute consensus about certain fundamental principles of the field. There is still a huge amount of open questions and controversial subjects in this discipline. Discussions in semantics, with entirely different approaches (such as generative, statistical, cognitive, or formal) are an example of this. In this context, the famous work *The Linguistics Wars*, by Randy Allen Harris (1993) discusses the still immature status of linguistics, at least in some domains. Harris famoulsy outlines the discussion between adherents and criticizers (such as Lakoff and Ross 1967/1976) of Chomsky's generative grammar.

However, I want to argue that at least in the development of the linguistic discipline of syntax, a significant level of maturity has been achieved. This might not be so in other linguistic disciplines, especially in semantics. I argue that the phase of theoretical change from Bloomfieldian structural linguistics to generative grammar meant a significant step towards the maturity of linguistics. I will rely on Bird's (2013) explication of the Kuhnian concept of maturity. He expresses as follows:

> Kuhn describes an immature science, in what he sometimes calls its 'pre-paradigm' period, as lacking consensus. Competing schools of thought possess differing procedures, theories, even metaphysical presuppositions. Consequently there is little opportunity for collective progress. Even localized progress by a particular school is made difficult, since much intellectual energy is put into arguing over the fundamentals with other schools instead of developing a research tradition (Bird 2013).

In what follows, I will outline and show that it was primarily the introduction of mathematical methods that enabled Post-Bloomfieldian linguists to explain syntactic phenomena in a better way than structural linguists. There is no doubt that today, there are still many different branches of linguistics (*i.e.* Cognitive, Generative, Typological, etc.), which stand in contrast to each other. However, there is nowadays a certain consensus on fundamental principles of syntax. These principles were discovered by Harris and Chomsky. From a historical perspective, I aim to provide answers to questions about radical theory change, with Harris and Chomsky as the founding fathers of transformational generative grammar. It should become clearer to understand the shortcomings of structural linguistics and in consequence to understand why this radical theoretical change from one paradigm to the new paradigm of generative grammar occurred. Through the contribution of this work, we should be able to see with more clarity where the gaps between both paradigms lie, and where similarities hold.

Let us now turn the focus on the role of mathematization. The discussion of the historical development of this part of the history of linguistics is of central importance for my dissertation. By showing that this phase of theory change lead to a significant progress in linguistics, I motivate the point that this part in the history of linguistics becomes interesting from the perspective of the philosophy of science. One could ask how radical the shift from Bloomfield to Harris and Chomsky was, and if there was indeed such a radical, “revolutionary" shift. This also makes it especially interesting for structural realism, since one could ask about the structures that might have been continuous in this case. As I will discuss and show below, there were some important changes in the ontology of the theories, but there were also structural continuities.

6.1.1 Linguistics matures

Leonard Bloomfield is widely recognized as one of the most important linguists in the first half of the twentieth century. Especially in the phase before Chomsky somehow *revolutionized* linguistics. Bloomfield's theory of structural linguistics provided the basic fundament for later linguistic theories. His theory was extremely influential in the community of linguists in that time. After the publication of his first book *An introduction to the study of language* in 1914, Bloomfield dedicated his work to the study of the Algonquian languages, especially to Menomini, as his influential work *Menomini Morphophonemics* shows. Bloomfield's main work is his 1933 *Language*. He presents his whole theory in this work. Especially his theory of meaning changed from a mentalist position in 1914, to a radical behaviorist one in 1933. Due to the whole of Bloomfield's work and the great influence it had, the so-called school of *American Structural Linguistics* arose. Until the upcoming of Chomsky's highly influential early work in the late fifties, structural linguistics was seen as the *standard* approach in linguistics.

Earlier and during the same time, many structuralist schools in different places were developed. All of them had in common that they related back to Ferdinand de Saussure's structuralist linguistics. The so-called Copenhagen-school, with its main representative figure Louis Hjelmslev and the Prague-school with its main figures Roman Jakobson and Nikolai Trubetzkoy are generally counted as the central figures. What all these structuralist approaches have in common is their focus on language as a structured (ordered and recurrent) phenomenon, and that the best way to study language is to understand it as consisting of certain smaller structural parts, into which it should be decomposed. The name structuralism may have little in common with other structuralisms in other fields like in philosophy, mathematics, anthropology or literary theory. The main motivation for calling these linguistic schools structuralism comes probably out of the history of phonology. There, one of the core figures was Jan Baudoin de Courtenay, who understood language as a composition of small, structured units, which actually realize sounds. It was Baudoin de Courtenay who introduced the notion of phoneme into linguistics. Later, it was the group of european structuralists like Trubetzkoy and Hjelmslev that systematized it[4].

Besides many other works, Bloomfield's 1926 *A Set of Postulates for the Science of Language* is his own intent of axiomatizing linguistics. He outlines a list of definitions and assumptions, which aim to state clearly what linguistics is about. In this sense, this work of Bloomfield counts as a work on the foundations of his own scientific discipline and can also be seen as a contribution to the philosophy of linguistics. It is of special importance for a logical reconstruction of Bloomfield's theory. Bloomfield states the importance of what he calls the *postulational method*:

> The method of postulates (that is, assumptions or axioms) and definitions is fully adequate to mathematics; as for other sciences, the more complex their subject-matter, the less amenable are they to this method, since, under it, every descriptive or historical fact becomes the subject of a new postulate. Nevertheless, the postulational method can further the study of language, because it forces us to state explicitly whatever we assume, to define our terms, and to decide what things may exist independently and what things are interdependent (*ibid.*: 153).

[4]For a detailed study of the history of structural linguistics, see Seuren (1998).

In this work, Bloomfield lists the fundamental postulates and definitions of his theory. He aims to contribute to a clarification to the object of study of the in 1926 yet *immature* science of linguistics.

Bloomfield's motivation of making linguistics a more precise science can be seen as continuous with what has been called the *Wissenschaftliche Weltauffassung* by members of the Vienna Circle as Otto Neurath and Rudolf Carnap. Bloomfield shared many meta-theoretical assumptions with Neurath and, common in the years before World War II, also to Bloomfield a *scientistic* world-view had become attractive. This can also be seen by the fact that Neurath invited Bloomfield to contribute in his "International Encyclopedia of Unified Science", where Bloomfield published his article (originally from 1939) *Linguistic Aspects of Science* in 1955[5]. Already in his 1926, Bloomfield points out quite clearly what he expects of a mature science of language:

> Also, the postulational method saves discussion, because it limits our statements to a defined terminology; in particular, it cuts us off from psychological dispute. Discussion of the fundamentals of our science seems to consist one half of obvious truisms, and one half of metaphysics; this is characteristic of matters which form no real part of a subject: they should properly be disposed of by merely naming certain concepts as belonging to the domain of other sciences (*ibid.*: 153-154).

I will now focus on Bloomfield's linguistic theory. His theory was widely accepted as the standard theory in linguistics in the first half of the twentieth century. The important point is that his theory was widely

[5] see Ernst *et al.* 2002, for more detailed information on Bloomfield's contact to Neurath and Carnap.

accepted, but it was not able to explain a lot about syntax. Typically, structural linguistics had a clear focus on the fields of phonology and morphology. There, linguists were able to do actual descriptive work on languages that were unknown by the community of linguists by that time.

An important principle for Bloomfield is that language manifests through acts of speech. Such acts are concrete actions of utterance. Languages are always spoken in linguistic communities. Another central feature of Bloomfield's theory is that recurrent sound features are linguistic forms, and that these forms receive meaning through recurrent stimulus-response-features, since Bloomfield endorsed a behaviorist picture of meaning. There is no semantics grounded on mental principles. The only way to study linguistic meaning is by behaviorist psychology (in Bloomfield's scientistic view - in 1933). In fact, by the time of 1914, Bloomfield endorsed a mentalist view of meaning, of which he had retrieved by 1933, most likely also inspired by logical positivism and, more generally, scientism.

Morphology and syntax

Interestingly, Bloomfield already mentioned what was systematized later by Harris: That certain morpheme-classes correctly combined build up a grammatical sentence. This insight allowed a significant systematization of syntax, since one could start classifying classes of morphemes and was in a position to describe language in a clearer way. Another important notion in Bloomfield is the notion of construction: "Each position in a construction can be filled only by certain forms" (Bloomfield 1926: 158). This notion as well presented a clarification of syntactic theory.

The insight that there exist "positions" within "constructions" already presented a first step towards a formalization of syntactic theory. We can see the following example, in order to make Bloomfield's point clear:

(vi) *The tree is old.*

(vii) *The _ is old.*

In the empty slot, only certain classes of morphemes (or forms) can occur. In this case, certain noun-classes. Furthermore, the notion of substitution was characterized by Bloomfield in the following way:

> A substitute is a linguistic form or grammatical feature which, under certain conventional circumstances, replaces any one of a class of linguistic forms ... thus, in English, the substitute *'I'* replaces any singular-number substantive expression, provided that this substantive expression denotes the speaker of the utterance in which the substitute is used ... the substitute replaces only forms of a certain class, which we may call the *domain* of the substitute; thus, the domain of the substitute *'I'* is the English form-class of substantive expressions (Bloomfield 1933: 247).

As well as in the case of constructions, the notion of substitution provided a clear fundament for parts of syntactic theory. By classifying certain expressions and then sorting out in which position these could stand within a bigger construction, a criterion for well-formed sentences was given.

Transformations and discourse analysis

The origins of Harris' transformational theory can be explicated by his interest in studying the different levels of language. Being an adherent of structural linguistic theory, Harris primarily searched for a criteria of differentiation between sentences. Before him, within structural linguistics, the analysis of language had been carried out from the most elementary level of phonemes up to the level of sentences, as Bloomfield developed it in his 1933 *Language*. Bloomfield's book was a standard introduction to linguistic theory for decades. In syntactic theory, Harris wanted to go further than Bloomfield and searched for a systematization of sentence-types. He developed a method that enabled linguists to describe how sentences are built and changed from one mood-type into another, like from active to passive. Within structural linguistics before Harris, it was possible to describe a text as a sequence of sentences, but with such a description, linguists were only able to describe the relations between the elements of one sentence, but not between a sentence and other sentences within the same text. Harris' transformational theory allowed to explain more linguistic phenomena. In this sense, it helped linguistic theory to mature significantly.

Within this context, Harris developed his *Discourse Analysis*. The method of discourse analysis proceeds as follows. It is a method of analyzing complete text corpora. The proper aim is to describe the relative distribution of the elements which occur in a text. This aim is achieved by building the classes of the elements, which have identical or equivalent environment in a text. Each sentence is then represented as a sequence of such classes. By obtaining a description of the structure of a text, it is possible to study the semantical information of the text. The idea was to provide semantical interpretations of text corpora. By providing such interpretations, it was thought, results for mechanical text processing could be obtained. Harris' motivation came also from a semantic standpoint. Since some definite semantic information was to be deduced from the structure of the text, these methods could assign only such structures as did not change this semantic information.

Phrase structure and formal grammar

Chomsky took Harris' ideas and developed them further. This starts already with his master's thesis (1951) *Morphophonemics of Modern Hebrew*, and later with his ground-breaking (1955) *The Logical Structure of Linguistic Theory* and his (1957) *Syntactic Structures*. As in Harris, the mathematization consists of the application of logical tools, *i.e.* first-order logic, especially predicate calculus, set-theoretic tools and, more abundantly than in Harris, graph-theoretic methods (*e.g.* syntax-trees). Chomsky went further and developed a formal theory of grammar, where transformations manipulated not just the surface strings, but the parse tree associated to them, making transformational grammar a system of tree automata. It was a further development towards the same direction. Parsing is the process of recognizing an utterance by decomposing it to a set of symbols and analyzing each one in the grammar of the language. As a result, the first step to describing the meaning of an utterance is to break it down part by part and look at its analyzed form.

The phrase structure rules that were introduced by Chomsky lead to a significant progress. In a sense, these rules were a further development on Bloomfield's first characterization of notions like position, construction or substitution. Such phrase structure rules determine how the constituents

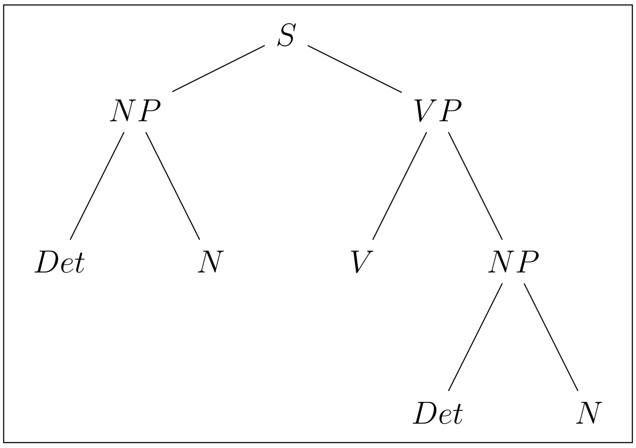

Figure 6.1: Example of a syntax-tree for the sentence: "The student buys the book". Such graphical representations present a central aspect of mathematization in inguistics.

of a phrase are ordered. According to Chomsky (1957), the phrase structure rules for the english noun phrase (NP) can be formulated as follows:

(viii) *John* (N)

(ix) *the car* (Det N)

(x) *a big car* (Det Adj N)

(xi) *a car in a garage* (Det N PP)

The general phrase structure rule for the english noun-phrase, according to Chomsy (1957):

NP $\rightarrow$ (Det) (Adj) N (PP)

This means that the typical noun phrase consists of a determiner (such as 'the'), if required, an adjective (such as 'green'), then a noun (such as 'idea'), and then a prepositional phrase at the end, if required. After having formalized linguistics in such a way, Chomsky also established the notion of formal grammar. A formal grammar of this type consists of:

1. A finite set of production rules (left-hand side → right-hand side) where each side consists of a sequence of the following symbols:

2. A finite set of nonterminal symbols (indicating that some production rule can yet be applied)

3. A finite set of terminal symbols (indicating that no production rule can be applied)

4. A start symbol (a distinguished nonterminal symbol)

According to Chomsky, a formal grammar generates a formal language, which is a (usually infinite) set of finite - length sequences of symbols (*i.e.* strings) that may be constructed by applying production rules to another sequence of symbols which initially contains just the start symbol. A rule may be applied to a sequence of symbols by replacing an occurrence of the symbols on the left-hand side of the rule with those that appear on the right-hand side. Such a grammar defines the formal language. The notions of deep structure and surface structure were introduced later (with Chomsky 1965). Chomsky's proposals from 1965 imply more controversial views on the relation between semantics and syntax.

For Chomsky, every sentence had a deep structure and a surface structure. In the beginning, Chomsky claimed that deep structure determined meaning, etc. For the 1965 Chomsky, even interrogative sentences like:

(xii) *Which car did John wash?*

had a deep structure, where their real meaning was

(xiii) *John washed that car.*

I have argued that Bloomfield already systematized an important part of syntactic theory. He introduced that certain linguistic forms can be grouped into classes. Such form-classes cannot be substituted by any other form-class. Harris took Bloomfield's ideas on syntax and developed them. By introducing his formalism, Harris took a first step towards a mathematization. This made it possible to give linguistic theory a stronger explanatory power and certainly helped linguistics to become a *mature* science. With Chomsky, syntactic theory became fully "formal", in the sense that it was possible to develop a formal grammar, as I have shown above.

The main advantage of applying mathematical methods is clearly that any domain of knowledge can be studied in a more precise way. It enables us to demonstrate objectively new results. One makes the achieved results actually revisable, intersubjectively, by anyone who knows certain formal methods. Only through mathematization, a scientific theory is supposed to have *predictive power*. This can be measured, as well as the success of the explanations it provides.

6.2 Bloomfield's structural linguistics

In this section, I will introduce the central notions and concepts of Bloomfield's theory. I will provide a list of the most important concepts of the theory, explaining them through examples and showing textual evidence by quoting original passages of Bloomfield's works.

One aim of this section is to provide a logical reconstruction of one of the main representative theories of linguistics, which is Bloomfield's theory of structural linguistics. This theory has often been interpreted as

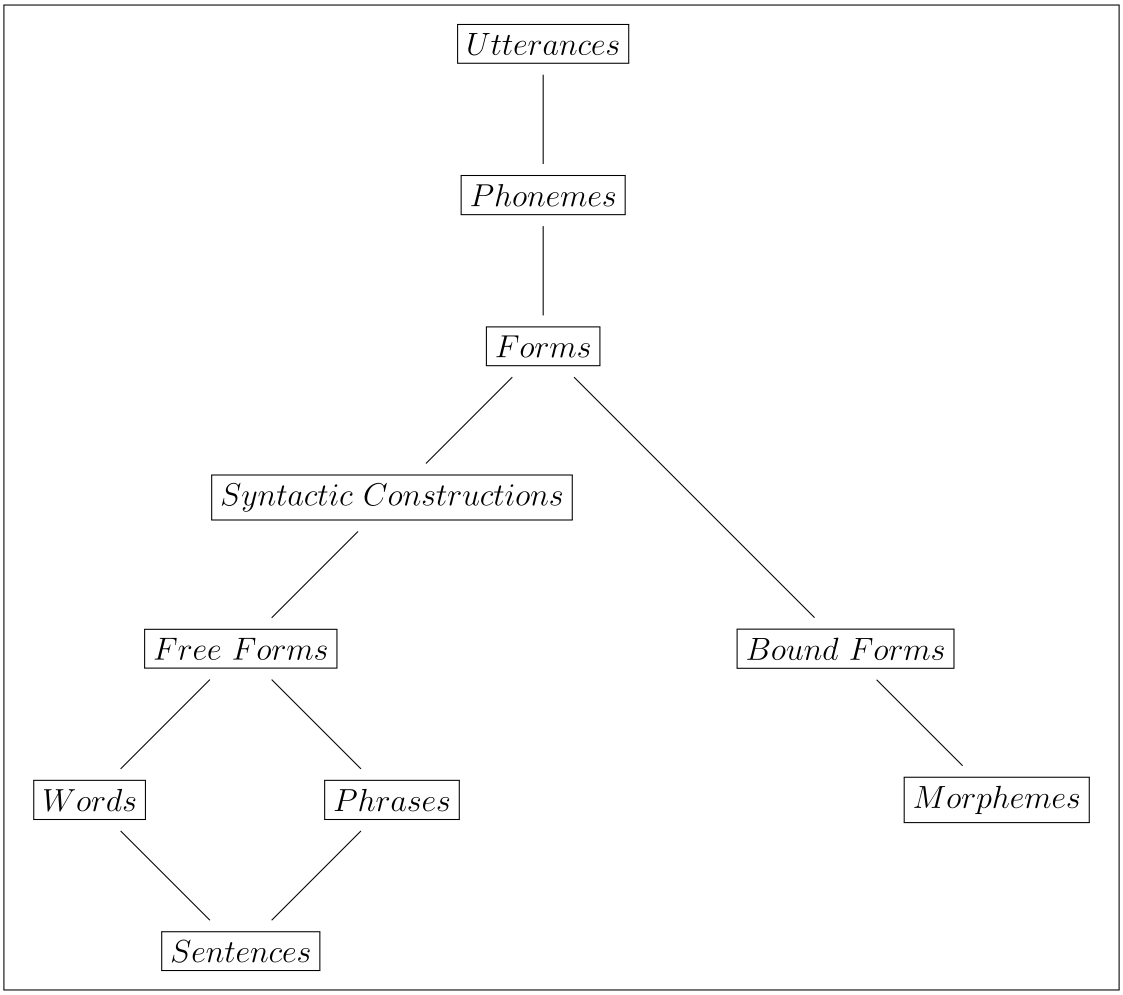

Figure 6.2: *The hierarchy of linguistic levels of Bloomfield's theory.*

the most influential and paradigmatic theory of language in the first half of the twentieth century[6]. From a historical perspective, an analysis and a reconstruction of Bloomfield's theory can provide answers to questions about radical theory change, with Harris and Chomsky as the founding fathers of transformational generative grammar. It should become clearer to understand the shortcomings of structural linguistics and in consequence to understand why this radical theoretical change from one paradigm to the new paradigm of transformational generative grammar occurred.

Other than the conceptual part, there is also a sociological explanation for this case of theory change. This has been pointed out by Frederick Newmeyer:

> We know that there was a Chomskyan revolution - but not because every linguist in the world was at one time, or is now, a generative grammarian. There was a Chomskyan revolution because anyone who hopes to win general acceptance for a new theory of language is obligated to show how the theory is better than Chomsky's. Indeed, the perceived need to outdo Chomsky has led him to be the most attacked linguist in history (Newmeyer 1996: 30).

Newmeyer's discussion has a strong sociological focus on the issue of theory change. By approaching the question of theory change from a less sociological, but a more conceptual and logical point of view, I think there can be deeper insights to the question of theory change.

My reconstruction of Bloomfield should be seen as a necessary preliminary work if one wants to find ultimate answers in this debate. Nev-

[6]See Newmeyer 1986, 1996 and Seuren 1998 for detailed historical reconstructions.

ertheless, it is an indirect contribution to the study of theory change in linguistics, since the central result of this reconstruction is the fact that Bloomfield's work had so much in common with the early work of Rudolf Carnap, methodologically and epistemologically. In this sense, this reconstruction provides new insights as it shows how structural linguistics and Carnap's *Aufbau* are related.

In this sense, the present section presents both a case study of general interest about the foundations of structural linguistics and a historical reconstruction of the relation between Bloomfield's and Carnap's early work. Both authors stand for structuralist approaches. The difference is that Carnap's structuralism is about epistemology, and Bloomfield's structuralism concerns language. I will outline that for Carnap, the fundament of his constitutional system were the so-called fundamental experiences (*Elementarerlebnisse*), and that Bloomfield grounded his system on the notion of utterance. By applying Carnap's method of quasianalysis, we will see how Bloomfield's theory can be constructed in an analogous way to Carnap's epistemology in the *Aufbau*.

6.2.1 Quasianalysis

During the last years, there has been a revival of Carnap's work, especially in the sense that a lot of contributions have been made to the historical reconstruction of Carnap and intents of applying it to contemporary philosophical questions (see Moulines 1991, Richardson 1997, Friedman 1999 and 2011, or Leitgeb 2007, among others). This work should be seen as a contribution to this revival, with the focus of enriching Carnap's approach even more by connecting it with fields of which we might have been unaware of until now, such as linguistics.

Bloomfield had contact with some of the main figures of logical positivism[7]. This is not surprising, if one takes a closer look into Bloomfield's work, as well as into some of Carnap's work, especially the *Aufbau*. For both, the relation of similarity was a fundamental concept. Carnap based his system of knowledge description in the *Aufbau* on the so-called *recollection of similarity*, Bloomfield based his theory on a relation of partial similarity between linguistic entities, as we will see below. Carnap started by reconstructing knowledge of an ideal agent on the basis of the *elementary experiences*. In this sense, his *Aufbau* was an epistemological program.

Below, I will show how one can make use of Carnap's quasianalysis to construct Bloomfield's theory. Under certain modifications, I think it gives us a powerful logical tool. As Moulines' (1991) states:

> Quasi-analysis is one of the most interesting methodological inventions of the first Carnap. It consists in formally deriving (= constructing) what we intuitively regard as parts of a whole from the wholes themselves taken as prior units. In this sense, the procedure is similar to ordinary conceptual analysis, but it differs from it in that it derives the (intuitive) parts as classes (of classes) of wholes ... The procedure is in itself interesting, quite independently of the epistemological use Carnap makes of it. It could be applied to other issues of conceptual analysis as well (*ibid.*: 272-273).

[7]Tomalin (2006) gives a detailed presentation and discussion of the knowledge that Bloomfield had of works of members of the Vienna Circle. According to Tomalin's reconstruction, especially Carnap's work in *The Logical Syntax of Language* had a strong influence on Bloomfield and on Harris and Chomsky as well.

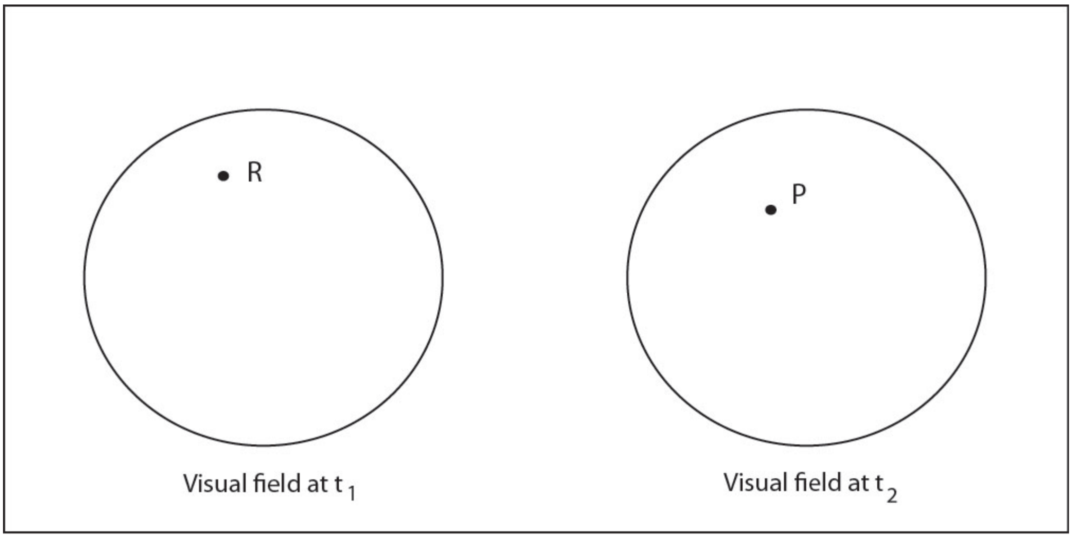

Figure 6.3: *The points 'R' (a red color spot) and 'P' (a pink color spot) represent color spots within the visual field, as it is presented by Carnapian quasianalysis. These two spots are not identical, but partially similar, since they are close to each other. For the purpose of our reconstruction of Bloomfield, we will think of these points as utterances.*

In the sense that Moulines proposes, I will apply quasianalysis as a tool for conceptual analysis to other fields, namely, to linguistics (see figure 6.3). By his method of quasianalysis, Carnap was able to describe our knowledge of the world stepwise, by moving on from the elementary experiences up to macroscopical objects. We will see below that this is analogous to Bloomfield's intent of describing natural language by relying on a relation of partial similarity between utterances and other linguistic entities. If quasianalysis is applied to a concrete physical system, such as speakers utterances, such problems might not appear. We will see below that, whereas for Bloomfield's theory, phonemes and forms are theoretical entities, we can think of Carnap's quality classes as being theoretical entities in the *Aufbau*.

Considering difficulties that may arise when quasianalysis is applied, I agree with Moulines (1991) that:

> These difficulties, however, should not be overrated. It is important to note that the counter-examples to quasi-analysis do not show that this method is inapplicable, but that, in some very particular cases, it won't lead to the results expected. All criticisms of Carnap's definitions ... recognize through the consideration of numerous examples that normally the right items can be abstracted from a given list of experiences through quasi-analysis (*ibid.*: 282).

For the reconstruction of Bloomfield's theory and the analogy to quasi-analysis, it will be useful to employ graph-theoretical notions. Leitgeb (2007) already approaches Carnap's quasianalysis with graph-theoretical methods. I will follow Leitgeb's proposal and employ the same notions, since they will prove to be useful for my reconstruction of Bloomfield's theory. It will turn out that phonemes and speech communities can be defined as maximal cliques, after the relations of partial similarity have been introduced. For matters of clarification, I will introduce the following basic graph-theoretical notions: A clique in an undirected graph $G = (V, E)$ is a subset of the vertex set $C \subseteq V$, such that for every two vertices in C, there exists an edge that connects both. A maximal clique is then a clique that cannot be extended by including one more adjacent vertex. Leitgeb (2007) argues for the representation of quasianalysis with graph-theoretical methods as follows:

> Think of a room with coloured objects, where sharing a colour is used as a similarity relation. Each colour may be supposed to embrace a certain range of hue, brightness, and intensity, and colours are permitted to 'overlap'. A set X of individuals

> which are brown (partially or completely) will then certainly be a clique with respect to similarity, since every two members of X share a colour. In order to turn from a set such as X to the set of brown individuals in this room, and accordingly for the other colours, one might take maximal cliques rather than just cliques simpliciter in order to constitute the colour properties. That is essentially the core of the method of quasi-analysis, a procedure by which Carnap's so-called 'similarity circles' (see Aufbau, Sections '70-73, 80-81, 97, 104), *i.e.*, our maximal cliques, are constituted (Leitgeb 2007: 187).

In what follows, we will see an example of how quasianalysis can be successfully applied to the reconstruction of Bloomfield's theory. First, I will give a list of the central concepts of Bloomfield's structural linguistics, followed by examples. This will be a first part of the logical reconstruction. In this reconstruction, the main focus lies on analyzing how Bloomfield constructs a natural language on the basis of a relation of partial similarity between utterances. By starting with the utterances, it is possible to construct phonology.

Before I start with my logical reconstruction of Bloomfield's theory, there is one further outstanding historical fact about the interrelation between Carnap's quasianalysis and linguistic methodology that needs to be outlined, *i.e.* the fact that Chomsky already recognized this methodological similarity in his master's thesis:

> Thus Carnap in the Aufbau . . . begins with a primitive relation between slices of experience and attempts to construct, by a series of definitions, the concepts of quality class, qual-

> ity, sensation, etc., *i.e.*, he tries to construct concepts for the most general description of experience. Similarly, it can be shown that the theoretical part of descriptive linguistics, beginning with three 2-place predicates of individuals, and restricting its individuals to a tiny domain of experience (*i.e.* speech sounds) can construct concepts such as 'phoneme', 'morpheme', etc., which are available for a general description of that part of experience called linguistic phenomena (Chomsky 1951: 1-2).

This highlights that the methodological analogy between Carnap's epistemological program and Bloomfield's approach to linguistics has to be extended to Chomsky. At least, we can safely presume that Chomsky was aware of this methodological similarity, and that he fully endorsed it.

6.2.2 Bloomfield's central notions

I will now introduce Bloomfield's central notions. For this purpose, I will quote relevant passages of Bloomfield's work and I will start with a set-theoretic and logical characterization of these notions.

I. **Utterances**: Bloomfield introduces the notion of utterance as one central notion of his theory. Utterances (as tokens) are acts of speech. The following quotes illustrate Bloomfield's position: "An act of speech is an utterance" (1926: 154), and "A speech-utterance is what mathematicians call a continuum; it can be viewed as consisting of any desired number of successive parts" (1933: 76).

Let U be a set of utterances, which will be qualified below. The set U is the basic domain of Bloomfield's theory.

Bloomfield does not mention whether his notion of utterances is always restricted to a specific point in time, a language and whether these utterances are all the possible utterances. He implicitly presupposes that each utterance is always made by a speaker of a specific language.

II. **Forms**: The following quote illustrates Bloomfield's notion of forms: "... a form is a recurrent vocal feature which has meaning, and a meaning is a recurrent stimulus-reaction feature which corresponds to a form (1926: 155)." Bloomfield's use of the notion of form suggests that forms are types and that they are abstract entities. This furthermore suggests that forms are theoretical entities, and likely to be understood as theoretical terms. Forms are non-observational, as opposed to utterances, with which the empirical linguist is confronted in the actual descriptive work. Furthermore, any word instantiates a form. Also the components of words, in their minimal occurrence, the morphemes, are forms. Following Bloomfield, a form is then any vocal feature which frequently is uttered and which is at least partially alike to further utterances. This will become clear as we move on with the reconstruction. Let $\mathfrak{F}$ be a set of forms. Furthermore, it is important to note that several linguistic forms can be concatenated. We introduce the function $concat : \mathfrak{F} \times \mathfrak{F} \rightarrow \mathfrak{F}$. This is the concatenation function on forms.

III. **Partial similarity**: The relation of partial similarity is of central importance in Bloomfield's theory. Very much analogous to Carnap's recollection of similarity in the *Aufbau*, Bloomfield builds up the whole of phonology on this notion. We will distinguish between two relations of partial similarity, the first one holding between utterances, and a second one which is necessary to constitute speech communities, as we will see below. Let $\approx^1$ be the relation of partial similarity one: $\approx^1 \subseteq U \times U$, meaning that an utterance u is in the relation $\approx^1$ to an utterance v iff u is at least partially similar to v. The relation is reflexive and symmetric, but not transitive. If it were transitive, it would mean that all utterances are partially similar to each other. But this is clearly not the case. There is without a doubt a partial similarity between the utterances 'car' and 'cat', but there is no partial similarity between 'car' and 'bet'. Let $\approx^2$ be the relation of partial similarity of the second type. This is a different relation than the already defined relation $\approx^1$, since $\approx^2$ holds between utterances of speakers in speech communities. The relation $\approx^1$ is represented in figure 6.4.

We will consider the following example: Imagine a speaker S. During a certain interval of time, this speaker utters the following words (in English):

(ivx) *I am in my house.*

(xv) *I am tired.*

(xvi) *He was bored.*

We are now able to identify partial similarities between parts of these three uttered sentences. 'I am' in (ivx) stands in this relation to 'I am' in sentence (xv), and 'tired' in sentence (xv) stands in this relation to 'bored' in sentence (xvi).

In order to make sense of Bloomfield's fundamental thesis of alikeness of utterances, which is of central importance to distinguish between speech communities, the following quote will be helpful:

> ...the study of significant speech-sounds is phonology or practical phonetics. Phonology involves the consideration of meanings. The meanings of speech-forms could be scientifically defined only if all branches of science, including, especially, psychology and physiology, were close to perfection. Until that time, phonology and, with it, all the semantic phase of language study, rests upon an assumption, the fundamental assumption of linguistics: we must assume that in every speech-community some utterances are alike in form and meaning (1933: 77-8).

By relying on his behaviorist criterion of meaning, Bloomfield needs to assume such a thesis. If this assumption is not made, it is impossible to move on and describe the structure and, in general, the grammar of a natural language. By working with this assumption, the structural linguist can start to distinguish one group of speaker from another one and, most importantly, to classify all so-called speech-forms. Out of these speech forms, the linguist classifies the whole grammar of a language.

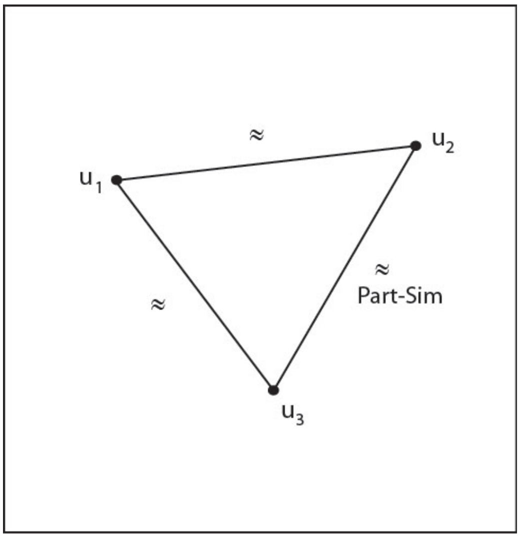

Figure 6.4: *This represents how utterances (u_1 to u_3) stand in the relation of partial similarity* $\approx^1$.

IV. **Phonemes**: The phonemes are related to the linguistic forms and are necessary to constitute the field of phonology within the whole discipline of linguistics. For Bloomfield:

> . . . a minimum same of vocal feature is a phoneme or distinctive sound. The number of different phonemes in a language is a small sub-multiple of the number of forms. Every form is made up wholly of phonemes (1926: 157). . . . we can find forms which partially resemble pin, by altering any one of three parts of the word. We can alter first one and then a second of the three parts and still have a partial resemblance . . . pin-tin-tan . . . and if we alter all three parts, no resemblance is left, as in pin-tin-tan-tack. Further experiment fails to reveal any more replaceable parts in the word pin: we conclude that the distinctive features of this word are the three indivisible units. Each of these units occurs also in other combina-

> tions, but cannot be further analyzed by partial resemblances: each of the three is a minimum unit of distinctive sound-feature, a phoneme (1933: 79).

We can see here that Bloomfield characterizes explicitly the notion of phoneme, by stating that what is a minimum unit of distinctive sound-feature, is a phoneme.

I will now introduce more formal notions. Let P be a set of phonemes. Above, I defined the relation of partial similarity between utterances: $\approx^1 \subseteq U \times U$: meaning that an utterance u is in the relation $\approx^1$ to an utterance v iff u is at least partially similar to v. The relation is reflexive and symmetric, but not transitive. The above quote fundaments the postulation of this relation, given that Bloomfield explicitly mentions the partial resemblance between utterances.

Let us consider the following example: Our speaker S utters the following words (with phonetic transcription in parentheses): *cough (kaf), car (kar), cat (kœt), cutter (kətə)*. Every concrete occurrence of a sound is a phone, and its abstracted and classified version is a phoneme. For Bloomfield, it is possible to individuate phones through his relation of partial similarity $\approx^1$. In the case of our example, this relation holds between all k that are uttered by S in all four utterances. Therefore, these four phones stand in the relation $\approx^1$. We are now able to define phones as follows:

Definition 3: x is a phoneme $:\leftrightarrow x$ is $\langle u, C\rangle$

where C is a maximal clique with respect to $\approx^1$ and u is an utterance, $u \in C$

The constitution of the maximal cliques of utterances is also shown in figure 6.5. Furthermore, we want to express that two or more phonemes can be combined in order to build bigger units of sound. Phonemes are connected and construct such larger units. These then can be connected to build up morphemes and bigger units such as words, phrases or sentences. Hence, we introduce an ordering relation on the set of phonemes. We call this the *parthood* relation for phonemes (symbolized by $\sqsubset$). It is a reflexive, antisymmetrical and transitive relation:

$\sqsubset \subset P \times P$

As an example, we can think of the word 'undeniable' which we can decompose into the three morphemes 'un', 'deny' and 'able', and into the three phonemes 'ən', 'dənaj', and 'əbəl' . By concatenating these three morphemes, the word 'undeniable' is built.

V. **Speech communities**: As Bloomfield states, "A speech community is a group of people who interact by means of speech" (1933: 42), and "The totality of utterances that can be made in a speech community is the language of that speech-community" (1926: 155). As simple examples, we can think of the group of speakers of English and the group of speakers of German. I will now introduce the following formal notions:

- Let T be a set of time intervals.

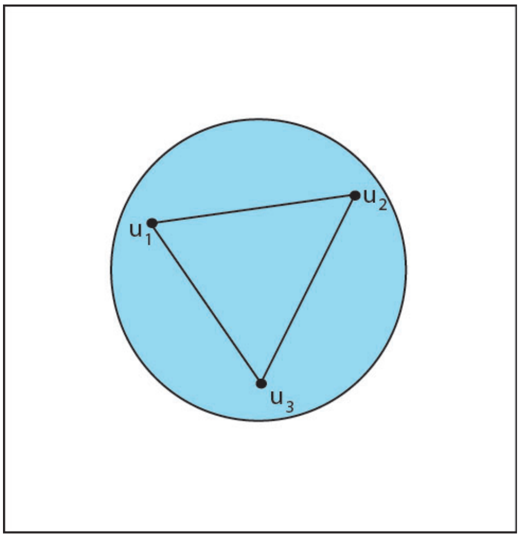

Figure 6.5: *On the basis of our relation of partial similarity $\approx^1$, we are able to build the maximal cliques of utterances. On the basis of this, we define phones as pairs of utterances and maximal cliques.*

- The function $t : U \rightarrow \mathfrak{T}$, from the set of utterances into the set of closed subintervals of T, where the value $t(u)$ of the function t at u is the least temporal interval in which the utterance u was produced (which we assume to exist).

  Let us consider the following example, to illustrate this point further: we imagine two speakers of English, our speaker S and one further speaker, called D. Both utter *house*. We label the first utterance as h_1, the second as h_2. Both utterances are partly alike, they stand in our similarity relation: $h_1 \approx^1 h_2$. They are different insofar that they are produced by different speakers, giving a different pronunciation to it[8]. They are different tokens of the same type of utterances. By studying utterances through this similarity relation, it is possible to individuate groups of speakers. As the similarity relation between utterances gets looser and is obtained

[8]This difference might be small or big. We can think of an English speaker from Australia and of a speaker form Cornwall, England. There, the difference between the two utterances would naturally be bigger than if both speakers were from Cornwall.

less and less, the distinction between two groups of speakers can be drawn. Naturally, the step from one group to another one is gradual. We can think of the group of speakers of Dutch and the group of speakers of German. The utterance *huis* from a Dutch speaker and the utterance *haus* from a German speaker are still partly alike, but the utterances *haus* and *maison* of French are not. On the one side, it would then be possible to set up a clear division line between German and French, but not between German and Dutch. It becomes clear that the similarity relation is not transitive.

Bloomfield's similarity relation is compelling in the sense that in fact, a clear distinction between groups of speakers cannot be drawn. Many times, *dialects* build the intermediary between groups of speakers. A speaker of Dutch might still understand a speaker of Low German (Plattdeutsch), a dialect spoken close to the Dutch border, but might in fact struggle to understand standard German, or another German dialect, like Swabian, spoken in southwestern Germany. For matters of simplicity, I will not discuss further sociolinguistical topics in this work. But it should be mentioned explicitly that there are of course many other complex sociolinguistical factors that influence on the devlopment and on the distinction between speech communities. Anyhow, I take Bloomfield's approach to be of high originality, and we have to see his theory in the light of the nineteen-thirties of the twentieth century. At that time, practically no work in sociolinguistics had been developed. In this sense, Bloomfield's work is a pioneer work in sociolinguistics.

- Furthermore, we need a formal characterization of speech communities. For this purpose, let L be a set of speakers, which will be qualified below.

  The constitution of speech communities is then as follows. Speech communities are maximal cliques with respect to the relation of partial similarity $\approx^2$ (see Figure 6.6). We can then proceed and define the partial similarity of speakers utterances as follows:

  > **Definition 4:** $S_1 \approx^2 S_2 :\leftrightarrow$ many utterances of S_1 are partially similar (in the sense of $\approx^2$) to many utterances of S_2.

  It should be clear that this is a vague characterization. I consider this to be unproblematic, since in the real world, boundaries between natural languages are in fact ambiguous and not strict. Speakers at the German/Dutch border may understand each other more easily, than a German speaker from Dresden and a speaker of Dutch.

  We can quantify over 'many' in different ways. It is an idealization, and we can consider 'many' in a strict, or in a looser sense. For example, we can think of the whole of all speakers of German as a speech community. But if we only take the German dialect of Bavarian, we can say that the speakers of Bavarian already build a speech community, since the utterances of all speakers of Bavarian are partially similar in a stricter sense than they are to all other speakers of German, in a looser sense.

  Moreover, it is important to note that there is also a way of considering small speech-communities, such as the community of all speakers of an endangered language, (say Otomí, spoken in central

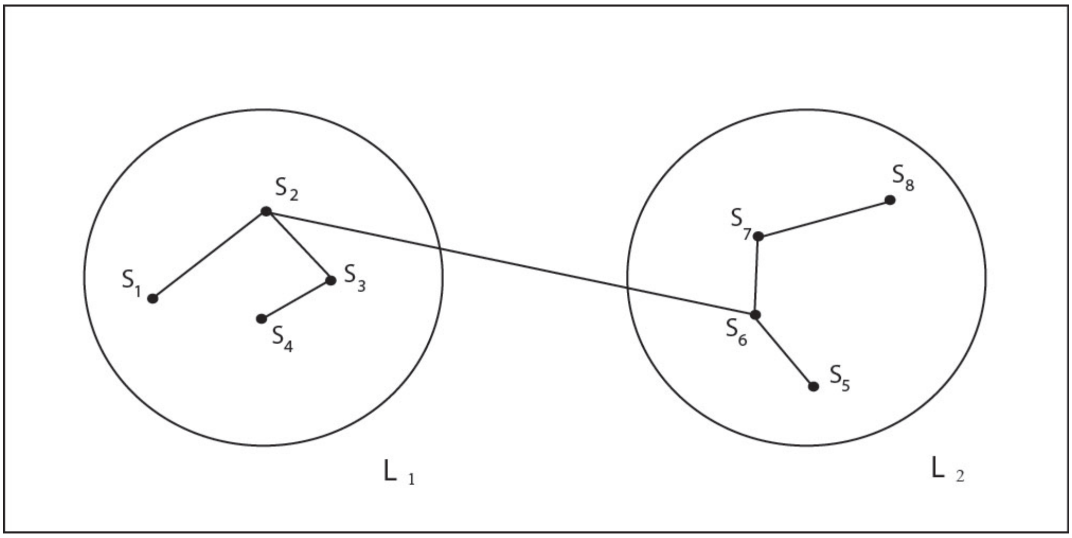

Figure 6.6: *The vertices represent speakers (S_n), the edges represent the relation of partial similarity $\approx^2$. Each circle represents a speech community, called L_1 and L_2.*

Mexico). A speech community with few speakers, (in the case of Otomí 20.000, according to Hecking *et al.* 1984), would not be a maximal clique, but a clique, in the sense of graph-theory. We can now define speech communities as follows:

Definition 5: L is a speech community $:\leftrightarrow$ L is a maximal clique with respect to $\approx^2$

The relation $\approx^2$ holds between speakers. On the basis of the partial similarity between the utterances of speakers, we are able to classify speech communities and to separate them from each other.

6.2.3 Potential models of SL

In what follows, I will define the set-theoretic predicates for Bloomfield's structural linguistics, giving it the set-theoretic predicate SL[1]. I will follow the usual way of structuralist methodology.

Definition 6: $M_p(SL) : x$ is a potential model of the theory of Bloomfield's structural linguistics ($x \in M_p(SL)$) iff there exist: $U, \mathfrak{F}, P, L, T, \mathfrak{T}, t, \approx^1, \approx^2, \sqsubset, concat$
such that:

1. U, $\mathfrak{F}$, P and L are finite and non-empty.
2. T is a closed interval of $\mathbb{R}$ of positive length.
3. $\mathfrak{T}$ is a set of closed subintervals of T of positive length.
4. $t : U \to \mathfrak{T}$
5. $\approx^1 \subseteq U \times U$
6. $\approx^2 \subseteq U \times U$
7. $\sqsubset \subset P \times P$
8. $concat : \mathfrak{F} \times \mathfrak{F} \to \mathfrak{F}$.

This represents the general framework of Bloomfield's theory. All concepts of the theory are explained within the potential model of SL. The basic intended interpretation is as explained above.

[1]It is a well known fact that Bloomfield had a behaviorist theory of meaning. Since his theory of semantics presents no further relation to the rest of his linguistic work, and, more importantly, since the shortcomings of behaviorist semantics have been abundantly discussed (see Chomsky 1959), I will not include Bloomfield's semantics into my reconstruction.

6.2.4 Actual models of SL

SL is an empirical theory of natural language. Being an empirical theory requires that it contains some laws, or at least lawlike statements. The issue of lawlikeness in the philosophy of science is varied and controversial. It might be intuitively clear that in physics, laws are easily identifiable and their range of applicability is almost always empirically testable. But not so in linguistics. If we want to contribute to the discussion of lawlikeness in linguistics, the correct way to go, I argue, will be to proceed from a concrete empirical theory of language, as it is our example of SL. If laws or lawlike statements can be identified in SL, it might help to establish more general conclusions on the issue of lawlikeness in linguistic theory. For the determination of the laws, or lawlike statements of SL, we define an actual model of SL. The fundamental principles of the theory, which are also required for the determination of the theoretical terms of SL, are given:

Definition 7: $M(SL)$: x is an actual model of the theory of Bloomfield's structural linguistics ($x \in M(SL)$) iff there exist $U, \mathfrak{F}, P, L, T, \mathfrak{T}, t, \approx^1, \approx^2$ $, \sqsubset, concat$, such that:

1. $x = \langle U, \mathfrak{F}, P, L, T, \mathfrak{T}, t, \approx^1, \approx^2, \sqsubset, concat \rangle \in M_p(SL)$
2. $\approx^1$ is reflexive and symmetric.
3. $\approx^2$ is reflexive and symmetric.
4. $\sqsubset$ is reflexive, antisymmetrical and transitive.
5. x is a phoneme $:\leftrightarrow$ x is a maximal clique with respect to $\approx^1$.

6. L is a speech community $:\leftrightarrow$ L is a maximal clique with respect to $\approx^2$.

7. $\forall f$ (f is a form) $\rightarrow \exists f_1$ (f_1 is a form and f_1 can be concatenated with f).

Basic intended interpretation: Condition 1 states that the actual model is also element of the potential model. But further conditions are necessary for becoming an actual model of SL. Condition 2 states that the relation of partial similarity $\approx^1$ is reflexive and symmetric, in the same way as condition 3 states this for the relation $\approx^2$. Condition 4 expresses the parthood relation for phonemes, whereas condition 5 expresses the definition of a phoneme. Condition 6 expresses the definition of speech communities L. Condition 7 expresses the fact that forms can be concatenated.

6.2.5 Theoretical terms in SL

In order to address the question of theoretical terms in SL, we adopt the structuralist criterion for theoreticity. In SL, we say that the forms ($\mathfrak{F}$), the phonemes (P), $\sqsubset$, *concat* and speech communities (L) are theoretical. They are newly introduced by Bloomfield's theory. Only if the conditions of the actual model $M(SL)$ are presupposed, they can be determined. The relations $\approx^1, \approx^2$ are basic observational terms. Their characteristic of being reflexive and symmetrical is an empirical claim and a generalization, based on what the structural linguist does in fact observe when languages are descriptively studied. Furthermore, utterances and the notion of parthood are observational, since both are directly observable. The linguist is first directly confronted with utterances, and after this, she will also be able to observe parts of utterances. This is why the notion of parthood should also be understood as being observational. The forms, phonemes and speech communities are theoretical terms, since they are in no relation to direct observation and no part of the phenomena that can be experienced. All three notions can only be constructed after the analysis of the observational phenomena has been carried out.

6.2.6 The partial potential models of SL

After having clarified and determined which terms are theoretical in Bloomfield's theory, we are able to define the Partial Potential Models of SL, where the theoretical terms are cut out. The partial potential models are the *data-models* of a theory. A linguist starts with the M_{pp} of a theory, once she describes a language. She will start by identifying utterances through similarities. This enables her to classify the forms, and then the phonology of a language, by making use of the relation $\approx^1$. After that, she will be able to define speech communities on the basis of the relation $\approx^2$.

Definition 8: $M_{pp}(SL)$: x is a partial potential model of the theory of Bloomfield's structural linguistics ($x \in M_p(SL)$ iff there exist: $U, T, \mathfrak{T}, t, \approx^1, \approx^2, t$ such that:

1. U is finite and non-empty.

2. T is a closed interval of $\mathbb{R}$ of positive length.

3. $\mathfrak{T}$ is a set of closed subintervals of T of positive length.

4. $t : U \to \mathfrak{T}$

5. $\approx^1 \subseteq U \times U$

6. $\approx^2 \subseteq U \times U$

6.2.7 The empirical claim of SL

As I have mentioned in the introductory section, the aim of a reconstruction of a particular empirical theory within the framework of the structuralist meta-theory is to formulate its empirical claim. Formally, the empirical claim can be best understood if we think of a core K as follows. Suppose we are given sets M_p and M_{pp}. Since M_{pp} is a set of all possible applications of some theory, we can interpret $\mathcal{P}(M_{pp})$ as the set of all combinations of applications for such a theory. Each element of $\mathcal{P}(M_{pp})$ is a set of possible applications and therefore a candidate for the set I of intended applications. Usually, not all elements of $\mathcal{P}(M_{pp})$ will have this property. So we can say that M, GC and GL are used as means of selection for picking out certain elements from $\mathcal{P}(M_{pp})$ or for picking out a subset of $\mathcal{P}(M_{pp})$.

The empirical claim consists in the statement that I, the set of intended applications, belongs to the content of K. In the framework of the structuralist meta-theory, the idealized empirical claim of T is that $I \in Cn(K)$. However, some empirical theories might have an empty empirical claim. This happens especially when no relevant constraints or links can be found. The elements of $Cn(K)$ can be described as combinations of partial potential models which can be "subsumed" under the theory, or which can be "treated by means of" T, or to which T can "be applied successfully". The empirical claim of SL can be expressed as follows:

$$K(SL) = \langle M_p(SL), M(SL), M_{pp}(SL)\rangle$$

$$T(SL) = \langle K(SL), I(SL)\rangle$$

$I(SL) \subseteq M_{pp}(SL)$ is such that members of $I(SL)$ are systems of utterances. The utterances build the empirical basis of $T(SL)$, on which SL is constructed via the relations $\approx^1$ and $\approx^2$.

The result of this reconstruction is that in fact, Bloomfield's theory has an empty empirical claim, since its theoretical terms can be defined explicitly. This goes very much along with Carnap as well, since he intended to give a framework for the construction of empirical science. Carnap's constitutional system is a framework for explaining the construction of scientific knowledge. In the same sense, Bloomfield wanted to define a framework for the description of language.

6.2.8 Conclusions for the reconstruction of SL

I have provided a formalization of Bloomfield's theory. We defined the models of SL in the framework of the structuralist meta-theory and I showed the information about laws in SL. The result of this reconstruction is that Bloomfield constructs phonology based on two relations of partial similarity and that it is also possible to individuate groups of speakers via his method. In a sense, Bloomfield's theory is empirically empty, but presents us rather with a conceptual and logical framework for linguistic theory. This holds especially for the disciplines of phonology and morphology. In this sense, with the logical reconstruction of Bloomfield, I provided a framework for linguistic theory, based on Bloomfield's structural linguistics.

One further important aspect is the historical fact that Bloomfield's method of distinguishing speech communitites was an early contribution to sociolinguistics. It is clear that nowadays, sociolinguistics has developed strongly and grown to be an independent discipline of linguistics. However, due to our reconstruction, it becomes apparent that Bloomfield made an early contribution to this field of linguistic inquiry.

One further result is a historical one. It is now clear that Bloomfield's method of developing a logico-conceptual framework for scientific inquiriy is analogous to Carnap's method in the *Aufbau*. Both authors rely on the method of quasianalysis, and on relations of partial similarity to construct their respective systems. Whereas Carnap outlined a structuralist program for the logic and the epistemology of science, Bloomfield developed a structuralist method of describing natural language based on utterances.

This result will be useful in order to clarify the relation between structural linguistics and transformational grammar. The former is clearly focused on phonology and morphology, whereas the latter approach is mostly an approach about syntax. In this sense, it might be compelling to ask if there could have been a radical theoretical change from structural linguistics to transformational grammar.

For the philosophy of science and its history, it is positive to see that methods that had been developed by Carnap found their applications in more remote fields such as linguistics. Carnap's methodological development of quasianalysis is applicable to the analysis of natural language, as Bloomfield showed us. This should also motivate us to search for further applications of this method in other scientific disciplines, and, most importantly, it shows us that Carnap's *Aufbau* is still of high actuality if we aim to reconstruct our knowledge of the world.

6.3 Harris' theory

In chapter 5 and in section 6.1, I have already presented and discussed the central elements of Harris' theory. In what follows, I will give a structuralist reconstruction of Harris' theory, similar to my reconstruction of Bloomfield's theory in the previous section.

6.3.1 Structuralist reconstruction of Harris' theory

I will begin with the definition of the potential models for the *theory of Harris' linguistics*, to which I will give the set-theoretic predicate THL.

6.3.2 Potential models of THL

Definition 9: $M_p(THL) : x$ is a potential model of the theory of Harris' linguistics ($x \in M_p(THL)$) iff there exist: $\mathfrak{F}, T, \mathfrak{T}, concat, \sqsubset, trans$ such that:

1. $x = \langle \mathfrak{F}, T, \mathfrak{T}, concat, \sqsubset, trans \rangle$

2. $\mathfrak{F}$ is finite and non-empty.

3. T is a closed interval of $\mathbb{R}$ of positive length.

4. $\mathfrak{T}$ is a set of closed subintervals of T of positive length.

5. $concat : \mathfrak{F} \times \mathfrak{F} \to \mathfrak{F}$.

6. $\sqsubset \subset \mathfrak{F} \times \mathfrak{F}$

7. $trans \subset \mathfrak{F} \times \mathfrak{F}$

I will now give the following intended interpretation. As in SL, $\mathfrak{F}$ is a set of linguistic forms. T is a set of time intervals, with $\mathfrak{T}$ being a set of closed subintervals of T. Furthermore, as in SL, there is a concatenation function on forms, *concat* and the parthood relation $\sqsubset$ on forms, since THL is a theory of syntax, and no longer classifies phonemes. What is new in THL, and of central importance, is the notion of transformation, represented by the relation *trans*. The introduction of the transformation relation on forms makes the ontology of Harris' theory radically different from the ontology of SL. It is true that the potential models of both theories are similar. However, the addition of the notion of transformation allowed Harris to explain more linguistic phenomena, as I have already explained in previous sections.

A significant change in the ontology of linguistic theory in this phase of theory change is of central importance for my argument on epistemic structural realism and theory change. If we look at both theories, SL and THL, they are similar, but at the end, there is the addition of transformations in Harris, which changes the ontology significantly. This could be seen as a case of radical theory change, where an anti-realist could find corroboration and argue against scientific realism. This is exactly where I will show that, despite the difference of ontologies, there is a continuity of structure, expressed by the relation of reduction between SL and THL. We will discuss this further in the following sections.

6.3.3 Actual models of THL

The actual models of THL are now as follows:

Definition 10: $M(THL)$: x is an actual model of the theory of Harris' linguistics ($x \in M(THL)$) iff there exist $\mathfrak{F}, T, \mathfrak{T}, concat, \sqsubset, trans$, such that:

1. $x = \langle \mathfrak{F}, T, \mathfrak{T}, concat, \sqsubset, trans \rangle \in M_p(TBL)$

2. $trans$ is reflexive, symmetrical and transitive.

3. $\forall f$ (f is a form) $\rightarrow \exists f_1$ (f_1 is a form and f_1 can be concatenated with f).

4. $\forall f$ (f is a form) $\rightarrow \exists f_1$ (f_1 is the transform of f).

The lawlike information about the transformation relation is added in $M(THL)$. Condition two expresses the form of relation that $trans$ is. Condition 3 states that forms can always be concatenated with other forms. Condiion 4 expresses the fact that forms have transforms, *i.e.* that forms can be transformed into other forms. We have seen this in detail in previous sections. The standard example being the active-passive transformation.

6.3.4 Theoretical terms in THL

By following the structuralist criterion for theoretical terms, we say that $trans$ is a T-theoretical relation in THL, since it is newly introduced by THL, and it has to be presupposed in order to determine THL completely. $\mathfrak{F}$ is no longer T-theoretical, since it was introduced by SL already.

6.3.5 The partial potential models of THL

Consequently, we now define the partial potential models for THL, where the T-theoretical notions are cut out.

Definition 11: $M_{pp}(THL) : x$ is a partial potential model of the theory of Harris' linguistics ($x \in M_p(THL)$) iff there exist: $\mathfrak{F}, T, \mathfrak{T}, concat, \sqsubset$, such that:

1. $x = \langle \mathfrak{F}, T, \mathfrak{T}, concat, \sqsubset \rangle$
2. $\mathfrak{F}$ is finite and non-empty.
3. T is a closed interval of $\mathbb{R}$ of positive length.
4. $\mathfrak{T}$ is a set of closed subintervals of T of positive length.
5. $concat : \mathfrak{F} \times \mathfrak{F} \to \mathfrak{F}$.
6. $\sqsubset \subset \mathfrak{F} \times \mathfrak{F}$

In this case, only the relation *trans* is cut out. We are now in a position where it is possible to have full insight into the structure of Harris' theory. It turns out that it is a similar theory to SL, however, it is different in its ontology in a crucial aspect, namely, the fact that the relation *trans* is introduced.

With this step, I will finish my reconstruction of THL, since all the relevant information for the question of intertheoretical relations to SL and to Chomsky's theory is obtained. In what follows, I will give a structuralist reconstruction of Chomsky's early theory, before I will show that there is in fact a structural continuity between these theories.

6.4 Chomsky's generative grammar

The development of generative grammar has caused enormous impact on linguistic theory. As it is well known, Chomsky developed a theory of language that was able to explain more linguistic phenomena than all previous theories, especially in the field of syntax.

Since Chomsky's theories have been developed over a time span that comprises more than half a century, and due to the fact that his theory has evolved strongly, it is important to be clear on the distinction of the theoretical development of the whole of his work. John Collins (2008: 5) provides a clarifying outline of Chomsky's work, and characterizes the different phases as follows:

	Period	Framework
1.	1955-59	Early transformational grammar
2.	1962-66	The Standard Theory
3.	1968-72	Extended Standard Theory
4.	1973-80	Revised Extended Standard Theory/Conditions
5.	1980-90	Government and binding / principles and parameters
6.	1991-99	The Minimalist Program
7.	2000-	Phase Derivation / Level Free

For the purpose of this dissertation, I will analyze the first two periods of Chomsky's work, which comprises his books *The Logical Structure of Linguistic Theory, Syntactic Structures*, and *Aspects of the Theory of Syntax*. I argue that the early phase of theoretical development between structural linguistics and generative grammar should not be interpreted as a paradigm shift in a Kuhnian sense. This shift was neither that radical, nor was it a "revolution". Most certainly, there was no incommensurability, since at the structural level, there is continuity (as I will show below). The crucial role of an intermediary between both paradigms was Harris. With respect to other possible paradigm shifts within the theoretical developments of Chomsky's work, I agree with Collins that "the history is complex; it is not a series of jumps into the unknown a la the standard caricature of Kuhn's picture of science (*ibid.*: 6)". It is precisely the change from structural linguistics to generative grammar that presents us with the idea of a radical, Kuhnian scientific revolution. After my reconstruction it will become clear that such a radical Kuhnian shift did not occur in a drastic sense. My thesis is strengthened by the fact that there is a form of structural continuity from structural linguistics to generative grammar.

Chomsky's introduction of the notions of deep structure and surface structure clearly presents a significant change of ontology in the theoretical development of syntactic theory, specifically in the phase of change from Harris to Chomsky. The following quote illustrates the role that Chomsky adscribes to these notions:

> A deep structure enters the semantic component and receives a semantic interpretation; it is mapped by transformational rules into a surface structure, which is then given a phonetic interpretation by the rules of the phonological component (Chomksy 1965: 141).

The mapping by transformational rules, from a deep structure into a surface structure, is the central element in this phase of Chomsky's theorizing. In my structuralist reconstruction of Chomsky's theory, this will become apparent.

6.4.1 Chomsky's theory TCL

Analogously to my reconstruction of Bloomfield's and Harris' theories, I will now procede to show how Chomsky's theory (giving it the set-theoretic predicate TCL) can be formalized within the framework of the structuralist meta-theory.

6.4.2 Potential models of TCL

Definition 12: $M_p(TCL)$: x is a potential model of the theory of Chomky's linguistics ($x \in M_p(TCL)$) iff there exist: $\mathfrak{F}, \mathfrak{F}^{\mathfrak{D}}, \mathfrak{F}^{\mathfrak{S}} concat, \sqsubset$
$, trans$
such that:

1. $M = \langle \mathfrak{F}, \mathfrak{F}^{\mathfrak{D}}, \mathfrak{F}^{\mathfrak{S}} concat, \sqsubset, trans \rangle$.

2. $\mathfrak{F}, \mathfrak{F}^{\mathfrak{D}} and \mathfrak{F}^{\mathfrak{S}}$ are finite, non-empty sets.

3. $\mathfrak{F}^{\mathfrak{D}} \subseteq \mathfrak{F}$.

4. $\mathfrak{F}^{\mathfrak{S}} \subseteq \mathfrak{F}$.

5. $\mathfrak{F}^{\mathfrak{D}} \bigcap \mathfrak{F}^{S} = \emptyset$.

6. $concat : \mathfrak{F} \times \mathfrak{F} \rightarrow \mathfrak{F}$.

7. $\sqsubset \subseteq \mathfrak{F} \times \mathfrak{F}$.

8. $trans \subset \mathfrak{F} \times \mathfrak{F}$

This represents the general framework of Chomsky's theory. The basic intended interpretation is as follows. There exist three base sets, $\mathfrak{F}$, $\mathfrak{F}^{\mathfrak{D}}$, and $\mathfrak{F}^{\mathfrak{S}}$. $\mathfrak{F}$ is a set of forms, just as in SL and in THL. What is new in TCL, and what presents a significant shift of ontology, are the sets $\mathfrak{F}^{\mathfrak{D}}$, and $\mathfrak{F}^{\mathfrak{S}}$. $\mathfrak{F}^{\mathfrak{D}}$, is a set of "deep-structure forms", and $\mathfrak{F}^{\mathfrak{S}}$ is a set of "surface-structure forms". This aims to adequately represent the fact that Chomsky introduced the notions of deep structure and surface structure, by expressing the fact that both, deep structure and surface structure are forms. Linguistic forms have both levels of structures, and hence, the set $\mathfrak{F}$ includes both $\mathfrak{F}^{\mathfrak{D}}$, and $\mathfrak{F}^{\mathfrak{S}}$ as subsets. $concat$, $\sqsubset$ and $trans$ are the same notions than in SL and in THL.

6.4.3 Actual models of TCL

For the determination of the laws, or lawlike statements of TCL, we define an actual model of TCL. The fundamental principles of the theory, which are also required for the determination of the theoretical terms of TCL, are given:

Definition 13: $M(TCL)$: x is an actual model of the theory of Chomsky's linguistics ($x \in M(TCL)$) iff there exist $\mathfrak{F}, \mathfrak{F}^{\mathfrak{D}}, \mathfrak{F}^{\mathfrak{S}} concat, \sqsubset, trans$, such that:

1. $x = \langle \mathfrak{F}, \mathfrak{F}^{\mathfrak{D}}, \mathfrak{F}^{\mathfrak{S}} concat, \sqsubset, trans \rangle \in M_p(TCL)$
2. $trans$ is reflexive, symmetrical and transitive.
3. $\forall f$ (f is a form) $\rightarrow \exists f_1$ (f_1 is a form and f_1 can be concatenated with f).

4. $\forall f$ (f is a form) $\to \exists f_2$ (f_2 is the transform of f).

5. $\forall f$ (f is a form) $\to \exists f_3$ (f_3 is the deep structure of f) & $\exists f_4$ (f_4 is the surface structure of f).

Basic intended interpretation: Condition 1 states that the actual model is also element of the potential model. As in THL, conditions 2, 3 and 4 express the lawlike facts about the relation *trans*, and the transformational apparatus. The new law is expressed by condition 5. This condition states the fact that all forms have a deep structure and a surface structure.

6.4.4 Theoretical terms in TCL

In TCL, $\mathfrak{F}^{\mathfrak{D}}$ and $\mathfrak{F}^{\mathfrak{S}}$ are T-theoretical. Both are newly introduced by TCL, and are required to satisy the fundamental laws of TCL. All other terms are T-non-theoretical, since they are already present in SL, or in THL.

6.4.5 The partial potential models of TCL

After having clarified and determined which terms are T-theoretical in Chomsky's theory, we are able to define the partial potential models of TCL, where the T-theoretical terms are cut out.

Definition 14: $M_{pp}(TCL)$: x is a partial potential model of the theory of Chomsky's linguistics ($x \in M_p(TCL)$) iff there exist: $\mathfrak{F}, concat, \sqsubset$ $, trans$ such that:

1. $M = \langle \mathfrak{F}, concat, \sqsubset, trans \rangle$.

2. $\mathfrak{F}$ is a finite, non-empty set.

3. $concat : \mathfrak{F} \times \mathfrak{F} \rightarrow \mathfrak{F}$.

4. $\sqsubset \subseteq \mathfrak{F} \times \mathfrak{F}$.

5. $trans \subset \mathfrak{F} \times \mathfrak{F}$

Analogously to the case of SL and THL, by this definition, we obtain the "data model" of TCL. The Chomskyan linguist starts by working with linguistic forms, which can be understood to be "somehow" empirical, and observational. Forms are observational in the following sense. The Chomskyan linguist will "see" some linguistic forms written down somewhere, and start manipulating these forms, by sorting out their deep structure and the surface structure and by determining phrase structure rules, etc. But in the beginning of the application of TCL, the linguist will start with the forms.

6.5 Structural continuity

As an example of structural continuity, I will first discuss a case of reduction in linguistics. The relation of reduction between theories will be taken as an exemplar case of structural continuity. This continuity is expressed in the reduction relation as defined in the framework of the structuralist meta-theory.

I will quickly introduce both the potential models of Bloomfield's and of Harris' theories of syntax. I restrict here to syntax because there, structural continuity is most eminent. For the purpose of this work, it is sufficient to outline that both theories are related through the structuralist reduction-relation (see chapter 3 for my outline of the concept of reduction of the structuralist meta-theory), concerning the linguistic sub-discipline of syntax.

M_B (for Bloomfield) is a set of models m_B, such that:

1. $m_B = \langle \mathfrak{F}, concat, \sqsubset \rangle$.
2. $\mathfrak{F}$ is a finite, non-empty set.
3. $concat : \mathfrak{F} \times \mathfrak{F} \to \mathfrak{F}$.
4. $\sqsubset \subseteq \mathfrak{F} \times \mathfrak{F}$.
5. $\forall f$ (f is a form) $\to \exists f_1$ (f_1 is a form and f_1 can be concatenated with f).

The basic intended interpretation is as above in the reconstruction of SL. We will now look at Harris' model of syntax:

M_H (for Harris) is a set of models m_H, such that:

1. $m_H = \langle \mathfrak{F}, concat, \sqsubset, trans \rangle$.
2. $\mathfrak{F}$ is a finite, non-empty set.
3. $concat : \mathfrak{F} \times \mathfrak{F} \to \mathfrak{F}$.
4. $\sqsubset \subseteq \mathfrak{F} \times \mathfrak{F}$.
5. $trans \subset \mathfrak{F} \times \mathfrak{F}$
6. $\forall f$ (f is a form) $\to \exists f_1$ (f_1 is a form and f_1 can be concatenated with f).

7. $\forall f$ (f is a form) $\rightarrow \exists f_2$ (f_2 is the transform of f).

The model of Harris' syntactic theory states the same as the model of Bloomfield, but expresses one more crucial fact, the fact that linguistic forms do always have a transform. The function *trans* expresses that forms are always transformed to other forms. This aims to adequately represent cases like the active - passive transformation.

6.5.1 Continuity from Bloomfield to Harris

Now, structural continuity between SL and THL is expressed through the reduction of SL to THL, in the following reduction relation ρ:

1. $\rho \subseteq M(THL) \times M(SL), \langle x^*, x\rangle \in \rho$, iff:
2. $x^* = \langle \mathfrak{F}, concat, \sqsubset, trans\rangle \in M(THS)$
3. $x = \langle \mathfrak{F}', concat', \sqsubset'\rangle \in M(SL)$
4. $\mathfrak{F} = \mathfrak{F}', concat = concat', \sqsubset = \sqsubset'$
5. $\forall f$ (f is a form) $\rightarrow \exists f_1$ (f_1 is a form and f_1 can be concatenated with f).
6. $\forall f$ (f is a form) $\rightarrow \exists f_2$ (f_2 is the transform of f).

Condition one states that the models of both theories are related through the reduction relation ρ, where in condition two and three, x^* and x are one set of the actual model (of syntax). In four, it is expressed that the entities of both theories are identical, both theories deal with the same entities, namely, linguistic forms and their combination. Condition five expresses the syntactic law of both theories, that every form can be concatenated with another form. The new law, which makes THL the reducing theory over SL, is expressed in condition 6, where it is stated that every form has a transform, such as the example sentence *John saw Jill*. We have seen that in our case-study, Bloomfield's theory is reduced to Harris' theory. But the important fact for structural realism is that, the structure of Bloomfield's theory is completely overtaken and continuous with the structure of Harris' theory. It is just that Harris' theory is more complex, it has one law that is not part of Bloomfield's theory, but all other parts of the structure of SL are continuous with THL. It is a structural continuity for the following reason. Both theories are represented as structures themselves. Now, the reduction relation provides the continuity of structure.

I argue that the structuralist meta-theory provides tools for a clarification of the notion of structural continuity. Empirical theories are taken to be structural entities themselves (in the formal sense explained above). Some information about intertheoretical relations might be trivial and not really informative. Anyhow, I argue, the structuralist framework provides formal notions for capturing all kinds of structural continuities, trivial and non-trivial. Here, I have given a case of a non-trivial reduction relation in linguistics.

Where Worrall mentions the continuous appearance of certain equations in different theories, the structuralist meta-theory provides us with a more universal and abstract notion of structural continuity of more sophisticated and detailed formal explanatory power. The notion of structural continuity, in this case, has the form as in the definition of the reduction above. But in each particular reconstruction of an empirical theory, a theory-net will have a certain structure. In this sense, when a theory has reached sufficient development and sophistication, it can be possible to identify a structural continuity of *some* form. The important question is whether there is a relation of structural continuity which provides us with any important information concerning questions on theory change and structural realism.

In the case of Bloomfield and Harris, it is the structure of their theories of syntax, that is preserved. As I have shown in the reduction relation above, Harris' theory adds more features to the theory, but all structures in Bloomfield are continuous with those in Harris. However, there is a significant change of ontology, since THL introduces the transformational apparatus, expressed by the transformation relation $trans$. This is something entirely new and, since it is part of the domain of THL, it is an addition to the ontology of syntactic theory, an example for theoretical change in syntactic theory.

6.5.2 Continuity from Harris to Chomsky

A stronger case of structural continuity occurs in the change from Bloomfield's and Harris' to Chomsky's theory. Let us remember the fact that structural realism comes into play when there is an apparent discontinuity on the referential level. The ontology of the theories in question changes, but at the structural level, there is still continuity. This happens in the shift from Bloomfield to Chomsky, since in Chomsky's theory, the notions of deep structure and surface structure are introduced. I will recall Chomsky's syntactic theory as follows:

M_{TCL} is a set of models m_{TCL}, such that:

1. $m_{TCL} = \langle \mathfrak{F}, \mathfrak{F}^{\mathfrak{D}}, \mathfrak{F}^{\mathfrak{S}} concat, \sqsubset, trans \rangle$.

2. $\mathfrak{F}, \mathfrak{F}^{\mathfrak{D}} and \mathfrak{F}^{\mathfrak{S}}$ are finite, non-empty sets.

3. $\mathfrak{F}^{\mathfrak{D}} \subseteq \mathfrak{F}$.

4. $\mathfrak{F}^{\mathfrak{S}} \subseteq \mathfrak{F}$.

5. $concat : \mathfrak{F} \times \mathfrak{F} \rightarrow \mathfrak{F}$.

6. $\sqsubset \subseteq \mathfrak{F} \times \mathfrak{F}$.

7. $trans \subset \mathfrak{F} \times \mathfrak{F}$

8. $\forall f$ (f is a form) $\rightarrow \exists f_1$ (f_1 is a form and f_1 can be concatenated with f).

9. $\forall f$ (f is a form) $\rightarrow \exists f_2$ (f_2 is the transform of f).

10. $\forall f$ (f is a form) $\rightarrow \exists f_3$ (f_3 is the deep structure of f) & $\exists f_4$ (f_4 is the surface structure of f).

Analogously to the reduction between SL and THL, there also holds a reduction relation (and hence a relation of structural continuity) between THL and TCL. This can be expressed as follows, in the reduction relation ρ:

1. $\rho \subseteq M(TCL) \times M(THS), \langle x^*, x \rangle \in \rho$, iff:

2. $x^* = \langle \mathfrak{F}, \mathfrak{F}^{\mathfrak{D}}, \mathfrak{F}^{\mathfrak{S}}, concat, \sqsubset, trans \rangle \in M(TCL)$

3. $x = \langle \mathfrak{F}', concat', \sqsubset', trans' \rangle \in M(THS)$

4. $\mathfrak{F} = \mathfrak{F}', concat = concat', \sqsubset = \sqsubset', trans = trans'$

5. $\forall f$ (f is a form) $\rightarrow \exists f_1$ (f_1 is a form and f_1 can be concatenated with f).

6. $\forall f$ (f is a form) $\rightarrow \exists f_2$ (f_2 is the transform of f).

7. $\forall f$ (f is a form) $\rightarrow \exists f_3$ (f_3 is the deep structure of f) & $\exists f_4$ (f_4 is the surface structure of f).

THL is reducible to TCL in a similar way than SL reduces to THL. In both cases, the ontology of the theories changes significantly. However, the reduced theory can be seen as a simpler version of the reducing one, since it explains the same facts, but the reducing theory explains more, adds entities and lawlike statements to its ontology and says "more" about linguistic phenomena. Where it is commonly held that Chomsky's theory strongly revolutionized syntactic theory, we can now see that this revolution was not as dramatic as it has been seen. Harris did not introduce the concepts of deep structure and surface structure to his ontology. In this sense, the ontology changes through the theory change from THL to TCL. But through the reduction relation, all the three theories are related, and this is where the structural continuity is expressed.

6.6 Conclusions

The mentioning of the structuralist concept of reduction in this chapter should be understood as an illustrative example in order to promote the idea of approaching structural realism by applying the framework of the structuralist meta-theory to it. It is clear that the whole framework of the structuralist meta-theory offers a whole series of other concepts which all might be applicable to address the structural realist concern for structural continuity.

I have argued that the structuralist concept of reduction fits for the purpose of formally representing structural continuity. I have shown that there is a case of reduction between theories in linguistics, where there is in fact structural continuity, expressed by this relation. SL is reducible to THL, and THL is reducible to TCL. This holds explicitly for the field of syntax, as I have shown. The main result of this case study is, besides the fact of a theory reduction in linguistics, that we can see that there is a case of structural continuity in linguistics, and that it is possible to formulate structural realism with a focus on theory change in linguistics. This also implies that one is in no need of a high degree of mathematization in the sciences, if the aim is to find structural continuities. This means furthermore that structural continuity can also be found in non-highly-mathematized fields of science, and shows that the framework of the structuralist meta-theory allows us to include the special sciences in case studies about theory change and scientific (and structural) realism.

Chapter 7

Objections and a defense

7.1 The objections

I will now present a series of objections, which have been raised to structural realism. The proposal of structural realism made by Worrall raised several objections. These objections have been presented especially by Stathis Psillos[7]. I will shortly mention Psillos' objections (and others). The following list summarizes the objections which are relevant for my proposal in this dissertation:

7.1.1 The standard-realism objection

Psillos argues that the notion of structural continuity can be fully explained by standard scientific realism. Mathematical equations have been retained because they form an integral part of the approximately true theoretical content of theories. But furthermore, there needs to be some theoretical content beyond the equations. Psillos explains this as follows:

[7] See Psillos 2001a and his 1999: 146-161. There, he presents several objections to structural realism.

> If the empirical success of a theory offers any grounds for thinking that some parts of a theory have 'latched on to' the world, those parts cannot be just some (uninterpreted) mathematical equations of the theory, but must include some theoretical assertions concerning some substantive properties as well as the law-like behaviour of the entities and mechanisms posited by the theory ... let me just stress the main point: if one admits that there is substantive (not just formal) retention at the structural-mathematical level, then one should admit that some theoretical content, too, gets retained. But such an admission would undercut the claim that predictive success vindicates only the mathematical structure of a theory (Psillos 1999: 148).

And for Psillos, one would end up with the standard scientific realism picture again. As soon as one accepts more than pure mathematical structure, he is more than a structural realist, Psillos argues. Worrall seems to state that all that is preserved, are the equations. It is not clear in which part of our theories the theoretical content enters. It is a part of our equations, but, as Psillos argues, there is also theoretical content that is not part of our equations. And if such content is preserved, we would get standard scientific realism again.

7.1.2 The uninterpreted-structures objection

That certain structures are retained through theoretical change does not imply that these structures (as equations) tell us anything about the structure of the world. It is not clear whether these equations represent relations between physical entities which would be otherwise unknowable. The descriptions of the world, obtained through our empirical theories, cannot be only expressed by equations. Some theoretical assumptions which are not part of equations are also required in order to justify the success of an empirical theory. In Psillos' words: "... in empirical science we should at least seek more than formal structure. Knowing that the world has a certain formal structure (as opposed to natural structure) allows no explanation and no prediction of the phenomena (Psillos 2001a: S21)".

One might think in a first step that this sounds very much like the standard-realism objection. However, the uninterpreted-structures objection is different in the following sense. One can interpret equations in a merely instrumentalist setting, *e.g.* one can give purely technical explanations on how certain functions look like, but this does not commit to scientific realism, since giving purely technical interpretations to mathematical structures does not imply any commitment the scientific realist wants to make.

7.1.3 The structure-nature objection

This objection states that the distinction between structure and nature of an entity, as Worrall proposes, has no justification. Following Psillos, scientists normally describe the nature of an entity by ascribing certain properties and relations to it. The nomological behavior is then expressed by some equations. There is no need for the distinction between structure and nature of an entity, since it is metaphysically inflationary. In Psillos' words:

> Is the nature of a theoretical entity something distinct from its structure? Equivalently, can one usefully conceive of the physical content of a mathematical symbol (that is, of the entity or process it stands for) as distinct from the totality of the interpreted mathematical equations in which it features, (that is, from the totality of laws which describe its behaviour)? When scientists talk about the nature of an entity, what they normally do -apart from positing a causal agent- is to ascribe to this entity a grouping of basic properties and relations. They then describe its law-like behaviour by means of a set of equations. In other words, they endow this causal agent with a certain causal structure, and they talk about the way in which this entity is structured. I think that talk of 'nature' over and above this structural description (physical and mathematical) of a causal agent is to hark back to the medieval discourse of 'forms' and 'substances'. Such talk has been overthrown by the scientific revolution of the seventeenth century (Psillos 1999: 149).

7.1.4 The structure-loss objection

Chakravartty (2004), Bueno (2008) and others claim that there are not only cases of structural continuity, but also of structural loss through theory change. This objection relies on historical facts of theoretical change and is hard to reject at first sight. Bueno rightly refers to Laudan (1996), who first discussed such cases of structural loss:

> However, structural realism also faces a further difficulty. It arises not from the existence of different structures that do the same job, but from the existence of structural losses in scientific change. There are well-known cases that support the existence of these losses. For example, when we moved from Descartes's celestial mechanics to Newton's, the structure provided by Descartes's theory of vortices was entirely lost. The latter theory explained why the planets moved in the direction that they did, and this was an issue left unexplained by Newton's own theory. In other words, some structure was lost in this case (Bueno 2008: 223-224).

It is right that in the shift from Descartes' to Newton's celestial mechanics, there was a loss of structure. Below, we will see how one can counter this objection, or at least turn it into the favour of the structural realist that aims to apply the structuralist meta-theory in the debate on structural realism.

7.1.5 Structure correspondence

Not an objection, but an open question in the debate is the following, formulated by Frigg and Votsis (2011):

How does the exact relation of correspondence between two structures, the one in the abandoned and the one in the successor theory, look like?

In other words, the question Frigg and Votsis ask is about the form of intertheoretical relations between the theories in question, and about how such relations between the structures of the theories in question look like in a formal sense. In the Fresnel-Maxwell case, how does the formal relation between Fresnel's and Maxwell's equations look like, since Fresnel's theory is the abandoned one and Maxwell's theory is the successor theory? It is clear that this question can only be answered by working out careful and detailed case studies, where the logical structure of the theories in question is reconstructed. It is exactly here, where I argue that the structuralist meta-theory has the adequate formal tools for explicating this open question. I have shown an example of this in the previous chapter. In that case, the relation of correspondence between the structures looks like the reduction relation in the structuralist meta-theory.

The matter of correspondence between the two structures is closely connected to some of the objections that I have mentioned. First, if we are able to explicate formally how such a correspondence looks like, we can refer to that relation of correspondence as the structure that guarantees a continuity between both theories. This structure is not just an uninterpreted one, since it will hold between two concrete empirical theories. Secondly, the explication of such a relation will also serve to show how a structural loss is not affecting structural realism. For this to work, of course, it is required that the correspondence relation between two structures includes all relevant facts of both theories.

I have already outlined the tools of the structuralist meta-theory in chapter 3, and I have shown a concrete case of structural continuity, formulated in the framework of this approach, in chapter 6. In this sense, the correspondence between two structures can be seen as a relation of reduction, as it is the case for Harris and Bloomfield, and in the case of Harris and Chomsky.

7.2 Defending structural realism

Finally, I want to propose the following answers to the objections to structural realism mentioned above. First, I argue that the *standard-realism objection* is blocked if we accept the framework of the structuralist meta-theory. In the case of the reduction from Bloomfield to Harris, there are not even equations that could be a part of these theories. This makes it such an important case, for if structural realism wants to be a tenable position, it should be applicable to all domains of empirical science, and not only to physics, or to sciences that have equations as a necessary part of them. By equations, I mean differential equations, as they appear in physics, for instance. But generally, there cannot be a need for mathematical equations (in the sense of differential equations) for structural realism to make sense.

What about linguistics, again? I do not think it is necessary to have such equations to make sense of structural realism. But of course, the more mathematized a scientific discipline becomes, the easier it gets for structural realism to sort out relevant structures that can become relevant for the discussion on structural continuity. A syntax-tree is also a mathematical structure, and henceforth it enters into the discussion on structural realism.

My answer to the first objection goes very much along with my answer to the next objection. To the *uninterpreted-structures objection*, I propose that if we approach structural realism with the framework of the structuralist meta-theory, we will not only talk about equations as structures, but of the whole empirical theories *as* structures, in a formal sense. We will never have the problem of having only uninterpreted structures, if we rely on structuralist reconstructions of *real* empirical theories. In the case of SL and THL, the structures of both theories are not uninterpreted, for they represent real physical entities, namely, linguistic forms, and the ways in which they are put together. It is important to mention that linguistic forms, as existing physical entities, have to be understood as tokens, just analogous to physics. When physicists measure the movement of electrons, they measure tokens, but when a theory about electrons is formulated, it is about types. This is the same case in linguistics. Within a theory, linguistic forms are types, but as entities, measured in a physical system (in this case, a natural language), they are tokens.

The *structure-nature objection*, I argue, can be answered in a Carnapian way, if we think of the *Aufbau* (§16), where Carnap explains the distinction between *structure* and *nature*, where he calls it the *material*. From the perspective of the structuralist meta-theory, our empirical theories describe the structure of the *material*, to speak with Carnap's words. The tools of the structuralist meta-theory allow to represent all empirical theories as structural entities. Hence, there is a well-established distinction between these two levels. It is true that Worrall's use of 'nature' is somehow misleading, for it gives the debate a problematic and unnecessary metaphysical touch. But if we rely on Carnap's concept 'the material', I argue, we don't get that surplus flavor of metaphysical connotation.

It is well known that Carnap was not a realist, and that he aimed to stay neutral with his program, in the *Aufbau* as well as in his later works. Nevertheless, I see that Carnap's structuralist methodology of the *Aufbau* can be applied to structural realism. I argue with Carnap that, for the structural realist, only the structural components guarantee objectivity, since all other ways of describing our knowledge (*e.g.* via ostension) will end up being subjective. Now, if we put this into the context of structural realism, I want to recall that one of Worrall's main points in his 1989 paper is exactly that the names of the entities to which we refer with the terms that are postulated in our theories can change completely, and that we therefore should better focus on certain structural than on referential continuities. In this sense, I find Carnap's structuralist methodology helpful in order to strengthen structural realism. And in this way, such a distinction between structure and nature does make sense, without being inflationary or metaphysically unnecessary.

To the *structure-loss objection* I propose the following answer. It is exactly through concrete case studies that one can show concrete cases of structure-loss. The same holds for the inverse case, *i.e.* one can also show how a structural continuity looks like. It might well be the case that there exist cases of structural loss. But what the adherent of this objection is urged to do is to work out careful case studies on these supposed structural losses. The same, again, for the structural realist. It is exactly one central aim of my dissertation to contribute to this with the case study of linguistics that I have provided here. It can be seen that, with the approach I am proposing, the question of structural continuity becomes a matter of accumulating convincing case studies. I argue that, once we have a certain amount of case studies, we can at least make an inductive step in the way Laudan (1981) does it with the pessimistic meta-induction, but towards the other direction.

It might well turn out that the result will be that there are more cases of losses than cases of continuities, but this is a question, I argue, which cannot be answered normatively, and must be pursued in a way of accumulating logical reconstructions of concrete cases of scientific change. The crucial point for the solution of the question of structural losses is concerned with the importance of every specific case-study. As the structural realist will have to find representative cases of structural continuities, the same holds for those arguing in favor of structural losses. Is the case of the continuity of structure from Fresnel's to Maxwell's theory convincing enough? Surely not. There is a need for more case-studies. At the end, both sides will have to argue for the importance of each case in question.

7.3 Structure choice and underdetermination

The problem of underdetermination (as outlined in chapter two) can also be approached from the perspective of structural realism (see Brading and Skyles 2012). These authors present a discussion of underdetermination and ontic structural realism, by discussing contemporary issues in the philosophy of physics, connected with ontic structural realism and the metaphysics of science.

My approach here is different. I argue that only after a rational and logical reconstruction of the respective empirical theory under issue, we are able to see the full structure of the theory, and to understand its full meaning. We always have to step to the meta-level of looking at the empirical theory in its reconstructed form, as a meta-representation. By doing so, we can have a clear vision of the theory in question. If problems of underdetermination emerge, one then needs to sort out the "best" and the "relevant" structure that is available.

This move aims at answering the problem of underdetermination by appealing to pragmatic or epistemic virtues. One can argue for the choice of a certain theory over another one, because it might be more elegant, economical, simple, coherent, or it might have a better explanatory structure. In this same way, we can also think of a choice we can make between structures, and not between theories in the sense of scientific realism. This means that, after having analyzed and reconstructed an empirical theory, we focus on the way in which we have provided the formal reconstruction. This means that we have to argue why our theory has been reconstructed in a specific formal framework, and not in another one. For example, one could reconstruct classical mechanics in a finitist framework, or in a framework that permits the existence of infinite sets. This difference is crucial, since it radically changes the underlying ontological assumptions that we make by when we use one specific framework.

Furthermore, we can think of the tools that are provided by the structuralist meta-theory, and of the reconstruction of linguistics I have given in this dissertation. One could always provide an alternative formal reconstruction, within another formal framework. This is in principle logically possible. In this case, it could be within the framework of first order logic, or within the framework of category theory. After having reconstructed the same empirical theory with different formal approaches, one could then see clearly the similarities and differences in the "structure" of the theory, by looking at its meta-representation. This move would allow us to decide, by relying on the classical epistemic virtues, which structure would fit best, and to argue why we would prefer one representation over others. In this sense, one could answer the problem of underdetermination.

Chapter 8

Structural realism and neutralism

8.1 Introduction

After having examined structural realism, I will turn the focus now on a view which is called *neutralism*. This view, I argue, should be seen as being emergent out of the debate on scientific realism, and especially of the debate on structural realism. I argue that, once we leave standard scientific realism beside and move to structural realism, we are able to defend a form of realism, although this form of realism is primarily focussed on the notion of structure and structural continuity. It turns out, however, that structural realism can still be sophisticated in a way that allows a strong modification. In fact, it changes from structural realism to neutralism, where one can remain "neutral" with respect to questions of scientific realism. In a way, one "arrives" at such a neutralist view, after having moved to structural realism, or at least to structuralist epistemologies, in the sense of Carnap. My intent to develop a neutralist view goes along with the fact that for most adherents of the structuralist meta-theory, neutralism is an attractive option. I will discuss this point in detail below. I will now discuss and develop a neutralist view for the dynamics of scientific theories, and in general for the debate on scientific realism.

This neutralist view aims to incorporate both scientific realism and anti-realism, but especially the developments of structural realism. In this sense, the move to neutralism goes from standard scientific realism, *through* structural realism to neutralism. The idea is that the structural descriptions of empirical theories that we can give by means of the framework of the structuralist meta-theory can strengthen structural realism, as I have shown. But they also guarantee objectivity and enable us to remain neutral.

Since the early work of Carnap (1928), and more explicitly during the last decades, the debate on scientific realism has been enriched by the discussion of neutralism. This view has been carefully examined by Stathis Psillos (1999) and Michael Friedman (1999, 2001 and 2011). In this chapter, I will start by discussing Carnap's neutralism, and Psillos' and Friedman's analysis of this view. I will then define different forms of neutralism, namely, metaphysical, semantic, framework-dependent, and interpretational. By doing so, I hope to clarify the distinctions between the different kinds of neutralism, by laying out a proper typology, but also to examine some limitations of the view.

Neutralism seems an attractive alternative to both realism and antirealism. As is well known, scientific realism involves a number of commitments, such as: the existence of unobservable objects, real modality in nature, and some correspondence between the unobservable superstructures of a theory and the corresponding objects in the world. Scientific antirealism, in turn, although typically involving fewer metaphysical commitments, still countenances some metaphysics. For example, in the case of constructive empiricism, there is an ultimate commitment to abstract objects, given that the aim of science is understood as the search for empirically adequate theories and such theories are formulated as families of models (Rosen 1994). But models are abstract objects, and the relation of empirical adequacy, for the constructive empiricist, requires the existence of an isomorphism between the empirical substructures of a model and the structures obtained by measuring relevant features of the world (van Fraassen 1980, p. 64). A neutralist stance would try to avoid such commitments, whether based on realism or on antirealism. This is very much in the spirit of a Carnapian *scientific philosophy*, which aims to guarantee objectivity without the metaphysical difficulties that realism and antirealism seem to generate. This goal, if it can be reached, makes a neutralist position very tempting. For these reasons, I think neutralism is an attractive option.

8.2 Defining neutralism

Neutralism can be thought of as an alternative to ontologically loaded views, such as realism and antirealism. There are also less committing versions of antirealism that are neutral about the existence of a certain range of objects, for instance, unobservable entities. This is the case of van Fraassen's constructive empiricism (van Fraassen 1980). But this form of antirealism is not neutral about the existence of other objects, such as observable entities, given that the constructive empiricist is realist about them.

8.2.1 A typology of neutralism

Given the formulation of scientific realism presented in chapter 2, and my discussion of structural realism, different types of neutralism can now be characterized. The purpose of developing a typology is to identify different levels of neutralism, even though, with some work, there may be some overlap. This just means that one can consistently maintain neutralism about several—in fact, about all—of the categories in this typology.

Metaphysical neutralism

This is a rather unpopular position, according to which one should be neutral about the question of what there is. This view can be motivated by some ideas alluded to in Carnap's *Aufbau*:

> Are the constituted forms "generated by thought", as the Marburg-School teaches, or are they "only recognized", as realism states? Constitutional theory uses a neutral language; and there forms are neither "generated", nor are they "recognized", but "constituted"; and it should be emphasized strongly already here that the word "constitute" is always meant completely neutral (Carnap 1928: §5).

Carnap notes that the application of his constitutional theory (outlined in the *Aufbau*) prevents the generation of pseudo-philosophical questions, such as the classical dispute between realists and idealists. In this sense, his proposal is metaphysically neutral, and is motivated by the attempt at avoiding engaging with pseudo-philosophical issues. Of course, not everyone shares Carnap's assessment that the dispute between realists and idealists addresses a pseudo-philosophical question. But even those who maintain that this is a genuine philosophical issue may find in the metaphysical neutralist position a safe haven, given the inherent difficulties in settling it.

Epistemological neutralism$_1$: Framework-dependent neutralism

Just as epistemological realism involves both the interpretation of scientific theories and the conceptual resources they provide, epistemological neutralism also has two components: one focusing on the relevant conceptual resources, which I want to call *framework-dependent neutralism*, and another highlighting the interpretation of the theories under consideration, which I will call *interpretational neutralism*.

According to framework-dependent neutralism, one should remain neutral with respect to the framework that is used to reconstruct scientific theories. The choice of a certain formal framework, at least in principle, should not shape one's ontology. One could use a variety of available frameworks, such as, naïve set-theory, Zermelo-Fraenkel set theory (with or without *Urelemente*), category theory, type theory, mereology (whether formulated in a first- or in a second-order language) etc. Since the neutralist is not committed to any particular framework, it's possible to avoid any ontologically problematic assumptions made by a framework while exploring the expressive resources it provides. I will discuss below to what extent this form of neutralism can, in fact, be implemented.

Epistemological neutralism$_2$: Interpretational neutralism

On this view, one should be neutral on how to interpret the results of scientific inquiry. In particular, given a scientific theory, one ought to remain neutral on how to interpret it. That is, the interpretational neutralist remains neutral about the different possible interpretations of the relevant theories, resisting the commitment to any one in particular. In this way, they provide understanding of significant features of the world that the theory alone does not settle, since an interpretation is supposed to supplement features of the original theory while still remaining compatible with it rather than becoming a rival theory. The Copenhagen interpretation and the many-worlds interpretation of quantum mechanics provide well-known examples. By not settling on any particular interpretation, this kind of neutralist emphasizes the contribution made by each interpretation to one's understanding of the world without the commitment to any of the assumptions (metaphysical, epistemological, or mathematical) made by the proposals in question.

Semantic neutralism

According to this view, one should be neutral on the reference of scientific terms, that is, on whether the terms of our scientific language refer to existing entities or not. The postulation of the existence of such entities is given a pragmatic justification. Neither realist nor antirealist accounts of the reference to such entities are acceptable to a semantic neutralist. This goes hand in hand with Carnap's well-known *Principle of Tolerance*:

> In logic, there are no morals. Everyone is at liberty to build up his own logic, *i.e.* his own form of language as he wishes.

> All that is required of him is that, if he wishes to discuss it, he must state his methods clearly, and give syntactical rules instead of philosophical arguments (Carnap 1937: 52).

This principle, in fact, meshes well with neutralism more generally. On Carnap's view, philosophical disputes should be a matter of a meta-theoretical discussion regarding logical syntax, instead of a debate in which terms such as 'real', 'existence', or 'world' occur. Once again, independently of whether one shares Carnap's assessment of meta-theoretical disputes, a semantic neutralist will avoid getting involved in settling the thorny issues of securing reference to scientific objects or explaining how to account for their failure.

It may be complained that the semantic neutralists are unable to accommodate significant features of scientific practice, in particular, to explain the success of science. Why is it that scientific theories are able to explain novel empirical phenomena? Since neutralists are not committed to (any particular account of) the reference of scientific terms (they remain neutral about this issue), the scientific realist will no doubt argue that scientific success will likely remain a mystery for them.

In response, one can note that the semantic neutralist does not deny that scientific terms refer—neutralism is not a form of dogmatic skepticism about science, after all—it does not assert that they do. And it is possible to explain the success of science without a commitment to the reference of scientific vocabulary: those scientific theories that were empirically unsuccessful were eventually rejected, and it is not surprising that theories that were not shown to be empirically inadequate remained (van Fraassen 1980).

Scientific realists may insist on the need to provide a mechanism in terms of which the success of novel predictions can be accounted for. But this assumes that there is something that still needs to be accommodated beyond the sheer fact that certain theories have not been shown to be empirically inadequate. The neutralist will resist the requirement that an underlying mechanism ought to be provided—typically, in terms of reference and truth, or, at least, approximate truth. For these notions take a particular stance on the issue of scientific progress, and one of the key motivations for the neutralist is to resist such maneuvers. This is, of course, a deflationary approach to this problem, but it is one that meshes very well with the deflationary approach to ontology that underlies the various forms of neutralism I have identified.

8.3 Carnap's neutralism

A neutral interpretation of our theories with respect to both scientific realism and scientific antirealism is a methodological move that goes very much along with Carnap's own proposal. As he tells us:

> It is obvious that there is a difference between the meanings of the instrumentalist and the realist ways of speaking. My own view, which I shall not elaborate here, is essentially this. I believe that the question should not be discussed in the form: "Are theoretical entities real?" but rather in the form: "Shall we prefer a language of physics (and of science in general) that contains theoretical terms, or a language without such terms?" From this point of view the question becomes one of preference and practical decision (Carnap 1974: 256).

Despite acknowledging the difference between realist and anti-realist interpretations of science, Carnap also highlights the central point that that difference ultimately amounts to a choice between two different frameworks: one that includes theoretical terms and another that doesn't. Carnap's neutralism amounts to insisting that the choice between such frameworks is ultimately a matter of preference, a "practical decision", as he notes.

Carnap's neutralism incorporates all four kinds of neutralism we discussed above. In particular, we find the collapse of the distinction between framework-dependent neutralism and interpretational neutralism, since he interprets the latter in terms of the former. On his view, the difference between realist and anti-realist interpretations of science (the issue about which the interpretational neutralist remains neutral about) is ultimately a difference between two rival frameworks, that is, two distinct languages in which to raise and examine questions about science (framework neutralism). Not surprisingly, Carnap also interprets semantic neutralism in terms of framework neutralism: whether the theoretical terms in one's language should be taken as referring or not is, for him, also a "practical decision" regarding the choice of one's language. In his hands, semantic neutralism also emerges from framework neutralism.

Finally, as noted above, Carnap embraces metaphysical neutralism by emphasizing that the language he adopts, for instance in the *Aufbau*, commits him neither to assert that objects exist independently of one's language (for these objects would then be recognized or discovered) nor does the language require him to generate such objects (which would then be created). Rather, Carnap insists that the language "constitutes" the objects in question, where such "constitution" is neutral between the two possibilities just highlighted. So even on the most fundamental issue about metaphysics—the basic nature of objects—Carnap adopts a neutralist stance.

Thus, Carnap's neutralism incorporates metaphysical, semantical, interpretational and framework-dependent components, and throughout his writings he emphasizes that the corresponding disputes are linguistic. As one may expect, given the centrality of framework-dependent neutralism in his thinking, Carnap also argues that the only way to discuss philosophical questions is to lay out the relevant syntactic rules and the reasons for the adoption of a given framework. However, in contrast with Carnap, I argue that the discussion of the syntax of the chosen framework is more than a linguistic dispute, since it aims to provide a comparison and an assessment of those frameworks that will be adopted in philosophical research. And as will become clear below, the adoption of a framework commits one to the particular ontology that is associated with that framework.

At this stage, one may worry whether it is possible to provide a genuine form of neutralism in the philosophy of science. In his discussion of Carnap's neutralism, Psillos has raised some concerns:

> Carnap's own aim is to defend a sort of genuine neutralism with respect to the question of the existential implications of scientific theories: no ontological commitments to unobservable entities are dictated by scientific theories, but scientific theories are not merely instruments for prediction and control either (Psillos 1999: 40).

Psillos concludes that Carnap eventually fails to achieve a neutralist stance, since his view ends up collapsing into one or the other of these extreme positions. On his view:

> It seems as though Carnap's neutralism is difficult to maintain: every attempt to restore an empiricist equidistance between scientific realism and instrumentalism makes him fall towards one of these positions. Carnap has to take sides, doesn't he? (Psillos 1999: 56).

This does not seem entirely clear, since the very idea of neutralism is not to take sides. In fact, the whole point of being a neutralist is precisely to navigate, as carefully as possible, between these extremes, and to avoid taking sides on metaphysical debates. And it is ultimately by framing the debate between realist and anti-realist interpretations of science as a choice between different frameworks—as a matter for a framework-dependent neutralism—that Carnap manages to achieve that.

Crucial for Psillos' case for the instability of Carnap's position is a passage in the first edition of Carnap's (1966) book. There Carnap claimed that the conflict between realism and anti-realism—he explicitly mentions instrumentalism—is basically linguistic, and that to state that a theory is a reliable instrument is basically equivalent to maintain that the theory is true. In his own words:

> It is obvious that there is a difference between the meanings of the instrumentalist and the realist ways of speaking. My own view, which I shall not elaborate here is that the conflict between the two approaches [realism and instrumentalism] is

> essentially linguistic. It is a question of which way of speaking is to be preferred under given circumstances. To say that a theory is a reliable instrument -that is, that the predictions of observable events that it yields will be confirmed-is essentially the same as saying that the theory is true and that the theoretical, unobservable entities it speaks about exist. Thus, there is no incompatibility between the thesis of the instrumentalist and that of the realist. At least so long as the former avoids such negative assertions ... but the theory does not consist of sentences which are either true or false, and the atoms, electrons and the like do not really exist' (Carnap 1966: 256, quoted in Psillos 1999: 56).

This passage is admittedly quite puzzling. Realism and anti-realism (in particular, instrumentalism) clearly are not compatible views. One would need to provide an exceedingly non-standard characterization of instrumentalism to make these views consistent with each other. For example, one could strip instrumentalism of its anti-realist components. In fact, this seems to be the route Carnap entertains at the end of the passage, suggesting that the instrumentalist could avoid asserting that scientific theories lack truth-value or that atoms and subatomic particles do not exist. But it's unclear that the resulting view would be a form of instrumentalism, since the distinctive features of the view would have been eliminated. Suppose, however, that the adjustments were made in the other direction. For instance, one could recommend that the realist should avoid asserting that scientific theories have a truth-value or that atoms and subatomic particles exist. In this case, it would be unclear that the resulting view would be a form of scientific realism, given the lack of commitment to those traits that are distinctive of the view. In either direction, the adjustments suggested clearly would not work.

To his credit, prompted by correspondence with Grover Maxwell, Carnap agreed to revise the passage I just quoted above (see Martin Gardner's forward to Carnap 1974: ix-x; see also Friedman 2011: 250, footnote 2). The result is the version I have reproduced in the beginning of this subsection, which was published in the revised edition of Carnap (1966):

> It is obvious that there is a difference between the meanings of the instrumentalist and the realist ways of speaking. My own view, which I shall not elaborate here, is essentially this. I believe that the question should not be discussed in the form: "Are theoretical entities real?" but rather in the form: "Shall we prefer a language of physics (and of science in general) that contains theoretical terms, or a language without such terms?" From this point of view the question becomes one of preference and practical decision (Carnap 1974: 256).

Carnap also added in a footnote:

> In my view greater clarity often results if discussions of whether certain entities are real are replaced by discussions of preference of language forms. This view is defended in detail in my "Empiricism, Semantics, and Ontology" [Carnap 1950] (Carnap 1974: 256, footnote 4).

This change is very telling. It explicitly highlights that Carnap is reinterpreting the ontological dispute between realists and anti-realists in terms of the choice between different frameworks. By doing that, although he cannot maintain that these rival views are compatible, he can preserve a neutral stance toward them. Framework-dependent neutralism is the key feature here.

More recently, Michael Friedman has suggested that Carnap's neutralism can be thought of as a symbiosis between van Fraassen's constructive empiricism and contemporary structural realism:

> The circumstance that Carnap has this much in common with both van Fraassen's instrumentalism and contemporary structural realism suggests, at least to me, that Carnap's attempt at neutrality may have succeeded after all. He may in fact have articulated a version of structuralism that recognizes the strengths of both instrumentalism and realism while simultaneously avoiding the philosophical "pseudo-questions" on which they appear substantively to differ. But this can only be fully appreciated, I shall argue, when we place Carnap's views on theoretical terms within his wider conception of the task of philosophy of science—which he calls *Wissenschaftslogik*, the "logic of science"—more generally (Friedman 2011: 252).

This is an intriguing claim, which suggestively brings Carnap's work directly to bear on recent discussions in the philosophy of science. In support of his view, Friedman starts by examining the relation between Carnap's conception and constructive empiricism, before exploring the connections between Carnap's view and structural realism. First, Friedman identifies a similarity in van Fraassen's characterization of the proper commitment of a scientific theory and Carnap's representation of the empirical content of a scientific theory *via* its associated Ramsey-sentence. He notes:

> . . . despite his lack of patience with Carnap's approach, van Fraassen's view of what a scientific theory should properly assert is virtually identical with Carnap's conception of the Ramsey-sentence representation of a theory's synthetic or empirical content. [On a footnote, Friedman adds: "This point is first emphasized in Demopoulos (2003)".] Van Fraassen thinks that we should only assert that the observational phenomena are *embeddable into* an abstract model for the theory, and the Ramsey-sentence, on Carnap's account, asserts precisely the same—that there is some abstract (mathematical) model of the theory such that all observable phenomena behave in the way that the theory requires (1966, 1974: 254-255): "Some physicists are content to think about such terms as 'electron' in the Ramsey way. They evade the question of existence by stating that there are certain observable events, in bubble chambers and so on, that can be described by certain mathematical functions, within the framework of a certain theoretical system. Beyond that they will assert nothing (Friedman 2011: 251-252).

There are, however, difficulties with this suggestion. One of the key benefits of van Fraassen's constructive empiricism is that, as opposed to the logical positivist view, it takes scientific theories literally. That is, if a scientific theory states that there are electrons, one will not try to reconstruct such a theory avoiding reference to electrons (van Fraassen

1980). Van Fraassen's innovation is to highlight that one can make perfectly good sense of scientific practice without the commitment to the existence of unobservable entities—as long as one does not assent to anything more than the theory's empirical adequacy (and remains agnostic about what goes on at the unobservable level). One of the central reasons why van Fraassen rejects the syntactic approach to theories is because it requires that such theories be reconstructed—reformulated, for instance, in terms of its Ramsey-sentence. As a result, one cannot make sense of scientific practice in its own terms, and a layer of artificial reconstruction is introduced. This point cannot be ignored; otherwise a significant benefit of constructive empiricism is lost.

In contrast, Carnap's strategy requires the reconstruction of every scientific theory, so that its Ramsey-sentence can be formulated. On his view, some physicists, who are sympathetic to the Ramsey-sentence approach, will not assert anything beyond the content of the theory's Ramsey-sentence. But this doesn't say anything about the full content of the original unreconstructed theory—precisely the theory the constructive empiricist is empiricist about—nor does it specify the nature of one's commitment to the original theory.

It should now be apparent that van Fraassen's constructive empiricism and Carnap's reconstructive view are importantly different, despite any formal similarity between them. To specify the content of one's empiricism by means of a Ramsey-sentence is significantly different from stating that a theory is empirically adequate—even if the same observational structures are picked out in each case. For the Ramsey-sentence weakens the theory, emptying it of any theoretical significance, in a way that the focus on its empirical adequacy doesn't. To embed the models of data into the empirical substructures of a given model of a theory—as van Fraassen requires for the theory's empirical adequacy ([1980], p. 64)—is to highlight the significant role that theoretical considerations play in understanding the phenomena, even if one need not take these considerations to be true, but empirically adequate only. In contrast, to formulate the Ramsey-sentence for a given scientific theory is to move away from the original theory in a double way: by reformulating the theory in the first place, and by weakening it further by explicitly identifying one's commitment to only what is expressed *via* the relevant Ramsey-sentence. The formal similarity between the two views underscores a fundamental difference in attitude toward the theoretical content of science: van Fraassen's conception is neither eliminativist nor re-constructionist regarding that content; Carnap's is.

This point goes through despite Carnap's acknowledgement that the Ramsey-sentence approach is problematic (Carnap 1966/1974). Not surprisingly, Carnap does not recommend that the original theory be replaced by its Ramsey-sentence, but thinks that that sentence's role is to specify the empirical content of a scientific theory, that is, it should be used in the "explication of experiential import" (Carnap 1963: 963). As Friedman (2011: 257) notes, Carnap ultimately suggests a combination of both the Ramsey-sentence and the Carnap sentence for the task of expressing the original theory, where the Carnap sentence is the conditional whose antecedent is the Ramsey-sentence for the theory and the consequent is the theory itself. Thus, the original theory is equivalent to the conjunction of the Carnap sentence with the Ramsey-sentence. However, Carnap's view requires the reconstruction of the theory so that its Ramsey-sentence can be specified. Moreover, the explanation of the phenomena ultimately is not articulated in terms of the theoretical content. At best, as Carnap emphasizes, the phenomena are *described* by the mathematical features that are part of the framework of the theory (Carnap 1966/1974: 254-255). In this sense, his proposal ends up downplaying the significance of the theoretical content, and ultimately becomes a form of eliminativism regarding that content, since it is not invoked in the explanation of the phenomena.

Furthermore, Friedman goes on to argue that Carnap's position also bears significant similarities with contemporary structural realism. I see this as a problematic interpretation. On his view:

> It is equally true, as Psillos has argued, that Carnap's view also has much in common with contemporary structural realism. Indeed, Carnap himself ends *Philosophical Foundations of Physics* with what looks like a ringing endorsement of that view (Friedman 2011: 252).

In support of this interpretation, Friedman quotes the concluding sentences of Carnap (1966/1974), in which, speaking of our understanding of quantum mechanics, Carnap notes:

> Some physicists believe that there is a good chance for a new breakthrough in the near future. Whether it will be soon or later, we may trust - provided the world's leading statesman refrain from the ultimate folly of nuclear war and permit humanity to survive -that science will continue to make great progress and lead us to ever deeper insights into the structure of the world (Carnap 1966/1974: 291-292).

The crucial point here is to understand Carnap's reference to the *structure of the world*. It's indeed tempting to read this as a forceful endorsement of contemporary structural realism. But in order to do that, 'structure' needs to be understood as those fundamental features of reality that are preserved through theory change, since that's the relevant sense in contemporary literature. But this kind of realist commitment is precisely the sort of metaphysical view that Carnap wants to avoid (as Friedman is, of course, aware). As Carnap emphasizes:

> The usual ontological questions about the "*reality*" (in an alleged metaphysical sense) of numbers, classes, space-time points, bodies, minds, etc., are pseudo-questions without cognitive content (Carnap 1956: 44-45; quoted in Friedman 2011: 254).

Thus, a better understanding of 'structure of the world' here would be in terms of something that is not committed to the reality of the particular features under consideration. More likely, in the relevant passage, Carnap is not using 'structure of the world' in any metaphysically robust sense. But this means, as one would expect from him, that he is avoiding precisely the realist components of structural realism. By making use of 'structure', Carnap does not aim at a structural realist picture, but more generally aims to say something like 'construction', 'ordering', or 'composition'. I argue that Friedman is reading too much into this passage, since we also have to keep in mind that the German word 'Aufbau' can well be translated into 'structure', but equally well into the other words I have just mentioned. In any case, Carnap endorsed a structuralist epistemology, but not structural realism per se, and the quote to which Friedman refers is not sufficient to prove otherwise.

The point here is that no such a combination of views—constructive empiricism and contemporary structural realism—seems possible at all, since clearly the views have incompatible components. What Carnap ultimately does is to implement a form of framework-dependent neutralism, which could be applied to a variety of different versions of realist and anti-realist views about science, but without ever combining them simultaneously; otherwise, the result would be an inconsistent combination of conceptions.

It is noteworthy that Carnap has held neutralism since his early work. Friedman (1999) highlights this, by mentioning that in the *Aufbau*:

> The world of physics is univocally determined *via* the physical-qualitative coordination plus conventional stipulations . . . the *Aufbau* instead anticipates Carnap's (1950) later strategy of "Empiricism, Semantics, and Ontology": the question of the reality of the external world dissolves into the "external question" of whether or not to accept and use the forms of expression of the "thing language" (Friedman 1999: 123-124).

Friedman is right in noting the continuity in Carnap's view on ontology, although the explicit distinction between internal and external questions won't be formulated by Carnap until the later paper from the 1950's. For the purpose of this dissertation, it is important to note that in distinguishing between internal and external questions, Carnap is ultimately supporting a form of neutralism, since questions of existence (of numbers, propositions, physical objects) are dependent on the explicit framework in which they are formulated. The relevant concepts need to be formulated in order that the corresponding question of the existence of objects falling under these concepts can be properly raised. It may then be simply an analytic issue, given the framework, whether numbers or propositions exist. But this does not settle the metaphysical question as to whether numbers, propositions or physical objects exist *independently of the relevant frameworks*. In Carnap's hands, as we saw, these questions become pragmatic issues involving the choice of a framework.

However, one has to acknowledge the fact that, in choosing between frameworks, Carnap is not, in effect, introducing different concepts of existence, one for each framework under consideration. He is trying to avoid any form of commitment to metaphysical issues of existence, and to claim that there are different concepts of existence is to take a stand on exactly that issue. In this sense, the form of ontological pluralism that Matti Eklund tentatively tries to rescue on Carnap's behalf probably would not have been of much help to Carnap himself, since it presupposes that there is a "multitude of existence-like concepts" (Eklund 2009: 148), which is a proposal Carnap would ultimately resist.

It seems in fact that Carnap's neutralism is both a methodological position and metaphilosophical. It is metaphilosophical in the sense that it aims to avoid philosophical disputes, it prescribes and discusses how to do philosophy.

However, one could argue that the following problem emerges for neutralism: If one is supposed to choose rationally between different frameworks, on what logical grounds would such a choice be made? As Florian Steinberger argued:

> According to Carnap's [neutralism][1], logical norms are self-imposed. We impose logical norms upon ourselves by adopting a linguistic framework that gives rise to the corresponding logical laws. But then on the basis of what rational norms do we choose a linguistic framework? Presumably, our choice ought to be guided by certain logical norms. It would appear that in order to be able to rationally impose logical laws on ourselves, we must already be subject to something like such norms. This problem strikes me as fundamental to Carnap's entire tolerance-based philosophy (Steinberger 2014: 18).

This may seem to be a fundamental problem for any neutralist approach. We cannot choose a framework before accepting certain logical principles. But this principles could be chosen by considering fragments of different logical systems and evaluating what follows from them. This would allow a neutralist to get off the ground.

There are, however, problems for Carnap's neutralism. In particular, his rejection of metaphysics as being meaningless is interestingly non-neutral. Carnap takes a particular anti-metaphysical stance on the status of metaphysics that violates, in part, his own neutralism. Even though he admits that the "constituted forms" that are discussed in the *Aufbau* are neutral, in that they are neither "generated" nor "recognized" (Carnap 1928: §5), he still seems to regard metaphysical views as deeply misguided and inadequate, specifically in the domain of science and in *scientific* philosophy:

> The decision concerning the two main questions about metaphysics, that is, if it makes sense at all and if it has a justifi-

[1] Steinberger uses the term voluntarism where I use the term neutralism.

> able [*daseinsberechtigt*] existence, and if so, if it is a science, depends apparently on what we call "metaphysics". ... If it is about the first knowledge ... we can use the name "fundamental science" [*Grundwissenschaft*] instead; if it is about the ultimate, most general knowledge, we can use the name "world view" [*Weltlehre*]. ... On the other hand, the name "metaphysics" is used for the result of a non rational, but merely intuitive process; and this seems to be the common use of language (Carnap 1928: §182).

His rejection of metaphysics becomes clearer later on in the same paragraph, where he argues:

> The fact that intuitive metaphysics makes use of words to express itself must not lead us to the opinion that it would belong to the domain of concepts, and in that sense, to the domain of (rational) science. ... Also in spheres of life other than the one involving conceptual knowledge, words are used; for example, in the practical transmission of will from one person to another, in art, in the field of myth (to which intuitive metaphysics perhaps belongs), which stands between science and art, and in other fields (Carnap 1928: §182).

Carnap's rejection of metaphysics, as not belonging to the domain of the rational and conceptual, is apparent. This results from the fact that he places (intuitive) metaphysics in the field of myth, and also that he sees an absence of rational concepts in metaphysics. For him, metaphysics falls outside the domain of the rational and the scientific, and there is no place for it in scientific philosophy, that is, in philosophy as

Carnap understands it. From the perspective of this discussion, rather than adopting a proper neutralist stance toward metaphysics, Carnap ultimately rejected metaphysics, and was not neutral in this sense.

Having said that, it should be clear that by choosing a certain formal framework, one implicitly accepts its ontological commitments. If we are rational agents, it seems that we are in principle unable to approach philosophical questions neutrally, because the framework has those commitments. But what kind of neutralism is ultimately possible? In order to examine this point, I will develop a neutral form of neutralism.

8.4 Neutral neutralism

I aim to defend a neutral form of neutralism that avoids the rejection of metaphysics championed by Carnap. I argue that the resulting form of neutralism does not face the difficulties faced by Carnap's, and it incorporates several advantages as well.

The neutral neutralist will embrace neutralism about all of the components of the typology that I have discussed above (in this respect, there is full agreement with Carnap). First, with regard to metaphysical neutralism, the neutral neutralist will resist the temptation of settling the metaphysical issue of what there is: are there universals, properties, relations, tropes, particulars, objects, individuals? Different metaphysical views have been advanced throughout the history of philosophy in an attempt to settle this issue. We find, for instance, one-category ontologies, based only on tropes, and suitable reduction relations with regard to the remaining categories (see, *e.g.*, Campbell 1990); two-category ontologies, either based on objects and universals plus suitable reduction relations (see, *e.g.*, Armstrong 1997), or based on objects and tropes plus reduction relations (see, *e.g.*, Martin 1980 and 1993); three-category ontologies, based on universals, objects, and tropes (see, *e.g.*, Lowe 2006), and four-category ontologies, based on universals, objects, tropes, and modes (see Lowe 2006). In each case, there are a variety of arguments in support of each particular ontology. They range from the minimal commitments involved when only tropes (and, thus, only particulars) are assumed in one's ontology all the way to the significant expressive advantages of having maximal commitments to a four-category ontology.

Not surprisingly, these arguments have been for the most part inconclusive, and there is no agreement as to which of these ontologies should be preferred. The epistemological considerations that support a meager ontology are counterbalanced by the expressive richness of less spare ontologies. The result is a standoff, and the neutral neutralist recommends suspending judgment about the issue. Not surprisingly perhaps, one can make perfectly good sense of scientific activity without settling metaphysical issues of this sort, given that scientific practice itself is neutral on the answers that can be given. Typically, that practice is compatible with any of the different metaphysical packages available.

A similar recommendation would be made in other sorts of metaphysical issues about the sciences in which the weight of the evidence does not favor one view over others, such as, whether there are laws of nature, natural kinds, dispositions, or causes—and if so, in each case, what their nature ultimately turns out to be. As opposed to Carnap, the neutral neutralist does not deny that these and other metaphysical issues are meaningful: there's nothing wrong with raising them, even if it may not be clear how they can be settled in the end.

Furthermore, the neutral neutralist will adopt neutralism about the particular frameworks that are invoked in a given context. The result is a form of framework-dependent neutralism. Depending on the particular framework one adopts, different commitments will be involved: some will be about mathematical objects and structures invoked in the formulation of scientific theories; others will involve assumptions about the nature of the objects, relations, events, and processes that are referred to in the relevant theories. Whatever these frameworks turn out to be, they each will embody commitments of a particular kind. The neutral neutralism will register these commitments in each case, and will contrast with different commitments involved in alternative frameworks.

The neutral neutralist, although clearly a pluralist about frameworks, is not a relativist about them. The claim is *not* that any framework is just as good as any other, and thus that everything goes. For example, a framework that is based on classical logic is not adequate to capture constructive features of mathematical reasoning. But one that relies on constructive logics is. Since there are several such logics, there are several different frameworks for the task at hand. Some frameworks are better than others for certain purposes, and hence they should be preferred. For example, certain constructive frameworks may have a richer class of consequences than others, but may be equivalent to other constructive frameworks on this score.

Typically, *more than one* framework can be adopted for the same purpose (and, clearly, several frameworks can be implemented for different purposes). Classical mechanics admits of Hamiltonian or Lagrangian formulations (Arnold 1989). Non-relativist quantum mechanics can be formulated in terms of matrices (matrice mechanics) or waves (wave mechanics) (von Neumann 1932). Quantum field theories, in turn, have no canonical formulation and can be expressed in a variety of different ways, depending on the constraints that are imposed (Kuhlmann 2010). There is, thus, a plurality of frameworks, but since some are better than others for certain purposes, relativism is still avoided.

My view also embraces neutralism about the interpretation of science, and thus will recommend an interpretational neutralism. The various interpretations of science also provide additions to scientific practice that are not strictly needed to make sense of that practice. Whether scientific activity is understood as involving the search for true (or approximately true) theories, or whether it is characterized as aiming for empirically adequate theories, is not something that can be established on the basis of the activity alone. After all, the practice itself does not require any of the particular interpretations in order to function, and in this sense it is neutral on interpretational issues.

Moreover, each interpretation requires specific commitments regarding issues the practice is typically silent about. For instance, realist views normally presuppose a particular conception of truth (or truth approximation) in order to specify the nature of the relation between theory and reality. Anti-realist views normally invoke norms that are weaker than truth, such as empirical adequacy or problem-solving effectiveness, but they also need to determine the nature of the relation they posit between reality and theory. In the end, however, it is unclear to what extent these additions are in fact needed to understand the sciences.

Furthermore, there are several arguments that favor realist interpretations (from the no-miracles argument to arguments regarding how to secure epistemic access to unobservable objects) and several arguments that favor anti-realist interpretations (from arguments based on Kuhnian losses and discontinuities in scientific change to arguments highlighting the fewer commitments anti-realists require to make sense of scientific practice). But it is unclear that these arguments successfully establish one party in the dispute. It's perhaps not surprising that none of the proposed forms of realism or anti-realism have received widespread acceptance so far. Once again, being unable to choose between these conflicting arguments, the neutral neutralist will suspend judgment about the relevant issues.

Also, neutral neutralism takes no stand on semantic considerations in the sciences. Thus, a form of semantic neutralism emerges. Whether scientific terms refer or not, and if they do, what exactly they refer to, is something the neutral neutralist will not attempt to settle. Many considerations seem to support the successful reference of scientific terms—typically invoked by standard scientific realists (such as Psillos 1999). But opposing considerations question whether this is indeed the case—having to do with concerns about the falsity of many scientific theories. The neutral neutralist is unable to settle which of these competing considerations provide the right answer in this case.

By reflecting on the fact that it is unclear how to settle the scientific realism debate, the neutral neutralist will encourage the development of alternative proposals that attempt to move away from the rigid categories of realism and anti-realism. In the end, these categories don't seem to be as illuminating as one would expect them to be to make sense of crucial details of scientific practice.

The natural ontological attitude

But perhaps there is an earlier form of neutralism in contemporary philosophy of science, namely, Arthur Fine's (1984a, 1984b) suggestive position: the natural ontological attitude (NOA). This proposal tries to resist the need for adding to scientific practice a layer of metaphysical and epistemological interpretation, commonly found in both realist and anti-realist approaches to science. Realists typically add a particular conception of truth—or approximate truth, or some other form of correspondence between theory and world—as part of their interpretation of science. Anti-realists, in turn, often resist the appropriateness of invoking truth—or the other alternative norms recommended by realists—in the context of theory assessment, and advance weaker norms instead, such as empirical adequacy, problem-solving effectiveness, or some other way of assessing the relation between theory and world. From the point of view of NOA, these are all unnecessary additions to what takes place in scientific practice:

> What binds realism and anti-realism together is this. They see science as a set of practices in need of an interpretation, and they see themselves as providing just the right interpretation .... The quickest way to get a feel for NOA is to understand it as undoing the idea of interpretation, and the correlative idea of invariance (or essence) ... NOA is inclined to reject all interpretations, theories, construals, pictures, etc. of truth, just as it rejects the special correspondence theory of realism and the acceptance pictures of the truth-mongering anti-realisms .... NOA is fundamentally a heuristic attitude,

> one that is compatible with quite different assessments of particular scientific investigations .... NOA tries to let science speak for itself, and it trusts in our native ability to get the message without having to rely on metaphysical or epistemological hearing aids (Fine 1984b: 61-63).

But it is unclear whether there is such a "natural ontological attitude" to begin with. NOA seems to presuppose that the identification of an ontological attitude could be made in a metaphysical or epistemological vacuum, as something that is simply given irrespectively of any particular philosophical commitments. But it is unclear exactly how, in letting "science speak for itself", one could detect the particular commitments "naturally". On the basis of what kind of evidence can one affirm that we have a "native ability" that allows us to avoid unnecessary metaphysical or epistemological commitments? The very content of a scientific theory and how it is supported by the evidence often depend on how such a theory is interpreted, that is, on whether it is taken as a description of an independently existing range of phenomena or of some artifact that needs to be explained away. For Fine, NOA puts us in a position in which we can avoid any unnecessary interpretation of science. However, it is not clear how that can be implemented, and even whether we can draw the line between the content of science and its interpretation as neatly and easily as he suggests. The mere appeal to our native ability to understand science is not sufficient to make sense of scientific practice. After all, that practice is often embedded in metaphysical and epistemological assumptions—although such assumptions are not typically made explicit, unless in those cases in which controversies arise.

If we contrast NOA and neutral neutralism, it seems that Fine's proposal is not a form of neutralism, since it accepts the commitments that are "naturally" found in scientific practice—whatever these commitments are and however they end up being expressed. However, absent some *account* of what these commitments are, it is not clear how one can identify them. To do so, we need a particular framework in which the relevant commitments are explicitly stated. In contrast, the neutral neutralist offers an alternative that explicitly identifies the commitments of particular views—in light of the specific framework that is adopted—but resists the attempt at settling which of these views is ultimately right.

Furthermore, even if NOA did provide a form of neutralism, it should be noted that it is primarily concerned with interpretational issues in science—in particular with the issue of how realist and anti-realist interpretations provide what turn out to be ultimately unnecessary maneuvers to the understanding of scientific practice. Thus, NOA would be, at best, a form of interpretational neutralism. But as my typology of neutralism indicates, there are additional issues about science that one may need to be neutralist about too (metaphysical, semantic, and framework-dependent). In this way, neutral neutralism provides a broader setting for neutralism than NOA does. Neutral neutralism, however, offers a characterization and a typology of the different neutralisms that can be formulated and upheld, as well as an assessment of their viability and adequacy.

Is the proposed neutral neutralism just Carnap's overall neutralism? Not really. As I have argued, Carnap's considered view involves a rejection of metaphysics as being cognitively inadequate: According to him, it is not conceptual in nature nor is it rational or scientific. In contrast, I propose no such rejection. Metaphysicians, including those working on the metaphysics of science, are certainly raising cognitively significant, meaningful, rational questions.

What emerges is a neutral form of neutralism that engages with debates in the sciences, including those concerned with scientific realism, structural realism, and anti-realism, but without taking a stand and the resulting troublesome and problematic commitments. In the end, what one gains is *understanding*: of how the world could be if the relevant theories under consideration were true (see van Fraassen 1991). One can learn something significant by examining the issues in this way quite independently of settling which of the proposed answers is ultimately correct.

8.5 A framework for neutralism

After having discussed the possibility of neutralism, and after having argued for what I have called neutral neutralism, I will now proceed and discuss if the framework of the structuralist meta-theory can be used to represent neutralist views.

8.5.1 Neutralism in the structuralist meta-theory

In the structuralist meta-theory, there is almost no explicit work on questions of the scientific realism debate (see Sneed 1983 for the only explicit work on scientific realism). In personal conversation with some of its main representative figures (Ulises Moulines, Wolfgang Balzer, Jose Díez, Pablo Lorenzano, and others), it has been repeatedly pointed out to me that questions of scientific realism are not the main interest of the structuralist meta-theory, and that most likely, the structuralist meta-theory would fit best in a neutralist picture. In this section, I will discuss this question further and analyze in detail if the structuralist meta-theory is in fact a neutralist view.

Theoretical terms

Let us first have a look at the criterion of theoretical terms that is endorsed by the structuralist meta-theory. The structuralist concept of theoretical terms reveals us that the adherents of the structuralist meta-theory do not endorse a realist view:

> Term t is T-theoretical if every t-determining model is a model of T. Or, in other words, if the set of all t-determining models is included in $M(T)$, or if any method of determination for t is contained in $M(T)$. The intuitive idea of theoreticity thus is the following. Term t being T-theoretical means that t can be determined only if T's fundamental laws are presupposed. In other words: The determination of t only works in situations in which T's fundamental laws are satisfied (Balzer *et al.* 1987: 65).

First, theoretical terms, their meaning and their reference, matters that are usually central to a scientific realist, are all left out by the structuralist criterion. Of course, the structuralist meta-theory is a model-theoretic conception of theories, where one implicitly accepts that the notion of truth, if there is one, would be Tarskian. But in this particular view, there is at no point in the literature a development of a concept of truth. Structuralists introduce T-theoretical terms in order to determine the structure of a theory, and to separate terms that are newly introduced into a theory from terms that 'come' from other theories, *via* intertheoretical relations. This is a merely methodological approach to theoretical terms. It is never asked whether such terms refer or what they mean, as long as they can be determined by means of the satisfaction of the fundamental laws of a theory in question.

Furthermore, it is also remarkable that in the whole literature of the structuralist meta-theory, classical notions of scientific realists such as truth and approximate truth, correspondence, predictions or any other notions that are needed in order to fulfill metaphysical, epistemological and semantic realism, are completely left beside. All this suggests that the structuralist meta-theory is not a realist view. But is it therefore necessarily an instrumentalist view? In what follows, I will discuss the two only textual evidences in the whole structuralist corpus, where a concrete answer to this question could be found, namely, Sneed's (1983) work and Stegmüller's notion of a relative a-priori.

The structuralist meta-theory relies on set theory as its basic formal framework, and the considerations above make this point very clearly. Given that set theory is not ontologically innocent, at least in its usual interpretation, some more work is needed in order to ensure that the structuralist meta-theory provides a framework-independent neutralism. Given the structuralist meta-theoreticians' goal of remaining neutral with respect to all theories to which their framework is applied, the commitment to abstract objects that emerges from set theory should be avoided. After all, a conflict may emerge between this goal and set theory's most natural interpretation.

If one assumes, for the sake of argument, a platonist interpretation of mathematics, then numbers, functions and sets are indeed importantly different from symphonies and theory-elements. In the structuralist meta-theory, we read:

> The fact that the formal characterization of theory-elements may include mathematical entities such as sets of real numbers and numerical functions (which constitute infinities in the peculiar sense of mathematics) should not be confused with the idea that they themselves coincide with those mathematical universes. Ontologically speaking, theory-elements are more like symphonies or political ideologies than like numbers. That they apparently are more amenable to formal representation than other cultural products is a purely contingent fact that has nothing to do with the possibility of an infinite construction (Balzer *et al.*: 412).

Whereas mathematical objects are abstract entities (which are neither spatially nor temporally located, nor are they causally active), symphonies and theory-elements are curiously hybrid objects: they are clearly temporal, but not spatial, entities. Beethoven's Ninth Symphony didn't exist in the thirteenth century, and arguably it will no longer exist if all of its scores and recordings are destroyed, and no one recalls how to play it anymore. Similarly, Newtonian mechanics didn't exist in the thirteenth century, and arguably it will no longer exist if all of its written formulations are destroyed, and no one recalls how to express it either. In this respect, both are temporal objects. But neither symphonies nor theory-elements are spatial objects, since arguably one cannot identify a symphony with its score (since different token scores still encode the same symphony), nor can one identify a theory-element with its physical inscription (there are, after all, different ways of formulating the same theory). There is, however, an important difference between symphonies and theory-elements: although theory-elements are not causally active, symphonies—when performed—can be heard, and thus have some causal power.

Despite the fact that structuralist meta-theorists suggest that theory-elements are closer to symphonies than to numbers (and other mathematical objects), the point still stands that, by invoking a particular mathematical framework (a particular set theory), they are committed to those entities that are posited by that framework—unless some nominalization strategy is presented that explains how one can use the framework without commitment to the corresponding entities that it posits. In the absence of such a nominalization strategy, one cannot simply assume that ontological commitments can be avoided.

One may argue that the choice of frameworks is ultimately a matter of convenience. For the structuralist proposal can also be developed within category theory (Mormann 1996). The fact that these different formulations—set-theoretic or categorial—are available suggests a form of neutralism about the underlying theoretical resources, which thus provides a form of framework neutralism.

Architectonic

Besides the formal requirements and implicit commitments that we have to accept when we decide to apply a certain framework, there are further meta-philosophical issues with the structuralist meta-theory that suggest a neutralist view. If we want to analyze the supposed neutralist commitments of the structuralist meta-theory, it is of central importance to take a look into what can be called the structuralist main work, *An Architectonic for Science* (Balzer *et al.* 1987). In this work, the formal framework of the whole approach is developed, applied and discussed. The authors develop many case studies, discuss issues of theory change and theoretical terms, and outline the technical tools for applying the structuralist meta-theory. Moreover, the authors of *Architectonic* clearly commit themselves to a structuralist, a holist and a neutralist view. Regarding structuralism, they note:

> Our present aim is more modest. We want to provide a description of those structural features of empirical knowledge which, we think, could serve to distinguish it in an interesting way from other things (*ibid.*: xvii).

Their commitment to holism is also highlighted as follows:

> This view commits us to a certain kind of “holism". We believe that those features of empirical science that fully distinguish it from non-scientific enterprises can only be seen clearly by viewing sufficiently “large" fragments of scientific knowledge. That is, they are only apparent when one has a sufficiently “global" perspective (*ibid.*: xvii-xviii).

Finally, their neutralism is characterized as follows:

> ... In a restricted sense ‘perspective’ means “historical perspective". Our representations of scientific knowledge should

> be “historically neutral" in the sense that knowledge in every historical time period should be treated in the same way. ...“perspective" requires that the representation scheme be “neutral" with respect to all the theories to which it is applied (*ibid.*: xvii-xviii).

The commitment to holism emerges from the formal framework that is used and the concrete case studies that are developed. The holism is restricted to theory-nets and is not of a global Quinean form. There is no *web of belief*, but only a *web of theories*. The holism of the structuralist meta-theory states, more moderately, that by looking at empirical theories from a more global perspective, some of their features might become more apparent. But it is clear that in the structuralist meta-theory, no one would claim that molecular biology has a strong and necessary intertheoretical connection with value theory in economics. In this sense, it is a restricted and moderate holism about scientific theories.

For my discussion of neutralism, one further concept is central for the structuralist meta-theory, the so-called *theory-holons*. These holons are bigger theory-nets; more exactly, they are sets of theory-elements related by intertheoretical links. Formally, they are defined as follows (for the full definition, see Balzer *et al.* 1987: 389):

> H is a theory-holon if, and only if, there exist N and λ, such that $H = \langle N, \lambda \rangle$ and

1. N is a non-empty set of theory-elements;
2. $\lambda : N \times N \to \bigcup\{\mathcal{P}(M_p(T) \times M_p(T'))/T, T' \in N\}$ is a partial function.

Given that the logical reconstructions provided by the structuralist meta-theorists are carried out in naïve set theory (Balzer *et al.* 1987: xii), implicit ontological commitments are made. Left untamed, naïve set theory is inconsistent, and hence a commitment to inconsistent objects immediately emerges. Presumably, this is not something structuralist meta-theorists intend, and perhaps rather than using naïve set theory, they aim to adopt an axiomatic formulation of set theory, such as Zermelo-Fraenkel set theory with the axiom of choice (ZFC). This would avoid the commitment to inconsistent objects (assuming that ZFC is consistent), but does entail the commitment to a multitude of sets, which are abstract objects.

One of the main purposes of this section is to shed more light on the ontological commitments of the structuralist program. It is problematic to maintain that the use of formal methods is just a heuristic tool for the representation of scientific theories, and does not have any additional commitments of its own, without, however, providing any account of how exactly such commitments are avoided.

It also comes out clearly that the structuralist meta-theory refuses to commit to a position in the debate about scientific realism:

> A theory-element $T = \langle K, I \rangle$ may be regarded as an instrument for formulating empirical claims. We do not think that this statement forces on us any particular epistemological position like instrumentalism, or anti-realism with respect to theoretical terms, as has sometimes been claimed by critics of our program ... Our main intention here is to give an account of how to construe empirical claims which we find in scientific practice, and this account does not seem to fit squarely

> into such labels as "instrumentalism", "anti-realism" or their opposites for that matter (*ibid.*: 90).

Here, one can see the explicit commitment to a neutral perspective. The authors apparently take it for granted that a formal framework, what they call a *representation scheme*, is neutral. I argue that this is not possible in general. Since one has to accept the underlying ontological postulates of whatever framework one applies. It is not the same to apply naïve set-theory or Zermelo-Fraenkel set theory. Although these set-theories share many ontological commitments, they also differ in many respects. I think that in this precise sense, one can never be completely neutral with respect to a representation scheme. What can be done is to adopt a pragmatic way of justifying the choice of a certain framework, in the sense of Carnap's *Principle of Tolerance*, as I have discussed above.

Sneed's 1983 paper

I will now analyze Sneed's paper on the relation between scientific realism and the structuralist meta-theory. So far, I have outlined that in *Architectonic*, its adherents argue for a neutralist and a moderate holist view. Sneed's work was published before *Architectonic*, and reveals that the same approach was already in place in the earlier phase of the structuralist meta-theory. Sneed motivates the structuralist meta-theory as follows: "Structuralism is essentially a view about the logical form of the claims of empirical theories and the nature of the predicates that are used to make these claims ... Roughly, the claims of empirical theories are rendered as set-theoretic versions of a Ramsey-sentence" (Sneed 1983: 350). This gives us a first hint regarding its motivation.

Later on, Sneed describes in which sense both the scientific realist and the structuralist would agree:

> ... Structuralists see the mathematical structures associated with a theory to be much more "essential" features of the theory than the claims it makes. The claims may change with the historical development of the theory, but the mathematical apparatus remains the same. Thus both structuralist and realist would agree that empirical science makes descriptive claims but disagree about the extent to which empirical scientists, speaking professionally, "mean what they say" (*ibid.*: 351).

If the mathematical apparatus remained the same, our representation scheme would always be the same, no matter what developments in mathematics influence on our framework. However, since there is clearly progress and theory change in mathematics as well, Sneed's view seems to bear problems. As noted above, in the applications of a framework, it is not the same to adopt a finitist set-theory or to posit the existence of infinite sets. There are always ontological commitments in the background.

Furthermore, the Kuhnian aspect in structuralism is also important. Structuralists usually argue that:

> Structuralism is not committed to the view that any kind of revolutionary development has, in fact, occurred at any point in the history of science. At most, structuralism provides the formal methods to reconstruct the theories associated with scientific traditions putatively separated by "revolution"

> ... revolutionary scientific change is just different from normal scientific change but not, in any apparent way "irrational" (*ibid.*: 354).

This reveals that Sneed does not accept a kind of revolutionary theory change in the sense of the early Kuhn. In fact, Kuhn recognizes the virtues of the structuralist framework, as I have discussed in chapter 3. This suggests that the structuralist motivations are in fact on the side of logical rigor, detail and objectivity, altogether Carnapian virtues, so to speak.

For Sneed, the difference between structuralism and scientific realism appears to be as follows: "Structuralist reconstructions of empirical theories attribute claims to them whose logical form departs so widely from the logical form of claims apparent in the literature of the theory that realists find them implausible (*ibid.*: 355)". He appeals to the fact that structuralists provide logical reconstructions of theories but make no metaphysical claims, as scientific realists do.

Later on, Sneed mentions that the distinction between T-theoretical and T-non-theoretical terms is rejected by realists: "The structuralist theoretical - non-theoretical distinction between the elements of the models for a theory entails an ontological distinction between properties or individuals that the theory is about which realism rejects (*ibid.*: 355)". Another point of conflict between both views appears to be that the classic scientific realist has a different conception of the meaning of theoretical terms:

> Structuralism is committed to a kind of 'contextualism' for the meaning of terms referring to theoretical elements that is

> incompatible with the realist's view that the reference of all predicates and singular terms in an empirical theory is nailed down 'near' the beginning of the theory's development (*ibid.*: 356).

By what Sneed calls contextualism, we see again the commitment to a form of holism by the structuralist meta-theory. One might criticize that Sneed takes it that scientific realists have a pretty simple notion of the meaning of theoretical terms, where it is in fact the case that the issue about the reference of theoretical terms is still a live question in the scientific-realism debate. It seems that this difference can be highlighted by carefully sorting out a theory of reference, as it has been done by contemporary scientific realists (see Psillos 1999). However, one has to be aware of the fact that Sneed's work dates from the phase of the scientific realism debate where Puntam just had developed what we call today a simple scientific realism (as I have discussed in chapter 2), and therefore one should not be too harsh on the evaluation of Sneed's views in the light of contemporary views. Sneed draws an interesting conclusion, relying on the structuralist criterion for theoreticity:

> One way of understanding the implications of structuralism for realism is then this. Assuming structuralism is true, if one wants to be a realist about the meaning of terms referring to theoretical elements, then one must also be a realist about theoretical laws (*ibid.*: 358).

This follows from the criterion for theoreticity, introduced above. We have seen that in the structuralist meta-theory, terms are introduced through the presupposition of the fundamental laws of the theory in question. In this sense, one has to be a realist about laws as well.

Sneed concludes without placing the structuralist meta-theory clearly on either the realist or the antirealist side. This could be seen as one indication in favor of neutralism, but is too weak as a single evidence, since it leaves too much room for speculation.

Stegmüller's relative a-priori

One further hint to neutralism in the structuralist meta-theory is Stegmül--ler's notion of a relative a-priori (see my discussion in chapter 3). This notion, as he develops it, suggests that the structuralist meta-theory is not neutralist at all. It rather suggests an ontologically committed view, at least in a very abstract sense.

Besides Stegmüller, also Friedman (2001) mentions a relativized a priori in the philosophy of science. His conclusion about a stable core, which can also undergo changes, is very much alike to the view advocated by Stegmüller:

> Reichenbach distinguishes two meanings of the Kantian a priori: necessary and unrevisable, fixed for all time, on the one hand, and "constitutive of the concept of the object of [scientific] knowledge," on the other. Reichenbach argues, on this basis, that the great lesson of the theory of relativity is that the former meaning must be dropped while the latter must be retained. Relativity theory involves a priori constitutive principles as necessary presuppositions of its properly empirical claims, just as much as did Newtonian mechanics, but these principles have essentially changed in the transition from the

> latter theory to the former . . . [what we end up with, in this tradition, is thus a relativized and dynamical conception of a priori mathematical-physical principles, which change and develop along with the development of the mathematical and physical sciences themselves, but which nevertheless retain the characteristically Kantian constitutive function of making the empirical natural knowledge thereby structured and framed by such principles first possible (Friedman 2001: 30-31).

If one accepts that there are a priori mathematical and physical principles, it might be hardly possible to be a neutralist. The neutralist would rather have to suspend judgement on this issue and appeal to the choice of a certain framework, in which such principles can be analyzed in the best way. In this sense, Stegmüller does not seem to defend a neutralist view, but rather a weak form of scientific realism, since the a priori he accepts would be relativized. But this is something that the usual scientific realist would not accept. Of course, such a realist view would most likely not be a semantic one, but at least metaphysical and epistemological.

It turns out that by looking into the views of the two early main figures of the structuralist meta-theory, Sneed and Stegmüller, we are not able to draw a general conclusion on their views. Sneed connects the structuralist meta-theory to the debate on scientific realism, but avoids a general conclusion on where the structuralist meta-theory stands in this debate. Stegmüller discusses a relativized a priori and seems to defend a weak form of scientific realism. But this alone is not sufficient for positioning his approach on either the realist or the antirealist side, since the structuralist meta-theory has developed in many different ways since both Sneed and Stegmüller developed it in the beginning.

Category theory

Despite the explicit commitment to naïve set-theory by the adherents of the structuralist meta-theory, Mormann (1996) shows that central concepts of this approach can be formulated alternatively in the framework of category theory. Mormann explicitly shows that this holds for the concepts of constraint, link and theory-holon. He argues that these concepts can be generalized when they are reformulated in a categorial setting, and that relevant categories can be formulated in each case.

From an ontological point of view, we may try to determine what are the commitments of the structuralist meta-theory. One may argue that, given the usual set-theoretic formulation of the view, it is at least committed to sets. By showing that core concepts of the structuralist meta-theory can be formulated in category theory, one could resist this conclusion. After all, the commitment to sets can be avoided by introducing categories as the basic framework. One would then have a significant form of framework neutralism.

It turns out, however, that the particular categorial framework adopted by Mormann presupposes set theory, given that a category is defined by a *collection* of objects such that for each *ordered pair* of objects, there is a *set* of morphisms involving these objects (see Mormann 1996: 269). Of course, the typical characterization of the concepts of *collection*, *ordered pair*, and *set* are all set-theoretic in nature. As a result, unless they are reformulated in some other way, in the particular version of category theory that Mormann employs, sets are ultimately presupposed. Thus, a truly neutralist maneuver, according to which it doesn't really matter which particular framework one adopts, cannot be made: sets are assigned a privileged status within the structuralist meta-theory if not in theory, certainly in practice.

8.6 Why frameworks matter

It may be argued that it should not matter which particular formal framework one ends up using to reconstruct a scientific theory: any such framework should do. But the situation is not so simple. Suppose one uses naïve set theory (in classical first-order logic) as the underlying framework. The theory has a number of benefits: it is simple, elegant, and extremely general. However, given that Russell's paradox can be formulated in it, the theory turns out to be inconsistent—and, given classical logic, trivial (that is, everything follows from it). Clearly, this would not be a suitable framework for the reconstruction of scientific theories.

In response, it may be noted that the relevant framework should be, at least, consistent. But this grants that particular features of the framework do matter after all—consistency being a significant feature of such frameworks, and one that, depending on the framework, is often not easy to establish. An additional concern emerges at this point. Suppose that one is trying to reconstruct Bohr's original model of the atom. As is well known, according to electrodynamics, that model would be unstable, since the electron would fall onto the nucleus. To secure the stability of the model, Bohr stipulated that such an event would not happen. As a result, Bohr's model was inconsistent with electrodynamics, and any faithful reconstruction of that model presumably should take into account this inconsistency. This means that no consistent framework that assumes classical logic would be adequate for this task. In the end, consistency cannot be a general requirement for any framework, for it is too strong and basically nothing would be reconstructable in any framework.

Moreover, I have emphasized that the structuralist meta-theory has been formulated in set theory, and it has also been formulated in category theory. From an ontological point of view, these two theories are significantly different: the former (in its usual characterization and assuming a Quinean doctrine of ontological commitment) is committed to sets, whereas the latter (also in its usual characterization) is committed to categories. But sets and categories are different kinds of things: sets have members, categories need not (they involve objects and morphisms); sets are not morphisms (although they can be associated with a characteristic function), and a category is not a set (nor does it even presuppose one). So we have here two different ontologies.

In light of this, what does the structuralist meta-theory ultimately presuppose, sets or categories? It may be argued that it does not presuppose either: it does not presuppose sets, since the view can be formulated in category theory; it does not presuppose categories, since the view can be formulated in set theory. It is, therefore, truly neutral between these two frameworks.

But is this maneuver adequate? Compare the analogous move about natural numbers. We can formulate number theory in set theory (in fact, as is well known, there are many different ways of doing that, using distinct set-theoretic reductions). We can also formulate number theory in category theory. Thus natural numbers, one could argue, do not presuppose sets, nor do they presuppose categories. They are truly independent from both. In fact, Fregeans (or neo-Fregeans) about numbers would insist on that, since for them numbers can be formulated in second-order logic plus some definitions alone.

But if we were to run the corresponding argument for the structuralist meta-theory, it is unclear what the view would be *independently* of some framework, that is, independently of either set theory or category theory. What would a theory-element, a global constraint or a global link *be* without the framework in which these components can be expressed? The proper answer is far from clear, since at least one framework seems to be indispensable, but each framework provides a different answer—one in terms of sets, the other in terms of categories. As a result, the ontological status of the structuralist meta-theory becomes puzzling.

So far, I have characterized a typology of neutralism and then outlined and discussed the relation between neutralism and the structuralist meta-theory. I will now give the following interpretation of neutralism with respect to the structuralist meta-theory:

1. **Metaphysical neutralism:** It seems clear that for the structuralist meta-theory, metaphysical neutralism seems to match. As we have seen above, adherents of this view explicitly commit to this form of neutralism. The possibility of applying the framework of the structuralist meta-theory to questions of theory change and structural realism suggests that it is indeed metaphysically neutral.

2. **Epistemological neutralism$_1$: Framework-dependent neutralism:** This view matches with the structuralist meta-theory as

well, since it usually employs naïve or Zermelo-Fraenkel set-theory, but there are important differences. As I have outlined, Mormann (1996) reconstructs the formal apparatus of the structuralist meta-theory in the framework of category-theory. This suggests that structuralists are flexible with respect to the choice of their formal tools and hence framework neutralists.

3. **Epistemological neutralism$_2$: Interpretational neutralism:** The structuralist meta-theory is not neutral with respect to issues of interpretation. The criterion for T-theoreticity is well-defined and applied broadly in the literature. A clearly defined criterion for the meaning of theoretical terms cannot be advocated by interpretational neutralists. I conclude that the structuralist meta-theory does not count as being interpretationally neutral.

4. **Semantic neutralism:** For the case of a semantic neutralist view, it seems that the structuralist meta-theory would enter this characterization. Whenever there is talk of scientific terms, these are taken to occur within scientific theories and especially in the case of the structuralist meta-theory, theoretical terms, receive their meaning only through those theories.

5. **Carnapian neutralism:** For the case of the structuralist meta-theory, a similarity to Carnap's approach becomes apparent. This becomes clear due to the fact that nowhere in the literature, questions of the realism debate are explicitly addressed. Only in Sneed's paper, we can find a comparison of realism and the structuralist

meta-theory. But this work is more a characterization, than a positioning of the structuralist meta-theory as a realist or antirealist position. Since in this view, the main aim is to provide a neutral description of the structure and dynamics of theories, it suggests in fact a compatibility with the Carnapian approach.

8.7 Conclusions

In this chapter, I have analyzed different forms of neutralism and I developed my own view, neutral neutralism. I and discussed whether the structuralist meta-theory can be called a neutralist position. It turned out that different forms of neutralism are accepted sometimes implicitly by this view. The analysis I provide here aims to establish more clarity with respect to the ontological commitments of neutralism itself and of the structuralist meta-theory. It is now clear in which way this view is neutralist, and also in which sense it relates to Carnap's neutralism.

Chapter 9

General conclusion

In this dissertation, I have shown how structural realism developed out of the debate on scientific realism. I discussed the influence that several authors had on the early development of structural realism and highlighted Carnap's early program of the *Aufbau* as especially relevant for the development for epistemic structural realism.

Furthermore, I have argued that an adequate tool for characterizing structural realism in a clear way is by means of the structuralist metatheory. By applying certain technical notions of this program, namely, the notion of reduction, the notion of structural continuity can be understood in a broader sense than only in the sense of a mere reappearance of certain mathematical equations, and the Ramsey-sentence. This result helps both to clarify Worrall's original proposal and to amplify structural realism. Nevertheless, It is clear that only after taking out further concrete case studies, structural realism will find its corroboration or refutation. These case studies have to be worked out of all scientific disciplines which provide representative cases of theory change. In this sense I have pointed out which way might be a promising one to pursue for both structural realists and its opponents in order to bring out clear results within the debate.

It should also be clear that by no means my case study of linguistics does provide an ultimate answer to all objections that have been raised against structural realism. Moreover, it is a specific proposal of how to approach epistemic structural realism with a focus on theory change in a new way in order to provide solutions to some of its problems.

By developing my view of pragmatic structural realism, I have highlighted a way out of the Newman objection. The price of sorting out "relevant" structures in each case might be too high for the pure structuralist. Nevertheless, we have seen that it is the only way of avoiding the Newman problem, and hence if we want to make sense of structural realism, such a pragmatic stance has to be taken.

The possibility of ontic structural realism has also been discussed in this dissertation. I concluded that there is indeed a way of accepting a form of ontic structural realism, based on linguistics. For this purpose, I highlighted Harris' notion of kernel sentence as the most important entity in his whole theory of syntax. It is, however, an open question in which way ontic structural realism should be accepted as a general claim, and there is a need for case studies in different disciplines. My result of ontic structural realism in linguistics has to be restricted to Harris' syntactic theory, and should not be understood for the whole of linguistics.

In chapter 7, I have answered some objections that have been made to structural realism, and I have argued that by adopting my version of structural realism, there is a way to avoid the problem of underdetermination.

Finally, in chapter 8 I argued that by modelling structural realism within the framework of the structuralist meta-theory, one is driven to the question of neutralism and, more specifically, to the question if the structuralist meta-theory is in the end neutral to questions of scientific and of structural realism, as some of its adherents claim. My form of neutralism, which I called neutral neutralism tries to incorporate the best of Carnap's proposals and challenges both scientific realism and anti-realism in their views.

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